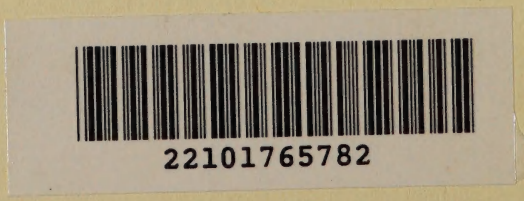

THE

# BRITISH AND FOREIGN

# MEDICAL REVIEW

OR

## QUARTERLY JOURNAL

OF

## PRACTICAL MEDICINE AND SURGERY

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EDITED BY

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# MEDICAL REVIEW,

FOR JULY, 1841.



## PART FIRST.

## Analytical and Critical Reviews.

### Art. I.

*Principes généraux de Statistique Médicale, ou Développement des Règles qui doivent présider à son emploi.* Par Jules Gavarret, Ancien Elève de l'Ecole polytechnique.—*Paris*, 1840. 8vo, pp. 312.

*General Principles of Medical Statistics, or a Development of the Rules which ought to preside over the use of Numbers in Medical Investigations.* By Jules Gavarret, formerly a pupil of the Polytechnic School.—*Paris*, 1840.

Medicine is both a science and an art, a collection of principles on the one hand, and of practical applications of those principles to individual cases on the other; as a science most imperfect, as an art most difficult. Originally medicine, like almost every other branch of human knowledge, was a mere art; that is to say, it possessed no rules, but those who practised it did in one case that which they had seen or known to be useful in an individual case which resembled it. Medicine ceased to be an art, and began to wear the form of a science, when principles took the place of facts; in other words, when men began to collect, arrange, and analyze individual and like instances, and to express that which was common to them all in the form of propositions or aphorisms, which might serve for instruction to the student and guides to the practitioner. In this sense of the term, Hippocrates was the father of physic—of physic not as an art but as a science, and those only deserve the name of successful cultivators of the science who deduce principles from the facts collected by themselves or others; he who deals merely with individual instances, whether as the groundwork of his opinions or the guides of his practice, deserves no higher praise than that awarded to the successful practitioner of an *art*.

The art of medicine, however, which preceded the science differed widely from the art which sprang from the science: the one, dealing only with individual cases, was mere empiricism; the other, establishing prin-

ciples, and reapplying those principles to cases similar to those out of which they were originally formed, is practical science. The art of medicine then, as it now exists, is the offspring of the science, and as such must look for all its improvement to the bounty of its parent. The time is long gone by when men could regard individual facts in any other light than as suggestions to new enquiries, hints for practice in the absence of distinct rules, and solitary materials for the formation of theories.

Keeping steadily in view the distinction thus laid down between the science and the art of medicine, and regarding the science as the true and only source of the art, let us confine ourselves at present to an examination of the science. Here we encounter the necessity for a more precise definition of the term *science of medicine* than that which we have incidentally given in distinguishing the science from the art. In a certain general sense of the term, the science of medicine may be said to consist of an assemblage of auxiliary sciences bearing either directly or remotely on the knowledge and cure of diseases, of those sciences which the student of medicine is required to know before he can commence the practice of his profession. The following scheme presents these sciences at one view, and enables us to explain the meaning which we propose to attach to the term science of medicine.

1. Anatomy (descriptive and structural), comparative anatomy, morbid anatomy; or the sciences of healthy and diseased structure.

2 Botany (with certain other parts of natural history), chemistry, materia medica; or the sciences descriptive of the materials used in the cure of disease, including the modes of preparing, preserving, and compounding them.

3. Physiology, pathology; or the sciences of healthy and diseased functions.

4. Therapeutics; or the science of healing, including all the knowledge which we acquire by experience or experiment of the virtues of remedies.

5. Medicine, surgery, midwifery, forensic medicine, hygiene; or the practical applications of the knowledge furnished by the foregoing branches of science.

The first two groups consist of sciences of description which owe their origin and improvement to the faithful exercise of the senses on visible and tangible objects, and rarely require the use of induction, or the extensive collection of similar instances which must precede the application of that powerful instrument of discovery. Morbid anatomy, indeed, at first sight appears to form an exception to this statement, but it is only when it is studied in immediate connexion with diseased function. The third and fourth groups (including physiology, pathology, and therapeutics,) bear directly on the investigation and cure of disease, a knowledge of healthy function being an essential preliminary to an investigation of disease, an acquaintance with disordered function being equally essential to its identification, and a knowledge of the virtues of remedies to its cure, whilst morbid anatomy studied in immediate connexion with pathology, instructs us as to its cause. These four sciences then, physiology, pathology, therapeutics, and morbid anatomy, brought to bear on the study and cure of diseases, constitute what we have chosen to term the science of medicine. This definition is adopted, not for its accuracy, but for its convenience, as it brings together all those sciences

which stand most in need of improved methods of investigation, and thus serves to make our enquiry more definite. Surgery and midwifery are not excluded from this definition, at least only such parts of them as are merely mechanical; and forensic medicine and hygiene, in as far as they require similar methods of investigation, may benefit indirectly by our enquiries.

We now return to the assertion with which we set out, that medicine considered as a science is most imperfect. Those who are about to enter upon its studies should know this; those who have completed them need scarcely to be reminded of a fact which every day's experience must impress more and more strongly on their minds. Now to what cause or combination of causes are we to attribute the acknowledged imperfection of medicine as a science, and its consequent difficulty as an art? Is that imperfection necessary or accidental? In other words, is it inherent in the subject itself, or does it depend upon want of sufficient materials, want of industry in its cultivators, or on some defect in our methods of investigation? These questions are of sufficient importance to merit a detailed examination. The only way to answer them satisfactorily is to compare medicine with other branches of human knowledge, and especially with those which have attained to the greatest degree of perfection.

There are certain "primary existences and relations," to use the words of Herschel, "which we cannot even conceive not to be, such as space, time, number, order, &c." Not only can we not conceive them not to exist, but all men who think about them must have very nearly the same notion concerning them, though they might find some difficulty in giving a satisfactory definition of them. Now the measure of space and time and indeed of quantity and magnitude in general, is number, and number has the same meaning for all mankind, and is the only thing about which difference of opinion is impossible. As, moreover, numbers are altogether independent of the things counted, and admit of being represented by symbols which are free from the errors of ordinary language, it is obvious that we can reason upon numbers as we can reason upon nothing else. The science of arithmetic, and of algebra (which is merely a more general and powerful arithmetic) being free from the errors of sense on the one hand, and the errors of language on the other, is as far as it goes a perfect science. Geometry too, and the whole circle of what are called the pure mathematics, being sciences of quantity, and therefore, in a certain sense, of number, partake of the absolute certainty of the sciences of arithmetic and algebra. Arithmetic and algebra deal with number without reference to the nature of the things counted, and they are busied in preparing instruments of calculation, as delicate as powerful, for the service of all science. The geometrical sciences resemble arithmetic and algebra in treating of relations of matter not less simple than that of number, such as magnitude, distance, and relative position, carefully excluding all reference to the material of which the things measured consist, and applying at every step the instruments of calculation furnished by the sciences of number. By this union of the numerical and geometrical sciences, fresh instruments of calculation are prepared ready to be applied to a thousand purposes, whether scientific or practical.

The first and simplest use to which these instruments of calculation were put was in the science of astronomy. This science is *par excellence* a science of numerical relations. Magnitude, distance, position, motion, and time are its elements, all of which admit of being expressed by numbers. Its facts are the simplest observations made by the eye, the most certain of our senses, aided by the telescope, the most complete instrument of man's invention, and by other mechanical contrivances remarkable for their accuracy no less than for their ingenuity. Matter enters no otherwise into the calculations of the astronomer than as it has bulk or distance, or relative position. His observations made, he follows out his calculations at his leisure in his study, representing his suns by points, and his distances by lines, sketching out his schemes of an universe on paper, following the track of a planet with his pen, foretelling future events as if he had himself planned them, and awaiting with tranquil confidence the fulfilment of his predictions. Calculation is the secret of the perfection of his science; it is this which gives it all its certainty; it is this which places it at the head of all other sciences, and makes it the model of a perfection which all of them strive to imitate. Here then we have the example of a science of *observation* scarcely inferior in certainty to the pure mathematics themselves, the work of unassisted reason.

But there are other properties of matter almost as simple as those which we have already specified, which admit of the application of the same instruments of calculation, and form the materials of sciences scarcely less perfect than astronomy itself. Such are weight, or to speak more correctly gravitation, and motion, of which the measures are distance and time. The application of calculation to these properties of matter constitutes the sciences of statics and dynamics, hydrostatics, and hydrodynamics—of matter in its two states of solid and liquid, at rest and in motion. In the case of dynamics, force is measured by motion, and motion by the distance traversed by the moving body in a given time. Now the distance may be represented by a straight line, and the time by a symbol, and the direction of the motion by an angle, so that here, as in the case of astronomy, the results of observation or experiment may be worked out in the closet, expressed in figures or traced with the pen. As the amount and effect of a single force may be represented by a straight line, so may the results of two forces acting on the same body be presented to the eye by curves. It is thus that the philosopher with his pen in his hand, and shut up in his closet, reasons upon lines and angles and triangles, and works with symbols and figures, until he succeeds in rearing sciences as true as they are practical, embracing with equal ease the smallest and the grandest objects, now tracing out the motion of an atom, and now following the planets in their course.

Optics, another science in which calculation plays an important part, has, like those just mentioned, attained a high degree of perfection. Here, too, the rays of light admit of being represented by lines, and their direction by angles; and symbols and numbers are employed as measures of quantity. But not to multiply examples, (for our space will not allow us to extend this introductory part of our subject), suffice it to observe that all sciences which deal with those simple relations and properties of matter which admit of being represented by lines or figures, or symbols,

and are in their very nature numerical, may attain to very high degrees of perfection.

It will be observed that all the sciences of which mention has been made make choice of one or two simple relations and properties of matter, and apply to them the subtle and powerful instruments of calculation already prepared by abstract reasoning; and that the observations or experiments which form the staple of those sciences are of the very simplest kind. Their certainty and perfection bear an exact proportion to the accuracy of the observations or experiments which they employ, and the extent to which they avail themselves of calculation.

It must not be supposed that even in the sciences of which mention has just been made all is certainty, and that no fallacies of any sort exist. Out of the pale of the pure mathematics there is no absolute certainty; the most perfect of the mixed mathematical sciences has its probabilities. Thus, to take astronomy as an example, wherever the senses are employed there may be fallacy, wherever instruments are used there must be error, and though it may appear a task of no great difficulty to ascertain the precise position of an object, or the relative position of two objects with regard to each other, experience shows that this cannot be effected with perfect precision. How, then, does the astronomer rectify the errors of his observations? Why, by multiplying them. For instance, he wishes to determine the precise position of two stars, but his instruments are not perfect nor his senses sufficiently delicate; and he finds that one observation makes the distance between the two objects too great, and another makes it too small, and a third and a fourth differ from both of them and from each other; and he may make hundreds of observations without finding any two precisely alike. What then does he do? He has recourse to the NUMERICAL METHOD—to that method about which medical men are still doubting and arguing, as if it could admit of any question, whether it ought or ought not to be applied. The astronomer takes the mean of all the observations which he has been able to procure, and these often amount to many hundreds or thousands, and adopts that mean as the nearest possible approach to the true number required. Here then the medical man has the example of a science remarkable for the perfection which it has attained, owing that perfection mainly to the use of instruments of calculation of absolute certainty and infinite power; but nevertheless employing the very method which he in his ignorance and inexperience is tempted to reject. Ought not this fact alone to remove his prejudices? Ought not the high authority of the most perfect science existing to weigh with him more strongly than a thousand arguments? It was by this very method on which the medical man places so slight a value that Laplace made some of his greatest discoveries.

Descending from the high ground occupied by these favoured sciences, and passing hastily by those of heat and electricity, which admit of the application of numbers, but await a more complete development, we encounter sciences which still make extensive use of calculation, but own a closer dependence on the exercise of the senses. Amongst these chemistry holds a prominent place, and deserves the attntion of the medical man as forming an essential part of his education. The properties of matter which are examined by the chemist are much more numerous

than those which engage the attention of the astronomer or the student of the statical and dynamical sciences. Availing himself of the labours of the geologist, mineralogist, botanist, and physiologist, in bringing together the materials on which he is to operate, he submits to a minute and careful analysis all the material substances which the earth affords, resolves them into their simplest elements, and tests where possible the accuracy of his analysis by the success of his synthesis. In the prosecution of his task he receives powerful assistance from agents which he has learned to create at will: fire and the several electric fluids become his ready instruments in subduing the stubborn matter from which they themselves were generated, and one form of matter aids him in bringing another under his control. Furnished in this manner with powerful assistance in the prosecution of his enquiries, he provides himself with instruments wherewith to measure the intensity of the several influences by which he surrounds himself. The barometer, the thermometer, the pyrometer, the hygrometer, and the balance give minute accuracy to all his results, and express those results in numbers. Figures, indeed, pervade the whole science of chemistry; all its statements wear a numerical form, and its theory of definite proportions, like the astronomer's theory of gravitation, is a numerical theory. It was this numerical theory which converted the art of chemistry into a science; it is this which lends it its chief attraction as a study; it is this which gives precision to all its practical operations.

In the case of chemistry, as in that of astronomy, the instruments which are employed are not, and cannot be, perfect; and sources of error may mingle with the most careful analysis; hence, as the astronomer multiplies his observations, so does the chemist his experiments, and both alike adopt the numerical method as a means of ensuring the accuracy which they prize so highly. The chemist owes all the power which he gains over matter, and all the knowledge which he accumulates concerning it, to his entire command of the things upon which, and the instruments with which he operates: the one are passive in his hands, the other obedient to his will. The perfection of his science springs from calculation, the certainty of his art from the power which he has of making the matter he employs identical with that of which he has already ascertained the properties. The element which he has once procured in its purity by his analysis, he can use in the same purity in his synthesis. The chemist triumphs over unorganized matter, decomposing and recomposing it at will, but organized matter still baffles all his attempts to reconstruct it, however perfectly he may have succeeded in resolving it into its original elements.* Here then we get the first glimpse of an essential distinction between the world of unorganized and the world of organized matter.

Chemistry investigates one order of properties only with which matter is endowed—its atomic affinities; but these are by no means the only ones which man is interested in understanding. The mechanical properties and uses of matter are in some respects still more important. The instruments which he makes use of in his scientific enquiries, and the materials which he employs in the arts of life are wrought out of the va-

* Some few of the excretions form an exception to this statement.

rious forms of matter which surround him, and he is deeply interested in gaining an accurate knowledge of their mechanical properties. This knowledge can be obtained only by repeated experiment. If every form of matter which goes by the same name had precisely the same properties, and could be identified without difficulty, one careful experiment would suffice to teach us all we want to know concerning it. But this is far from being the case. The rough materials which man finds ready to his hand, and those of which he is in a certain sense the maker, resemble each other without being absolutely identical. To ascertain the mechanical properties of the wood, or stone, or iron, which he uses in his buildings or his machines, he must make not one but many experiments, and must take the mean of all as the nearest possible approximation to the true value of which he is in search. So that here again we have the application of that numerical method, which we have already seen doing good service in the hands of the astronomer and the chemist.

All that we know then of those sciences which have attained the highest degrees of theoretical perfection and practical exactness convinces us that they owe these great advantages to calculation. All scientific experience points to this as the grand secret of their superiority, the source both of their certainty and their power. Calculation, as has been well observed, is the very "soul of science," the universal measure of all our knowledge, the sure guide to all its practical applications. Astronomy and chemistry, the one a science of observation, the other of experiment, owe the perfection they have reached mainly to this cause. A numerical theory embraces and combines all their scattered facts, and presides over all their operations; the instruments which they employ give their indications in the language of numbers; and the errors to which they are liable are corrected by a numerical method. But of these two sciences there can be no doubt which is the most perfect. Astronomy holds the first place, because it has to do with simpler relations of matter, employs fewer instruments in its service, depends less upon the uncertain exercise of the senses, and possesses a more comprehensive theory. All that has now been stated, then, leads to the conclusion that the certainty of a science is mainly determined by the extent to which it admits of the application of numbers.

The foregoing observations apply merely to those sciences which have to do with unorganized matter, and its relations and properties; and though they point to the principal source of their perfection and power, they do not reveal the whole secret of their superiority. Taking the practical applications of science as the test of its perfection,—and we know no other or better test,—we at once discover a sufficient cause of the acknowledged superiority of the sciences which deal with unorganized matter over those which have living beings for their study and object. Astronomy, as has been already stated, is more perfect than chemistry, because it deals not with *properties*, but with *relations* of matter. Among those sciences which deal with properties of matter, that must needs be most perfect which has to do with matter which either is, or may be made to be, identical with all other matter bearing the same name. This is the peculiar privilege of the chemist. He can procure several forms of matter in a state of complete purity, and having once examined all their properties, and given to each an appropriate name, he can reproduce

them at will, combine them at pleasure, and foretell the results which will take place with perfect ease and certainty. Not so, however, with the mechanic or engineer. He uses materials which, bearing the same name, resemble each other without being positively identical; the several fragments of wood, and stone, and iron, which he employs in his fabrics, are not in every respect the same, though they pass by the same name, and are sawn from the same tree, or hewn from the same quarry, or smelted in the same furnace. If availing himself of experiments on the strength of any one specimen of these materials, or even of an average of several trials, he were to construct his fabrics of merely that degree of strength which such previous trials indicated, he would often find his best labours thrown away. To ensure solidity and durability to the works of his hands, he must remember that his materials are not identical with those which bear the same name, and he must be prepared greatly to surpass the limits of the possible variation in their properties.

We have now the date necessary for the solution of our first question, *Are the imperfections of medicine considered as a science inherent in the subject itself?* They are. The physician, unlike the mathematician, is not the creator of his own science; unlike the astronomer, he has no simple relations of matter to deal with; he cannot, like the chemist, make any two things which he examines or uses identical; the objects of his study are more variable than the winds and tides, and the materials with which he works infinitely more difficult to adapt to their uses than the matter which the mechanic or the engineer presses into his service. In all his preliminary studies (with the exception of inorganic chemistry), in all his original enquiries, in all his practical applications, he encounters the varying effects and complicated phenomena of Life. The human frame unites within itself all that is most wonderful in contrivance and most elaborate in workmanship. Its structure as much surpasses the most skilful work of man's hands, as its functions do the play of his most ingenious mechanism, and its products the results of his most refined chemistry. That which he knows bears no proportion to that of which he is entirely ignorant; what he sees he sees but darkly; much of what he does he does but guessingly. He seeks for causes, but they elude his search; the vital principle which contains the solution of his difficulties baffles him at every turn; he strives, as it were, to seize it by force, but the violence which he uses defeats itself, and the tortured body dies that it may preserve the secret of its life. Such, and so inscrutable is the body in health; disease surrounds it with new mysteries. Its structure passes through strange transformations, its functions undergo wonderful changes, a new chemistry presides over its secretions, and new principles seem to pervade its every part. Exposed from without to a thousand varying influences; subject within to innumerable changes; governed by a subtle principle which pervades every part, but seems to have no single centre of action; the tenement and instrument of a mind which it both obeys and governs; the human body forms, beyond all comparison, the most difficult, the most complicated study which offers itself to our choice.

Imperfect our knowledge of such a structure is, and must ever be; if we could confine our enquiries to a single human being, or even if every human being were in all its parts and all its functions, the counterpart

of every other; if external influences produced the same effects on all, and internal changes followed the same march in all; even then we could scarcely hope successfully to fathom so many mysteries, and unravel such intricate combinations. But so far from one human body being identical with or even similar to every other, each differs from another, in outward form, in inward structure; in health, in disease; in the degree of influence which external things exert upon it; in the effects of food and remedies. The food which nourishes one man shall act as a poison on another, the remedy which produces one effect in one case shall be powerless in a second, and in a third shall have an effect the direct reverse of that which it usually produces. But passing by these idiosyncrasies, as being of rare occurrence, and therefore comparatively unimportant, we encounter in human beings themselves, and in the several parts and functions of their bodies, differences in degree not less extraordinary than those differences in kind. If, for instance, we compare two persons of the same age and sex, both in the enjoyment of what we term perfect health, we shall find that they differ widely in size, in stature, in strength, in complexion, and in feature; their minds, too, as far as we have the means of judging of them, are as different as their bodies. If from the general appearance we turn to the individual functions performed by the several parts of their frames, we discover differences in degree, if possible, still more extraordinary. Taking as an example the function of the circulation, as that for which we have the most exact measure, the pulse of the one shall beat fifty times in a minute, that of the other nearly twice as often, the respiration shall vary within as wide limits, and differences scarcely less marked shall be discovered in all the other functions. Suppose the same parties seized with the same disease, can we doubt that they would exhibit differences as strongly marked in their symptoms; that they might require corresponding differences in treatment; that the same treatment might cure the one which proved fatal to the other; or that if the disease proved fatal in both instances, we should discover some striking structural changes in the one, not present in the other? We should, moreover, find the original structure of the one in every part of the frame different from that of the other.

Such being the intricacies of the framework, and of the functions of the body in health and disease, let us next enquire what means we have of unravelling them. None but the diligent exercise of the senses, and that not ignorantly, but knowing that which has been done by others; not passively, but actively; not carelessly, but with a plan; not aimlessly, but with an object. Time was when men thought to make discoveries by reasoning upon abstractions, and playing with words; imagining that because reason could count and measure, she could almost dispense with the aid of the senses, and find out what nature did by merely speculating upon what she ought to do. Happily for the cause of science, men have been taught the right use of reason, and the true value of observation and experiment. The method which Hippocrates practised and Bacon enforced has become with us a habit. "*Homo, naturæ minister et interpres, tantum facit et intelligit quantum de naturæ ordine re vel mente observaverit, nec amplius scit aut potest.*" These words describe the true relation in which man stands to nature, and trace out the eternal limits

of his knowledge and his power. The physician describes the source of his knowledge in five words, "Ars medica tota in observationibus." But the term observation is here used in its true and not in its vulgar meaning; not as the mere passive exercise of the senses, but as the union of thought and perception; of thought electing an object, maturing a plan, guarding against every source of error, inventing instruments, improving methods, arranging and classifying the facts collected, and lastly, submitting them to analysis. The simple employment of the senses is not observation, nor is the frequent exercise of them experience: it is, in the true sense of these terms, that the one is the parent of the science of medicine, and the other of the art.

Observation, then, in this its highest sense,—observation invested with the power of experiment,—is the source of all the knowledge which we have obtained or can hope to obtain of the human body in health or disease. But observation, with a few rare and doubtful exceptions, is the source of all other knowledge. Why then are improved methods of investigation more especially necessary to the physician? Because medicine is, beyond all comparison, more difficult than any other science; because it is, in all its parts, a science of observation; and because the objects which it embraces are infinitely more variable and more complex than any others which man can contemplate. The only science which admits of any comparison with it in this respect is Meteorology; but even this, imperfect as it is, and much as it depends upon observation, does not present a tithe of the complexity of medicine. The most compound meteorological changes are brought about by comparatively few elements, and these individually admit of the application of accurate instruments. The thermometer, the barometer, the hygrometer, the electrometer, and the anemometer, (with the exception of the last, instruments of very accurate construction,) apply a numerical measure to the principal elements of all atmospheric changes, and yield assistance far more powerful than any which the physician can ever hope to obtain. With few exceptions the elements which make up the complex phenomena of health and disease admit of no exact measure, and must be described in the inexact and imperfect language of the senses. When the physician has done all he possibly can with the few instruments which he possesses, he has done little towards an exact description of a disease. He can count the pulse, but its frequency is only one measure of the circulation; he may, in like manner, register the number of respirations; but here again he has but one comparatively unimportant indication of the state of the respiratory function; he may ascertain the temperature of the body, and weigh and test its excretions, but after he has done all this, he has only begun his description of the disease. Then again its causes, its consequences, and the effects of the teatment adopted are to be carefully enquired into, and every day's progress to be recorded with a minuteness which fatigues and disheartens the observer. And yet all these things are to be attended to, and all these things are varying every day and every hour. Compare with this complication, this almost inextricable confusion, what science we will, we shall find none half so difficult as this; none which stands so much in need of every aid which experience can suggest, or ingenuity invent.

We might, without much difficulty, have extended the foregoing remarks, for which we are chiefly indebted to the short essay of Dr. Guy,* so as to include other causes of the imperfection of medical science, and the comparative superiority of certain other branches of human knowledge. As our space will not allow us to extend our enquiries further, we shall content ourselves with throwing into the form of short propositions some of the principal causes of the advantages which the more favoured sciences possess, and of the necessary disadvantages under which medicine is placed.

1. The only sciences which are necessarily certain are the arithmetical and geometrical sciences; of which the object is to furnish "instruments of calculation," of infinite power and absolute certainty for the service of all the other sciences.

2. The extent to which these instruments of calculation can be applied to the objects of sense is the true measure of the perfection of the sciences.

3. The objects of sense are either *relations* or *properties* of matter: and the relations of matter being more simple than its properties, and at the same time more easily submitted to measurement, the sciences which deal with the relations of matter are more perfect than those which have to do with its properties.

4. Of the sciences which deal with *relations* of matter, those are the most perfect which treat of the simplest relations; in like manner, of the sciences which have to do with *properties* of matter, those are the most perfect which treat of the simplest properties.

5. Those sciences are the most perfect which deal with the simplest relations or properties of matter, admit of the most extensive use of calculation, employ the most perfect instruments, and possess the most comprehensive numerical theory.

6. Medicine is necessarily a most imperfect science, because it treats of the most complex properties of matter in its most complicated form, (that is to say, organized;) because it does not admit of the use of the most perfect instruments of calculation; because it possesses very few accurate instruments compared to the great number of objects to be examined; and because it has no numerical theory.

7. Lastly. Medicine considered either as an art or a science is and must continue to be imperfect, because the several objects of its study, though they bear the same name, neither are nor can be made counterparts of each other.

Medicine, therefore, as compared with other branches of human knowledge, is, and must necessarily be, imperfect; but though this be admitted to the fullest extent, it does not follow that it allows of no improvement. On the contrary, it may be safely affirmed that there is no science existing which admits of so much improvement. If this be true, in what direction, it may be asked, shall we look for the means of its advancement? What are the remediable defects under which it la-

* On the Value of the Numerical Method as applied to Science, but especially to Physiology and Medicine. By William Augustus Guy, M.B., Cantab., Professor of Forensic Medicine, King's College, London. Reprinted from the Journal of the Statistical Society of London. 1839.

bours? what the impediments to its progress which we may reasonably hope to see removed?

We have already stated that the foundation of medical science must be laid in observation, in the sense in which we have employed that term. But the foundation must be much broader and deeper than it is, before we can hope to see a superstructure reared upon it worthy of the name of a science. And yet at first sight there would seem to be no want of facts. Our journals, our reviews, our monographs teem with facts enough to construct a dozen sciences; our press is most prolific in single cases, and small groups of observations, nor is it altogether barren of collections on a larger scale. But these are as nothing compared to the wants of so vast a subject. We not only want many facts, but we want many *comparable* facts. Putting aside our false facts as worse than useless, our imperfect facts as deceptive, our unmeaning facts as impertinent, and our wonderful histories as the mere curiosities of medical literature, how few will remain which can be safely employed in the building up of a science of medicine. What we want then, in the first place, is a greater number of comparable facts. These facts must be formed into groups, these groups must admit of comparison with other groups; that which is common to all the facts must be expressed in language at once concise and intelligible, in language which may, like the single facts themselves, admit of strict comparison. There is no language which answers to this description but the language of numbers, and the language of numbers is the language of science. The object of the foregoing remarks has been to prove this, and to place medicine under the obligation of listening to stronger arguments than those of reason, the arguments of example and authority. We are advocates then of the *numerical method*, not as a method adapted to the use of this or that science, but as a universal method indispensably necessary to the advancement and improvement of all the sciences of observation and experiment.

The term numerical method is here used in preference to the word statistics. Properly speaking, statistics means the science of states, (from the German *staat*,)* and it is nearly synonymous with the terms "political science," "political economy," "social science." The name owes its origin to Achenwal, professor of history in Göttingen, who published an historical work in 1749, in which the term *scientia statistica* occurs for the first time. The use of numbers as a means of comparison in this work of Achenwal led to the strange mistake of regarding their employment as a new method, and after a time every application of numbers in the service of the sciences of observation was dignified with the sounding title of statistics; and we have at last become as familiar with the term medical statistics as with the phrase medical science. We are interested in pointing out this mistaken use of the term statistics because, as we have already stated, we wish to show that so far from the employment of numbers in medical investigations deserving to be characterized by a new word, or looked upon as an innovation, their non-employment till a recent period proves that medicine has profited little by the ex-

* Dufau, in his Traité de Statistique, derives this term from the Latin status.

ample of the more perfect sciences, and gives us good ground for hope that improved methods of investigation may do much to retrieve its character, confer upon it a greater degree of certainty, and enlarge the sphere of its practical usefulness.

The numerical method is sometimes erroneously regarded as a mere substitution of figures for words. Against this mistake Gavarret strongly protests, and with good reason, though the mere substitution of figures for words is a great improvement in our scientific methods, seeing that figures admit of strict comparison which words do not. "The *sometimes* of the cautious is the *often* of the sanguine, the *always* of the empiric, and the *never* of the sceptic; but the numbers 1, 10, 100, 1000, have but one meaning for all mankind." If this sentence embodied all the advantage to be expected from the substitution of figures for words, it would furnish strong reasons for the exclusive use of figures; but the numerical method, as we have just stated, is something more than this. It is not merely a language, but a science. Just as the pure mathematics furnish the instruments of calculation for the service of those sciences which deal with fixed and certain quantities and measures, so does the numerical method supply instruments of calculation for varying quantities, and for events brought about by many conjoined causes. We have already spoken of its employment by the astronomer, the mechanic, and the chemist, for the purpose of correcting the errors of observation and experiment; we have now to speak of other and higher benefits which it confers on the science of the physician.

The science of medicine is conversant with more than one order of facts. For the most part it has to do with what may be termed compound facts; that is to say, with large groups of circumstances. Such for instance are diseases traced through their entire course, and faithfully described in all their symptoms. But these compound facts admit of being separated into their elements, or into the simple facts of which they consist. Particular symptoms of disease, for instance, may become the subjects of separate consideration and enquiry, or our attention may be confined to the result of the treatment which we employ; in other words, to the event of the disease. Single symptoms, or isolated facts or questions on the one hand, and events on the other, are the proper objects of the numerical method; compound facts require the application of a different method first pointed out by Lord Bacon, and subsequently matured into a uniform plan by the late Dr. Todd, of Brighton.* The first order of facts are the subjects of our present enquiry; the second will be examined on a future occasion.

Symptoms of diseases, or particular facts connected with them, may be studied either as events of more or less frequent occurrence, or as circumstances varying in intensity in different cases, the object being to determine the average degree of that intensity. Considered merely as events, without regard to the degree in which they are developed, the symptoms of disease resemble other events, and the same rules which apply to events in general apply to these in particular. Regarded in this light, therefore, the individual symptoms or circumstances of diseases

* The Book of Analysis, or a new Method of Experience. London, 1831, 8vo.

may be discussed when we come to speak of events in general. But we shall first say a few words on symptoms regarded as of variable intensity.

In order that our knowledge of symptoms in respect of intensity may form a part of the science of medicine it must be complete and accurate; embracing the rule in all its degrees, and the exceptions in all their variety. If the symptom consist in an entire change of some healthy function, that healthy function must be accurately examined, and its healthy state must be our standard of comparison. If, on the other hand, it be something entirely foreign to the healthy state of the body, it must be examined alone, but with equal care. In any case, if it be such as not only to exist in varying degrees, but to admit of being expressed by figures, all the observations which we make with respect to it must wear a numerical shape. For instance, the frequency of the pulse and respiration, the temperature, the quantities of some of the excretions, especially the urine, and the specific gravity of that liquid, admitting of being expressed by figures, ought never to be stated in any other terms, and that our knowledge may be accurate, it must extend to a great number of cases and embrace every probable degree of intensity and every exception to the rule; and the results must be stated in the same accurate language, the mean as well as the extreme values being in every case expressed. As it is no part of our present object to discuss the practical application of the knowledge thus collected, we shall not attempt to show the use to which both these values may be put in the actual practice of our profession; suffice it to observe that too little attention seems to have been paid to the extreme values in the observations hitherto collected. The number of facts which it may be necessary to collect in any particular instance, in order that we may possess an accurate mean and the real extremes, is not easily determined. The simplest rule appears to be "to divide the whole number of observations into groups of equal size, and compare them the one with the other; if the average value of each group is the same, we may safely conclude that we have arrived at the true mean: if not, we must increase the number of our observations, and the size of our groups, till the desired equality is obtained. For instance, one hundred observations having been collected, are divided into four parts, containing twenty-five observations each; if, on comparing these four parts with one another, we find that they yield the same average result, we have good reason to regard such result as the real average. But if the average results are different, we must divide the one hundred observations into a smaller number of parts, and if necessary, increase the number of our facts. If each collection of fifty or of one hundred observations, as the case may be, yields the same average, we may confidently regard that average as the true one." The same method may be employed for determining the real extremes both in excess and defect.

We now turn to the more interesting and important subject of events brought about by a great number of conjoint causes, all of which may vary within wide limits of intensity in the several instances observed; and where the object is to collect and express numerically the frequency of the events in question.

As an example of such events we will take the alternative of death or recovery, the consequence of a given method of treatment in a given

form of disease. Here the causes which combine to bring about the one or the other of these two events are extremely numerous. Gavarret divides them into five distinct groups, as follows :

1. *Individual conditions.* Age, sex, temperament, constitution, previous diseases, and state of health at the time of the invasion of the disease in question.

2. *Hygienic conditions antecedent to the invasion of the disease.* Profession, social position, mode of life, ventilation, state of dwelling, kind of nourishment, moral influences.

3. *Hygienic conditions during the treatment.* Healthiness of the place in which the patient is treated, moral influences which act upon him during the course of his disease, and the exactness with which the orders of the medical attendant are followed.

4. *The disease itself.* Nature of the disease, extent and degree of organic lesions, and of the influence which they exert on the economy, period which elapses between the attack of the disease and the commencement of the treatment, and the several complications which may arise in the course of the disease.

5. *Treatment employed.* This head includes not merely the remedy made use of, but the dose administered, and the various auxiliary remedies used to meet occasional symptoms.

It would perhaps be possible to extend this list on the one hand, and to simplify it on the other ; but assuming that it is a tolerably correct representation of the several circumstances which may affect the success of a plan of treatment adopted in any given disease, it must be obvious that, if the disease in question be one of any severity, the event of it may be greatly influenced by any one of these several circumstances, and as it is altogether impossible to estimate the influence which they would severally exert, we must rest contented with an accurate observation of the result obtained in all the instances. Confining our attention, therefore, to the result, what are the rules which are to guide us in obtaining that result, and in applying our knowledge of it to the prediction of the fate of patients labouring under the same malady ?

The first thing to be attended to is the quality of our observations, the second is their number. It is obvious that we can scarcely err with regard to the mere event, provided we take the most ordinary degree of pains : in this it will be easy to make all our observations strictly comparable. But there is ample room for error in the selection of our cases, and in the circumstances under which we place our patients during the course of their malady. The disease may be loosely defined, and we may confound together cases of a totally different nature, or we may be calling a mere symptom a disease, and thus throw together under the same title things in themselves essentially different. For example, not long ago, a man would have given himself some credit for an industrious observation of cases of ascites, and would have thought that he exercised discrimination enough if he assured himself that all the cases which he reported were bonâ fide cases of that disease ; but his facts would have been almost useless, because they would not have been strictly comparable. What a heterogeneous assortment of observations should we not have had ! Ascites from chronic peritonitis, from diseased kidney, from obstruction to the circulation through the venæ portarum, from diseased

heart, from certain forms of pulmonary disease, from scarlatina, from cold, &c. all grouped together under the same name, all treated alike, and all leading to one of two results, recovery or death. The observations, then, which we group together under the same name, must be strictly comparable facts, in order that our conclusions may have any value.

But not only must the diseases which we call by the same name be the same things, own the same causes, and have the same things in common, (at least such as are most characteristic, as the albuminous urine in cases of ascites from renal disease,) but the circumstances under which our patients are placed must be, as far as possible, the same. They must occupy the same locality, receive the same attention, partake of the same diet, and, as far as practicable, be placed under the same treatment.

Those things, then, which are in the power of the physician must be made the same in all the cases observed, or the result must needs be faulty. But those circumstances over which the physician has control form but a small part of the influences brought to bear on the several subjects of his observations: there still remains a long list of circumstances peculiar to each individual over which the physician can exercise no control, and these must have a powerful effect on the result of his treatment. The existence of such individual peculiarities precludes the possibility of meeting with any strictly comparable facts, and this circumstance has been unjustly advanced as an objection against the application of numbers to medical enquiries. This objection goes on the mistaken supposition, that numbers being made up of units can have no value, unless the units themselves be absolutely identical. The answer to this objection is at once supplied by the fact already stated, that the astronomer is constantly making use of measurements not strictly comparable, and applying to those measurements the numerical method which we are now advocating. Insurance societies of all kinds are constantly making the most important use of events brought about by causes not less variable and entangled than those of which we now speak, and the results which they attain justify their calculations. By parity of reasoning, then, the physician may expect to derive advantage from grouping together observations not absolutely comparable, and applying to them the same calculations which his experience teaches him lead in the hands of others to true and valuable results. With regard, then, to the quality of our facts, it is sufficient that they be strictly comparable in those particulars over which we can exercise control; or, to use the language of our author, we must take care to secure "*l'invariabilité de l'ensemble des causes possibles.*"

Having provided for the sameness of our observations, as far as it lies in our own power to do so, the next question regards the number of observations which ought to be brought together, in order that the result of our treatment may be a true result. This question brings us to the most important part of the work before us. Our author, as has been already stated, places the objects and use of the numerical method much higher than the mere substitution of figures for words. According to him medical statistics, or, as we prefer to call it, the numerical method, is "la théorie des grands nombres," the application of the calculus of probabilities to the science of the physician, "le complément le plus indis-

pensable de la méthode expérimentale." Adopting the sentiment of Laplace, "Le système tout entier des connaissances humaines se rattache à la théorie des probabilités," he proceeds to apply the calculus of probabilities to the solution of some of the most important questions which can engage the attention of the physician. Amongst them the most important is the result of the treatment which he adopts.

The first principle on which our author insists is, that the number of our observations should be considerable. Now this principle will be readily conceded, for every day's experience convinces us that a small number of observations is altogether insufficient to establish any result whatever on a firm basis, whilst on the other hand it shows the sufficiency of very considerable numbers. Every one knows, for instance, that at games of hazard individuals sometimes gain or lose large sums of money in consequence of what is called a run of good or ill luck. Who that has been in the habit of collecting observations does not remember instances in which the chances of two events being equal or very nearly so, one of them has occurred for many times in succession? Such things are constantly happening to the physician; and not to the physician only, for even the mathematician has occasionally encountered these coincidences. What can more strikingly prove the insufficiency of small numbers than the following instance, drawn from the more exact parts of science. The celebrated mathematician Euler, making use of a certain formula, and giving to the abstract quantities contained in it the values 0, 1, 2, 3, &c. in succession, found that all the resulting numbers up to the 40th were *prime* numbers; that is, numbers which have no divisors, or which cannot be divided into any number of equal integral parts, less than the number of units of which they are composed: hence it might be supposed that the law was general. It happened, however, that in the forty-first term the result was a *composite* number. Can any instance be imagined more conclusive than this as to the necessity for establishing our general principles on a sufficient number of cases. The sufficiency of large numbers of facts, on the other hand, is proved by ample experience. The success of the bank in gambling transactions where the capital is large, and of insurance societies where the tables employed are derived from a sufficient number of facts, are conclusive on this point. The uniform results, too, obtained in successive years from large collections of instances of the same kind is well known. Thus, if we compare the number of male and female baptisms registered in England in 1821 and the nine following years, we find that, in these successive years, for 1000 girls baptized there were 1048, 1047, 1047, 1049, 1046, 1047, 1043, 1043, and 1034 boys.* Taking the first eight of these years (for it seems not improbable that the last may be a misprint for 1043) the difference between the highest and the lowest number is less than 6 in 1000, a difference so small as safely to be disregarded in a general result.

Assuming, then, that the necessity for employing large numbers of comparable facts in forming general conclusions will be readily conceded, the next question which arises is, how many facts will be sufficient to establish such a general result as the efficacy or inefficacy of a given plan of treatment in a given disease? Our readers must not be dis-

* De Morgan on Probability, p. 120.

heartened when they are told, that even the most industrious observers of the present day have fallen very far short of the strict requirements of the numerical method, and the indications afforded by the calculus of probabilities. M. Louis, the justly celebrated medical statistician, falls under our author's censure for having asserted the slight efficacy of bleeding in pneumonia, erysipelas of the face, and cynanche tonsillaris, on the strength of one hundred cases of the first disease, forty-four of the second, and twenty-three of the third, and he lays it down as an undoubted principle that "every *statistique*, in order to furnish admissible indications, ought to consist of *many hundreds of observations.*" If Louis, to whom the profession is under such obligations for taking the lead in this grand improvement lies open to censure, what shall we say of the majority of his followers, and in what terms shall we speak of those who still persist in foregoing the aid of numbers, and in drawing important conclusions from one or two scattered, and not comparable facts.* To bring together such large numbers of observations is, in the present state of medical science, and with the existing opportunities and industry of medical men, impossible: hence it is most important to discover some means by which we may know the degree of reliance to be placed upon smaller numbers of observations, and within what limits the results derived from such small numbers may be looked upon as entitled to confidence.

After what has been already stated, the insufficiency of small numbers of observation will not be doubted, nor will it be difficult to understand how the results of observation will become more and more worthy of confidence as the number of observations increases, until at length the general principles established by their aid may be as firmly relied on as the most certain truths of the pure mathematics. Few, for instance, will be inclined to withhold their assent from the proposition established by the facts already quoted, that, in this country, the number of male births is to the number of female births as nearly 21 to 20, and they will scarcely consider the evidence on which this fact rests as less satisfactory than the evidence of their own senses and reason that two straight lines cannot inclose a space. But the question still recurs—are there any means by which we can ascertain the degree of reliance to be placed upon any given number of observations, or by which we can determine the limits of error to which they are subject? THE CALCULUS OF PROBABILITIES supplies us with a method, a method which we may employ with confidence for precisely the same reason that we may use numbers with confidence, namely, the success which has attended its use in the hands of the cultivators of the more certain sciences. To develope this method, and to give examples of its application, is the chief object of Gavarret's excellent work, which we strongly recommend to the perusal of all who are interested in the advancement of the *science* of medicine.

It is necessary to take this method for granted, as it can only be understood by those well versed in mathematics, and it must be received on

* It must be borne in mind, that Gavarret's observations refer to events brought about by vast numbers of external and internal causes combined, and not to states of system which depend upon internal causes alone, such as the functions of the body in health and disease. In this case, though our observations must be often repeated, they need not be so numerous.

the same authority on which we receive the other methods employed by the astronomer, namely, the perfection of his science and the success of his predictions. It is asserted, then, on this high authority, that where an event has been *observed* to happen a certain number of times in a given number of cases, the probability of its happening is not represented by the actual number observed, but lies between limits somewhat greater and somewhat less than that number; and that these limits vary with the number of observations, being wider as the observations are few in number, and approaching each other more and more as they increase. The following example, taken from tables furnished by Gavarret, will sufficiently explain our meaning.

Supposing two collections of observations on the efficacy of a certain remedy in a certain disease to be made, one consisting of 300, the other of 1000, and that the number of cures effected is in each case nine tenths of the whole number of cases; that is to say, suppose 270 to recover out of the 300 cases, and 900 out of 1000; then the numbers 270 and 900 do not give the same degree of confidence, but are subject to differing degrees of error, and are only approximations to the truth. The cures in each instance amount to nine tenths of the whole number treated, or to ·90000 in ·100000. Now for the smaller number 300, the limits of possible error will be ·048990 added to ·900000, or ·948990; and ·048990 taken from ·900000, or ·851010; whilst for the larger number 1000, the limits will be ·026833 added to ·900000, or ·926833, and ·026833 taken from ·900000 or ·873167. In other words, when 300 observations only are collected, the results obtained are much wider from the limits which calculation assigns than when 1000 observations are employed. Supposing for instance that a person having observed 300 cases of any disease, states that 270 cures were effected by a certain remedy, and concludes from this fact that 9 out of 10 patients will be cured in future by the same means, we start forward with the objection—"Your facts are not sufficiently numerous to warrant such a statement; it may have been so in your 300 cases, but 300 observations of any sort whatsoever are liable to a certain fallacy, which can be measured by calculation; and on applying this measure we find that you are not justified in foretelling 9 recoveries in 10 in future, but merely some number between 95 and 85 per cent. Even if you had collected 1000 cases and found that $\frac{9}{10}$ of your cases recovered, all that you could assert for the future would be, that some number between 87 and 93 per cent. would get well."

In the Appendix to Gavarret's work some interesting examples are given, illustrative of the insufficiency of small numbers of facts. We will select one only of these.

M. Louis, in his "Recherches sur la Fièvre Typhoïde," has endeavoured to illustrate the treatment of this disease by carefully analysing 140 cases. The results of these cases are as follows:

52 deaths; 88 recoveries: Total 140.

The mortality therefore is $\frac{52}{140}$, or 0·37143. If we take this result as a strict expression of the efficacy of the treatment adopted in this disease, we shall have the following proposition. Under the influence of the treatment adopted the mortality of typhus fever is represented by 37,143 deaths in 100,000 cases, or approximatively 37 deaths in 100 cases.

If now we make use of the calculations already referred to, we shall find that this assertion is subject to considerable correction, and that the mortality, instead of being exactly represented by the results of this small number of cases, may vary within the following limits:

$$0{\cdot}37143 + 0{\cdot}11550 = 0{\cdot}48693, \text{ and}$$
$$0{\cdot}37143 - 0{\cdot}11550 = 0{\cdot}25593.$$

Thus all that we learn from this limited experience is this—that under the treatment adopted the mortality may lie somewhere between 48,693 and 25,593 deaths in 100,000 cases, or approximatively between 49 and 26 per cent. In other words, that on employing precisely the same treatment upon a very large number of patients attacked with typhus fever, we may lose from a quarter to a half of our patients.

For further illustrations of the necessity for the employment of the calculus of probabilities as a corrective of the results deduced from small numbers of facts, we must refer our readers to the original work.*

The calculations of which we now speak have nothing whatever to do with the *nature* of the facts observed, but merely with their number, and hence any objection which may seem to lie against the employment for medical purposes of formulæ supplied by the pure mathematics, will at once disappear. The only authority which need be adduced in support of this application of numbers to observation is that of the successful cultivators of the most perfect sciences, who employ the same method in their accurate investigations, just as they employed the numerical method long before it was used by the medical man.

The questions which we propounded are now answered. The imperfection of medicine as a science is partly due to the inherent difficulty of the subject, and partly to defective methods of investigation. We have not enough facts, and our men of science are not industrious enough to supply the large demands which are made upon them.

The object of the foregoing remarks is to make the more perfect sciences an example to our own. By showing how important a part calculation plays in those sciences which have attained the highest degree of perfection, we think that we have pointed out the true path to the improvement of the science of medicine. The substitution of figures for words, though a step gained, is not all that is required: the numbers which we deal with must be large ones, and where they are not so, we

* For the benefit of such of our medical brethren as are conversant with the mathematics, we subjoin the formula by means of which the limits of error are obtained.

If $m$ represent the number of times that one of two events (call it A) has happened, $n$ the number of times that another event B has happened, and $\mu$ the total number of observations collected, so that $m + n = \mu$; then the number which expresses the observed frequency of the event A, is not the true number, but merely an approximation to it more or less close as the number of observations is greater or less. That number will in any case lie between

$$\frac{m}{\mu} + 2\sqrt{\frac{2 \cdot m \cdot n}{\mu^3}}$$

and

$$\frac{m}{\mu} - 2\sqrt{\frac{2 \cdot m \cdot n}{\mu^3}}$$

or at least, there are 212 chances to one in favour of its being comprised within those limits.

must make allowances for error proportioned to the number of our observations. To make the principles of our science as close approximations to truth as possible, the facts from which those principles are deduced must be very numerous. Where objects are to be examined of variable magnitude, we must make as many observations as shall include all the varieties which nature presents; where events are the subjects of our enquiry we must collect as many instances as shall enable us to find at least very close approximations to the actual frequency of their occurrence. But the units of which our numbers consist must be comparable facts. Where to obtain these facts is the difficulty. How few men possess the opportunity, how very few the inclination to make such large collections of facts! and what reward can they expect for their industry? The *practical* man who delights in single cases, and glories in those who deal with them, will laugh to scorn the plodding collector of facts by the hundred, and confound the principles which he draws from them with the hypotheses of some dreaming enthusiast. He who devotes himself to the science of medicine must expect little sympathy from the mere votary of the art. And though the science is, and must ever be, the parent of the art, the man of science must look for little gratitude from those on whose behalf he labours—the mere practitioners of the art. Where then must he look for his reward, whether of fame or wealth? Something of the one he may gain from posterity, but little of the other from his contemporaries. He must love science then for its own sake, and labour for its advancement without hope of recompense, satisfied if he leaves the most imperfect science of the day somewhat more certain than he found it. In the meantime he must be prepared to sacrifice those advantages on which men set the greatest store, and for which they make the greatest exertions. We might add much more on this topic, but we are warned to bring our remarks to a close.

Once more, then, the SCIENCE OF MEDICINE wants facts—comparable facts—numerous facts: well observed, carefully arranged, minutely classified, and acutely analyzed. Her language must be the language of figures; her test, the calculus of probabilities; her example, the most perfect and exact among the sciences of observation and experiment.

## Art. II.

*Recherches Anatomiques, Pathologiques et Thérapeutiques, sur la Maladie connue sous les noms de Fièvre Typhoïde, Putride, Adynamique, Ataxique, Bilieuse, Muqueuse, Gastro-entérite, Entérite Folliculeuse, Dothinentérie, &c. comparée avec les Maladies aigues les plus ordinaires.* Par P. C. A. Louis, Médecin de l'Hôtel-Dieu, Président perpétuel de la Société Médicale d'Observation de Paris, &c.—*Paris,* 1841. Deuxième Edition, 2 tomes 8vo, pp. 542, 523.

*Anatomical, Pathological, and Therapeutical Researches on the Disease known by the names of Typhoid Fever,* &c. By P. C. A. Louis, Physician to the Hôtel-Dieu, perpetual President of the Medical Society of Observation, &c.—*Paris,* 1841.

Few works have exercised a more important influence on the medical science of their age than that before us, whether we consider it as contributing to dethrone the long reigning (but at the time of its appearance already tottering) dogma of the essentiality of *all* fevers, or as demonstrating the fallacy of the seductive and grasping doctrine of Broussais, or as containing the earliest exhibition of the practical workings of the system of studying the science devised by its gifted author: probably its chiefest merit, because that ensuring the largest amount of practical good, arises out of the characteristic last referred to. Before the production of these volumes it might, on the evidence of the previous history of our science, have been justly doubted whether any pathologist might arise capable of acting uniformly and exclusively on the principle, in the recognition of which the successful cultivation of all sciences of observation is involved, enunciated in the following words: "je sais que la vérité est dans les choses et non dans mon esprit qui les juge; et que moins je mets du mien dans les jugemens que j'en porte, plus je suis sûr d'approcher de la vérité." It was not to be doubted that one day some keen spirit must speculatively admit the applicability of Rousseau's apophthegm to the study of medicine, and recognize its dependence upon the philosophy of Bacon; that this had actually been done, though imperfectly, by others is even strictly true. But to announce and profess the notion that any medical doctrine having pretensions to an origin in sound philosophy, must of necessity be inductive and purely inductive in its nature, was one thing;—to demonstrate the truth of this by practical evidence another. To work upon such notion to the exclusion of all others for years, to overcome the almost unconquerable tendency of the mind to speculate and not to *induce*, to bestow lengthened toil in the discovery of certain results, while others of equal apparent value might, on the old and legitimised system, have been quasi-established by a quiet exercise of the simplest thought, required a degree of exalted zeal, of perfect mental self-control and of enlightened love of practical truth for its own sake, which the tendencies of the past cultivators of our science gave us no right to expect in their successors.

And the present appears an exceedingly appropriate time for the introduction of this work to the British public. There never was in truth a period when all questions relating to the nature, modes of origin and propagation of fever, excited more attention among us than the present,

or when conscientious men more eagerly desired an accurate examination, based upon facts and—as far as this is possible—upon facts *alone*, of the great question of the identity or dissimilarity of the fièvre typhoïde of Paris and the typhus fever of this country. To institute this examination as effectively as we can with the *positive* materials within our reach will, we believe, be to render a service to all interested in the philosophic settlement of such questions. As the essential and necessary basis of the enquiry, we shall set out with an analysis of the work before us, as complete and circumstantial as the nature of this journal will justify. A mere outline of its contents would not suffice for our purpose. Though it is, we are quite aware, very commonly supposed that the leading facts established by M. Louis are at the present day accurately understood among us, the truth is, on the contrary, that evidences of individual ignorance of the essential substance of his doctrine are daily made public; and still, as of old, the idea of acquiring acquaintance with details is, practically speaking, heartily scoffed at.

In addition to the distinction between this and other *ex professo* treatises on fever afforded by its strictly inductive character, there is another peculiarity in its construction, which at once exhibits the comprehensiveness of the views taken of disease by its author, and adds vast weight to—or, more correctly speaking, creates the importance of—the general deductions drawn by him in respect of typhoid fever from his different series of particular facts. It is this. Recognizing the important truth, that in order to establish the real value of any symptom or set of symptoms in connexion with any given disease, it is necessary to ascertain not only the degree of frequency with which it or they may attend that disease, but also the degree to which they are—either singly or in combination, in the same or in a different order—peculiar or proper thereto, he conceived the series of elaborate and minute comparisons instituted in these volumes, on the occasion of each symptom and lesion, between typhoid fever on the one hand, and all other acute diseases collectively on the other. The work thus supplies an analysis of nearly nine hundred cases of acute disease, and should rather be entitled a treatise upon the philosophical pathology of acute disease generally, and of typhoid fever in particular, than a simple monograph of the latter affection.

The work is divided into four parts. In the First of these are reported the histories of eighteen subjects whose death occurred at different periods of typhoid fever, exhibiting the varieties in the condition of the parts implicated, dependent on the greater or less duration of the malady. The Second contains a general description of the anatomical changes observed in all the fatal cases both of typhoid fever and of other acute diseases,—an exposition of the causes of death in both series of cases, and a brief summary of all the morbid alterations described. In the Third part, the symptoms in the fatal cases and in those terminating by recovery of the patient are analyzed; the subjects of "latent" and of "simulated" typhoid fever are examined; the epiphenomenon of intestinal perforation described; and the causes of the disease enquired into. The Fourth division embraces the subject of treatment. Under these different heads some new matter appears in the present edition. The anatomical chapters are enriched with observations on the work of M. Chomel, with contributions from M. Barth, and a minute description,

furnished by MM. Rilliet and Taupin, of the lesions discovered in the bodies of children, (from ætat. two to fifteen, both inclusive,) dying of the typhoid affection. The symptoms occurring in infancy are closely related; the chapters on diagnosis, etiology, and treatment are considerably enlarged.

With the collection of eighteen cases we need detain the reader no further than to recommend them to his notice, as models of what such cases should be, as prototypes of those we should desire ourselves to bring forward (were this possible) either in substantiation or in refutation of the application of the general inferences from them, to the disease existing in these isles. The histories of 32 other fatal cases are interspersed through the work and from 46 of the 50 thus reported the general anatomical description is derived. Of these 46 cases:

| | | | | | |
|---|---|---|---|---|---|
| 10 | terminated fatally | from the | 8th | to the 15th day: | 1st period. |
| 7 | .. | | 16th | .. 20th .. | 2d .. |
| 20 | .. | | 20th | .. 30th .. | 3d .. |
| 9 | .. | | after the | 30th .. | 4th .. |

The lesions of the digestive organs are first examined. The pharynx was the seat of morbid change in 8 cases; of ulceration in 6, of submucous purulent infiltration, or of false membrane deposited on its free surface in 2. In no instance were the follicles of the pharynx (M. Chomel's experience is to the same effect) diseased in the manner of the crypts of the intestine. The ulcerations were not found in any subject dying before the 15th day, and in one case only proving fatal after the 30th. In 70 subjects dying of other acute diseases, exclusive of variola, no single instance of pharyngeal ulceration occurred; a fact which though not sufficing to prove such lesion peculiar, among all acute diseases (except smallpox), to the typhoid affection, yet demonstrating its great diagnostic importance in, and entitling it to the rank of a secondary lesion of, that disease.

Similar ulcerations existed in the œsophagus in seven cases, generally attended with some more or less marked lesion of the mucous membrane of the stomach, in no instance having led to perforation, and observed in such subjects only as died after the 16th day. No such ulceration was discovered in the victims of other acute affections; though extensive destruction of the mucous, with softening and attenuation of the submucous, tunic (a lesion terminating in perforation of the œsophagus in two cases of typhoid fever, more recently observed by M. Barth,) occurred in three instances. The inference drawn in respect of pharyngeal common ulceration, is clearly applicable to the œsophageal also.

It results from the general comparison made by M. Louis, in respect of the mucous membrane of the stomach, that this tissue was in the two orders of fatal cases (typhoid and common acute) affected with the same species of lesions, and in very nearly equal proportions. Thus softening and attenuation were observed in $\frac{1}{5}$ of the typhoid cases and in $\frac{1}{6}$ of those dying of equally acute complaints; ulceration in $\frac{1}{12}$ of the former, and $\frac{1}{24}$ of the latter; simple softening in somewhat less than $\frac{1}{6}$ of the first, and in $\frac{1}{7}$ of the second category of patients; mammillation inversely in about the same ratio; lastly, the mucous membrane presented its normal characters in $\frac{2}{7}$ ($\frac{10}{35}$) of the typhoid subjects, and in $\frac{1}{5}$ ($\frac{7}{35}$) of

the others. Upon these important facts hinges the total subversion of the gastric theory of Broussais; they as clearly demonstrate that the typhoid fever of France is not a *gastro-enteritis* as induction can. They show that it would be just as rational to term pneumonia a *gastro*-pneumonia, because affections of the gastric lining membrane occasionally arise in its course, as to call typhoid fever a *gastro*-enteritis. But further, in consequence of the multiplicity of the diseases, during which such gastric complications are wont to be developed, and of the fact that such development does not take place until a more or less advanced period of the original malady, there follows one of the most legitimately deduced and most important laws of diseased action, thus stated by its discoverer: "In a certain number of cases in which an acute affection, of whatever nature this may be, gives rise to a febrile movement of any duration, the mucous membrane of the stomach becomes, at a variable period of that affection, the seat of an anatomical change proportional in degree to the predisposition of the subject; a change which hastens to a greater or less extent the fatal termination of the case, and may in some instances be the true cause of death."

It is unnecessary to notice particularly the intimate characters of any of the lesions of tissue referred to, with the exception of softening and attenuation attended with destruction. M. Louis, grounding his judgment on the absence of signs of inflammation around the softened part and on the coexistence of softening and attenuation of the submucous tunic—this too without surrounding evidences of inflammation—a phenomenon not observed in undoubted cases of inflammation of mucous tissues, questions its inflammatory character. He is inclined to admit that it may depend in some cases (as Dr. Carswell has shown to be the fact in rabbits) on dissolution by the gastric juice; but demurs to the general application of that distinguished observer's results to the human subject. His argument, drawn from the fact that death occurs occasionally in the human being under circumstances which would in animals infallibly entail dissolution and probably perforation, without any such effect being induced (an argument already very naturally applied by Mr. Taylor with similar intention),* incontrovertibly disproves the justness of such general application. It also shows that in cases wherein the human stomach appears to undergo the species of chemical change in question, there are some subsidiary conditions necessary to its production, not yet understood. M. Louis makes it manifest that these are probably unconnected with the state of the surrounding atmosphere, as such presumed dissolution occurs in the same proportion of cases in hot and cold weather. Mr. Taylor maintains that they are referrible to a morbid state of the gastric juice itself, a notion to which some obvious objections may be raised.

The mucous membrane of the duodenum was affected in fourteen out of twenty-two cases; discoloured in six, grayish in two of these in which death occurred at an advanced period of the affection; studded with one or two small superficial ulcerations in two cases. With the exception of the non-existence of the latter lesion, this portion of the intestinal tract

* Vide Brit. and For. Med. Rev., vol. IX. p. 375. April, 1840.

presented the same species of changes in the typhoid as in the other category of subjects.

Enlargement of the caliber of the small intestine was detected in fourteen out of thirty-nine typhoid subjects, exclusive of those dying from perforation of the ileum; and appears to be more frequent in the earlier periods than in the fourth,—a fact, showing that meteorism (the cause of the enlargement in question) cannot be directly ascribed to ulceration of the bowel. Invagination of the bowel occurred in three cases, without giving rise to any particular symptoms. Ascarides were found in a certain but undetermined number of cases; there was always a greater or less quantity of mucus in the intestine except in one case of perforation; and commonly an abundant supply of fluid bile. An orange tint in the neighbourhood of red spots appeared traceable to exhalation of and subsequent changes undergone by blood.

The colour of the mucous tunic varied with the period at which the disease had proved fatal. It was white in

| | | | | |
|---|---|---|---|---|
| 4 | out of | 10 | subjects of the | 1st series |
| 3 | .. | 7 | .. | 2d |
| 5 | .. | 20 | .. | 3d |
| 1 | .. | 9 | .. | 4th |

in other words the earlier the occurrence of death, the stronger the chance, as might have been anticipated from the facts already disclosed, of finding its hue natural. The converse is true of *gray* discoloration: this was observed in only two subjects among those who succumbed before the twentieth day, in seven of those who perished from the twentieth and thirtieth, and in four of those lingering beyond that period. In a third part of the cases the surface was red; a yellow tinge, evidently from bilious stain, appeared in four instances only. It follows then that the gray hue is almost peculiar to the intestine of subjects dying after the twentieth day; and that this is a transformation of the red, the observed condition of the parts essentially implicated in the disease appears, as we shall presently see, to demonstrate.

The mucous membrane (excluding from this statement that of the patches of Peyer) was of natural consistence in nine out of forty-two subjects, and in a greater proportion of individuals dying after than before the 30th day. It had lost its natural share of consistence to a greater or less degree in all the other cases, and in the following proportions in the different periods:

| | | | | | |
|---|---|---|---|---|---|
| in 7 | of the | 1st series | in 16 | of the | 3d series. |
| 5 | | 2d | 5 | | 4th |

The secondary character of this softening clearly appears from its absence in a number of the subjects whose decease occurred from the 8th to the 15th day of the affection; but its intimate nature is far from being equally manifest. A luminous enquiry into the point, based on observed facts, leads the author to the following conclusion, which we are induced to extract on account of its important bearing on a general question of pathological anatomy:

"Every consideration, consequently, appears in favour of the notion that simple softening of the mucous coat of the small intestine, unaccompanied with thickening or redness, is, at least in part, the result of incipient cadaveric decomposition. I say in part, because the existence of this softening in a cer-

tain number of subjects dying in the cold season of the year, shows that some other cause must also be in action,—whether this be some, as yet unascertained, lesion of the mucous tunic itself, or an altered condition of the liquid and other contents of the bowel."

The agminated glands, patches or glands of Peyer, were more or less diseased in every case, through an extent of the intestine varying from two to eight feet. This lesion presented itself under two distinct aspects, distinguished by the epithets of *soft* and *hard.* To commence with the former: The most advanced changes invariably appeared in the patches nearest the cæcum; a sound patch rarely appeared between two diseased ones, and the transition from the morbid to the natural state was commonly sudden. The patches affected with the least marked disease were but slightly prominent, and of delicate pink hue; their mucous membrane slightly softened; the orifices of the crypts imperceptible. These different characters assumed more marked development in the adjoining patches, the free surface of these appearing finely mammillated, the orifices of the crypts manifest, the corresponding submucous tissue more or less thickened and red. In a still more advanced stage the redness, thickening, and softening of the tissues implicated, had acquired further development; mammillation ceased to be visible, nor could the mucous tunic, as in the earlier stages, be removed from the subjacent tissue, which had itself meanwhile grown thicker and redder than before. Nearer the cæcum ulceration exhibited itself, at first superficial, occurring in a single or in several points, sometimes of very small size, in other instances producing total destruction of the mucous coat of a patch, either through the union of several minute ulcers, or extension of a single one. The ulcerative process successively destroyed the submucous, muscular, and peritoneal tissues, leading to complete perforation in eight out of fifty fatal cases. The form of these ulcerations is not characteristic; it may be round or oval, its edges smooth or denticulated, vertical or bevilled off. These lesions were most prominently marked in subjects dying between the 15th and 30th days; any ulcerations discoverable before that period were generally small, and few in number, or, as was the case in two instances, these were absent altogether,—redness, softening, and thickening constituting the only morbid changes therein. This absence of ulceration was, however, also established in two cases terminating fatally, at so advanced a period as the 25th and 29th days; a very striking exception to the ordinary course of things. The usual appearances discovered after the 30th day indicated a retrograde action in the patches: the strong red hue had given place to one less marked and mixed with gray or blue, while the tissue itself had become firmer, and lost much of its abnormal thickness; at a still later period the edges of the ulcerations were sunken, and their field covered wholly or partially with a shining, polished, and extremely delicate pellicle of serous aspect, and continuous with the submucous tissue beyond the confines of the ulcer. These evidences of cicatrization were never detected before the 37th day of the disease, and always in the immediate vicinity of the cæcum; reparation commenced where the diseased action had first set in.

The *hard* patches were distinguished from those we have described by the condition, not of the mucous but of the submucous tissue. In this

variety of the disease the latter tissue was "transformed" into a homogeneous matter, without apparent organization, of a pale pink or yellowish tint, presenting a dry and shining surface on division. Before ulceration the surface of this variety of patch is plane and uniform; after that change the patches become more or less deeply furrowed and uneven. The portion of abnormal stratum in contact with the mucous membrane is soft and friable; its deeper layers possess the consistence of the lymphatic glands. Thirteen of the 46 subjects examined furnished examples of this variety of patch; in three cases it coexisted with the other. The hard species occurred with much greater proportional frequency among subjects dying from the 8th to the 15th day, than at a later period; and in no instance was it discovered, where death had supervened after the 30th day. The inference M. Louis felt inclined to draw from these facts as to the more fatal character of the form of lesion now under consideration, is supported by the subsequent observation of M. Barth, at the Hôtel-Dieu: this physician found the mean duration of the disease to be only 19 days in 8 cases of the hard, 36 in 19 of the soft, variety. Reasoning upon the invariable coexistence of the lesion of the submucous with that of the mucous tissue under all circumstances, M. Louis concludes that both become affected simultaneously; a consideration sufficient in itself to demonstrate the specific character of the disease, as in ordinary cases of mucous inflammation the subjacent tissue does not, in the majority of instances, participate in the inflammation. The fact that the morbid change of the cellular tissue of the hard patches had in several instances attained a very marked degree of advancement, while that of the investing mucous tunic was only moderately softened, seems to warrant our entertaining the belief, "that in a certain share of cases the alteration of the cellular commences before that of the corresponding mucous membrane." M. Chomel goes further than this, considering himself enabled, by his own observations, to affirm that, in some instances at least, the disease distinctly originates in the tissue beneath the mucous coat. And though the legitimacy of his conclusion is fairly questioned by M. Louis, inasmuch as his premises do not appear to warrant it, still that it is a correct one as applied to other facts is rendered probable by the microscopical investigations of Boehm, on the agminated glands in the "abdominal typhus" of Germany. "The first and evident change," to use this observer's language, "(as Carus has found,) consists in an exudation, which takes place in the submucous tissue . . . Thus the inflammation of the intestine [more correctly of the *mucous membrane of the patches*] is not to be considered as *primary*, but as *consequent* on the distension and irritation of the mucous membrane of the corpuscules covering the exudation . . . The mucous membrane being destroyed, the bottom of the ulcer is formed by the exuded matter, which is dense, lardaceous, hard, often rough, uneven, furrowed, and surrounded by callous margins often of a peculiar yellow colour."* Notwithstanding the counterargument urged by M. Louis to the effect that in one case proving fatal from perforation on the 36th day, the submucous tissue, "though thickened, retained its ordinary white colour," we think it may be accepted, not certainly as a demonstrated fact, but as a very likely

* Vide Brit. and For. Med. Rev., vol. I. p. 524. April, 1836.

one, that the hard form of the disease originates in the submucous tissue.* M. Louis differs from M. Chomel in regarding the lesion of both varieties of patch as identical, in regard of the mucous membrane; nor does he consider the lesion of the latter tunic in the soft patches as essentially gangrenous, though gangrene may supervene as a termination of preceding inflammation. Reasoning upon the presumed gangrenous nature of this lesion, M. Chomel concludes that the hard patches only are susceptible of resolution, but our author correctly observes that the cases, adduced in the work of that observer, as proving the reality of such susceptibility on the part of these patches, are very far from establishing the point.

The solitary glands were more or less diseased in twelve cases, and with one exception, the changes affecting them were most advanced in the vicinity of the cæcum.

From a review of the lesions observed in subjects dying of other acute diseases, it follows that, setting aside the abnormal state of Peyer's glands of which not a single example occurred except as an attendant upon the typhoid affection, the morbid changes in the small intestine were the same in nature, and very closely so in proportional frequency of occurrence, as in those who succumbed to the latter malady. Hence the inference that the law already explained as regulating the development of gastric lesion during protracted febrile action prevails also in regard of the small intestine.

The caliber of the large intestine was enlarged in consequence of meteorism in twenty-two out of thirty-nine cases; in three fourths of these the distension was so considerable as to obstruct the actions of the thoracic and abdominal viscera, and occasionally drive the liver considerably beyond its natural level superiorly. This distension of the intestinal walls, not traceable to any lesion of the mucous membrane, considerably more frequent in subjects dying between the 20th and 30th days, than before or after that period, was attended with thickening of the muscular coat; a fact illustrating the rapidity with which muscular hypertrophy may be established, and sufficing to prove, independently of any other phenomena, that the development of gas had occurred during life. The mucous coat was in four instances studded with *hard* patches of the same character as those of the small intestine, but of much smaller size, their diameter varying from three to four lines, non-friable on the surface, and in one instance only ulcerated superficially. Ulcerations, to the number of two or three in each instance, varying in diameter from four to thirty lines, capable of cicatrization, as ascertained by M. Barth in two cases of death on the 43d and 120th days, presented themselves in fourteen subjects, in the following proportions in each series:

| | | | | | |
|---|---|---|---|---|---|
| 1 | case out of | 10 | of the | 1st | series. |
| 2 | .. | 7 | .. | 2d | .. |
| 9 | .. | 20 | .. | 3d | .. |
| 2 | .. | 9 | .. | 4th | .. |

With the exception of these hard patches the lesions discoverable in the large intestines of subjects dying from all other acute affections were of the same description as in the instance of typhoid patients; nor did the proportion of these, except as regards meteorism and ulceration, mate-

* This must be understood to refer to subjects of fifteen years of age and upwards alone.

rially differ in the two categories: this appears distinctly from the subjoined table:

| | | Deaths. 45 from typhoid fever | 69 from other acute diseases. |
|---|---|---|---|
| Mucous Membrane. | Universally red | 3 | 3 |
| | Partially red | 10 | 12 |
| | Grayish | 9 | 7 |
| | Universally softened | 16 | 22 |
| | Partially softened | 14 | 25 |
| | Ulcerated | 14 | 3* |
| The bowel tympanitic | | 25† | 2 |

The lesions of the abdominal lymphatic system are in this affection marked and characteristic. The mesenteric glands corresponding to the diseased patches were invariably diseased; in the early stages enlarged, red, and softened, at a more advanced period infiltrated or studded with specks of pus. In some individuals whose death had taken place between the 20th and 30th days, the red discoloration had given way to a grayish hue, and the glands lost, to a certain extent, their abnormal enlargement and softening,—direct indications of their being on the return to the normal state. The mesenteric glands corresponding to healthy patches presented changes of the same kind, but less marked, in $\frac{1}{4}$ of the subjects. The mesocolic were the seat, but not in every instance, of an analogous form of lesion; the cervical and gastric occasionally red and enlarged; those of the biliary ducts intensely inflamed in 2 cases. In other acute affections the condition of these parts was extremely different, and from the facts now adduced the following propositions very clearly follow: 1. The lymphatic glands may become diseased through the influence of causes existing in the system generally, independently of the mucous membranes, with which they are in physiological connexion. 2. The latter are frequently diseased without implication of the corresponding glands, although the lesion of the former may have existed for some time before death. 3. Softening and enlargement of the mesenteric glands, when existing to any extent, constitute a lesion proper, among acute diseases, to the typhoid affection, and are of almost as great importance in this point of view as the lesion of Peyer's glands themselves. 4. There is a special tendency to disease of the lymphatic glands generally in the typhoid affection. The importance of the first two propositions springs from the refutation they convey of some favorite dogmata of Broussais.

Of hardly less constant occurrence than the affections of the patches of Peyer and of the mesenteric glands is a peculiar morbid condition of the spleen: in four cases only was this organ healthy, twice in subjects dying between the 20th and 30th days, twice in cases terminating fatally after that period. The organ was ordinarily enlarged and softened, in many instances had attained four or five times its normal size; and was invariably much enlarged when the additional lesion, softening, existed to a considerable amount. Different varieties of discoloration coexisted with these changes. Now, inasmuch as the four exceptional cases occurred among individuals dying after the 20th day, and the organ was

* This is of course exclusive of dysentery.

† The observed number 22 out of 39 is here raised to the ratio of 45.

never found healthy before that period, it becomes exceedingly probable that the spleen is in reality implicated in every case at an early period of the disease, a notion confirmed by the semeiology of the malady. Respecting the nature of these combined lesions,—while he rejects the notion of their being inflammatory on account of, 1st, the invariable absence of pus; 2dly, the fact of the splenic parenchyma being *universally* and *equably* affected throughout, (a singularly uncommon state, if indeed it ever arise, of parenchymata confessedly inflamed;) and 3dly, the invariably sound condition of the investing membrane of the organ,—M. Louis shows it to be impossible, in the existing state of knowledge, to draw any determinate inference.

In thirty-two only of eighty-four individuals dying of other acute diseases was the spleen notably affected, and in these cases the changes it had undergone were much less marked than in typhoid subjects. Particular ages appear to be without influence on the development of this lesion, nor is any connexion traceable between it, the existence of diarrhœa or of disease of the stomach or of the large intestines.

Continuing the examination of the abdominal viscera we come to the liver, an organ softened in 22 of 46 typhoid cases, commonly throughout its entire extent, and at the same time pale and friable,—changes of equally frequent occurrence at all periods of the complaint. And the inferior importance of this lesion, as exhibited in the comparative rarity of its existence, is thrown into stronger relief by the consideration of the circumstances under which it was observed; from these it appears that, though in some measure dependent on some predisposing condition of the solids or liquids, the softening was materially influenced by physical agency, especially by temperature. The changes discovered in the liver of the other category of subjects, resembled in frequency of occurrence and in character those of the typhoid class; a somewhat greater frequency of softening in the latter being the only notable distinction. Curiously enough, emphysema of the hepatic tissue was detected in three instances, coinciding with similar development of gas in the common cellular membrane, while not a single example of the former phenomenon presented itself among the typhoid subjects.* The liver was fatty in two patients, one dying of peritonitis, the other of an indeterminate affection.

The bile was very abundant, fluid, reddish or greenish, in the majority of typhoid cases, and sometimes turbid; the gall-bladder contained pus in three instances, in which its mucous membrane was red and thickened. The same law prevailed here as in regard of the organs generally, in respect of the period at which the most advanced disease exhibited itself; they were more serious when death occurred at an early than at an advanced period. In the victims of other acute affections the lesions of the biliary apparatus were of the same character, but of much less frequent occurrence than in the typhoid class.

Enlargement, softening, and violet-red discoloration of the kidneys existed in a few subjects, most marked in those cut off at an early period. The same modes of lesion were discovered in other subjects, not however with the same frequency or to the same extent. In commenting upon

* That such emphysema sometimes occurs in the typhoid fever of Paris, at least as a post-mortem phenomenon, could scarcely be doubted à priori, and is shown to be the fact by M. Chomel's cases; but this does not diminish the interest of M. Louis' results.

the statements of M. Rayer regarding the development of nephritis in typhoid fever, M. Louis observes that the general law of the greater frequency of secondary inflammatory lesions in that malady suffices to explain the fact, though he does not deny the influence of retention of urine upon its development. This is hardly different from the doctrine laid down by M. Rayer himself, as may be seen by referring to his second volume, or to the review of it in this Journal. (Vol. X. p. 302.)

Two typhoid subjects were manifestly affected with pyelitis; the mucous membrane of the pelvis of the kidney being red, thickened, and bathed in one of them with pus. The only remarkable point in the condition of the bladder, besides its being extremely distended with urine in a fifth of the subjects, was the existence of a minute ulceration in one case. One typhoid subject only had suppurative inflammation of the parotid glands, the affection of these parts was equally rare in other acute diseases; the pancreas did not in any instance present notable deviation from the normal state.

We shall pass more rapidly over the lesions detected in other organs, these having on analysis been demonstrated to be without intimate relation to the phenomena of the disease. The heart was healthy in somewhat more than half the cases, more or less softened in the others, and sometimes to an extreme degree. Under these circumstances its tissue was of livid colour, its walls attenuated and easily torn, its contents a few drops of blood, mingled with air or clotted and non-fibrinous. M. Louis' conclusion as to the cause and mode of production of this softening, agrees precisely with that already explained in respect of the similar change observed in the liver. In almost every instance wherein the heart appeared softened, the internal surface of the aorta was tinged red, and its lining membrane occasionally softened and thickened; a phenomenon produced by imbibition, favoured by some peculiar non-inflammatory condition of the blood or arterial tissue, and in some instances probably setting in prior to death. The discoloration in question rarely occurred, and was always trifling in amount, when the heart retained its normal characters.

Redness, peripheral thickening, and investment with false membrane existed in the epiglottis in two cases; partial destruction, combined with thickening, in the sixth part of the subjects; the larynx was lined with false membrane in three instances, the seat of a small ulceration in one other. M. Louis found the lungs healthy or nearly so, in somewhat less than half the patients, whose death occurred from the 8th to the 20th day, and in somewhat more than the fourth part of those who died at a more advanced period; in a state of splenization or hepatization, or of both combined, in all the others. The pleura contained sanguinolent serosity in nearly half the cases. All these abnormal states were observed with very nearly the same frequency in subjects dying of other acute disorders, with the exception of course of pneumonia.*

The arachnoid contained two or three small spoonfuls of transparent serosity in three cases, of turbid in one; in another instance a soft, ex-

* Not of pleurisy, as the reader might perhaps expect us to add. Our author has shown that pleurisy rarely, if ever, proves fatal, unless in subjects labouring under chronic disease; such subjects are of course excluded from consideration throughout the present comparisons.

tremely recent pseudo-membrane. The subarachnoid cellular tissue was more or less infiltrated in 28 subjects,—to any considerable extent in 4 only, and in 3 the phenomenon was distinctly ascribable to the unusual duration of the "agony." The pia mater was injected in somewhat less than half the cases, to a remarkable degree in 11 subjects; more frequently in those dying from the 8th to the 20th day, than after that period; the superior cerebral veins distended with a tolerably large quantity of blood in one fifth of the cases. The cortical substance of the cerebrum was more or less red or pink coloured in 17 cases; the medullary injected to a moderate extent in the majority of subjects. Both were slightly softened in 7 instances; there was local softening in two others. The cerebellum presented the same species of lesions in a less number of cases. The injection of the pia mater and medullary substance, the pink hue of the cortical substance, and undue firmness of the entire mass, are more frequent and more marked, the more rapid the death of the subject: the contrary was the truth with sub-arachnoid infiltration, effusion of serosity into the lateral ventricles and softening. Of the non-inflammatory nature of these lesions no question can be raised after the study of the acute enquiry into the point by M. Louis; the length of this prevents us from extracting it entire,—to condense would be to mutilate.

Whatever importance the reader may feel inclined to attach to these conditions of the encephalon as connected with the symptomatology of the disease, the fact that similar cerebral conditions were observed with almost the same frequency in persons dying of all other acute affections indiscriminately, exhibits clearly their insignificance in this point of view. The only condition of the viscus apparently peculiar to typhoid fever—namely, a certain viscidity of tissue existing in one half the cases—has no claim, in the present state of knowledge at least, to consideration.

The remains of erysipelas existed in 4 subjects; the skin was thickened or attenuated, partially ulcerated or completely destroyed where blisters had been applied. In one instance only was the cellular membrane emphysematous, whereas it was so in 8 of the non-typhoid class of individuals. This latter phenomenon is by M. Louis ascribed in considerable measure to a morbid state of the fluids, favorable to the establishment of putrefaction; if this be the case, it would follow that the state in question is less frequent in typhoid fever than in other acute diseases.

Such then are the anatomical changes observed in subjects dying with the group of symptoms constituting the typhoid fever of Paris,—such their frequency, such their degree. Reviewing these we find that the sole lesions of invariable occurrence are seated in the patches of Peyer and mesenteric glands; is there any other legitimate conclusion from this fact than that these lesions are, in the language of the author, "inseparable from the existence of the affection under consideration, and constitute its anatomical character?"* And further, as the lesion of the

* We need scarcely point out to the reader that this conclusion differs *toto cœlo* from that almost invariably ascribed to M. Louis in this country,—namely, that the affection of the patches of Peyer is the *cause* of typhoid fever. The one inference is not only just, but a manifest involution of the premises; the other utterly unwarranted thereby, and unworthy of the humblest follower of inductive medicine. There are some minds of such curious perceptive power that they are unable to comprehend the wide difference between *cause* and *anatomical character*, and who, we presume, would charge any pathologist maintaining that the pustules of smallpox constitute the anatomical character of that disease,—in other words, the only material evidence of its existence,—of ranking them as its cause. With individuals of such mental caliber it is vain to argue.

agminated glands had acquired a certain degree of intensity in subjects opened on the 8th day of the disease; as in the greater number of cases the initiatory symptoms indicated a morbid change in the intestinal canal, (while the fact of the comparatively slight implication of the *general* mucous tract excludes the idea of their having originated in the changes developed in it,) the inference is, that the affection of Peyer's glands commenced with the first functional derangements. And the characteristic value of this particular lesion is thrown more prominently forward by the fact that the great majority of others occurred, as we have seen, with similar frequency in the course of acute disease generally.* The only exception of consequence to this statement appears to arise from the very frequent development of ulceration in the membranous tissue of typhoid subjects—a phenomenon occurring with singular rarity in other acute maladies, and hence forming a distinguishing feature of the disease, while it justifies the view of M. Louis, that "typhoid fever bears in this respect the same relation to other acute diseases, that phthisis does to chronic."

The cases upon which this anatomical doctrine is based, having been collected at an hospital from which individuals under 16 years of age are commonly excluded, it could not justly be applied to the disease as existing in children. Direct observation had however subsequently proved beyond doubt that the same mode and form of lesion of the intestinal glands prevail before as after the sixteenth year; and the recent extensive and minute investigations of MM. Rilliet and Taupin, at the Hôpital des Enfans, have demonstrated not only the general identity of the morbid condition of the glands, but the close similarity of the secondary lesions developed in the infant malady. These secondary lesions, which we have in the greater number of instances distinctly pointed out, depending upon the continuance of febrile action, the obvious inference is, that the existence of an inflammation entails that of other lesions, inflammatory or otherwise. Now the importance of this law in respect of the natural history of disease will, we believe, be questioned by none; but has it no practical application? has it no reference to the practice of revulsion and cauterization in acute diseases,—modes of treatment to which certain persons are wont to ascribe most marvellous effects? To us it appears to show that the alleged efficiency of agents of the class in question may fairly be disputed. And in this it appears to corroborate the results of direct clinical experience; for our own parts we are disposed to affirm that there is not a single one of the class of *acute* visceral inflammatory affections in which blistering and other modes of counter-irritation have been *demonstrated* to exercise a salutary therapeutical influence,—that is, to lessen the mortality produced by, or diminish the mean duration of, any malady referrible to that category.†

* Although in the extensive series of cases which fell under the observation of M. Louis, no example presented itself of the special lesion of the agminated glands, except in cases of typhoid fever, yet we do not mean here to prejudge the question of the possibility of such occurrence,—this will come to be discussed in a future article.

† We have elsewhere (Brit. and For. Med. Rev., vol. IX. p. 317,) noticed one of the probable causes of the exaggerated idea of the utility of blisters in certain affections. We should be glad to find that we have in our turn fallen into the opposite error of undervaluing them; but we may premise that our conviction of being correct—founded as it is upon the accurate investigations of unbiassed observers,—will not be shaken by the outcry of persons arrogating to themselves the title of "practical," who, if they have not cured all manner of maladies with blisters—we admit this beforehand—have no doubt employed them, in current phrase, with the "*happiest effects.*"

There is yet another aspect in which these secondary lesions assume pathological importance. If it be true that in the great majority of cases individuals who die with a group of symptoms confessedly referrible (not as to a cause but as to an observed coexistence,) to a given physical change of tissue—their anatomical character—the extent, nature, or seat of this morbid change, suffices to explain the decease of the individual; it is no less certain that in many instances this is inadequate to account for the observed issue of the disease. In these instances death may be either fairly ascribed to secondary lesions, or the event, as occasionally happens, is not to be explained by the gross physical changes discoverable in all the tissues collectively. Now in 18 of the fatal cases of typhoid fever (two-fifths of the whole) the condition of the patches of Peyer, of the intervening mucous membrane and mesenteric glands, was insufficient to account for death; and in 14 of them the event was traceable to the secondary lesions; while in the remaining 4 even these failed to explain it. Facts of the latter exceptional order have been appealed to with no small exultation by those who have satisfied themselves with a superficial knowledge of M. Louis' doctrine, as subversive of the importance attached by him to the Peyerian lesion. No better example could be adduced of the necessity of collecting a large mass of facts of all descriptions, before the attempt be made to determine the value or bearing of the phenomena of any individual case. The assumed importance of the disparity of the lesion and symptoms sinks into insignificance in respect of the notion which it is appealed to to support, on the announcement that a similar disparity was detected in the series of cases compared with the typhoid,—that in 5 out of 34 cases of pneumonia (to take a single example) the pulmonary lesion was insufficient to account for death, and in two of these the secondary lesions similarly ineffectual. And if the fact be taken into consideration that in several instances the typhoid lesions had commenced, or even made considerable progress in, a retrograde course, the proportion of cases in which the fatal issue appears a natural result of the state of the organs, will equal at least that observed in other acute diseases.

We have now reached the third part of the work, devoted to the symptomatology of the malady. Here M. Louis, having commenced with a general view of the symptoms, proceeds to make each of these the subject of separate examination. The time of origin, duration, and intensity of each; their relation to the different lesions discovered in *fatal* cases, are successively established; and then the same series of enquiries instituted in respect of the symptoms of individuals, who having had the disease *severely* or *mildly*, ended by recovering. The same course is next pursued in regard of the symptoms attending other acute diseases. Our means will not permit us to do more than make the reader acquainted with the chief characters of the symptoms observed in typhoid fever: we shall consider together the symptoms of fatal, severe, and mild cases, as to follow the distinction of these three classes, established by M. Louis, would inevitably carry us beyond reasonable limits.

*Diarrhœa* occurred in 129 out of 134 cases; in 58 of 125 on the first day of illness; was very considerable in 36 of 120, and of a mean duration of 20·5 days in cases of recovery.

*Abdominal pain* was detected in 112 of 127 subjects, arose on the first

day in 37 of 109, was commonly obtuse, of a duration varying from 24 hours to a month, and ordinarily seated in the iliac fossæ, especially the right, but in some instances affecting the abdomen generally.

*Meteorism* existed in 89 of 134 subjects, at some period of the disease was occasionally considerable, never appeared earlier than the seventh day, and lasted on an average from 4 to 15 days.

*Epigastric pain* was present in 59 among 96 patients; in 19 of 59 from the outset of the disease; in some subjects it was doubtful whether this was seated in the stomach or colon.

*Nausea* occurred in 37 of 95 patients, on the first day in 11 of 37; varying in duration from a few moments to 20 days.

*Vomiting* occurred in 29 among 94 subjects; in 7 of 28 on the first day.

*Cephalalgia* was noted in 127 out of 134 cases; appearing from the first in 39 out of 42 fatal cases, and in 74 of 85 subjects who recovered; the mean and ordinary duration being 9 days; it was violent in a very few instances only; in the rest it scarcely engaged the attention of the patients.

*Somnolence* exhibited itself in 109 of 134 cases, rarely before the sixth day; was slight at the outset in all; in 10 of 40 severe cases of recovery strongly marked; in these cases its mean duration was eight days; with hardly an exception it appeared before delirium.

*Delirium* was noted in 70 of 133 cases, and rarely commenced before the eighth day; ran a mean duration of ten days in fatal cases, of 6·5 days in severe cases terminating by recovery.

*Epistaxis* appeared in 49 out of 74 subjects; in 6 of these on the first day.

*Tinnitus aurium* was experienced by 36 out of 99 subjects, in 7 only on the first day.

*Deafness*, occurring commonly towards the middle or close of the disease, was manifest in 38 among 99 patients; in the worse order of cases was sometimes extreme, and varied in point of duration from one or two to twenty days.

Of 114 subjects in whom this was looked for, 91 presented a *peculiar eruption*, consisting of small florid papular elevations of lenticular shape, disappearing under pressure, few in number in the majority of cases, principally observable on the abdomen, but when abundant exhibiting themselves on the limbs; developed first, in all probability, between the sixth and ninth days; one set of papulæ disappearing and giving place to another, the whole running a mean course of about eight days.

*Sudamina* made their appearance in 28 of 43 cases, never being observed before the twelfth day; their diameter varied from two to four millimeters; they were produced by elevation of the epidermis with a colourless transparent fluid, and their development observed no distinct relation to the intensity of the sweats.

*Erysipelas* declared itself in 6 of 46 fatal cases, in 3 of 57 severe cases of recovery, in none of those of mild character.

The *Pulse* varied in character; thus of 41 subjects who died, 13 had a large and full pulse till the closing days, 8 had it remarkably large, 20 small; in respect of quickness it averaged 80-96 in 8, upwards of 100 in the rest; in 7 it was intermittent, in 34 regular.

*Buffy blood* was observed in 5 of 12 subjects who died, and were bled;

in 1 of these the buff was firm, thick, and semitransparent; in the others soft, gray, and gelatiniform. In 8 of 32 cases of recovery the blood was buffy; in 1 the buff thick, yellow, and transparent; in 1 dense and red, in 6 yellowish, and commonly thin, soft and gelatiniform.

It appears that the duration and intensity of the diarrhœa corresponded with those of the disease. The matters discharged were commonly thin, rarely containing small pieces of solid substance, a little mucus in four cases only; in two subjects they resembled coffee grounds; two other patients discharged some blood temporarily. The latter description of stool being of singular rarity in other acute diseases, with the exception of dysentery, its occurrence *may* become useful in a diagnostic point of view, though it is probable that when it arises, the other symptoms will be sufficiently characteristic. In respect of other acute diseases, this intestinal symptom confirms the result already referred to respecting the implication of the digestive tube in all febrile affections.

The objection which might be raised to the notion of early development of the intestinal lesion on the score of the absence of pain at the outset in a certain number of cases, M. Louis considers to be refuted by the fact that pain is not a constant attendant upon inflammation, in cases where this is even *seen* to exist. Thus, he has observed inflammation of the mucous membrane of the velum palati and of the tongue, erysipelas, &c. unattended with pain. He maintains accordingly that pain "is one of the least constant symptoms of disease, whatever be its seat, and one of the least calculated to throw light upon the diagnosis; nevertheless the period of origin of disease may in a considerable number of instances be inferred by its aid with precision, and in this respect it assumes much importance."

Upon the cause of meteorism the facts under review appear to throw no light; that it cannot be attributed to putridity follows from its having decreased in several subjects during the last days of existence; nor can it, at least exclusively, arise from alteration of the properties of the blood, as such alteration attends numerous acute diseases unaccompanied with meteorism; nor, we may add, can it depend upon decomposition of the feces, for the small intestine is its chief seat, whereas these sojourn in the large. The phenomenon appears to possess somewhat of a specific character.

The study of the gastric symptoms in the different classes of affections observed by M. Louis shows that, in the first place, affections of the mucous coat of the stomach often run a latent course during acute disease, a fact confirming the importance attached to the law respecting the development of lesions of the alimentary canal under these circumstances. Acquaintance with this law teaches us to be constantly prepared for the development of complications materially modifying the prognosis, while symptoms give us no hint of their advent. From M. Louis' cases we also learn that epigastric pain, coupled with bilious vomiting, almost invariably announces a serious lesion of the mucous membrane of the stomach; and that such lesion generally arises at an advanced period of the affection. On the other hand neither epigastric pain, nor even such pain attended with nausea, are infallibly indicative of an appreciable change in the tissues of the stomach, although they are in some instances the only observed effects of such change. This disclosure of the diag-

nostic inefficiency of epigastric tenderness, made at a time when the speculations of Broussais were cherished by the crowd, may be readily conceived to have excited notable indignation. Nor was this lessened by the added statement that the very affection upon which Broussais based his system—gastritis—is one of those with which pathologists are most imperfectly acquainted,—for this very natural reason that simple gastritis, terminating by the death of the individual it attacks, is of exceedingly rare occurrence, to such a degree that during a period of sixteen years, in which he has examined more than 1300 bodies, M. Louis has not met with a single example of that form of the affection. If it be true that the lesions of gastritis may be more frequently studied on the dead subject as secondary developments, it is equally certain that the examination of these had, before the investigations of M. Louis, been rarely, carelessly, and in fact but accidentally, attempted.

The conditions of the tongue and of the gastric surface in the fatal cases are exhibited in the subjoined table, which we have formed from the author's facts:

*Thirty-six Fatal Cases.*

| MUCOUS MEMBRANE OF THE STOMACH. | TONGUE | | |
|---|---|---|---|
| | Natural in 16 | Generally dry, bright red at the edges, reddish in the centre, rarely brownish in 12 | Dry, with crusted fur, brownish, more or less hard, in rare instances bright red in 8 |
| Softened and attenuated | 2 | 2 | |
| Mammillated with or without ulceration, ordinarily softened, thickened, or discoloured | 9 | 5 | Thickened and mammillated without softening 1 |
| Ulcerated | 1 | | 1 |
| | | With red or brownish punctuation 3 | |
| Softened simply | 2 | | With redness 4 |
| Healthy | 2 | 2 | 2 |

In two instances the tongue was more or less thickened and furrowed, in two others covered with a whitish pultaceous exudation. From the above table some inferences of consequence are derivable: 1. The tongue retains its natural characters in somewhat more than a third of fatal cases of typhoid fever. 2. The tongue may be natural and the gastric surface profoundly diseased. 3. The tongue may present the most marked evidences of implication in the disease, and yet the stomach be found not only not seriously diseased, but on the contrary perfectly healthy. 4. The same description of gastric lesion coexists with all varieties of condition of the tongue; and let it not be supposed that the cases, wherein the tongue maintained its healthy attributes, were mild in respect of other symptoms; independently of their being fatal cases, the very contrary was the fact. The analysis by MM. Rilliet and Taupin of fatal cases in children, of cases of recovery, and of all other acute affections in the adult, confirms the induction here drawn of the reciprocal independence of gastric and lingual lesion,—certainly not one of the least important corrections of professed experience which researches on the numerical system have established.

The function of deglutition was seriously obstructed in 10 of the 46 fatal cases as follows:

| | |
|---|---|
| 2 of 10 of the first series | 4 of 20 of the third series |
| 2 7 second | 2 9 fourth |

In eight of these cases the occurrence of the symptom was easily explained by the state of the parts, these being the seat either of abscess, infiltration with pus, plastic inflammation, muscular hypertrophy, or ulceration. In the two other cases the inside of the mouth was not carefully examined during life, so that it cannot be considered certain, though this be probable, that the inability to swallow was in them purely nervous. It must not be forgotten on the other hand that advanced disorganization of the pharynx or œsophagus may exist without dysphagia: such was the fact in four cases; here however the presence of delirium accounted for the absence of the ordinary effect. It is worthy of note, as observed by M. Louis, that when delirious patients obstinately refuse drink, without being able to assign their motives, a lesion of the pharynx is almost invariably detected after death.

Cephalalgia, as appears from the facts referred to, is one of the most common symptoms of acute febrile disease of every kind; nevertheless as it is a more frequent, indeed an almost constant attendant on typhoid fever, and in the vast majority of cases, whether fatal, severe, or mild (113 out of 127), originates from the very outset of the disease, its absence at the outset of a febrile disorder, more especially if other symptoms of typhoid fever be wanting, may be regarded as a very probable sign that the affection is not typhoid.

The characters of typhoid somnolence are sufficiently distinctive. In two variolous patients this symptom appeared on the third day of the feverish state, and it is obvious that as the general symptoms are at that period the same as in many cases of typhoid fever, the malady might be easily mistaken for the latter. However as typhoid somnolence rarely (6 times in 134 cases) comes on before the fourth day, that of the variolous eruption, its occurrence previous to that day warrants a strong suspicion that the affection is of the eruptive class.

M. Louis enquires at great length into the question as to the reality of the alleged existence of an appreciable relation between the occurrence of delirium, and the condition of the brain or other organs. The conclusion in respect of the brain is in the negative, and for the reasons deducible from the following table:

| State of the brain. | 12 subjects Non-delirious, or so only for 24 hours, or during the last 2 or 3 days of life. | 12 subjects With the most violent form of delirium. |
|---|---|---|
| Cortical substance more or less pink | 4 | 5 |
| Perfectly healthy | 6 | 5 |
| | One thalamus slightly softened | |
| Brain injected | 1 | Very much so 1 |
| Brain less firm than in health | 1 | 1 |

And further, in two of these twenty-four individuals the brain appeared abnormally firm; one of the two had violent delirium, the intelligence of the other remained unaffected. Now as the anatomical conditions of the brain in the two categories of subjects may almost be said to have been identical, while the states of the intellectual faculties were diametrically opposed, the negation of any necessary nexus between lesion of the brain and delirium in typhoid fever is justly predicable. Close study of the symptoms in cases of recovery corroborates this inference. Among other points of evidence in this direction is the fact that in upwards of 400 cases of recovery (herein are included those observed by M. Louis since the publication of his first edition,) this observer has met with a single example only of persistence of delirium after the cessation of febrile reaction; a fact, he well observes, incompatible with, and in itself sufficient to disprove the correctness of the notion that the delirium in this disease depends upon appreciable cerebral lesion.

How stands the question with regard to the relation of gastric changes and delirium? Let us display M. Louis' facts, bearing upon this point also, tabularly:

| Mucous membrane of the stomach. | 12 subjects Non-delirious, or so for last two or three days of life. | 12 subjects More or less violently delirious. |
|---|---|---|
| Healthy except from slight pink tint | 1 | Natural 2 |
| Slightly softened at great cul-de-sac or pylorus | 3 | 2 |
| Softened and attenuated | | 2 |
| Seat of a few small ulcerations | 2 | |
| Mammillated, and commonly changed in colour, consistence, and thickness | 6 | 6 |

On the independence of the two conditions under consideration here displayed it is unnecessary to insist; but it is important to add that neither the stomach *nor* the brain presented any remarkable change in several cases. The general inference drawn by M. Louis as to the causation of the delirium is that "as one lesion only is of constant occurrence and of similar character in all these subjects—namely that of Peyer's glands—in that lesion and doubtless in the agencies, whatever they may have been, favouring the development of the affection, resides the cause of delirium, and more especially of somnolence." But that the febrile action present is the intermediate and more direct cause appears evident from the extremely general occurrence of such action in the subjects of all acute diseases, whereof it constituted the only common phenomenon.

The spasmodic symptoms, observed in patients labouring under the typhoid affection, were of two kinds, muscular stiffness and alternate muscular contraction and relaxation. To the latter class are referrible subsultus tendinum, hiccup, &c. The vastly greater frequency of symptoms of this order in fatal cases than in those of recovery shows that their

occurrence materially increases the unfavorable tendency of the prognosis; and as no single instance of persistent contraction of the muscles of the neck and arms was observed in cases of recovery, this symptom may be regarded as of almost fatal augury.

In commenting upon the prostration of strength attending typhoid fever, in the majority of cases from the outset (ten out of nineteen subjects were obliged to give up their ordinary occupations on the day of attack), M. Louis carefully points out the exceptions occasionally observed. Thus an individual who died on the 24th day, continued to work during the fifteen first, and was able to get out of bed without help on the day of his death.

Out of 700 cases of acute disease of which 130 terminated fatally, 8 only were attended with œdema of the lower extremities: 6 of these were cases of pneumonia or intense pulmonary catarrh, 1 of pericarditis, 1 of scarlatina. Hence it follows that the occurrence of œdema in a subject affected with acute disease should direct the attention of the physician to the lungs or heart, provided there be no other symptom of Bright's disease present.

Redness of the conjunctiva, observed in 16 of 27 fatal cases, was the produce either of uniform discoloration or of injection. Almost every individual suffered from slight intolerance of light in the sitting posture. Total spasmodic occlusion occurred towards the close in 4 fatal cases.

Extreme deafness does not increase, as has sometimes been stated, the probability of a fatal issue. Suppuration of the auditory canal occurred in a few instances; a state which has been observed to terminate in perforation of the tympanum.

The eruption, termed *pink lenticular*, consists of minute spots, variable in number, of the shape and tint indicated by their name, slightly papular or elevated above the surrounding surface, disappearing completely under, and reappearing on the removal of, pressure—characters all of them distinguishing these maculæ from true petechiæ. The frequently late arrival of patients in hospital prevents the earliest possible period of their development from being easily discovered. The mean time of their appearance has already been estimated approximatively. It is remarkable that the frequency of their occurrence and their abundance are not in the direct ratio of the severity of the disease. Desquamation of the epidermis, attended or not with development of sudamina, was observed in some cases.

The statement of the conditions of the pulse shows, as might have been anticipated, that the absence of great rapidity is a favorable sign. In respect of relation between the states of the pulse and of the tissues of the heart, M. Louis found that "of 41 subjects whose pulse was properly examined, 13 suffered from softening of that organ, and almost all of them to a remarkable extent. On the other hand, 11 of 17 patients in whom the pulse was unequal, irregular, intermittent, small, weak, tremulous, and contracted, for a shorter or longer period, presented the same lesion on post-mortem examination; . . . . . this organ was consequently in the natural state in $\frac{2}{3}$ only of the cases in which the pulse possessed the characters referred to." This induction gives additional verisimilitude to the inference already drawn respecting the period at

which such softening originates; while it points out a means of ascertaining that period during life with an approximation to certainty.

The voice was changed in character, or suppressed, in cases attended with the development of pseudo-membrane in the air-passages; M. Louis has since the publication of his first edition met with an instance wherein complete aphonia persisted upwards of thirty days, and was probably dependent on ulceration of the larynx.

Some circumstances give diagnostic value to the ronchus referred to under the head cough: its universality and loudness distinguish it from that of acute primary bronchitis; and a special characteristic arises from the want of agreement between its extent and violence and the amount of accompanying dyspnœa.

The state of the blood in the veins after death did not always correspond with that observed during life; fibrinous clots were occasionally discovered in subjects whose blood furnished only a soft gelatiniform clot, when abstracted from a vein. From this M. Louis is inclined to infer that the morbid states of the blood, as those of the viscera, pursue a retrograde course after a certain period of the disease. He refers briefly to the recent researches of MM. Andral and Gavarret,* which, while they prove the fact of diminution in the ratio of fibrine in the blood to be greater in typhoid fever than that observed in any other disease, and directly proportional to the severity of the malady, also show that the same species of diminution arises in eruptive and simple continued fevers, in congestions and in cerebral hemorrhage. The globules are found, especially if the enquiry be undertaken at any early period of the affection, far from having diminished, to have notably increased in proportional quantity.

*Diagnosis.* In the chapter on diagnosis, to which we have now conducted the reader, are considered the grounds for affirming the existence of this particular disease in severe and slight cases, the peculiarities of *latent* and *simulated* typhoid fever, the question of the identity or non-identity of the disease with Camp typhus and the typhus fever of these islands.

Our examination of this chapter will be appropriately introduced by a transcript of M. Louis' excellent definition or rather condensed description of the disease:

"The typhoid affection may be characterized as an acute disease, attended with febrile action of variable intensity; of uncertain duration; proper to young subjects, more especially to those recently exposed to external conditions, to which they had previously been unused; commencing with violent rigors, anorexia, thirst, and in the very great majority of cases with diarrhœa; attended at an early period with a degree of weakness more than commensurate with the severity of the other symptoms, next with somnolence, giddiness, disturbed vision, stupor, delirium, meteorism, enlargement of the spleen, the formation of sudamina, of peculiar lenticular pink spots, of eschars over the sacrum, of ulcerations in the situations where blisters may have been applied; with deafness, various spasmodic motions or persistent contraction; occasionally with intestinal hemorrhage; very rarely with aphonia: a series of symptoms which progressively increase in intensity, when the disease is destined to terminate

* Vide Brit. and For. Med. Rev., vol. XI. p. 242. Jan. 1841.

fatally, and diminish more or less rapidly until they have totally disappeared if the contrary be the result: the whole group being characterized anatomically by a special morbid change of the glandular patches of the ileum." (Vol. ii. p. 195.)

With the exposition of the number and mode of catenation of these symptoms warranting the observer in diagnosticating typhoid fever we cannot tarry, but the minute study of these paragraphs is indispensable, were it only to demonstrate the occasionally almost insurmountable difficulty of the diagnosis. We find that so late as 1830 M. Louis himself, the greater part of whose medical life has been devoted to minute investigation of the malady, mistook symptoms originating in central softening of the brain for those of the malady in question. And yet this is the affection respecting the diagnosis of which the mere dicta of practitioners, who have never demonstrated directly or indirectly their capabilities for the task, are unhesitatingly accepted as correct,—the word of clinical tyros even deemed a sufficient guarantee,—nay more the random assertion of hospital patients of the lowest grade registered without comment in lieu of more worthy evidence, and hence virtually made to establish the existence or non-existence of "typhus" in a relative, a family, the inhabitants of a street, or the population of a district!* From the case referred to it follows that an affirmation of the existence of typhoid fever is not perfectly warrantable even "where there is considerable fever, accompanied with intense cephalalgia, great prostration of strength, dryness and brown discolorations of the tongue, *if no abdominal symptom be present;* . . . . . to authorize the physician in such diagnosis beyond all fear of error there should be present in addition a certain amount of meteorism, a few lenticular spots, disturbance of the functions of the organs of sense, enlargement of the spleen, and nasal hemorrhage."

M. Louis establishes an elaborate parallel between enteritis and the typhoid affection; diseases which the specious fallacies of Broussais had succeeded in confounding. Made acquainted with the error of these by the publication of our author's first edition, his countrymen now without an exception recognize the essential dissimilarity of the two maladies. In England, however, we have rarely known typhoid fever become the topic of conversation, without hearing it affirmed that M. Louis considers the disease an enteritis or a gastro-enteritis. Much has already appeared in the foregoing pages subversive of this error, but as the existence of correct notions on this point is of essential consequence, we transcribe M. Louis' tabular view of the symptoms in a number of cases of each of the affections in question. The cases of typhoid fever belong to a different series from those analyzed in the work.

* It is a notorious fact that in these islands the question of the contagious origin of the disease has in multitudes of instances been determined, not from personal examination, by the physician or by competent delegates, of the individuals alleged to have communicated the disease, but by the mere circumstance of such allegation having been made. Upon this point we shall have occasion to speak further in a second article, when considering the nature of the evidence existing upon numerous questions connected with the typhoid fever of this country.

| TYPHOID AFFECTION. | ENTERITIS. |
|---|---|
| 17 cases of recovery. | 23 cases of recovery. |
| 1. *Diarrhœa.* | |
| In 14 out of 15 cases: from 6 to 10 stools in 5 cases, less in the others. | In 23 out of 23 cases: from 7 to 30 stools in 21 cases, less in the others. |
| 2. *Abdominal Pain.* | |
| In 8 out of 11 cases. | In 22 out of 23 cases. |
| 3. *Meteorism.* | |
| In 12 out of 17 cases. | In 1 out of 23 cases. |
| 4. *Spleen.** | |
| Enlarged in 11 out of 15 cases. | Not enlarged in any. |
| 5. *Epigastrium.* | |
| Somewhat painful during cough in 1 case. | Not painful. |
| 6. *Nausea.* | |
| In 1 case. | In 2 cases. |
| 7. *Appetite.* | |
| Null in every case, until convalescence. | Null in 5 out of 23 cases on the admission of the patients; rapidly recovered. |
| 8. *Tongue.* | |
| Thickened and red in 3 cases; dry and red in 2; dry and black in 1. | Sometimes whitish, without other change. |
| 9. *Fauces.* | |
| Inflammation of the velum and uvula in 2 cases; of the pharynx and tonsils in 1. | Natural. |
| 10. *Cephalalgia.* | |
| In every case from the outset and often intense. | Slight in a single case. |
| 11. *Somnolence.* | |
| Tolerably marked in 5 cases. | Null. |
| 12. *Intelligence.* | |
| More or less disordered in 6 cases. | Intermittent delirium (removed by bark) in 1 case. |
| 13. *Strength.* | |
| Extreme weakness in 7 cases. | No prostration of strength. |
| 14. *Dazzling before Eyes.* | |
| In the only 6 cases in which mentioned. | None. |
| 15. *Spasms.* | |
| Of the lips in 11 cases, without subsultus. | None. |
| 16. *Tinnitus Aurium.* | |
| In 8 cases. | None. |
| 17. *Deafness.* | |
| In 3 cases. | None. |
| 18. *Vision.* | |
| Disturbed in 7 cases. | Unimpaired. |
| 19. *Epistaxis.* | |
| In $\frac{7}{10}$ of the cases. | None. |
| 20. *Pink lenticular Maculæ.* | |
| In 15 out of 16 cases. | None. |
| 21. *Sudamina.* | |
| In 9 out of 12 cases. | In 1 case of dysentery complicated with pneumonia. |

* The fact of such enlargement is to be roughly ascertained by the freedom from or presence of fulness and tension in the left hypochondrium; the increase of size to be measured with precision by percussion.

| TYPHOID AFFECTION. | ENTERITIS. |
|---|---|
| 22. *Rigors.* | |
| In 11 out of 12 cases. | In 5 out of 23. |
| 23. *Heat of Skin.* | |
| Unnaturally great in every case. | Slightly so in 4 cases of dysentery. |
| 24. *Sweats.* | |
| Copious in 5 cases. | In 9 cases, the only ones in which the symptom mentioned. |
| 25. *Pulse.* | |
| Above 100 in 7 cases. | 80 in 3 cases for one day; between 50 and 70 in the others. |
| 26. *Duration.* | |
| Mean = 25 days in the case of patients admitted from the 6th to the 9th day; = 30 in those admitted after that period. | Mean = 3 or 4 days counting from the admission of the patients; they left after a sojourn of 8 days. |
| 27. *Mortality.* | |
| Besides the 17 cases under consideration 4 fatal ones occurred within the same period, which gives a mortality of 1 in 5. | None. |
| 28. *Age.* | |
| Mean = 23·5<br>Extremes = 13 and 35. | Mean = 36.<br>Extremes = 15 and 70. |

*Modifications of form of typhoid fever.* The author next proceeds to the consideration of those categories of cases, the existence of which appears on first sight to shake materially the foundation on which his doc trine is erected. The first of these comprehends five cases, to which the title of "latent typhoid" is given, wherein death arose from perforation of the ileum, previously to which occurrence the symptoms had been of such character as if not to exclude, at least not to suggest, the idea of their dependence upon typhoid fever. The second refers to four cases characterized by intensely marked symptoms, while the Peyerian lesion was deficient on first view if not in specific character, at least in the development which was fairly to be anticipated. In a third class are reported three cases in which the majority of the symptoms of the typhoid affection occurred without specific change in the patches of Peyer. The apparent objection derivable from the first order of facts against the constant coexistence of the group of symptoms and anatomical change described in these volumes, is met by the consideration that a similar latent course occasionally masks the symptoms of the severest acute and chronic diseases,—that pneumonia even would frequently escape detection were the functional phenomena alone trusted to in the attempt to discover its existence, and that individuals sometimes perish from tuberculous perforation of the pleura in whom the ordinary symptoms of phthisis had never developed themselves. Now from cases like these the deduction would not be that there is no particular series of symptoms referrible to pneumonia and to phthisis, but simply that the ascertained effects of these maladies are occasionally not evolved. The objection which may be urged on the score of the second set of cases is open to similar refutation: cases are on record, and we have ourselves seen such, where pneumonia has been attended with the severest symptoms and eventually terminated fatally, and yet a *small* portion of a *single* lung only been found affected with the *second* degree of inflam-

mation. Such instances might easily be multiplied. It has been objected to M. Louis, in respect of the third class of cases that in refusing these the title of typhoid fever, because the patches of Peyer were healthy, he falls into a *petitio principii*. It is argued by M. Dalmas, "you propose to enquire if the group of symptoms termed typhoid fever is invariably attended by a given lesion, and when the lesion which occurs most frequently happens to be absent, instead of drawing the conclusion that such lesion is not of constant occurrence, you infer that the case was not one of typhoid fever,—because the lesion, which you call characteristic, was absent. But the precise question is whether that lesion is or is not characteristic." This argument is indubitably a strong one, but *audi alteram partem*. Analogous circumstances arise, it is urged, in the instance of diseases the anatomical character of which is thoroughly and definitively established by general experience; the only fair version of the difficulty attending these cases of "simulated typhoid fever" is that M. Louis was deceived by appearances, as others have been in respect of the affections just alluded to. "The symptoms of croup," says M. Valleix,* "have been supposed in certain instances to be present by most skilful practitioners—tracheotomy even been on the point of being performed for their relief, when death has cut off the patient and not a particle of false membrane appeared in the larynx. What is the conclusion? That croup occurs without false membrane? Certainly not, but that the symptoms of croup were *simulated* by those of spasmodic stridulous laryngitis,—that the case in short was one of pseudo-croup, and the diagnosis erroneous. Again, meningitis has often been presumed to exist, in cases wherein the dissection disclosed none of its anatomical characters. Nay, cases are cited in which all the rational, and even the physical signs of vesical calculus, among others the shock of the sound against a resisting body having been manifestly present, the patient has been cut and no stone discovered."† What was the inference here, but that men of the first powers are under certain circumstances liable to be deceived. And this style of reasoning derives support from the fact that within the last ten years, a period during which the diagnosis of the malady may be considered to have attained extreme perfection, not any, or scarcely any, exceptional cases have been encountered.—For our own parts, during an experience of four years in the Parisian hospitals, we never saw a single instance of the symptoms occurring without the lesion, or of the development of the latter unattended by the former;—but the Peyerian change was in more than one case so slightly marked, as to be barely capable of identification. But more of this hereafter.

M. Louis enquires into the similarity or dissimilarity of Camp typhus and typhoid fever, and deriving his knowledge more especially from the Memoir on the subject by M. Gauthier de Claubry, coincides with him in considering the symptoms of the former affection, as far as the vague language of its describers warrants the formation of an opinion, to be those of typhoid fever. The justness of this conclusion is considered incontestable, in respect of the occurrence of a certain number of symp-

* Archives Générales de Médecine, Janvier, 1839.

† We remember to have heard M. Roux mention many years since his having been five or six times unfortunate enough to lithotomize non-calculous patients.

toms—diarrhœa, abdominal pain, meteorism, lenticular maculæ, occasionally petechiæ, nasal hemorrhage, deafness, disturbed vision, somnolence, stupor, and delirium. In both diseases the course of the disease may be rapid or slow; the mean age of their victims is very nearly the same, the occurrence of typhus after the fiftieth year being apparently very rare; while both diseases, with extremely rare exceptions, affect the same individual once only. Where the viscera have been examined with any degree of closeness, the anatomical changes appear to be those observed in the typhoid affection,—gangrenous eschars, ulcerations of the mucous membrane of the small intestine, tumefaction, softening, and gray discoloration of the mesenteric glands corresponding to the seat of the intestinal ulcerations. These data appear to M. Louis sufficient to warrant an affirmation of the "identity" of the two diseases. Neither the silence of the describers of "typhus," respecting perforation of the intestine—an occurrence far from uncommon in the course of typhoid fever—nor the alleged fact that the symptoms of "typhus" have of late years been observed at Rochefort and Toulon without the existence of the intestinal lesion being established, appear to our author to constitute sufficient motives for renouncing or altering the opinion just expressed. The latter circumstance appears to him simply to indicate "the simultaneous existence of two severe diseases,"—an inference into the justness of which we shall have occasion hereafter to enquire, when we examine the section of these volumes referring to the typhus of our own country.

Perforation of the small intestine proved the immediate cause of death in 8 out of 55 fatal cases. Commonly single, the perforation was in some instances double or triple, seated in the vicinity of the cæcum in the midst of the ulcerated patches. The most remarkable circumstance perhaps connected with it was its occurrence in the majority of instances in mild cases; to such an extent that five of these belonged to the class *latent*. Perforation took place on the 12th day of the disease in one case, the 18th in two, from the 22d to the 42d in the remaining five. In five cases the *sudden* supervention of the usual symptoms of intense peritonitis announced the accident; in three it followed almost a latent course, though proving as promptly fatal as in the others. In certain instances the first severity of the abdominal pain may be mitigated; but the persistence of retraction of the features, of nausea, or vomiting, and of rigors show that the real gravity of the case has in no wise diminished. In some subjects the prevalence of cerebral symptoms notably masks those of perforation. The period elapsing between the occurrence of the accident and death varied, except in the instance of one subject, between 20 and 54 hours; the individual in question survived until the seventh day.

*Prognosis.* M. Louis' chapter on prognosis does no more than exhibit in a condensed form the facts scattered through his previous pages, whereby the almost impossibility of forming an assured judgment as to the issue of the malady is rendered very painfully evident. The relative prognostic value of particular symptoms may be gathered from the subjoined table, which we have drawn up from our author's descriptions; from this it appears that those enumerated under the head D, are the only ones at all warranting a fatal prognosis, and even these appear to arise—in infinitely rare cases, it is true—in individuals destined to recover.

The fact just referred to respecting the more usual occurrence of perforation in mild than in severe cases, displays the complexity of the question of individual prognosis in another aspect.

| A. *Symptoms which occur with the same or almost the same frequency and intensity in fatal cases and in those of recovery.* | B. *Symptoms which occur with greater frequency, but not to any notable degree, in fatal cases.* | C. *Symptoms occurring with much greater frequency in fatal cases.* | D. *Symptoms peculiar to, or with the rarest exceptions peculiar to, fatal cases.* |
|---|---|---|---|
| Diarrhœa.<br>Gastric symptoms.<br>Morbid states of the tongue.<br>Prostration of strength.<br>Epistaxis.<br>Deafness.<br>Tinnitus aurium.<br>Sudamina.<br>Lenticular eruption. | Involuntary dejections.<br>Meteorism.<br>Dysphagia.<br>Ocular injection.<br>Marked rapidity of pulse.<br>Irregularity or intermittence of the pulse. | Intestinal hemorrhage.<br>Delirium *appearing the first day.*<br>Agitation.<br>Somnolence.<br>Spasmodic movements.<br>Extreme prostration of strength from the first.<br>Permanent disturbance of vision in the recumbent position.<br>Erysipelas.<br>Persistence of a small contracted pulse. | Somnolence appearing the *first* day.<br>Perversion of intelligence exhibited by patient declaring himself well, while in reality suffering under very severe symptoms.<br>Persistent muscular contraction. |

There is one circumstance which, according to M. Chomel's experience, may justify a positive opinion as to the fatal issue of the malady, namely, severe relapse after short remission of the symptoms. Slow invasion of the disease also augurs ill for its mode of termination. Certain circumstances connected with the general condition of the individual modify the prognosis; of these the most important is age. The younger the subject, provided he be an adult or nearly so, the stronger his chance of recovery; M. Louis has in the course of the last ten years seen only one person aged less than twenty succumb. From a very admirable recapitulation made by M. Chomel in 1835, having for its basis the cases observed in his wards at the Hôtel-Dieu for three years, and of which we have the numerical results now before us, it follows that the prognosis is most favorable in the case of individuals under twenty years of age, of the female sex, attacked during the summer and resident at Paris more than two years.

*Etiology.* Passing to the chapter on etiology, we find M. Louis displaying the ages of his patients as follows:

| Ages. | Fatal cases. | Recoveries. |
|---|---|---|
| 17 to 20 | 14 (15½-20) | 31 |
| 20 — 25 | 20 | 39 |
| 25 — 30 | 11 | 13 |
| 30 — 39 | 5 (= 50) | 5 (= 88) |

The mean age in the fatal cases was 23, in those of recovery 21; the favorable chances afforded by youthfulness appear more distinctly from considering the much greater proportion of recoveries than of deaths before the twenty-fifth year.

Of 138 typhoid subjects, 32 only were females. M. Louis, however, correctly considers this result not to be confided in as the expression of a general law, inasmuch as the number of males constantly arriving in Paris, and unable to remain at home during illness, is probably much greater than of females,—a notion confirmed by facts observed in some rural districts of France. The mortality in M. Louis' cases was equal in both sexes, wherein his experience differs from that of M. Chomel.

The division of typhoid individuals into two classes in respect of trades, those requiring considerable expenditure of strength and the contrary, shows these conditions to be unimportant in respect of the development of the disease.

| | Laborious trades. | Non-laborious trades. |
|---|---|---|
| No. of cases | 77 | 55 |
| Deaths | 28 | 18 |

And the unimportance of the trifling excess in the ratio, furnished by individuals following laborious occupations, is manifest from the fact that in all probability the *laborious* are the best-peopled trades, if we may so speak, in Paris.

From the facts observed by M. Louis it would follow that strength or feebleness of constitution, excessive manual labour, moral suffering, and over indulgence in alcoholic beverages, do not distinctly act as causes of the malady. "Nor can a sojourn during the night in cellars inhabited by an excessive number of persons be esteemed a cause of the disease; the eighteenth part only of the subjects observed having been sufferers from such deficiency of healthful lodgement."

Removal from the provinces to Paris favours the outbreak of the disease; but the longer the stay of the patient in that town, the greater, as already mentioned, are his chances of recovery. This is shown in the following table:

| Length of residence in Paris. | Deaths. | Recoveries. |
|---|---|---|
| 2 to 3 weeks | 10 | |
| 2 weeks to 3 months | ... | 7 |
| 3 months to 5 months | 8 | 19 |
| 6 10 | 10 | 19 |
| 11 20 | 9 | 20 |
| 20 30 | 5 | 12 |
| 30 40 | ... | 1 |
| 3 years to 8 years | 2 | 7 |
| | 44 | 85 |

The mortality among those who had been in Paris a less period than ten months was 38·36 per cent.; among those whose residence had been longer, 28·6 per cent. The proportions expressed in the table, though approximations to the truth, cannot, as M. Louis himself well observes, be considered possessed of perfect precision; their claim to this character is based on the presumption, "that all the working classes in Paris, of all ages, seek hospital aid in the same proportion;" now this is not exactly the truth, as a tolerably large share of those who have inhabited Paris for years remain at their own homes. The question naturally arises, what is the cause of this predilection of the disease for the class of persons referred to? We have just seen that misery of circumstances

cannot have had any material influence on the production of the disease; and the bearing of the fact already noticed coincides with that of another observation by M. Louis, namely, that "the majority of the patients were of good constitution, their flesh was firm, their complexion of the ordinary kind, and they possessed a fair share of fatness when they were admitted into hospital; they were consequently in a condition subversive of the notion of wasting away, or of the prolonged action of some deleterious agency, capable of influencing the function of nutrition." It is true, nevertheless, observes our author, that practitioners commonly ascribe very powerful influence to bad nourishment in the production of typhoid fever; but while he recognizes, as all must, the reality of the pernicious influence of such nourishment on health, and in the development of disease in general, he finds himself obliged to declare that he has looked in vain for "incontestible proof that such influence is more marked in respect of typhoid fever than of any other disease, even of that typhoid fever which follows in the train of armies, under circumstances wherein a multitude of sources of insalubrity coexist,—atmospheric exposure, privation of food, excesses of every kind, and, as regards the sick and wounded, accumulation of a number of persons in the same place." The well-known experiments of M. Gaspard are correctly viewed by M. Louis as proving nothing more than that the injection of putrid matter into the veins of animals will give rise to a series of severe symptoms frequently assuming a typhoid character, (as may be the case with almost every acute disease to which the frame is liable), while the lesions produced do not wear the characters of those of typhoid fever, and seem to originate in almost all organs indiscriminately. Besides, it is perfectly illogical to compare the agency of a cause acting violently and suddenly with one producing its effects gently and slowly. Had M. Gaspard shown that animals fed for a length of time upon putrid substances became eventually the subjects of symptoms and lesions analogous to those of typhoid fever, his experiments would have assumed a very different share of importance.

The archives of the Medical Society of Observation contain the record of a case observed at the Hôpital St. Louis by M. Letenneur, which *apparently* supplies an argument of much sounder character and higher pretensions in the direction under consideration. An individual aged 21, of good constitution, having inhabited Paris for fifteen months, of sober habits, habitually using nutritious food, occupying a well ventilated room on the fourth story, and working by day in a spacious workshop with a southern aspect, ordinarily enjoying excellent health, but for the last sixteen months suffering from obstinate blennorrhœa, which had resisted all the usual modes of treatment, was admitted into hospital with the symptoms of an acute affection. Upon enquiry it appeared, that he had for some time been in the habit of drinking twice a day a glass of water in which mashed straw had been macerated for ten or eleven days, (a popular remedy in Paris for gleet); and, according to his fellow-workmen, had consumed altogether four litres (about five quarts) of the liquid. When the first symptoms of his existing malady declared themselves, the urethral discharge had then almost disappeared. The smell of the liquid was so horrible and so tenacious, that repeated washing failed to remove it from the vessel used for maceration. The patient had been five days

ill on that of his admission: the case followed the course of typhoid fever; and after death, which occurred on the tenth day, the characteristic lesion of the patches of Peyer exhibited itself: all these points are clearly shown in the full narrative of the case, from which we have merely extracted the more striking particulars. But, argues M. Louis, this case is more apparently than really conclusive: it is a solitary one, and the development of typhoid fever may have been a mere coincidence, especially as individuals constantly enjoying most nutritious diet are not exempt from the disease. Besides, if the ingestion of putrid water were often attended with consequences such as those some persons might in the present instance feel disposed to attribute to it, the remedy would decidedly be less cherished than it is by the lower orders in Paris. And in support of M. Louis' arguments may be adduced the facts, that quantities of putrescent fish are habitually consumed in some of the northern islands, and that the Esquimaux subsist in great measure upon rancid oil, without being on this account, if the narration of travellers may be credited, particularly subject to the ravages of typhoid fever. It is further urged by M. Louis that, while the disease is not observed to occur after the age of fifty, it is precisely among workmen who have passed that age that the greatest difficulty in earning sufficient and healthful nutriment prevails.

*Contagion.* One of the novelties enriching the present edition of M. Louis' volumes is a chapter on the subject of contagion. Of the reality of contagion as a cause of the propagation of typhoid fever the following facts, among many of similar character, afford in M. Louis' opinion full demonstration. M. Bretonneau, of Tours, has seen the disease pass from one village into another previously free from it, after the arrival in the latter of a person who had sojourned for a more or less long period in the former. M. Gendron observed, that after the arrival of a diseased individual in a village, the malady spread from house to house, when communication had occurred between the inhabitants of these; and that the persons most invariably and earliest seized were those who had been most assiduous in their attentions to individuals suffering under the malady. Similar facts have been observed in country hamlets by M. Putegnat. In all these cases the lesion of the patches of Peyer was invariably discovered when an autopsy took place. Respecting the contagiousness of the disease, exemplified by such occurrences as these, M. Louis does not think warrantable doubt can be raised, so far as rural districts are concerned. If admitted to prevail in these, it would seem an *à priori* necessity to recognize its existence in towns. The Parisian physicians, wisely rejecting on general principles such mode of reasoning, believed themselves justified by an appeal to direct observation in denying the reality of contagiousness in the capital they inhabit. It has been urged by M. Andral, that the disease is not observed to be transmitted in hospitals from a typhoid patient to those occupying the adjacent beds. And again, circumstances most favorable to the action of contagion appear to exist in the instance of medical students who closely tend their typhoid companions in small and ill-ventilated rooms, such as they commonly inhabit; yet the same observer has never met with a single instance of its development under these conditions. Against the views, apparently derivable from these facts, appears, in the first place, the general argument stated by M. Bretonneau, that it must be extremely

difficult from the very nature of things to trace the movement of contagion in populous towns. In respect of M. Andral's objections it is urged by M. Louis that the transmission of indubitably contagious maladies, such as measles, is scarcely ever observed in hospitals for adults; and if the validity of this argument be demurred to on the ground of measles being a disease of infancy, the demurrer must be entered in the case of typhoid fever also, for this too occurs in a very large proportion of cases at a similarly early period of life. Besides the fact of measles being a more contagious disease than typhoid fever is, according to M. Louis, questioned by none. Further, our author has three times observed the development of the disease in his own wards; two of these cases occurred during an epidemic. The argument drawn from the safety of students filling the office of *garde-malades* appears to M. Louis to lose its importance, from the fact that these youths constantly relieve each other in their office of friendship, and hence are not continuously exposed to the chances of contagion for any lengthened period. The large share of artisans and students who are seized with typhoid fever within a limited period after their arrival in Paris, argues in favour of its contagious nature. Admitting, in truth, the reality of contagion, it is easily intelligible that youths arriving in a town where the disease constantly exists, should contract it in greater or less number through direct or indirect communication with affected individuals. It may be objected to the notion of the contagionists, that typhoid fever differs from confessedly contagious disorders, in that the anatomical change characterizing it, originates in the great majority of cases with the first symptoms. The premonitory symptoms of the exanthemata are here referred to; M. Louis exposes the trivial character of the objection. There are some other interesting points in this chapter discussed or referred to, of which, as well as of the whole line of argument here adopted, we reserve our examination for a future occasion. To that period we also postpone the consideration of M. Louis' therapeutical results.

The volumes, of which we for the present take leave, are appropriately dedicated to the members of the Medical Society of Observation. Few, indeed, could urge a stronger claim for such distinction than they who have exhibited unwearied zeal in following out the system of their illustrious president, and accumulating additional demonstration of the value of that system by applying it to previously unexplored branches of enquiry. But if M. Louis be indebted to the ardour of his disciples, the benefit he has conferred upon them, and indeed upon all observers who may be charged with "the atrocious crime of being *young men*," is of even a higher order. He has established and promulgated the validity of a method of studying disease, which invests the young with equally great power to extend the limits of their science as the old, provided they approach their task with steadfastness, energy, and honesty, without which none can hope to secure even a single pebble on the shore of that great ocean—truth. We need only place before the reader the names of Valleix, Bizot, Barth, Grisolle, Rufz, Gerhard, Jackson, Maunoir, Woillez, Rilliet, Taupin, Nélaton, and D'Espine, to remind him of the extent and valuable character of services which youth, guided in its labours by minute observation and the numerical method, may render to positive medicine. And there is a related good accomplished, as well

for its cultivators as for the science itself, by M. Louis, the true bearing and importance of which do not appear to us to have been correctly understood or estimated. He has perfected an engine whereby *authority* may be resisted. He has unmasked that curious despotism (assumed by certain individuals in consequence commonly of their worldly position, or conferred on them by others who hoped thereby in turn to wield it,) which had been enabled to crush the efforts of young and ardent searchers after truth, did the results of these efforts—no matter by what diligence of observation and closeness of induction they were obtained, no matter how clearly the style of the narrative demonstrated the capability and conscientious exactitude of the observer—clash in the remotest degree with the dogmata it pleased themselves to originate or foster.* While the system of *authority* prevailed there was but one mode of reception for the results of *unpatronized* labour:

> "Tous ces discours sont des sottises,
> Venant d'un homme sans crédit;
> Ce seraient paroles exquises,
> Si c'était un grand qui les dit."

Had M. Louis done nothing more than thoroughly exposed the hollowness of all evidence founded on the authority of name, while he raised to its proper level that of facts, the single achievement would have ensured him an imperishable name among the benefactors of medical science. He has thereby, in truth, destroyed an ever active organ of evil, "the greatest and most irreconcileable enemy to truth and argument," (to use the energetic language of Bishop Hoadley, with a still higher topic for his theme,) "that this world ever furnished forth. All the sophistry, all the colour of plausibility, all the argument and cunning of the subtlest disputer in the world, may be laid open and turned to the advantage of that very truth which they designed to hide or to depress, but against *authority* there is no defence. It was authority which would have prevented all reformation where it is, and which has put a barrier against it where it is not."

* The spirit of unbounded respect for authority is, it would appear, only "scotched, not killed," on this side the channel. At least, we have been recently called on to notice a fact tending to exhibit the vitality of the mischief in a manner painful to all conscientious lovers of science. A learned and most amiable reviewer at the commencement of an article upon the recent work of certainly a very eminent and excellent man, but still a man, not only disclaims all intention to enquire into the correctness of a solitary one of the multitude of opinions he is about, by transcription, to aid in disseminating, but expresses his conviction that none will be found possessed of the temerity or malignity to venture upon so signal a breach of conventionalism. The temerity and malignity to labour in the cause of truth! Suppose a man distinguished for expertness as a chemist and an astronomer were, under the influence of "temporary mental derangement," to assert the moon to be made of cheese, the very first frosty night our philosophical reviewer of the *authority* school would, doubtless, distinguish the rind, smell the caseous odour, and clearly perceive parties of mites taking an airing on its surface, and probably exert his critical powers to determine whether the product of celestial churning belonged to the Stilton, the Chedder, or the Gloucester varieties.

## Art. III.

*Observations on the Religious Delusions of Insane Persons, and on the Practicability, Safety, and Expediency of imparting to them Christian Instruction; with which are combined a copious practical Description and Illustration of all the principal Varieties of Mental Disease, and its appropriate Medical and Moral Treatment.* By Nathaniel Bingham.—*London*, 1841. 8vo, pp. 213.

The delicate subject of administering religious instruction and consolation to lunatics has been very variously approached by modern physicians; and we fear that the religious excitement prevalent in almost every sect at the present period is not very favorable to its just consideration. Fanatics of every shade and persuasion will rush into extremes, and those who are not fanatics may be provoked into unseemly opposition. Yet the subject is a very serious one, and every medical superintendent of a lunatic asylum must feel the difficulty of determining on the course to be pursued, when error on the one side may cause important religious aid to be withheld, and on the other, a most hurtful excitement to be applied. Some excellent observations on the subject occur in Dr. Burrows's Commentaries on Insanity; but the subject has been apparently avoided by some subsequent writers. The few remarks made by Esquirol on this point are characterized by his usual good sense.

The simple truth seems to be this: as far as religious impressions can be made available to the government of the affections and conduct, they may be usefully encouraged even in a lunatic asylum: if they disturb the mind, they must be avoided, like every other cause of hurtful excitement. This, we suspect would not satisfy Mr. Bingham. He quotes the opinions of Dr. and Mrs. Button, the late surgeon and assistant-matron at Hanwell, to the effect, that lunatics "possess the power of reasoning in every degree, and in the greatest variety;" and that "the difficulty which lies in the way of their instruction arises, not from their want of abilities, but from their dreadful depravity." Mr. Bingham's notes of the opinions of Dr. and Mrs. Button appear to have been made some years ago; and he must surely be incorrect in ascribing such extravagant notions to them. At the time alluded to, nearly the whole staff of Hanwell were reputed to be methodists; but methodists, we imagine, would not accuse all lunatics of dreadful depravity, or assert that they were capable of reasoning in every degree. Sir William Ellis was, we believe, of the methodist persuasion; yet he employs much more reasonable language, far less offensive and unjust to the poor lunatics under his care. The subject of religion was scarcely, if at all, alluded to by him until he made his Fifth Annual Report of Hanwell; wherein he said:

"In former years, from the very incorrect notions entertained of this disease, religious and moral instruction of any kind was never thought of being afforded to the insane. Happily a better knowledge and a better state of feeling now exist. And it is at this time generally admitted that though on some points the mind may be insane, yet on others it may be perfectly rational. And it is no ordinary blessing to many of the sufferers that a just sense of religion often remains,

when every other feeling seems obliterated. . . . . Here the patients have the instruction of the Rev. J. Stoddart, the chaplain to the institution, and a more orderly and attentive congregation cannot be assembled together. Some of the committee and other gentlemen have frequently been present, and have expressed their astonishment and delight at witnessing the reverence and decorum of the patients." (Report. Dec. 1835.)

This is moderate and judicious; neither on the one hand containing any insinuation that in unsound mind, the mind is sane on every point, according to the alleged discovery of Dr. Button, nor conveying any libellous sentence upon poor suffering lunatics, as if their depravity was greater than that of the world in which they became mad. In the same reasonable spirit we presume Sir Alexander Morrison to have spoken, on that "fine summer's day" when Mr. Bingham describes himself as walking towards the Middlesex Lunatic Asylum with him; when "a lady of the party" asked Sir Alexander whether he thought the inhabitants of the great house before them were capable of profiting by religious instruction, and "many of them as capable as you or I, was his prompt and emphatic answer." So also Dr. M'Kinnon, of the Aberdeen asylum, when he assured Mr. Bingham that the greater part of his patients attended divine service twice on a Sunday, and that no congregation behaved better, does not seem to have brought this forward as any proof of their dreadful depravity. These words, we confess, haunt us. We should be sorry to think that they were ever seriously used to characterize lunatics by any one still exercising a control over an institution filled with them. We should apprehend that a view of them so limited and illiberal would taint the whole management, and even the whole manner of those entertaining it; and we cannot acquit Mr. Bingham of impropriety as respects recording it, whether Dr. Button be accountable for it or not.

It is rather remarkable that Sir William Ellis did not notice the subject of religious instruction in any of his subsequent reports; remarkable because we believe he was particularly open to religious impressions, and must therefore have abstained from the topic on account of the difficulty of saying what perhaps could alone be said without exaggeration, and at the same time of satisfying those whose ardent expectations in relation to it were not corrected by practical experience.

In the only Report published by Dr. Millingen, the successor of Sir William Ellis, the subject is not alluded to; but in his Aphorisms, which are quoted by Mr. Bingham, he expresses himself rather in a military way concerning it:

"Nothing can be more absurd," says Dr. Millingen, "than the assertion of the great benefit that arises from the patients being obliged to attend divine worship. The apparent tranquillity and attention with which they seem to listen to the chaplain's exhortations are merely mechanical. A lunatic seemed much affected at a sermon, and even shed tears with seeming contrition; the subject of the discourse was the Trinity. When questioned on the homily which thus affected him, he said it was one of the most beautiful sermons he had ever heard, all about the Emperor of Russia and the King of Prussia. Were it not for the moral discipline enforced in these asylums, and the presence of the keepers, their congregations would very frequently exhibit anything but a devotional appearance. During fourteen months' superintendence of Hanwell, out of upwards of one thousand patients, I had only four who were fit to receive religious consolation in sickness and on the bed of death."

As the lunatic congregation at the chapel at Hanwell on a Sunday is probably the largest of the kind in the world, we imagine the further testimony of the superintending officers of that institution down to the present time, although Mr. Bingham has not apparently looked into it, may be considered worthy of attention. In Dr. Conolly's First Report, in October 1839, he says:

"Since the commencement of August, the morning service has been read at eleven o'clock every Sunday, and the evening service read and a sermon preached at half-past three. Each service occupies about one hour. Some of the officers of the establishment invariably attend, and generally about 200 of the patients. A psalm and hymn are usually sung by the congregation, accompanied by the organ; and the responses to the commandments are chanted; by which means the service is so varied as to obviate fatigue or restlessness. The demeanour of the patients on these occasions is for the most part admirable. Few spectacles can be more interesting, or more affecting than that of so many lunatic persons, many of whom are at other times violent, noisy, agitated, and talkative, exercising so remarkable a degree of control over their behaviour for such a length of time. The practice of this control is, unquestionably, the principal advantage which many of the patients are capable of deriving from attendance on these services. Care is taken that they appear decently dressed; several of those who can read are supplied with prayer-books; and they evidently look forward to Sunday with pleasure; and are mortified when any accident interferes with their attendance in chapel. Yet a very small number of them seem to have any distinct religious impressions. Many are prone to the terrors of an alarmed conscience, and believe that evil spirits are immediately busy around them. A few present examples of religious conceit; several consider themselves to be divine persons. One asserts that he is the Almighty; and refuses to go to chapel, although he rings the chapel bell very diligently. In very few of the patients does religion appear to be a source of hope and tranquillity. The cautions which these circumstances render necessary in the attempt to administer religious instruction to them are too obvious to require to be dwelt upon."

The subject is again touched upon in the Second Report of the same physician, after the additional experience of another year:

"About 300 patients have attended at chapel twice every Sunday during the year. No familiarity with this spectacle can weaken its impressive effect. Occasional oddities of conduct and slight irregularities are displayed; but the general demeanour of the congregation is most remarkable.

"It happens almost every week that patients who are recovering from attacks of excitement, are petitioners for the indulgence of going to chapel. Whenever there is reason to think they may be trusted, their desire is complied with; and, generally speaking, they entirely control their conduct during the service. Exceptions of course sometimes occur; so many irritable beings being collected together.

"Among the old and incurable patients many are constant attendants at chapel; and, with respect to several of them, no manifestation of insanity is made which would lead a person unacquainted with them to conjecture that they were lunatics. Among the men, there are many examples of this kind; and the gravity and respectability of their appearance would cause an observer to be extremely struck with the contrast presented by their incoherent conversation when spoken to. Some of the men and women also, who would be deeply mortified by being excluded from chapel, practise a few eccentricities when there, but generally of so harmless a kind, that to exclude them would seem to border on cruelty. So accustomed are the patients to preserve their composure during the hour of service, that if, as sometimes happens, an epileptic patient utters a loud scream, falls into a fit, and requires to be taken out by the keepers or nurses, very

few of the patients quit their seats; and those in the immediate neighbourhood of the person affected usually render what assistance they can, and then quietly resume their places.

"Although so many male and female patients attend the chapel regularly, the physician has only found it practicable to recommend a very small number to the private attention of the chaplain. Whenever a desire is expressed to see him it is complied with; and to those who are seriously ill, it is often suggested that they should have some conversation with him. This is sometimes declined, but more commonly accepted. To make similar propositions to the numerous patients who think that nothing whatever is the matter with them, would probably give rise to morbid trains of ideas, which it is better not to excite. There are also some patients in the asylum whose thoughts perpetually dwell on religious topics; but with so much wildness and enthusiasm as to render it prudent not to encourage but rather to avert such ideas from their minds. Some of the patients admitted with a propensity to suicide, have appeared to be comforted by conversation with the clergyman. A great number of the patients are gratified by being allowed to have a Bible and Prayer-book."

We have quoted the preceding passages with the more satisfaction, because whilst they betoken no indifference to the subject of religion, they evince no signs of temporising in a matter on which to speak without cant is often to incur a great weight of theological hatred. We not long ago visited an asylum in which the chaplain, dissatisfied with the simple restriction of his visits to the patients who wished to see him, straightway proclaimed one of the officers to be a Unitarian, another an Infidel, a third a Socialist, and all the rest the church's enemies. In that asylum we listened, with dismay, to emphatic assurances to the melancholy and the mad that the devil was ever seeking to devour them. We heard them instructed in the reason wherefore in the bottomless pit they would be bound with chains instead of cords, (a sort of restraint-simile); namely, because the chains were the stronger; and for equally sound reasons they were given to understand that their sins were written not in common ink, but in marking ink, which would not wash out. They were invited, God help them! to the Lord's supper, not in our Lord's affectionate and affecting words, "Do this in remembrance of me;" but in the fierce language in which St. Paul perhaps not undeservedly addressed the Romans, when he told the sacrificers to Mars and Venus, who affected also to be Christians, that they could not partake of the Lord's cup and of the cup of devils! We were happy, at the time, to think that the poor denounced mad people were insensible to these insults; which, if they had possessed the exquisite reasoning power spoken of as belonging to them, must deeply have stung them, notwithstanding their "dreadful depravity." We dare say that precedents for all this strong language may be cited from the older divines; but our remarks apply to it as addressed to lunatics. It may be necessary to awaken hardened sinners by hard words; but the error seems to be that of confounding insane persons with hardened sinners.

Every reasonable person will see that the subject should be calmly entered upon; and we hope that in proportion to every medical man's anxiety for every lunatic to enjoy spiritual as well as all other advantages, will be his exertion to prevent such attempts to intimidate lunatics on religious matters, as would really be madder than madness itself. Mr. Bingham has, we think, entered on the subject with strong prepossessions, in consequence of which he sets aside, as he himself states, the

testimony "of a medical friend, who has seen as many insane patients as any gentleman in the kingdom;" and who told him that he had not "witnessed among them the good effect of religious instruction;" a gentleman, too, who does not question its propriety, but who doubts its being practicable to make a salutary impression among them. In the same way Mr. Bingham avows that he rejects the opinion of another witness from a quarter where he knows "the experiment has been long tried on the usual plan." This at least shows considerable adhesiveness in Mr. Bingham to his own opinion; which he at the same time affirms to be based on extensive observation. He thinks, without any perceptible reason for so thinking, as it appears to us, that the religious services in asylums are too generally merely formal; and even that patients are discouraged from attending. But we agree with him that, although the effect of a sermon may be inconsiderable, that of occasional short conversations may be beneficial; and he is perfectly correct in stating that many of the patients in asylums have been left all their lives in the grossest ignorance of religious truths. We can also readily conceive his eagerness to profit by their situation, if possible, "before they are sufficiently well to be discharged." But they must be affectionately addressed; they must love and respect their pastor; he must assume no airs of secular superiority; he must refrain from the scolding tone of one preaching to hardened convicts; and he must have sense and discretion enough to know when to desist, and when to hold his peace. If he has not this sense and discretion, and much real benevolence also, his labours will be vain, and his interference mischievous. The great principle in the government of lunatics is to refrain from irritating them; and, on every occasion, if not quite impossible, to proceed by methods of persuasion instead of force. If a few words in season are beneficial, a very few words out of season are equally pernicious. When these precautions are not despised, the chaplain may become a highly useful officer; and if he is a man of sense he will not consider it any degradation to be an auxiliary to the physician. Then it may be really ascertained in what proportion and description of cases the insane patient was capable of listening with patience and edification to religious conversation; and how much advantage the means of cure may receive from this exalted means. This will never be ascertained by those who consider the inmates of a lunatic asylum, or even those of them who delight in attending the religious services, as quite as capable of receiving the attention of a clergyman as any other persons of their class of life. The very maintenance of such ground would prove that the clergyman wanted the knowledge requisite for the office he had undertaken. But if, in addition to this want of knowledge, he should unhappily imbibe the notion of their "dreadful depravity," no more mischievous person could possibly be admitted into the wards. Indeed everything depends on the choice of a chaplain; and the attention paid to his qualifications cannot be too careful. When the choice is injudicious, the physician has an enemy within his gates, to whom to yield is to abandon his duty, and with whom to contend is to expose himself to imputations of disrespect for the clergy, the church, and even religion itself.

Mr. Bingham's sensible remarks on the Moral Treatment convince us that he would himself observe the utmost care in applying religious means

to lunatics; and he seems perfectly aware that there are some to whom such means could not be applied, and some to whom they might do mischief. He appears to us, to say the truth, when applying himself to the practical consideration of the subject, to have corrected his own views. At all events his remarks on the Mode of conducting the Religious Instruction of the Insane are characterized by judgment as well as earnestness. He observes, that the whole household of a lunatic asylum should unite in the attempts to make religious instruction or religious services acceptable to the patients; and that the spirit of the place should not be sectarian, but that which is preserved in the bond of peace. He lays down very good precepts touching the duties of the chaplain; whose office he regards as one enabling him materially to aid the superintendent, and to give his undivided attention to doing good. The suggestion of classing the more religious patients together, under a "pious keeper," is one which it would probably be found difficult to carry into effect; and the various means, admirable in themselves, which Mr. Bingham suggests as promotive of religious feelings in the insane, require the utmost good sense on the part of those endeavouring to apply them. Mr. Bingham does not overlook the frequent occurrence of religious despondency among the insane, and the cautious conduct to be observed towards those thus affected. He at the same time points out the common error of considering all cases in which the insanity is manifested by excessive devotion and enthusiastic religious talk, as cases originating in religious enthusiasm; showing that the devotional character is often only a part of the insanity, and disappears entirely on the patient's recovery from the disease; the malady itself arising from some disorder of the stomach or bowels. His opinion is, however, that the temporary religious feeling may be improved into a salutary habit, if the patient has the advantage of a minister or a religious friend. Mr. Bingham is, at the same time, quite explicit on the subject of other cases, in which excessive religious zeal has passed into insanity, and in which he acknowledges the necessity of withdrawing books, preachers, and injudicious friends. He does not, even in these cases, advise a total discouragement of religious ideas; by which he conceives the patient might be led to despair. In cases of melancholia he quotes Sir William Ellis and Dr. Conolly as having witnessed the good effects of music; and although such effects are probably not very durable, the frequent appliance of this and other means of promoting cheerfulness may be more permanently effectual. Reasoning, Mr. Bingham well observes, is of very little service in the case of delusions, whether religious or of any other kind; and the best thing is to lead the mind away from the delusive subject as much as possible. This is far preferable even to appearing to coincide in such delusions. They should seldom be alluded to; be contradicted calmly, if at all; and never ridiculed. Thus managed, they will often die away of themselves. Here, as in all parts of the treatment of insanity, there is wisdom in that avoiding of irritation of which we have already spoken. "Patients," says Mr. Bingham, "are sometimes *suddenly* set free from their illusions, but they are never talked out of them." In some instances of melancholy, he is of opinion that even the positive *excitement* of religion may be usefully tried; and the doctrines of Christianity preached with impassioned eloquence with good effect.

Upon the whole, there is so much that is deserving of cordial approval in Mr. Bingham's little work, that we feel justified in recommending it to all who have the care of the insane. The whole subject is attended with peculiar difficulty, and should be met in a candid, earnest, and philanthropic spirit. Above all things, let the attempt to extend religious instruction to lunatics be animated by a due consideration of their state and character; their state, the most pitiable that is incidental to God's reasonable creatures; and their character, not as spoken of by some of Mr. Bingham's witnesses, dreadfully depraved; but such as described by one who knew them well, and served them faithfully, and described them truly:—and whose words are quoted by Mr. Bingham. It is thus that Pinel speaks of them:

"I cannot here avoid giving my most decided testimony to the moral qualities of maniacs. I have nowhere met, except in romances, with fonder husbands, more affectionate parents, more impassioned lovers, more pure and exalted patriots, than in the lunatic asylum, during their intervals of calmness and reason. A man of sensibility may go there every day of his life, and witness scenes of indescribable tenderness associated to a most estimable virtue.

"Let us then approach them with kindlier feelings than have been usually cherished. Let us treat them as friends and as children. Let everything possible be done to alleviate their sufferings, to console and make them happy. And if we cannot win them over to piety, let them not have to say that our harsh conduct has confirmed their dislike to our principles."

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## Art. IV.

*A Treatise on the Nervous Diseases of Women; comprising an Enquiry into the Nature, Causes, and Treatment of Spinal and Hysterical Affections.* By Thomas Laycock, M.D.—*London*, 1840. 8vo, pp. 356.

This volume being on a very important subject, we shall enter into a somewhat minute examination of its contents. These are very systematically arranged under the three general heads of Special Physiology, General Pathology and Therapeutics, and Special Pathology and Therapeutics, each part being subdivided into numerous chapters.

Part I. The first chapter of the first part is occupied with Principles and Definitions; and this is divided into two sections, of which the first includes what are considered by Dr. Laycock the general principles of the class of diseases of which he treats. These principles are four:

1. *The nervous system is the seat of hysteric diseases.* This is established, in the opinion of Dr. Laycock, by the "concurrent testimony" of all modern writers. Now, as we by no means assent to the general proposition that "what everybody says must be true," we are not prepared to surrender our own judgment to the overpowering force brought up in array by our author; and we think he shows a little indefiniteness in the ideas he attaches to the term "seat of disease." That the phenomena of hysteria are principally produced through the instrumentality of the nervous system cannot be questioned; but it does not follow that it is the part of the structure primarily affected. Indeed, Dr. Laycock himself defines the morbid state he is about to discuss as one in which there is no organic disease (by which we suppose that he means no structural change) in the nervous system. As we are not believers in the pos-

sibility of the occurrence of any morbid phenomena, without a disorder either of the structure exhibiting them or of the stimuli exciting them, we must ask Dr. Laycock whence the disordered action of the nervous system witnessed in hysteria originates. If in some primary change in the nervous system itself, then there *is* an organic disease; if from sympathetic relation with some other organ, then the nervous system is *not* the seat of the disease. We believe that from either of these sources, hysteria may develope itself. We are perfectly ready to go along with Dr. Laycock in the affirmation that the phenomena of hysteria immediately arise from a disordered condition of the nervous system; and this is all that he requires for his subsequent argument.

2. We are compelled to take exception also to his second principle, that *hysteria is peculiar to females;* for, as he himself admits, the phenomena of this disease occasionally present themselves in the male sex. According to Dr. Laycock, this exception serves but to prove the rule; but if the rule be understood to mean, as the words manifestly import, that there is something in the nervous system of women which renders them alone susceptible of the disease, it is obviously untenable.

3. The third principle, that *women of susceptible nervous system are more liable than others*, needs no comment, since universal observation supports it.

4. The last, however, that *hysteric diseases appear only during that period of life in which the reproductive organs perform their functions*, may require, like the second, a little qualification for exceptional cases. We trust that in these remarks we shall not be thought guilty of a carping and merely verbal criticism. Were these principles enunciated a little less *ex cathedrâ*, we should have felt ourselves less called upon to animadvert upon them; but the somewhat dogmatic style in which they are put forth challenges doubt as to their universality. A few statements of a similar character occur elsewhere, such as the following, with which we can by no means accord: "The advance in our acquaintance with the anatomy and functions of the brain and nerves, made during the last thirty years, is such as to render it certain that ultimately all generalizations, as well pathological as physiological, must be solely based upon the anatomy and physiology of the nervous system." Our opinion is just the contrary; for we hold that the tendency of physiological science at the present time is to reduce the vast amount of phenomena which, for want of some better explanation of them, have been regarded as originating in the nervous system, and to distinguish those which are *influenced by* that system from those which are *essentially dependent* upon it. We may refer, in illustration, to what we have formerly said upon the physiology of the capillary circulation, and the pathology of inflammation, (see vol. VIII., p. 188, &c.)

In the second section of the first chapter are discussed the General Relations of the Reproductive Organs; and here we find ourselves able to go along with our author with little exception. The reproductive organs are defined as essentially consisting of the ovaria in the one sex and the testes in the other, for which adequate reasons are given. The following very just observation concludes this section:

"It is by no means certain that all the sexual differences depend upon the sexual organs; on the contrary, it is highly probable that there is an organi-

zation and development of the primitive tissues, to which these are subservient; in short, that the ovaries and testicles being subject in common with the whole system to this primary impulse or nisus, owe to it their own distinctive characters, and have the secondary influences they exert modified in such a manner in consequence, as to bring out the special difference of the sexes, as seen in the presence, texture, and configuration of certain special organs." (p. 13.)

This view is far more conformable to the general analogies of nature than that which supposes the sex to be determined by an *accidental* enlargement or contraction of certain arterial branches during the progress of development.

In chap. ii. are displayed the *Special Relations of the Reproductive Organs*, which are very systematically treated of under the following heads: 1. Relations to the Cutaneous Products, in regard to the colours, appendages, subtegumentary fat, and odours, peculiar to each sex in different tribes of animals; all of which peculiarities are the most strongly marked at the period of greatest sexual excitement. The extent of the subject of course prevents anything more than a sketch from being here admissible; and this is very clearly given. 2. The Pathological Relations of the Cutaneous Products to the Ovaria and Testes. Under this head a great number of interesting facts are brought together relative to the changes which occur in the skin and its appendages, in consequence of disease, removal, or senile loss of function, of the reproductive organs. 3. The Physiological and Pathological Relations of the Ovaria and Testes to the Lumbar Region. "There is an influence," says Dr. Laycock, "exerted by the sexual organs on the muscular and osseous structures connected with this region, which is possessed by no other viscus; this is very distinctly shown by the difference of size of the male and female pelvis and abdomen." Now it appears to us that this doctrine is completely at variance with that which we just now quoted with approbation; since the larger size of the pelvis cannot be regarded as a *consequence* of the development of the ovaria, it being one of the concurrent changes originating in the same general nisus. 4. The Physiological and Pathological Relations of the Ovaria and Testes to the Thoracic Region. The general relation between the sexual organs and the neck and throat has been a matter of common observation from the time of Hippocrates downwards. A superior size of the neck and thoracic region is observable in the greater proportion of male animals; though there are some curious exceptions. The contained organs, too, are more developed; and the superior power of the voice of the male, almost throughout the animal kingdom, together with the approximations to the contrary character which occur in both sexes under the influence of sexual disease, attest in a remarkable manner the relation between them. Certain diseases of the thoracic viscera are much more frequent in men than in women; such are angina pectoris, aneurism of the heart and large vessels, and thoracic asthma. The relations of the glandular structures of this region to the sexual system are very remarkable. They are elucidated rather by pathological phenomena than by comparative observation of the lower animals. The affection of the testes in man and of the mammæ of women (perhaps also of the ovaria) as a consequence of parotitis, and the increased secretion from the salivary glands, are familiar instances. 5. The Physiological and Pathological Relations of the

Ovaria and Testes to the Organs immediately subservient to Reproduction. These, in order to avoid repetition, are only partially discussed here; the principal attention being given to the phenomena of menstruation, on which the most recent information is given. Dr. Laycock reverts to the doctrine of the older authors, in considering the period of menstruation as analogous to that of heat in the lower animals; there being at this period an excited state of the ovarium, which is accompanied by a shedding of ova ready to be fructified. "But we shall find," he adds, "that this periodic movement is not limited to the ovaria; but that it is an affection of the general system in which the ovaria partake; and that it is through these the secondary system in connexion with them is influenced, and all the attendant phenomena (those of menstruation) excited." (p. 43.)

In chapter iii. *the Periodic Movements in the Reproductive Organs of Woman* are considered. The most obvious of these, that of menstruation, is treated of in section i. This is considered by Dr. Laycock as only an example of a large class of changes in the human system, which are governed by certain *periodic* influences.

"Not only are there periodic influences ever acting upon man which are derived from without, but from the moment of his conception (and perhaps before,) series of changes commence, enchained by definite laws, and depending for their due development upon agencies seated in the individual organism. The more prominent links of these series are too obvious to escape notice; and the septennial phases of man's existence were remarked at an early period. The quotidian movement, as indicated by sleep and wakefulness, and the regular daily recurrence of appetites and functions, could not fail to be observed; and the monthly movement, from its marked occurrence in females, was as easily distinguishable. Since the phenomena of these periodic movements are open to observation, there can be no doubt that it is possible to arrive at a knowledge of the principles by which they are governed." (p. 45.)

In pursuit of this enquiry, our author brings together a number of instances in which menstruation occurs at regular periods differing from that ordinarily known, as, for instance, two, three, or five weeks; and he concludes that "although there be no flow of the catamenia every week, yet a change takes place at that period in many females, more or less felt as the system is more or less robust; but strongly shown when excitability of the nervous system, either general or local, exists, and the phenomena of disease are developed, as paralysis, spasm, insanity, or hemorrhages." We should like to know a little more minutely the evidence upon which this rather novel conclusion is founded. If true, it is certainly important; but we are by no means satisfied of its validity. That the menstrual change occurs during pregnancy was maintained by the ancients; and Dr. Laycock thinks it extraordinary that the doctrine should have fallen into neglect. There is a looseness of statement, however, on this point, which leaves us in doubt as to our author's meaning. Does the menstrual change in the ovaria take place as well during pregnancy as in the previous condition? Or is it only the attendant phenomena, the pain in the loins, the tumescence of the mammæ, &c. which occur? Dr. Laycock endeavours to strengthen his position as to the *weekly* excitement of the sexual system of the human female by a reference to the periods of utero-gestation of the lower animals, a large and valuable collection of information on which head, as also on the period

of incubation in oviparous animals (which is evidently an analogous state), is contained in section ii. of this chapter. In spite of several exceptional cases, Dr. Laycock concludes that in all mammalia the period of utero-gestation may be stated in terms of *weeks*, in other words, is a definite multiple of the hebdomadal period; and that the period of incubation in birds is normally the same, although liable to be influenced by temperature, domestication, and other circumstances.

In the third section are collected facts relating to the Periodic Changes of Insects, which are regarded by Dr. Laycock as strengthening his position, that periodic changes occur in animals at periods of weeks or multiples of weeks. These facts are not stated in sufficient number or with sufficient definiteness to be satisfactory to us; but, as Dr. Laycock remarks, a *rigidly exact* series of observations on the time occupied between the various changes and developments of insects is a desideratum.

In the fourth section is briefly noticed the Periodic Recurrence of the "Heat" of Mammals, already spoken of as analogous to the menstruation of the human female. This also is known to recur at definite intervals; and although usually absent during pregnancy, yet it occasionally (like the catamenia in some women) displays itself throughout. The *rutting* of the males is somewhat analogous to the heat of the female; and its periodical recurrence appears to be as regular. In both, according to Dr. Laycock, the hebdomadal period or its multiples may be observed.

In the fifth section are considered the Vernal and Autumnal Changes which all living beings undergo, and which seem especially to affect the sexual system. The following is Dr. Laycock's general summary of the questions discussed in this chapter:

"First, that at hebdomadal periods or their multiples, a change occurs in the system of all animals; and that at puberty, and each menstrual period subsequently, the sexual organs of the female share in this change in common with the whole system, the phenomena resulting [in the human and some other species] being those of menstruation; and that, consequently, the flow of the catamenia is no more than a local effect of the general cause. Second, that the system of all animals undergoes changes about the vernal and autumnal equinoxes, and probably about the summer solstice, in which the generative organs and their dependencies partake; and that the causes of these changes modify in mankind all diseases, but those more particularly connected with the reproductive functions." (p. 70.)

A short chapter (iv.) next follows on *the Relations of the Reproductive Organs to the Nervous System in general;* a subject which is obviously of very extended scope, and which might, we think, have been with advantage treated a little more fully by our author. Among the influences on the system at large produced by the state of the reproductive functions, Dr. Laycock notices the pugnacious propensities remarkable in the males, and the artfulness of the females, qualities which are not without their parallel in the human race. Another remarkable effect of the change in the system at large, induced during the performance of the reproductive functions, is a loss of appetite or cessation of its indulgence, such as is peculiarly manifested in insects. The influence of physical love on the appetite of men and women is a matter of daily observation; and bulimia, pica, and strange longings are morbid modifications of the appetite, and belong to the same class of phenomena as this anorexia, —like it, being characteristic of the pregnant, chlorotic, and hysterical

female. The whole nervous system is excited by the sexual stimulus, as much as by medicines which have a direct and powerful influence on it. It has been remarked that in frogs, just before the period of copulation in spring, and in the autumn also, the nervous system is endowed with a peculiar irritability; so that the slightest touch will at these times produce the effects of powerful excitants at others. A corresponding state occurs in the morbid sexual excitement of women, known as nymphomania; in which the system becomes endowed with all the irritability observed in hydrophobia, the slightest touch (as in frogs) inducing tetanic spasms. In satyriasis the symptoms are somewhat analogous. Our practical readers will doubtless call to mind phenomena of a similar description in milder cases of hysteria; and it is interesting to be aware that they find a parallel in the natural condition of another species.

In chapter v. are discussed the *Mental and Corporeal Peculiarities of Woman.* The *affectability* of her nervous system is first alluded to, as the governing principle, so to speak, of her constitution; an observation in the truth of which observers of human nature in all ages have concurred. This peculiarity is at the same time the cause of her weakness and frailty, and of the strength and nobleness of her character under particular circumstances. Dr. Laycock then adverts to those peculiarities of her configuration, which, though influenced perhaps by the development of the ovaria, are by no means dependent on them; and quotes Meckel and others in regard to the very early period of fœtal development at which these distinctive marks may be perceived, a period anterior to that at which any perceptible difference in the sexual organs manifests itself. Even at a more advanced period, there is still a manifest difference in the corporeal structure of the sexes, long before the period at which the generative system begins to exert its peculiar influence; this has been well shown by the researches of Quetelet. Further, the male and female which are deprived of these organs at a very early period retain these peculiarities through the whole of life; thus the castrated boar is readily known from the spayed sow by his larger limbs and more massive bones, although both may have belonged to the same litter, and may have been mutilated at the same early age. From these facts, Dr. Laycock is inclined to the opinion of Cabanis, that the peculiarities of the sexes, whether mental or corporeal, including those referred to the genital organs, depend upon some particular organization of the primitive nervous system. Now this hypothesis assumes that the development of the nervous system governs that of all other parts, a doctrine which is neither conformable to analogy nor to observed facts. If, for example, we extend our survey to the sexual system of plants, and investigate the conditions under which the sexes of the diœcious species are respectively produced, we find them subject to no influence from a nervous system; and there have been instances of abnormal development of human fœtuses in which the absence of both brain and spinal cord have demonstrated the essential independence, on the part of the formative processes, of the agency of the nervous system. We have stopped to advert again to this question of the influence of the nervous system, because we deem it of the first importance that, if principles are to be sought out and followed at all, they should be correct ones. Dr. Laycock does not rest satisfied, however, with this doctrine; but, perceiving that it offers

nothing very tangible for consideration, he directs attention to the vascular system as the one which presents the most evident differences. We think that in this course he is more theoretically correct than he himself supposes; for on the functions of the vascular system depend alike the first formation of nervous matter and the development of its peculiar properties, and also the excitement of these properties to action; and it is here, therefore, that we should look for the first cause of the peculiarities of that action. In the following remarks there is much justice and acuteness:

"It is clear from these statements that the vascular, no less than the nervous, system is implicated in hysterical disorders; and it is equally obvious that the term hysteria is singularly inapplicable, and the term nervous diseases altogether vague. Perhaps a good general term for the whole class of hysteric and hypochondriacal diseases would be *neuræmia*, (*νεῦρον* and *ἅιμα*), and *neuræmic*; being distinguished from organic diseases of the nervous system in this, that in the latter it is not so much the blood as the blood-vessels which are diseased, as in paralysis, apoplexy, &c.; thus *neuræmic* neuralgia would be distinguished from real tic-douloureux dependent on organic disease. It is true one class passes insensibly into the other; but this happens in all classifications of vital structures and functions." (p. 84.)

The distinction here drawn is an important one. It has been repeatedly shown that the quality of the blood is lowered by frequent hemorrhage, and that this very condition predisposes to further hemorrhage, in consequence of the altered state of the tissues surrounding the vessels, the more ready permeation of the diluted fluid, and the tendency to stagnation which results from its diminished power of ministering to the proper functions of the capillary circulation, and of moving through these minute tubes. This condition of the blood is, therefore, a most fertile source of morbid action; and is nowhere more so than in the nervous system, the dependence of whose functions upon those of the vascular is so extremely close. Yet its results are often spoken of as *functional* diseases; as if there were any morbid action which had not *some* material cause.

The succeeding chapter, in which are considered some points in the metaphysics and physiology of the nervous system, is to our minds the least satisfactory of the whole work. There is a good deal of vague and pointless allusion to the doctrines taught in the ancient schools of philosophy, mixed up with speculations respecting the molecular actions of matter, which do not seem to lead to any definite end; in fact we are at a loss to know what our author himself had in view.

In chap. vii. is given a general sketch of the present state of our knowledge of "the brain and sensitive nerves as the organs of consciousness;" and the next chapter contains, under the title of "the movements of animals in relation to consciousness," an outline of Dr. M. Hall's and Mr. Grainger's researches on the phenomena of reflex action. These topics are rather beyond the proper limits of such a work as Dr. Laycock's; but he has introduced them as preliminary to the subject of the next chapters,—"The instinctive actions in relation to consciousness: the brain subject to the laws of reflex action;"—and "the action of the will, and of internal and external stimuli, on the hemispherical ganglia." In the former of these it is shown that the actions commonly regarded as *instinctive*, and those resulting from the *direct* influence of the passions

and emotions, are of the same general character as those termed reflex by Dr. M. Hall; being performed without the intervention of the will, through a system of nerves distinct from those which convey the influence of volition, and being immediately excited by external stimuli. On this question we have recently dwelt at length, and shall not therefore here revert to it.

Our author's chief object in the succeeding chapter is to direct attention to a class of phenomena, which he believes to constitute an important part of those ordinarily known as the nervous diseases of women, and which result from the power of the will, not only over the motor but over the sensorial fibres of the brain. This is exerted in the ordinary acts of memory, in which a change is voluntarily produced in the sensorial fibres, corresponding to that which the object itself, when presented to the organs of sense, had occasioned. On this principle, Dr. Laycock considers, may be explained many of the phenomena of Mesmerism, which are at first produced by an act of the "sensorial will," but which subsequently become so easily excited, as to recur almost involuntarily, or upon some slight external stimulus. Dr. Holland has shown the importance of the same general principle, in the aggravating influence of attention to the state of the digestive system, on the part of hypochondriacal patients; and Dr. Laycock refers to the recent work of Professor Dubois upon hypochondriasis, as developing it still more fully. This writer divides the phenomena into three stages: in the first the mind only is affected; the patient is harassed by imaginary diseases, and concentrates his attention upon one or other of the viscera—hereby changes in their innervation are excited, when the second stage is developed; and in the third, these merely nervous affections terminate in organic disease of the affected organs. "The effect of the phenomena of Mesmerism," it is very justly observed by Dr. Laycock, "will be useful in this, that the attention of philosophers will now be more than ever directed to the action of the will on the sensorial fibres of the brain, and through these on the sensitive nerves,—to the laws by which it is governed, and to the changes in distant organs which follow its agency."

Our author next proceeds to consider "the pathological and physiological relations of the nervous system to the ovaria," to which chap. xi. is devoted; and he begins with a general survey of the pathological relations of the spinal cord, especially as regards those complex and puzzling phenomena which result from an irritation in some part of the body, very remote from that in which they manifest themselves. It has been long since remarked, that a disorder in any of the viscera was likely to affect the portion of the spinal cord with which it was connected, and thence to produce sympathetic feelings or movements in various parts of the body. Thus, intestinal or visceral irritation is well known to cause convulsions; and paraplegia has been seen to exist in several instances without appreciable disease in the spinal cord or its membranes; the kidneys being in a state of suppuration, and having produced a functional disturbance of the nervous centres with which they are connected. Not only may the system at large, or various parts of it, be thus morbidly affected by a local disorder, but its condition will be also very much influenced by the state of the mind, especially of the feelings and passions;

which probably act upon the roots of the sensory nerves, and produce changes in them which are referred by the mind to their peripheral extremities, and which may (on the principle just referred to) become an actual cause of disease in the parts to which they are distributed. Dr. Laycock alludes to the singular march of erratic erysipelas, (well described by Dr. Graves) as easily understood on the supposition that the disease is caused by a morbid state of the nervous centres from which the nerves of the part take their origin: the disease advancing symmetrically, and invariably attacking in succession those parts of the skin which are supplied from the respiratory tract. If these general views be correct, it is very easy to understand how the connexion of the uterus and ovaria with the lumbar portion of the spinal cord should give rise to so great a variety of both functional and structural disorders; and how also these organs should be sympathetically affected by remote causes, operating through the passions and feelings of the mind, as well as more directly through the body. The value of the speculation, however, can only be determined by an extensive series of observations made with special reference to it.

Part II. *On General Pathology and Therapeutics.* In order to avoid repetition, as well as the appearance of ungraciousness towards our author in the too frequent dissent from his opinions, we shall, in commencing our notice of this portion of his work, state our general objections to some doctrines and some peculiarities of reasoning by which it is pervaded. When speaking of the periodical movements of the living body, we had to call in question the accuracy of Dr. Laycock's notion of hebdomadal terms. The evidence adduced in its favour is to our mind extremely feeble; whereas, nothing short of demonstration could sanction the adoption of a principle which would change the whole aspect of theory with respect to the diseases of women, and thence with respect to all disease, inasmuch as it would tend to subvert those general pathological views which have hitherto been considered as equally applicable to both sexes. For example, our author tells us that "the fourth day, and the seventh, eleventh, and fourteenth, are critical days, and connect the doctrine of crises with the menstrual period." (p. 150.)

Now, to say nothing of the arbitrary solution of a much disputed question—whether critical days have any real existence—we have here an hypothetical relation set up between certain crises, which, if they exist at all, are common to the diseases of both sexes, and a periodic function which is peculiar to one sex. If critical days be connected with menstruation in woman, what are they connected with in man? And if Dr. Laycock's view of the subject be just, must we not adopt perfectly distinct principles of pathology and treatment in fever as occurring in the two sexes?

Another remark we have to make is that our author appears to have adopted too implicitly, and applied too extensively, an idea of Dr. Henry Holland's, which we admit, nevertheless, to be well worthy of attention; namely, that many of the anomalous diseases of women may, in truth, be referrible to the gouty diathesis, which, being prevented by some peculiarity of the female constitution from developing itself in any of its more familiar forms, assumes others less readily cognizable.

The following sweeping assertion will sufficiently show the extent to which Dr. Laycock has been carried away by this notion:

"It may be stated here generally that, of the many cases related by authors as anomalous hysteric diseases, by far the greater portion were connected with a gouty diathesis, as indicated by the formation of calculi, by the occurrence of regular paroxysms of gout, and by the descent of the individual from gouty ancestors; they are cases, in fact, which would have been better understood and better treated, if they had been termed anomalous gout; but as the subjects were young females, they were, of course, set down as anomalous hysteria." (p. 163.)

Nothing can be more unsatisfactory than a *general* appeal to authors, when a new position is to be established or a disputed point settled.

Such premature pathological conclusions as these may naturally be expected to lead to questionable views of practice. Speaking of the material cause of gout, which he conceives to be an excess of urea or other constituent of the urine, Dr. Laycock remarks that, "When we consider the composition of this *materies morbi* in its general relations, many useful inferences, both theoretical and practical, may be deduced. Nitrogen is in the largest proportion, and the most important part of any of its constituents, and hence the utility of such muscular exercise in gout and hysteria as will 'use up' (if I may be permitted the phrase) the superabundant nitrogen present in the system. Hence also the propriety of a diminished consumption of animal food, and regular exercise as a prophylactic in both diseases; and hence also the applicability of the same medicinal agents to the cure of both." (p. 170.)

We suspect that the benefits arising from muscular exercise in the cases alluded to are attributable quite as much to the free distribution of the blood and the due appropriation of nervous power as to the consumption of nitrogen. Dr. Laycock's proposition that a diminished consumption of animal food is prophylactic of hysteria is too general to be either reasonably maintained or easily confuted, but we are quite sure that there is a very large proportion of cases to which it will not apply. We are equally sure that nothing but an hypothesis could have driven our author to so singular an assertion as that the same medicinal agents are applicable to the treatment of hysteria and of gout. The only legitimate use at present to be made of the presumed origin of some anomalous diseases of women in the gouty diathesis is to observe on a large scale, and to record numerically, the diseases prevalent among the females of gouty families. This might lead to the discovery of important relations; but we apprehend much evil from hasty assumptions on the subject. The speciousness and apparent extensibility of the notion in question are likely enough to render it popular, and we should not be much surprised, some time hence, to meet at every turn a clandestine gout skulking in neurotic garments, and to find colchicum the fashionable medicine of the day.

A third general objection we have to make to our author's manner of handling his subject is his proclivity to vague analogies. For example, he occupies a considerable space, and gives himself no little trouble, in showing a sort of affinity between the symptoms of hysteria and those produced by certain animal, vegetable, and mineral poisons; those caused by excessive loss of blood; and those attendant on chorea, tarantismus, laryngismus stridulus, and other convulsive affections; but what is this, after all, but making a long-winded announcement of the fact that the

effects of morbid or deleterious causes which powerfully influence the nervous centres, have more or less resemblance to one another, inasmuch as they implicate the same organs and disturb the same functions? Analogies of a similar kind, might, with as much truth and as little utility, be shown to exist among various phlegmasiæ, local injuries, and other causes which severely disturb the functions of the vascular system.

The remarks we have already made may have prepared the reader for the last general exception we have to take against Dr. Laycock's mode of philosophising.

It may perhaps be deemed a Milesian piece of criticism on our part to make a *general* objection to *generalities;* we do so, however, to supersede the necessity of noticing all the instances, which we regret to say are not few, where five minutes' reflection would have preserved our author from making general assertions which he would find it extremely difficult to support by facts. Some passages, on which we shall have occasion to comment in the sequel, will be found amply to justify our present remarks.

Having made these preliminary strictures, we are in a condition to follow our author through the principal details of his subject. In chap. i. of part ii. he makes some general remarks on the nervous diseases of women, and expresses his conviction that "the best mode of threading the labyrinth in which these diseases are involved is, to avoid the usual scholastic method of treating them, and instead of considering their proximate, remote, predisposing, or exciting causes, their various modifications, and their general pathology, to examine into their physiological and pathological relations, taking as our guide the general principles of the division devoted to their special physiology." He proceeds in chap. ii. to treat of the "general relations of the nervous diseases of women to growth, development, and decline," and the first section is on the "relations to growth and development antecedent to puberty." We cannot say that we here perceive much direction in his remarks. He contends, perhaps justly, that the excitability of the cerebral hemispheres is less in infancy and childhood than in adult age, and supports his opinion by the remark of Sir B. Brodie, that the proportion of recoveries from wounds of the brain is especially small in adult patients. Dr. Laycock adds a table, giving the results of eighty-one cases of injury of the brain, collected from various publications, and from his own notes. It is as follows:

| | Whole number. | Recovered. | Died. |
|---|---|---|---|
| Patients under 10 years of age | 8 | 6 | 2 |
| Patients between 10 and 15 (inclusive) | 24 | 18 | 6 |
| Patients between 16 and 21 (inclusive) | 8 | 7 | 1 |
| Adults of all ages above 21 | 41 | 14 | 27 |
| Total | 81 | 45 | 36 |

All tables of this kind are valuable, but we do not think the one before us greatly favours the notion in support of which it is adduced. In the first place, we cannot admit the result of injuries of the brain as a fair measure of its excitability at different ages, because other organs as well as the brain undergo modifications by age, and the relation of the brain to these organs is also modified; and such modifications have probably

great influence on the result of injuries of the brain as regards life or death, independently of the amount of damage sustained by the brain itself. Secondly, though the table shows that the mortality is greatest in adults, it does not make it least in infants; which it should do in order to bear out the views which we shall immediately find our author propounding. He regards dentition as a period of general development, and maintains that its painful or irregular progress may be much oftener an effect coexistent with convulsions, than a cause of the latter. Here we believe that he is correct; but when he endeavours to trace an analogy between the phenomena of the first dentition and those of hysteria, and declares that there is a peculiar state of the nervous system of infants during dentition, analogous to that at the commencement of menstruation, and its periodic return, we have only to say that we think some of our preliminary remarks here become particularly applicable. At the period of the second dentition, Dr. Laycock conceives that the brain has acquired a higher excitability, while the sensibility of the mucous membrane has diminished, and this in proportion to the growth of the individual: so that few of the phenomena which attend the first dentition are observed in the second: the exceptions to this rule being found in delicate, excitable children, whose development is retarded. These circumstances, combined with the influence of incipient puberty, occasion, according to our author, a diversity in the phenomena of nervous disease in the two sexes, convulsive affections assuming the form of epilepsy in boys and of chorea in girls. The latter affection he regards as the precursor of hysteria; in fact, as the hysteria of the female child: and he observes, that "it will be found accompanied by the same phenomena, *mutatis mutandis*, as accompany the first evolution of the teeth." (p. 136.)

The necessity for the qualification here introduced cannot be better shown than by recurring to the statement made by our author, only in the preceding page, that "few of the symptoms which accompany the first dentition are observed in the second." Now, since the phenomena of chorea are associated with those of the *second* dentition, and this presents phenomena widely different from the *first*, it appears to us that *mutatis mutandis* here amounts pretty nearly to *mutatis omnibus*, with which qualification we freely admit the analogy between chorea and the first dentition. Such contradictions as these are the inevitable consequences of reasoning, or speculating rather, on intricate and extensive subjects without sufficient data. We must further observe that Dr. Laycock's affirmation with respect to epilepsy, as the convulsive affection of boys and chorea of girls, is a very gratuitous one, and opposed to the best authorities, as well as to daily observation. With respect to the relative frequency of epilepsy in the two sexes, the statements of authors vary so much as to lead to the inference that the occurrence of the disease is not much influenced by sex, though it is probable, on the whole, that males are most subject to it.* On the other hand, though chorea

* Dr. Copland says, "The influence of *sex* is not remarkable; and is not manifested until after the second dentition. According to Esquirol and Foville, females are more subject to the disease after this epoch than males. At the end of 1813, 162 male epileptics were in the Bicêtre, and 289 female cases in the Salpêtrière. J. Frank found that of seventy-five patients, forty were females; but he agrees with Celsus, Heberden,

is decidedly more frequent in females, the preponderance is not such as to justify us in regarding it as at all peculiar to them. Indeed Dr. Laycock himself supplies us with abundant proof to the contrary in a table constructed by M. Rufz, of the Hôpital des Enfans Malades. Out of 189 cases, fifty-one were males; and our author, after citing Drs. Manson, Heberden, and Elliotson, acquiesces after all in the common estimate, according to which the proportion of girls affected with chorea to that of boys is about three to one. It is unaccountable that a man of Dr. Laycock's sense should thus make round assertions, as if for the mere pleasure of disproving them himself. This would be of less consequence if the disproved assertions were excluded from the author's reasoning, but they too often continue to form an essential part of his argument.

We should mention, that the table by M. Rufz, published in the Archives Générales for February, 1834, is referred to by Dr. Laycock, to show that the liability to chorea increases as the period of puberty approaches. (pp. 135-6.)

Sect. ii. treats of the "relations to the period between puberty and marriage, or between the ages of fourteen and twenty-one." The author makes some observations on chlorosis as one of the earliest diseases of the period which commences with the evolution of the sexual organs, and then on the hysterical disposition which accompanies a more advanced state of development. He regards hysteria as capable of hereditary transmission, in which we agree with him; and he makes some good remarks on the influence of the present system of moral and physical education in deranging the nervous functions of young females. He notices the effect of inordinate impressions on the external senses, and especially of music when too ardently cultivated, and animadverts with justice on the general want of attention to these points.

The third and concluding section of this chapter contains some brief remarks, which need not detain us, on the "relations to the period between the age of twenty-one and the cessation of the catamenia."

Chap. iii., comprising only half a page, is on the "relations of the nervous diseases of women to the uterus and ovaria." The author very properly concludes that hysteria is by no means necessarily dependent on disease of the uterine system, and that chronic hysteritis is in many cases an effect, not a cause, of the hysterical state. He affirms also that when the reproductive organs are the seat of the exciting cause of hysteria, the morbid changes they exhibit resemble those induced by conception rather than by organic disease. He states that the most constant appearance observed in the bodies of those who have died of hysteria has been enlargement of the ovaria, with vesicles containing an albuminous fluid, or resembling the Graafian, that such were the appearances observed by Villermay in a young female who died of hysteria induced by fright, and that the same author cites Riolan, Blanchard, Binninger, Vesalius, Diemerbroeck, and Morgagni, as having witnessed somewhat similar changes. The evidence, we think, is here too slight to justify any

and Soemmering, in believing that if a strict diagnosis were established between this and other convulsive diseases to which females are very liable, particularly several of those seizures described in the article *Convulsions*, the predominance would be found on the side of the males; and Drs. Cooke, Elliotson, and Cheyne, are of the same opinion." —*Dictionary of Prac. Med.*

general conclusion; we are not, however, inclined to dispute that of our author, being ourselves in a state of acknowledged nescience.

Chap. iv. is on "the relations of nervous diseases to periodic movements." It contains two sections: the first treating of the "relations to periodic movements dependent on cosmological influences;" the second of "relations to the weekly periodic movement." The author's observations on these subjects are rather indefinite, as might indeed be expected from the absence of all sufficient materials for reasoning; nevertheless, they may be read with advantage, as pointing out the recurrence of various forms of disease at various periodic terms, a series of phenomena which, for a long time to come, we should content ourselves with observing, recording, and comparing; any attempt at theory being in the present state of our knowledge entirely futile.

Chap. v. is on the "relations of nervous diseases to alterations in the composition of the blood." The author states that he does not here propose "to review all the extensive relations which nervous diseases bear to the composition of this fluid, but only to notice those points which are practically useful, or may lead to more enlarged views, or more correct observations." These points he conceives to be "the effects of blood-letting, and other exhausting agencies, of certain poisons which act by entering the circulation, and the phenomena of gout considered as being caused by a materies morbi circulating with the blood." (p. 152.) This chapter contains allusions to some curious cases, and the enumeration of a great variety of nervous phenomena produced by divers causes, and involved by the author in those analogies to which we have already represented him as, in our opinion, unduly addicted. Indeed our general remark on his proneness to vague analogy was more immediately suggested by the perusal of this chapter. Towards the conclusion of it, he dwells at considerable length on the relations of gout and hysteria, and their joint connexion with derangement of the renal functions. Having glanced, in a cursory manner, at a variety of instances illustrative of the gouty origin of certain nervous affections of men as well as women, he adverts particularly to the occasional occurrence of an erratic secretion of urine, or some of its constituents, both in gout and hysteria, referring to another part of his work for examples of such phenomena in the latter disease. We give the following rather long quotation, as affording a fair example of our author's style, and as conveying his ideas more distinctly than could be done by an abstract:

"The connexion between total suppression of the urine and gout, but especially between gout and gravel, has been frequently noticed. It is well known that all articles of diet which, either from their peculiar composition, or their action on the organs of assimilation, increase the quantity of lithic acid, will excite a fit of gout or gravel. The favorable termination of a fit of the gout is always accompanied by an increased excretion of lithic acid from the kidneys. Colchicum also, which is almost a specific for gouty diseases, increases this excretion; the quantity being nearly doubled, according to Professor Chelius, after the medicine has been taken for twelve days. The urine of gouty subjects is in general acid, but during the paroxysm it becomes alkaline or neutral, and so continues until the termination of the paroxysm, when it becomes loaded with the lithic acid deposit. Cases of gravel indeed are simply those in which the acid crystallizes so soon as excreted by the kidneys; in cases of gout the excretion is suspended; and these lithic formations, according to Dr. Prout, seldom

occur before the age of forty, a period of life corresponding to that in which gout is first developed in those with the hereditary predisposition. Facts being such, the direct inference is, that the presence of an excess of lithic acid, or other of the urinary constituents, is the immediate cause of a paroxysm of gout; and that those individuals are of a gouty constitution who, from hereditary conformation of the organs of assimilation or excretion, either have an unusual quantity of uric acid produced, or a deficiency of power in the kidneys to eliminate it from the system, as maintained by Dr. Holland. And such inference is quite in accordance with all toxicological, and (less directly) with many pathological phenomena, apparently unconnected with gout. . . . . It is not generally known that Jahn found crystals of the gouty salts in the blood of gouty patients; a fact, however, of the greatest importance. Turck, a recent French author, attributes gout to the presence either of too much acid or too much alkali, which latter he connects with a set of acid and alkali secreting organs, and these with positive and negative electricity. While the preceding statements with regard to the pathology of gout illustrate and confirm the views taken respecting the ovarian origin of hysteric diseases, they open out a large field of enquiry into the relations existing between the ovaria and testes on the one hand, and the kidneys on the other, as well as between both and the two large classes of diseases I have just compared. According to the researches of Wolff and Rathke (confirmed by Muller), the formation of the kidneys in the embryo is preceded by a substance termed by Rathke Wolffian bodies, after their discoverer. Like the true kidneys in fishes and batrachian animals (to which they are analogous in structure) the Wolffian bodies extend along the whole length of the spine, from the heart to the end of the intestine; a fact, as we shall see from the next statement, highly interesting, because it proves the extensive connexion the genito-urinary system has with the spinal cord. In mammalia a vessel proceeds from these bodies, which opens into the vas deferens, or fallopian tube. In the higher vertebrata these false kidneys are used to contribute to the development of both the true kidneys and the testes and ovaria, the latter first appearing attached to their anterior margin by a fold of peritoneum. In the human embryo, at the seventh week, they are slender elongated bodies, situate below the kidneys, their permanent position in the lower vertebrata." (pp. 166-8.)

The above embryological sketch affords, as Dr. Laycock thinks, a key to the explanation of many anomalous symptoms and phenomena of gout, hysteria, and some diseases connected with them. "We can," he says "have no difficulty in conceiving how the kidneys may, in many instances, have distinct sympathies, similar to those of the ovaria and testes; or in understanding how the latter may modify the functions of the former, and so contribute to the establishment of diseases which originate in a defective elimination of the urinary constituents." (p. 168.)

We have intimated our conviction that Dr. Laycock's mind is too much possessed with the relation between gouty and nervous disease; nevertheless, regarding the question as an important one, we strongly recommend his observations to attentive perusal, since they display both ability and an extensive knowledge of his subject.

Chap. vi. treats of the "relations of the emotions, passions, and propensities." The first section is on the emotions and passions, the second on the propensities, and the third on the causes which modify the influence of the passions on woman. Dr. Laycock is not diffuse on any of these heads. Their full consideration would indeed occupy a voluminous work, and would require an immense range of observation and great analytical power on the part of the writer. Some of our author's remarks on imitative movements, in the second section, appear to us to

merit attention, especially that relating to their excito-motory character; this subject, however, we must waive, for reasons stated in a preceding part of the present article.

In the same section we find one of those outrageous affirmations which we have encountered so frequently in Dr. Laycock's pages, that they cease to excite astonishment; "females and children," says he, "are more liable than men, indeed are alone liable, to epidemic convulsions." (p. 180.) He adds, however, we presume by way of qualification, that the mind "may be so excited by oratory, or by religious exercises, that a temporary susceptibility is developed," and he refers, as an example, to the dancing mania of the fourteenth century; but the qualification in fact would nearly swallow up the assertion, even if the latter were not, on other grounds, grossly and obviously incorrect; for it so happens that a great majority of the convulsive epidemics which have at different times occurred have been in close connexion with religious fanaticism, and, as in the instance of the dancing mania, have affected men as well as women.

With regard to *endemic* convulsions there are few cases which can properly be brought under this denomination, but these are quite unfavorable to our author's assertion. The leaping ague of some parts of Scotland may be considered as both epidemic and endemic, since it is nearly peculiar to those regions, and is chiefly prevalent at certain times. It attacks people of all ages and both sexes. Tetanus, which may in some sort be considered as an endemic convulsive disease of certain tropical countries, attacks males more frequently than females.

What then becomes of Dr. Laycock's affirmation that women and children are alone liable to epidemic or endemic convulsions? We are quite aware that he knows its inaccuracy just as well as we do, and our only wonder is what can induce him to make such random assertions.

In chap. vii. Dr. Laycock discusses the "relations of the nervous diseases of women to the nervous system in general." He commences with the recapitulation of a general law before laid down, namely, "that gradual impairment of the functions of the nervous system is preceded by exalted energy and affectibility," or, as it is more distinctly expressed when first anounced, (chap. x. part i.) "that when the functions of the motor and sensitive system are gradually impaired, there is exalted affectibility and energy observed before the paralysis." We are by no means satisfied of the universality of this law, though we admit that it applies in some instances. An objection immediately occurs to us in *amaurosis.* Is this always preceded by increased excitability of the optic nerve? So much the reverse, that Beer and others divide simple amaurosis into the case in which blindness depends on the undue exaltation of the sensibility of the optic nerve, and that in which it depends primarily on the depression of such sensibility; and every practitioner who has paid any attention to amaurotic affections, must know that in the incipient stage, some patients experience distress, and are unable to see except in shady places, while others court a glare of light. We do not think we should have to go far in quest of additional instances opposed to this law of Dr. Laycock's; fortunately, however, there is nothing practically objectionable in the application he makes of it.

"Referring," he says, "with this law in recollection to the statements

in part i. respecting the influence and anatomical and physiological connexions of the ovaria, we can have no difficulty in understanding how these organs, by acting upon the dorso-lumbar portion of the cord and the respiratory ganglia, may give rise to the most varied symptoms of disease in distant and different organs, varying in intensity from the most to the least severe, and implicating the sensitive, motor, and organic nerves." (p. 184.)

The first section of this chapter treats of "the relations of some general paroxysmal affections to the cerebellum and contiguous ganglia," and the second, of "the anatomical relations of the cerebellum and contiguous ganglia." But this subject need not be spoken of here, as it is noticed in another article in the present Number. See the review of the "Library of Medicine."

Chap. viii. is on "the relations of nervous diseases to the extremities of the spinal cord." Dr. Laycock here insists on the fact that in hysterical or *neuremic* diseases a metastarsis of morbid action may take place from one end of the spinal cord to the other.

"For the symptoms implicating the pelvic viscera will occasionally disappear, and vomiting, dyspnœa, cough, palpitation, and cephalea supervene, to disappear in their turn, at the menstrual or hebdomadal period, with a completeness really surprising, and be replaced by menorrhagia, diarrhœa, ischuria, constipation, vesical paralysis, and neuralgic pains of the abdomen and lower extremities, or some other affection implicating organs in connexion with the dorso-lumbar portion of the cord." (p. 198.)

The accuracy of these remarks, except in as far as they relate to hebdomadal matters, will, we believe, be admitted by every observing practitioner. With respect to the portion of the cord most liable to be affected, Dr. Laycock remarks that the pelvic viscera are most frequently implicated in those women who have the reproductive system highly developed, while the thoracic viscera, or those connected with the respiratory ganglia, are most liable to disturbance in women who approach the masculine conformation, as viragoes, women past middle life, and those who have borne no children, (p. 198.) This conclusion is certainly such as one would be inclined à priori to deduce from the premises, and we think we may also say that our general observation here coincides with our author's.

Chap. ix. treats of the "relations to the lateral halves of the nervous system."

Many facts are here adduced to show that there is a difference of development in the right and left halves of the body, both with respect to the generative and other organs. In the human subject the right has evidently the predominance, and this seems to be the case with reference to some organs among mammalia, as the liver and lungs.

In all classes of animals some difference exists in the right and left organs of generation, one side preponderating in some, and the other in the other. Dr. Laycock brings forward various facts in comparative anatomy illustrative of this subject, and makes some interesting remarks, for which we regret we cannot find space.

In chap. x. our author considers the "relations of the nervous diseases of women to the pathological changes in the nervous centres." These he dismisses, judiciously we think, with a few brief remarks, and a

simple allusion to the opinion of Ehrenberg, that the nervous fibres are composed of globules immediately excreted from those of the blood—an opinion which, if admitted, would remove all difficulty in comprehending how agents which alter the composition of the blood so readily affect the nervous system. Dr. Laycock conceives that the pathological changes under consideration probably consist in impaired functions of the capillaries, or molecular changes in the neurine, not amounting to organic disease, unless the operation of the exciting causes be long continued.

Chap. xi. is on "general diagnosis, prognosis, and treatment." It contains, under two sections, some judicious remarks, which being of a general character need not detain us.

Part III. We now come to the third part of Dr. Laycock's work, namely, that on "special pathology and therapeutics," and here we rejoice to be able to meet him on less debatable ground than we have hitherto been treading. The arrangement he follows is based upon special physiology, and eight chapters are respectively occupied with the nervous diseases implicating the cutaneous structures; those affecting organs in relation with the dorso-lumbar portion of the spinal cord; those affecting the liver, spleen, and pancreas; those affecting the viscera, in connexion with the respiratory ganglia; affections of the motor system in general; paroxysmal diseases; affections of the sensitive nerves; and cerebral diseases.

In the discussion of these subjects, Dr. Laycock evinces the judgment and knowledge of an accomplished physician. We think this portion of his work particularly valuable, as exhibiting a very complete and accurate delineation of the irregular forms of hysteria, so common in practice, yet so little recognized by the mass of practitioners. We trust that the perusal of Dr. Laycock's book may convince many of his brethren of what is the simple truth, that they are every day mistaking hysteria for inflammation or organic disease; frightening the patient and her friends with a dismal prognosis, when there is in fact no danger; and retarding recovery, and permanently injuring the constitution by bleeding, starving, and confinement, when fresh air, nourishing diet, and plenty of exercise would be much more to the purpose.

From the heads of chapters just enumerated, the reader will at once perceive that an attempt to analyze this portion of the work would swell the present article to more than twice the length it has already attained. It frequently happens, in fact, that it is not the best part of a book that most requires or admits of an analytical review; and we therefore trust that the author will not think we take more delight in censure than in praise, because we dismiss what we think the best part of his work with a general commendation, while we have made particular strictures on the defects of other parts.

To conclude, we think that Dr. Laycock's book, though justly open to the censure of being far too theoretical, too vague, and frequently inaccurate in its general statements, is, nevertheless, meritorious as a whole. The author gives evidence of an intimate acquaintance with his subject and of a long and thoughtful addiction to it. His style of writing is clear, and generally good; and we may add that, to his credit, he has treated in a strictly scientific and professional spirit a subject which might easily have been twisted to *popular*, in other words to self-interested ends.

Art. V.

1. *Traité Complet, Théorique et Pratique, des Maladies du Foie.* Par Aug. Bonnet, Docteur en Médecine de la Faculté de Paris, &c. &c. New Edition, corrected and augmented.—*Paris*, 1841. 8vo, pp. 396.
*Complete Theoretical and Practical Treatise on Diseases of the Liver.* By Aug. Bonnet, M.D., of the faculty of Paris, &c. &c.—*Paris*, 1841.

2. *A Practical Treatise on the Diseases of the Liver and Biliary Passages.* By William Thomson, M.D., Fellow of the Royal Colleges of Physicians and Surgeons, and one of the Physicians to the Royal Infirmary of Edinburgh.—*Edinburgh*, 1841. 8vo, pp. 306.

3. *The Madras Quarterly Medical Journal.* Edited by Samuel Rogers, Assistant Surgeon. (Vol. II, for Jan., April, July, Oct. 1840.) —*Madras*, 1840. 8vo, pp. 462.

It is not unusual to find an author manifesting a disproportionate anxiety to establish a claim to originality in some theoretical view involving no practical consequence of any moment, while, in the opinion of the reader, he might, if disposed to congratulate himself, have selected other and better grounds. We think the foregoing remark applicable to M. Bonnet, who challenges consideration to himself for certain views which he alleges to be new, but which we think are not materially so, and which, granting them to be new, are certainly less important than M. Bonnet seems to suppose them. Fortunately his work possesses other and more solid grounds of reputation.

After some "Preliminary Observations," in which are given, with brevity and perspicuity, the Anatomy and Physiology of the Liver, the proper subject of the work commences, and the first chapter is devoted to the "Semeiology of Irritation of the Liver."

M. Bonnet sets out with stating that there are cases in which irritation of the liver is so slight as not to amount to a phlegmasia. He thinks that this stage or phase of hepatic derangement has been hitherto overlooked by authors, who, as he supposes, never recognize hepatic derangement unless as it presents itself *after* having merged, in a greater or less degree, into Hepatitis. He claims to be the first who has detected and drawn the characters of hepatic irritation in this its incipient form, while uncomplicated with inflammation.

We shall be better able to judge whether M. Bonnet's claim to originality on this point be well founded after having seen what, according to him, constitutes "the first degree of hepatic irritation." He owns the diagnosis to be difficult, but states it to be as follows: It being granted, he says, that an increase in the functions of an organ proves the amount of vitality in that organ to be beyond the normal degree (an inference which we for our parts are far from assenting to), one cannot but admit that a more than usually abundant secretion of bile denotes that the liver is the subject of irritation. Whenever, therefore, we find an individual with a bitter taste in the mouth, yellow and furred tongue, vomiting of a fluid of a green or yellow colour and bitter savour, a jaundice hue about the lips, nose, and on the conjunctiva, yet without fulness or tenderness of the right hypochonder (the absence of which last signs is the proof that the affection has not yet passed into inflammation), we are to conclude that we have to deal with the first degree of hepatic irritation. If it be objected, he further remarks, that hepatic irritation, far

from being accompanied with an increased, is often characterized by a suspended secretion of bile, his answer is that the latter is never the case until the irritation has risen into some degree of phlogosis.

Now, with all deference to M. Bonnet, there is not, we apprehend, much novelty in the above statement. We believe we could point in a variety of authors to views essentially the same. We are sure, moreover, that the distinction which M. Bonnet sets forth as novel is, whether or not expressed in so many words, present to the minds of most practitioners, and that it generally regulates practice. We cannot discover wherein "the first degree of hepatic irritation" differs materially from what is ordinarily called functional derangement of the liver, manifesting itself by excessive biliary discharges whether in vomiting or stool. We admit that M. Bonnet, in pointing out that the suspension of the bile in such cases does not take place until some degree of phlegmasia has set in, and is of course symptomatic of the disease having reached that stage, draws a distinction more definite perhaps than is usually done, and one that is useful in diagnosis. Still, we repeat, there is no material novelty in it.

M. Bonnet gives it as his opinion that the majority of practitioners, both in their works and practice, have, till now, supposed themselves to be treating pure parenchymatous hepatitis, while, in fact, their own descriptions show that they have in many cases had a complicated affection to deal with, namely Gastro-Hepato-Peritonitis, as M. Bonnet designates it. He imagines, as we shall see in the sequel, that parenchymatous hepatitis, as a simple and primitive affection, is of rare occurrence; that it seldom exists uncomplicated with gastro-enteritis, peritonitis, or encephalitis. He thinks that a survey of symptoms will, in general, prove the above complicated character of many cases which are set down as those of simple parenchymatous hepatitis; and believes himself to be able accurately to distinguish, amid the *tout ensemble* of symptoms, those referrible to the stomachic and peritoneal affections, from those directly due to the affection of the substance of the liver.

The symptoms, viewed generally, of Gastro-Hepato-Peritonitis are as follows: tension of the right hypochonder; sensibility to pressure; pain sometimes dull, profound, answering ordinarily to the right hypochonder, but having its seat occasionally in the epigastrium or the left hypochonder, and accompanied with a feeling of uneasiness, fulness, suffocation; sometimes acute, pungent, lancinating, analogous to that of an inflamed pleura, and extending in certain cases from the extremities of the right false ribs to the clavicle and arm of the same side; augmentation of the volume of the liver; decubitus difficult, sometimes impossible, now on the one side, now on the other; dyspnœa; respiration increased on the left side, diminished on the right, and null in the abdomen; dry cough, hiccup, nausea, vomiting, intense thirst, bitterness of mouth; tongue red on its edges, and covered in its middle with a yellow or green fur; commonly a yellow tint of the eyes and of the skin; constipation; white stools, or else superabundant secretion of bile, more acrid than in the normal state; urine yellow, scanty, having an oily appearance, depositing a brick-like sediment; skin dry and burning; finally, a pulse frequent and often hard, but in some cases unequal and even intermitting.

Such are the conjoint symptoms of the complicated affection Gastro-Hepato-Peritonitis. Amongst these, those referrible to peritonitis

are: tension of the right hypochonder; the sensibility to pressure; the acute, pungent, lancinating pain, extending to the clavicle and arm of the same side; difficult or impossible decubitus on the *right* side; there spiration diminished on the right side and null in the abdomen; the dry cough and the hiccup. Those referrible to the gastro-enteritis are the nausea, vomiting, intense thirst, redness of tongue, dry burning skin, frequent and often hard pulse. Those directly due to phlegmasia of the hepatic parenchyma remain, namely, dull, deep pain, answering generally to the right hypochonder, but having its seat sometimes at the epigastrium or left side, accompanied with a feeling of uneasiness, fulness, suffocation; decubitus difficult or impossible on the *left* side; bitterness of mouth, yellow tongue, jaundice-tint generally of eyes and skin; either white stools, or more or less abundant and acrid bilious ones; urine yellow, scanty, oily in its appearance and depositing the brick-like sediment. M. Bonnet believes justly that parenchymatous hepatitis never advances far without involving the above complications and giving rise, of course, to the above symptoms.

We do not hesitate to admit that it is practically important to ascertain, as far as possible, whether and in what degree the peritoneal covering and the gastro-enteric mucous membrane participate in any primary affection of the substance of the liver, as thereby the prognosis and the treatment must be very considerably modified. We also acknowledge the accuracy with which M. Bonnet classifies the symptoms of the complicated affection, and refers them to the respective lesions which give rise to them. But we are disposed in this, as in the former case, to suspect that M. Bonnet underrates the discernment of his medical brethren, when he supposes that they are in the habit of overlooking the peritoneal and gastro-enteric complications of hepatitis, and of believing they have pure hepatitis to contend with, when the complications referred to are also present. The superficial acute tenderness which exists when the peritoneal coat of the liver is inflamed cannot be misinterpreted by any attentive physician; the gastro-enteric irritation, in all cases of acute hepatitis, is too prominent to escape the remark of even the careless. Not only the possibility but also the high probability of such complications cannot fail to be present to the mind of every practitioner of the slightest experience.

M. Bonnet distinguishes, as has been usually done, two forms of hepatic irritation, the primitive and the consecutive. Of the primitive form, the only causes which he admits are "a blow; a fall on the right hypochonder; a penetrating wound of the abdomen; a violent shock in the vertical line of the body, such as happens from a fall on the feet, hips, or knees." (p. 197.) However, in the succeeding page, he seems to acknowledge, if we understand him aright, other causes of primitive hepatitis.

"It occasionally happens," he says, "that under the influence of an obstacle to the circulation, situated in the chest or in the abdominal cavity, the blood accumulates in the liver and distends it beyond measure. The congestion is here entirely mechanical; but one easily perceives that if such congestion persists or is often repeated, it may become a cause of inflammation to the tissue in which it has its seat. Ought hepatitis of this origin to be regarded as idiopathic? Some will be surprised that I reply in the affirmative; but I wish them to observe that the only intelligible course in etiology is not to regard any

morbid affection as consecutive to another, except when the latter has produced it directly. Now, it is not the obstacle to the circulation which, in the case in question, occasions, properly speaking, the hepatitis; but, in fact, the blood, which, accumulated in the liver, becomes by its presence a cause of stimulation to that viscus; the immediate cause of the evil resides in the parenchyma itself. Why, therefore, should not hepatitis, which developes itself in this manner, not be regarded as primitive or idiopathic?" (p. 198.)

Granting it to be so (although the distinction seems more theoretical than of any practical utility), still primitive hepatitis of this origin does not come within range of any of the causes immediately before assigned by M. Bonnet himself. In the above statement, however, of the causes of primitive hepatitis, we do not find much to object to. Let us now see what are the only causes which he recognizes of the consecutive affection. "Consecutive hepatic irritation," he remarks (p. 199), "is always the result of a gastro-enteritis, an inflammation of the peritoneum, or of an encephalitis."

His own illustrations of this theory are not on the whole very clear and satisfactory. We see that he admits only three causes of hepatic irritation. Of these, he considers very justly gastro-enteritis as by far the most frequent. It is through the medium of this last affection, indeed, that he supposes one of the other two, namely, encephalitis, very frequently operates on the liver; and through the same medium, he believes that a multitude of other remote morbid influences or affections act in producing hepatic irritation.

"Dark and fat viands," he remarks, "..... hot seasonings, as pepper ..... generous wines, spirituous liquors, emetics, and purgatives employed continuously; all the agents, in short, known under the name of stimulants, exert their first effect on the stomach and duodenum. *It is never, except when they have produced a gastro-duodenitis*, and that this last has transmitted itself to the liver by the biliary passages or by the veins which originate on the internal surface of the mucous membrane, that the signs of hepatic irritation manifest themselves." (p. 206.)

This view of the matter is certainly just so far; as we believe that a gastro-duodenitis never occurs without inducing irritation greater or less of the liver; but we think M. Bonnet errs in supposing that unduly rich, fat, and stimulant aliment exerts a deleterious influence on the function of the liver in no other way than by the medium of the gastro-duodenal mucous membrane, and the production of a gastro-duodenitis. Aliment of the kind referred to, is generally considered, and we believe justly, as capable of affecting the function in another and indirect manner, namely, by giving such a constitution to the circulating fluid, as, by augmenting the eliminatory duty of the liver, shall derange that organ.

In short, it may here be observed generally, that M. Bonnet makes no reference to the liver as a depuratory organ, nor notices in any part the possibility of some of its derangements being due to lesions of its depuratory function, induced irrespectively of any mediate affection of the stomach and duodenum. We conceive that this omission constitutes a more remarkable and serious "lacune" in his treatise than the one which he refers to at page 222 does in the work of Broussais. The same anxiety to generalize as much as possible the causes of consecutive hepatic irritation, and the same neglect to take into consideration the

depuratory function of the liver, and the antagonist or rather supplementary relation which the organ very evidently bears to another important secreting and excreting viscus, namely, the lungs, lead him to represent the biliary affections of tropical climes as also solely owing to a prior affection of the digestive organs.

"Most authors," he observes, "have maintained that residence in a hot climate is one of the conditions which contribute remarkably to the development of diseases of the liver. It is true that such affections are exceedingly common in certain countries, as Egypt and the East Indies; but this is not owing to heat affecting directly the organ which secretes the bile. The most ordinary effect of elevated temperature is to render the digestive passages very excitable, and to dispose peculiarly to gastro-intestinal irritations. These irritations once declared propagate themselves to the neighbouring tissues; hence the frequency of the hepatitis of warm countries." (p. 204.)

Now, the experiments and observations of physiologists of an earlier period, and more lately those of Autenrieth, Tiedemann, and Gmelin, to which M. Bonnet, and we think Dr. Thomson also, makes no reference, tend to show that there is a vicarious action between the liver and the lungs. They even seem to prove that the size of the former organ is, if not always at least generally, in the inverse ratio of that of the latter, making it extremely probable that they are designed and adapted, to some extent at least, mutually to supplement each other. The liver, it is known, eliminates from the blood the excess of certain matters principally composed of carbon and hydrogen, and also fatty matter. The lungs also free the circulation from substances containing a great proportion of carbon. It is true that as regards the liver the carbon is still, when excreted, in the oxidizable state, and when separated by the lungs is united with oxygen; but this seems to prove nothing against the fact of the two organs being vicarious or supplementary in certain cases. Now it has been ascertained that, in states of high atmospheric heat, the lungs excrete less carbon than usual. It seems exceedingly probable that as, in the same circumstances, the action of the liver is augmented, the latter acts vicariously of the lungs; that thus, its function may be increased independently of any affection of the digestive organs; and as, according to M. Bonnet's own showing, undue augmentation of an organ's function cannot take place without irritation, hence one way, not referred to or accounted for by him, in which the liver may be consecutively deranged.

We have stated that the liver also excretes fatty matter. Now we would, in part, account for the frequency of biliary affections in *gouty* subjects, from the constitution of the circulating fluids peculiar to such subjects, and not from a "repercussion" of the gout on the digestive organs and thence on the liver.

"A chill," observes M. Bonnet, "from exposure to a current of air or immersion of part or the whole of the body in cold water, the repercussion of an exantheme, of gout, or of rheumatism, are capable, as we have said above, of acting directly on the liver; but most commonly when inflammation of that organ ensues from sudden suppression of cutaneous perspiration, the disappearance of an exantheme, &c., these causes begin by exciting either a gastro-enteritis or a peritonitis, according as the patient is predisposed to the one or the other of these maladies, by the state of the atmosphere, his regimen, habits, constitution." (p. 207.)

We are of opinion that several of the causes here referred to do not operate on the liver in the manner supposed by Bonnet, but by certain changes produced in the constitution of the blood, by which the excretive function of the organ is affected. In the above quotation, M. Bonnet speaks of the repercussion of an exanthema, of gout, rheumatism, &c. being capable of "acting directly on the liver," and afterwards he makes the following observations:

"Excess of study, violent passions, profound chagrins, accesses of anger, insolation, do not act directly on the liver. All these causes commence by producing an irritation of the brain and its membranes; this, when it is intense or long continued, reacts sometimes *sympathetically on the hepatic parenchyma itself;* but most frequently it transmits itself to the gastro-intestinal mucous membrane, and it is only after this that hepatitis takes place." (p. 213.)

There are two points connected with the above statement which M. Bonnet leaves unexplained, but respecting which we should have wished much to have had some precise enunciation of his views. The first is, in what manner and in what circumstances the repercussion of exanthems, gout, rheumatism, &c. "act directly on the liver;" in short, we should like to know what, according to M. Bonnet's understanding of it, are the meaning and amount of these words. The second point is, how irritation of the brain and its investments acts sympathetically on the liver. It is obvious, from M. Bonnet's words, that he supposes such irritation may and sometimes does so act, and this independently of any mediate affection of the stomach and duodenum. Besides that we are of opinion that M. Bonnet's omission to point out, in a satisfactory manner, the channel and mode of these morbid influences and effects constitutes a radical defect in his theory, we must state it as our impression that he has himself supplied the refutation of his own hypothesis, by admitting that there are several ways in which hepatic irritation may be originated other than the three which, at the outset, he stated as the sole ones: to wit, a gastro-enteritis, a peritonitis, or an encephalitis.

The view that many derangements of the liver are owing to previous error of the gastro-duodenal mucous membrane is not a new one. Broussais, we know, had, long before Bonnet, announced and insisted on it. Bonnet himself admits this:

"Such are my ideas of the mode in which hepatic irritation is produced. They approach nearly, I own, to those of Broussais on the same point; but they are not identical, as he pretends in his Annals. ..... Independently, in fact, that this physician has not recognized any degree of irritation of the liver, but that which constitutes the hepatitis of authors, he says nowhere in his writings that this last may be caused by peritonitis." (p. 22.)

Now we have two remarks to make here. In the first place we doubt that a genuine hepatitis, involving the substance of the liver, is ever due to a peritoneal affection, except in cases in which the liver has been previously the seat of chronic disease, and even of some degree of structural change; and in which also, the person in whom it occurred was of a peculiarly cachectic diathesis. We have never ourselves seen any case of peritonitis, however grave and even though terminating fatally, in which it could be affirmed that the hepatitis complicated with it was purely consecutive of the peritoneal affection. No doubt, when the peritoneal investment of the liver itself is the seat of inflammation, a superficial

affection of the parenchyma usually follows; but not a general and profound hepatitis. Secondly, we do not see that M. Bonnet has any ground for representing as novel the fact that peritonitis may, in the degree and in the circumstances above indicated, induce hepatitis; nor can we admit that Broussais was not cognizant of such a contingency; for although that eminent man may not have stated the possibility of such a complication in so many words, it is very clear, from various parts of his writings, that in respect to him, it could not be called new or unknown.

In the second part of his treatise, M. Bonnet treats of what he calls "maladies of the liver, other than hepatic irritation,"—of which we shall presently give a brief sketch or rather enumeration. Meanwhile we shall follow the author's plan, and advert to the treatment of hepatic irritation.

Whatever opinions we may entertain of some of the theoretical views of M. Bonnet, we must acknowledge that we regard his treatment as simple, sound, and efficacious. We cannot do better than give it in his own words. It will be seen that it is neither, on the one hand, so tame as the practice of his countrymen sometimes is, nor, on the other, so rash and bloody as are the measures of some medical men on this side of the channel. After observing that in cases in which inflammation of the liver depends on a lesion of the digestive passages the best way is to attack the two affections simultaneously, he continues:

"In all cases in which acute hepatitis depends on a gastro-enteritis, and the subject is plethoric, we should commence the treatment by bloodletting from the arm. If, in spite of this depletion, the pulse remains large, hard, and full, we must repeat it, and then have recourse to local emissions. These last suffice almost always with persons who are neither plethoric nor strong, or whose phlegmasia is not intense; but it is necessary, even in such cases, that the emissions be abundant and often repeated. We are in general too reserved in our employment of them. . . . . We must enforce the most severe abstinence, and prescribe cold acidulated drinks, as lemonade, &c." (p. 229.)

He recommends the simultaneous employment of clysters to which nitrate of potass has been added, then baths, and gentle purgatives.

We should in this country reckon the above treatment too mild perhaps for many cases of hepatitis. It is evident that Bonnet is swayed by the same groundless apprehensions which so fettered Broussais in regard to the supposed hazard of exhibiting purgatives on an excited mucous membrane. Yet apparently he is aware of the good effects of purgatives, for in reference to their employment in chronic hepatitis, he remarks:

"The English, who make very little enquiry as to the condition of the digestive passages, and who give to every patient, without exception, calomel, alone or combined with aloes, gamboge, &c. effect, if we are to believe them, many cures. There is a slight exaggeration in what our neighbours on the other side of the channel tell us of their practice in this respect; but it is certain that they often cure. Now although, among their successful cases, there may be some which ought to be referred to the efforts of nature or the absence of gastric irritation, there are others which are really due to the action of the medicine on the inflamed stomach; and one cannot but admit that stimulants which act by astringing (resserrant) the tissues, or which provoke an abundant secretion of mucosities from the surface of the gastro-intestinal mucous membrane, sometimes dissipate both the irritation of that membrane, and the hepatitis itself. There can be no doubt in my opinion as to this." (p. 259.)

Yet immediately after thus expressing himself, he says of mineral waters: "taken internally, their only operation is to stimulate the gastro-intestinal mucous membrane. . . . . It is scarcely possible, theoretically speaking, to depend on their efficacy." We differ very decidedly from M. Bonnet in this view, and we find that, by his own admission, Bordeu, Portal, Bouvard, and many other distinguished physicians had prescribed these waters with great benefit.

We would call attention to Bonnet's mention of baths in the treatment of hepatitis. Portal seems to have employed them with advantage; and there is no doubt that in hyperemic states of the liver and of the abdominal organs in general, baths are of the greatest efficacy as auxiliaries to other means,—as, by encouraging superficial circulation, they relieve central congestion. It is, however, a singular proof of M. Bonnet's ignorance or disregard of the modes of treatment followed by practitioners of other countries, that he makes no mention, so far as we can discover, of the nitro-muriatic acid bath; of the remarkable good effects of which in certain cases of chronic hepatitis and functional derangements of the liver, there can be no doubt; notwithstanding the doubts which Dr. Thomson (p. 290 of his Practical Treatise) is pleased to throw out on the subject.

M. Bonnet thinks leeching at the anus has no special efficacy whatever in relieving the hepatic venous system, and thinks it indicated only in cases in which hepatitis has supervened on the non-appearance or suppression of a periodical hemorrhoidal discharge, or of menstruation. On this subject many eminent practitioners differ from him. Dr. Conwell remarks that "local depletion is most efficaciously performed by the application of leeches round the anus, over the abdominal parietes and the perineum, as the blood drawn from those parts reduces the quantity about to be poured directly into these sources of the portal system, whether it be derived from arterial or venous capillaries." (Treatise on the Liver, sect. 185.) We have ourselves seen great obvious benefit in many cases from the application of leeches about the anus; but, in order to this effect, the number of leeches applied must be considerably greater than it frequently is.

Bonnet is also opposed to the employment of cupping-glasses and blisters, which, he remarks (p. 232,) "if not always hurtful, are at least perfectly useless in the treatment of acute hepatitis." We know Mr. Annesley thinks that cupping-glasses, by the tightening of the integuments and the pain, which in this way they cause, are apt to augment the inflammatory action, and that various eminent practitioners are opposed to the use of epispastics, especially of cantharides, in acute hepatitis. But we must confess that we have not by any means so unfavorable views of these remedies as M. Bonnet seems to entertain. We have seen the greatest benefit, even in acute hepatitis, from the application of cupping-glasses. As to blisters, we should certainly not have recourse to them in this form of hepatitis, both because their use in it is questionable, and their presence might prevent the application of the more direct and less equivocal means, leeching and cupping. But in some forms of chronic hepatitis their good effects are unquestionable.

The remainder of M. Bonnet's work is occupied with the rarer affections of the liver; and we do not find that, in treating these, he has

advanced anything remarkably new or important; although his observations in general are interesting and intelligent. We shall enumerate a few of the "diseases other than hepatic irritation." These are, hepatic asthenia, atrophy of the liver, passive sanguineous congestions of the organ, hepatic hemorrhages, erectile tumours, hepatic phthisis, biliary calculi, hydatids, hepatic tumours, jaundice, diseases of the excretory function of the liver.

Hepatic asthenia consists "in a debility greater or less of the biliary apparatus," (p. 273,) in which, "the quantity of bile secreted is too little to stimulate efficiently the intestinal mucous membrane." M. Bonnet seems to have found pills of aloes, calomel, and scammony useful in this affection.

The "hepatic phthisis," remarks M. Bonnet, (p. 307,) "of our predecessors is nothing else than chronic hepatitis, which has lasted long, and produced wasting, consumption, and death. It is, therefore, perfectly useless to constitute it a particular morbid state, or to enter into details respecting it."

In respect to jaundice, M. Bonnet is disposed to regard all the modern theories as either false or at least not proven, and as furnishing uncertain grounds of practice; such as that this malady is due to a dissociation of the elements of the blood, or to a morbid constitution of that fluid, or to the liver not duly separating from the circulation the biliary principles. He thinks the view which the ancient physicians took of jaundice, as the result of some positive obstruction to the excretion of the bile, and the consequent absorption of that liquid into the sanguineous circulation, the only or, at least, the most sure and the most trustworthy, as a ground of treatment. And undoubtedly M. Bonnet has reason in thinking that, in almost every case of jaundice, it is safest for us to *assume* that the excretion of the bile is obstructed by some mechanical cause, and to direct our practice accordingly. At the same time, while we admit that our knowledge of the causes of jaundice, other than gallstones or some mechanical or structural impediment in the biliary passages, is still imperfect, we think there are sufficient reasons for believing that the disease is occasionally owing to some morbid change in the constitution of the blood, or abnormal action in the secretory function of the liver, in consequence of which, matters which should have been eliminated from the circulation are retained in it.

In M. Bonnet's concluding section, "alterations of the bile," there is nothing worthy of remark.

We pass now to the Treatise of Dr. Thomson. Much less original in its character than that of Bonnet, it contains much more information. It is, in truth, a most laborious and complete compilation, but nothing more; everything valuable and important comprised in it existing already in print. We do not recollect a single observation, doctrinal or practical, of the slightest moment, which Dr. Thomson is not careful to corroborate by some quotation from a previous writer; thus himself showing that the sole merit of his work lies in being a collation and digest of the opinions and practice of various writers on liver disease.

As Dr. Thomson, though following a different method, goes over nearly the same ground as Bonnet, as he seems to draw no small portion of his materials from this writer, and as, agreeably to the remark just

made, the greater part of his book is but a rehearsal of opinions and practice already before the public, we do not deem it requisite to enter on a minute examination of his volume, but shall simply advert to one or two points which seem to require special notice.

In the preface Dr. Thomson remarks, that "he thinks it right to mention that the opinions relative to the employment of mercury in affections of the biliary organs, which he has endeavoured to illustrate and support, have long been entertained by his father, and guided his practice in this class of affections: and that they were inculcated by him in the course of lectures on the practice of physic, which he delivered for several sessions in the Edinburgh School of Medicine." We do not know whether Dr. Thomson means to claim for his father the having propounded some new views as to the employment of mercury in liver complaints, or simply to adduce his father's eminent authority in favour of certain views which had been held by others before him. We must infer that the latter is the meaning of Dr. Thomson, since he himself remarks, page 253 of his Treatise: "It is but justice to the late Dr. Trotter to observe that, in his view of the nervous temperament, he was one of the first physicians in this country who opposed the fashionable practice of administering mercurial medicines in the treatment of the so-called bilious complaints. In that opposition he was ably seconded by Dr. Saunders." Now the third edition of the latter gentleman's work on the Liver was published in 1803, and his Observations on the Hepatitis of India, which contain the cautions referred to in respect to the use of mercury, were published in 1809. But besides this, we confess ourselves at a loss to discover what are the peculiar opinions to which Dr. Thomson inclines with regard to the use of mercury, for he summons almost equally respectable authorities on either side of the question; and is careful (a peculiarity which pervades his book) to give nothing but a most negative and indefinite opinion of his own. In short, the whole of his section on mercury consists in quotations, alternately of writers who are of opinion that mercury is principally to be relied on in the treatment of hepatitis, and of others who take a more qualified view of its use and expediency; and although we may gather, though not without difficulty, from the general strain of the section, that Dr. Thomson is disposed to side with the latter authorities, yet nowhere do we find him giving that clear expression of his views and practice which the formal announcement in his preface had led us to expect. At page 294, Dr. Thomson declares "we shall not take upon ourselves to affirm in what cases of diseases of the biliary organs, mercury ought or ought not to be had recourse to," &c. If he is not prepared to do so, and if his views are so undetermined as to the powers of mercury and the morbid exigencies which indicate it, we should think it was hardly necessary for him to have asked particular attention to his opinions respecting these points.

It would be out of place to enter here on a minute description of the merits of mercury in hepatic derangements. We freely admit that it is an agent which has been much abused: but while we concede thus much, we must express our conviction that as a cholagogue drug—as a means of overcoming that excretory torpor of the liver in which the bile is duly secreted, but not emulged, in which the biliary tubes and receptacles become distended, (a condition which if allowed to continue may give

rise to hyperæmia and congestion, and ultimately inflammation,)—there is nothing so valuable as mercury. When there is reason to suppose that pale-coloured stools are owing to a mechanical cause, as a gallstone or some structural change in the ducts, mercury, as a *cholagogue*, is of course out of the question; and in acute hepatitis,—although as a purgative it is not contra-indicated, but is, on the contrary, combined or followed by saline draughts, &c. highly useful,—no certain advantage is to be expected from it as a constitutional means. In this case, the ordinary antiphlogistic treatment, by bloodletting, local and general, by saline purgatives, acidulous drinks, and abstinence, is alone or chiefly to be trusted to; and mercury, administered so as to affect the mouth, far from benefiting, appears sometimes positively to exasperate the inflammatory action. This seems to be the opinion of most practitioners, and of those in particular whose experience of hepatic cases has been the most ample. After suitable depletion by the lancet, mercury is useful in restoring suspended secretion, whether of the liver or of the intestinal canal, and in thereby still further subduing inflammatory action, or in preventing a reaccession of it.

It appears to us that there can be few members of the profession who practise it with more desponding feelings and anticipations than Dr. Thomson. The cases are rare indeed in which he does not emit sceptical opinions as to the efficiency of the modes of treatment to which he adverts. And thus, in regard to mercury, he remarks, page 279, in the course of some observations as to the power which this medicine possesses over the structural alterations of the liver, "With regard to mercury, as to other resolvents, and perhaps all other medicines, it may fairly be questioned whether the practitioner ever effects by its means what nature never succeeds in accomplishing for herself—whether any of the structural alterations of the liver, not of an inflammatory character, ever disappears under, or in consequence of, its administration or use." We are of opinion that there is no reason to doubt that, whether by exerting some power on the absorbent function, or by removing capillary congestion, or by modifying in some way the action of the secreting vessels, mercury does possess an unequivocal influence over some sorts of morbid formations in various textures and parts more accessible to our observation than the liver; and therefore that this fact, along with other strong presumptive proofs of its being occasionally efficient in reducing enlargement along with sensible induration of the organ in question, makes it extremely probable at least, that it is sometimes successful in removing structural changes of the liver. We are quite willing to admit that mercury is, in all such cases, only an auxiliary to a greater power; or, as Dr. Thomson expresses it, that "there are salutary changes which nature occasionally accomplishes for herself, or with such aid only as is required to give fair play to her own efforts;" but the same may be said of every medicine and of every case in which medicine is had recourse to; and it is moreover to be observed that if, without the aid referred to, nature would have been inadequate to bring about those salutary changes, the agent which enables her to do so, or which removes obstacles thereto, must be regarded as playing an essential part in the fortunate process. We do not suppose that any intelligent man ever attributed more to medicinal agency than what we have stated; but surely

it is going beyond the limits of philosophical doubt to deny thus much to it: "By alteratives," says Müller, "an organ morbidly changed in composition cannot be rendered sound by, as it were, a chemical process; but such a slight chemical change can be produced as shall render it possible for nature to restore the healthy constitution of the part by the process of nutrition." It is not to be denied that occasionally mercury seems to accelerate the progress of morbid formation, and to give it a more malignant character. This happens in cases in which the disease is originally of an intractable nature, or in which the constitution or diathesis of the patient is cachectic; and mercury in such cases, no doubt, seems but to facilitate the deposition of the morbid growth.

We shall only remark, in conclusion, that our French neighbours are not now so scrupulous as to the use of mercury in the treatment of hepatic disease as Dr. Thomson appears to suppose. Bonnet seems frequently to employ it, as Portal had done before him, and as Andral now does. Its use is, moreover, becoming every day more common in France. We would also here remark that, as to those cases referred to by Dr. Thomson, in which iodine, nitric acid, &c. were supposed, even by Twining, to augment the hepatic derangements which they were meant to cure, we have ourselves observed that it was only when imprudently prescribed that these substances irritated the gastro-duodenal mucous membrane, and that the hepatic derangement followed; but in no case, in which the former mishap did not take place, did the latter manifest itself: we consequently infer that we may persist in the use of iodine and nitric acid, without fear of injuring the liver, so long as the stomach and duodenum betray no signs of suffering.

Dr. Thomson's work, although well written, reads rather tamely, from the absence of original observation and vigorous opinion. We think that an author, even when he professes merely to compile, ought to digest, sift, and decide among the conflicting statements which he quotes, and not to leave this task to the reader. The principal merit of Dr. Thomson's volume is, as we have already stated, that it gives an accurate *coup d'œil* of facts and doctrines already in print, but scattered through a variety of volumes.

We purposely omitted to notice in their place M. Bonnet's remarks on hepatic abscess and its treatment, (p. 182,) in order that we might refer to them in connexion with some very interesting cases of exploratory puncture, detailed in the second volume of the Madras Medical Journal: a work which exhibits in a most favorable light, the scientific knowledge and professional zeal of our brethren in India, and does great credit to all concerned in its establishment and maintenance. Bonnet gives several successful cases of the evacuation, by operation, of hepatic abscesses: the case mentioned at page 237, in which an abscess of the liver first burst into the lungs, and again forming, was opened from the abdomen, and in which the patient recovered, is an exceedingly rare and remarkable one. As to the propriety of puncturing an abscess of this organ, of the existence of which there is no doubt, and which points in some accessible part, there cannot be the slightest hesitation. But as exploratory puncture supposes that when it is performed, there is some degree of doubt as to the existence and the site of the abscess, and as consequently some practitioners may have scru-

ples, whether in such circumstances, the operation is warrantable and safe, we shall endeavour to estimate what points the cases referred to appear to establish.

In the first place, the operation is in some cases not only safe but successful, and obviously the means of saving the life of the patient. It was so in the case detailed by Dr. Everard. (Journal, p. 229.) Although "there were evidently no adhesions between the liver and the abdominal parietes ; . . . the deputy-inspector . . . thrust a trocar into the liver through the epigastrium, without waiting to make any preparatory operation to induce adhesions between the parts." In another case of a private soldier, detailed by Dr. Mouat, (Journal, p. 226,) the patient "was twice punctured. The first operation failed to reach the cyst, but the second puncture did; when a trocar was introduced and a large abscess evacuated." This man and another on whom the operation was performed, died ; but in both cases, no bad consequences ensued from the use of the trocar; no effusion either of blood or matter had taken place into the abdomen ; and death was owing to the presence of several other abscesses. There is every reason to presume that in the former of these cases life would have been preserved had the abscess which was punctured been the only one. Regarding the general merits of the operation, Dr. Murray (p. 238) remarks :

"I consider that with a good anatomical, pathological knowledge of the region (i. e. of the liver) in our mind's eye, to enable us to avoid the large hepatic vessels, the gall-bladder, the colon, and the stomach, there is abundance of evidence to authorize us to, nay, it is our bounden duty to explore the liver, without hesitation or delay, in most cases where pathognomonic symptoms of abscess in it exist. . . . . By early accurate diagnosis and active constitutional treatment, a favorable termination may very often be happily brought about in hepatic diseases, without the necessity of operative procedure; but when abscess has once formed, we know how little advantage is to be expected from persisting in the use of mercury or any other medicine. Therefore let the question be fairly put, does not the trocar with a regulated diet hold out a better prospect of success?" (Journal, p. 472.)

We, however, think M. Recamier's mode of exploration much preferable to the one adopted in the above cases. This practitioner punctures, in the first instance, with a tube of a capillary fineness, over which he places a cupping-glass, by means of which he draws forth some of the contents of the tumour, and ascertains of what nature these are. We are not aware that Recamier is accustomed to adopt even this cautious mode of procedure, except in cases where there is an obvious and defined tumour; nor do we know that he employs it in the purely exploratory manner of some practitioners in this country, and exemplified in the cases detailed above. But it is obvious that his method is perfectly applicable to all such cases, and would certainly tend to render the operation more safe and sure.

"All our punctures," says Dr. Murray, "should be made from the abdominal cavity,—entering the trocar or explorer under the edge of the cartilages of the seventh, eighth, or ninth ribs, as circumstances may indicate. We may often indeed get nearer to the abscess through one of the intercostal spaces: *and I think primary exploration may sometimes be advantageously made in this situation by a very minute flat canular instrument;* but from not having seen any patient recover where the matter was evacuated in this direction (through the dia-

phragm); from finding that the action of the fibres of the diaphragm impedes the free discharge of the matter, something like a valve; from observing that air sometimes enters the wound when made there; and from considering that the opening is not so dependent through the walls of the thorax, as when made through the abdominal parietes,—I beg to recommend the latter mode in all cases." (Journal, p. 239.)

At page 22 et seq. of the Journal from which we have been quoting, Dr. Mouat discusses, at some length and in relation to hepatic abscess, the alleged occurrence of purulent metastasis, and of discharges of pus by stool, by the kidneys, and by the lungs. He gives a table of 112 cases, of which it is asserted that "109 passed purulent matter by urine; 33 of the number also by stool; and 8 of them pus by expectoration." Dr. Mouat, however, immediately after wisely puts the question: "Were these discharges really pus? And, if purulent matter, how can we account for its appearance in the excretions? For the former we have no unerring or certain test." Subsequently, Dr. Mouat states it as his opinion that, as far as his observations extend, "the appearance of the purulent matter seen in the fæcal and urinary discharges had passed through the circulatory mass." The possibility of the removal in this way of the pus of abscesses was and is held by Morgagni, Petit, Legallois, Larrey, Velpeau, Hennen, Copland; and has been variously explained and accounted for. Hunter denied it: and later researches and more accurate anatomical observation seem to prove that, unless where there is a direct communication between the abscess and the mucous surface, whether intestinal, pulmonary, or vesical, from which the purulent discharge takes place, no such discharge can occur; that it is impossible that the pus of an abscess can pass through the capillary circulation and then be excreted from a mucous surface: and that the alleged purulent discharges of this kind were either such only in appearance, or, if truly purulent, directly proceded from the organ or surface itself on which they showed themselves. This view is founded on very strong and as it seems to us incontrovertible evidence. Not to refer to the negative fact that Andral, a careful observer, has never, notwithstanding repeated and anxious examination, detected pus in the lymphatics in the neighbourhood of abscesses, but what was the product of inflammation in these lymphatics themselves, it has been ascertained by Weber that the globules of pus are always large, and in general are twice as large as the red particles of the blood, and therefore the former cannot possibly pass by the same routes as the latter. Moreover there are no apertures in the walls either of the lymphatics or of any of the secreting mucous surfaces, so large as to admit of the globules of pus, either by entering or escaping thereby; so that it is in the first place impossible, except mechanically, namely, by some rupture or ulceration in the walls of the lymphatics, that pus can find access into these vessels; and, in the second place, it is equally impossible, even after they have thus got into the circulation, that they can pass the capillaries, or, even supposing this obstacle overcome, that they find exit from any secreting or excreting mucous surface, except by a lesion of that surface of the most serious kind. We have only to add that the official reports, as given in this Journal, of the present mode of treating hepatic affections in India do not countenance the opinion, which Dr. Thomson's statements seem to imply, that mercury is less confided in

than formerly. We do now not enquire whether the employment of that agent, to the extent in which it is there used, is proper and defensible or the reverse: but that it is still principally trusted to may be learned from such statements as the following:

"If the original inflammation of the liver be slight, then moderate depletion and mercurialization will effect the solution of the circulating clot, and also the effused clot in the inflamed organ, and consequently remove the obstruction to the inflammation in the inflamed organ. Again, in very acute inflammations, the tendency of (to?) adhesion in the circulating blood, and the effused clot is so great, that copious depletion and mercurial saturation are required to overcome it. . . . In slight attacks, mercury has speedy success, and this is accompanied by glandular salivation. . . . . After admission, when it was detected that the liver was affected, bleeding and leeching were had recourse to, with large doses of calomel, sometimes amounting to a scruple, sometimes to ten grains. . . . . The quantity of blue pill and calomel, when calculated, was often large altogether," &c. (Madras Medical Journal: July, 1840. Art. I. Medical History of H. M. 39th Regiment, &c.)

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## Art. VI.

*The Library of Medicine.* Arranged and edited by Alexander Tweedie, M.D. F.R.S., &c. (Practical Medicine.) Five Volumes. —*London*, 1840. 8vo, pp. 440, 353, 385, 361, 346.

The readers of this Journal have already (Br. and For. Med. Rev., vol. X., p. 231,) been made acquainted with the general plan of the "Library of Medicine," and been given some notion of the aim and merits of its two introductory essays—that on General Pathology by Dr. Symonds, and that on Inflammation by Dr. Alison. We now proceed to furnish them with such an analysis of the five volumes devoted to the subject of Practical Medicine as the pretensions of these demand and circumstances permit.

Vol. I. In addition to the two articles just referred to, this volume contains essays on "Primary," Eruptive, and Puerperal Fevers, by Drs. Christison, Shapter, Gregory, Burrowes, and Locock, and concludes with a description of Cutaneous Diseases, by Dr. Schedel.

General Doctrines of Fever: Continued Fever. These papers, contributed by Dr. Christison, will be noticed with other works upon continued fever in our next Number.

Plague. This article is by Dr. Shapter, and is a compilation from which it would be unreasonable to withhold the praise due to judicious selection and arrangement of borrowed materials. The subsequent descriptions of Dr. Shapter are indeed distinguished by sounder intelligence of the subject on the part of their author than the opening paragraph gives promise of. In this, plague is stated to be "an *exanthematous* disease, the eruption consisting of buboes, carbuncles, and pustules, white, livid, or black, and generally attended with malignant and very fatal fever." This definition, which Dr. Shapter bears not the weight of originating, evidently sins in a multitude of manners both logical and

critical. We shall only here ask how Dr. Shapter justifies the strange perversion whereby buboes, carbuncles, and pustules are spoken of as the characteristics of an *exanthema?* Medical language is assuredly sufficiently loaded with equivocal terms and ambiguous phraseologies to render any attempt to add to their number, to say the least, superfluous.

The recapitulation of the arguments respecting contagion is well executed. The arguments and alleged facts adduced by the contagionists and their opponents are perhaps more likely to be fairly estimated by those knowing the scourge but by report, than by those who, living within the sphere of its inflictions, have interests, fears, or prejudices to be affected by the decision. Dr. Shapter inclines to believe, upon mature consideration of the arguments of each party, that plague is "essentially an endemic disease;" that through the influence of circumstances hitherto unascertained, but connected with season, it occasionally becomes epidemic; that "during the continuance of the epidemic effect, a principle is given off from the body, which *if very concentrated and pent up in confined and unwholesome situations* may generate the disease, so that, though not originally contagious, it may in that way, by accumulation of animal miasms, become contagious." The disease, then, according to Dr. Shapter, is evidently not contagious in the legitimate acceptation of the term; yet he appears to swerve from this conclusion, by maintaining it to be "not improbable that when the disease is communicated from person to person, it is by the inhaling the pestiferous breath or exhalations which emanate from the body of the patient; but at the same time that this influence of the atmosphere of contagion is very limited in its power and extent. A person who is himself uninfected cannot, according to Dr. Shapter, produce the disease in others by being "as it were the bearer of it." He adds that "the communication of plague by inoculation with the matter from a bubo or with any other morbid product, has by no means been proved; on the contrary, there is every reason to believe that the disease cannot be produced by these means." This is a perfectly correct estimate of experimental results:* persons have been frequently inoculated with the matter of buboes without the disease being produced; and in the instances of the few subjects in whom the occurrence of the malady has followed the operation, the *post hoc propter hoc* argument is inadmissible, inasmuch as they were exposed to the local influences generating the disease, and might have had it independently of the experiment performed.

The chapter upon Intermittent Fever is also the production of Dr. Shapter; and having herein more the tone of an observer, the writer appears to greater advantage than in his other writings. There is nothing in the description of the malady, however, meriting particular notice, except, we regret to say, a very remarkable omission, that of all allusion to the valuable researches of Professor Piorry into the condition of the spleen in intermittent fevers, an omission the less excusable because the results of these researches were some while since transferred to the literature of our own country in the pages of this Journal.† It will be

* Vide Br. and For. Med. Rev., vol. V. p. 560. April, 1838.

† Ibid., vol. VI. p. 141. July, 1838.

sufficient to quote the following statements to prove the importance of M. Piorry's enquiries: "The effect produced by quinine upon tertians and quartans is *proportionate to the reduction of the spleen;* so that the disease is cured simultaneously with restoration of this viscus to its healthy dimensions. On the other hand, though the symptoms be arrested, they will be liable to recur as long as the spleen exceeds its proper size. As the spleen attains its greatest enlargement at an early period of the disease, it proves that *it is not the paroxysms that produce the hypertrophy, but rather the enlarged organ that maintains the disease.*"

Dr. Shapter has been intrusted with the subjects of Remittent and Yellow Fever also. His account of the latter has unfortunately lost the greater part of any interest which might once have appertained to it by the subsequent publication of M. Louis' work on the disease, a work containing, as our readers are aware, the results of the first energetic and well-guided attempt to unravel the obscure anatomy of the malady. Dr. Shapter's essay is, however, well put together, and did it contain nothing else to admire, the subjoined sentence alone would render it deserving of purchase: "In order to prove the agency of contagion, a condition is absolutely necessary, which hitherto has never been properly attended to, viz. that the persons to whom the disease has been supposed to be communicable, should not reside in the same situation or locality as those by whom it is believed to be communicated, as in such case their being subject to the same influences as those already diseased, entirely invalidates any argument that may be offered in support of the operation of contagion." It is not writers on *yellow* fever alone who may be benefited by pondering upon this incontrovertible position.

Dr. Shapter informs us that Sir W. Burnett, Physician General of the Navy, "became so convinced of the absolute necessity of the free abstraction of blood in yellow fever, that he issued orders to the surgeons of the fleet, when stationed in the Mediterranean, to the effect that they should pursue this practice in every case of yellow fever that came under their inspection."* On the other hand, numerous English observers, together with "nearly all the practitioners in the West Indies, and many of the American and Spanish physicians," are cited as affirming, that they who bleed a man suffering under yellow fever "are guilty of nothing short of murder." Now there are certain persons terming themselves "practical men," who sneer at the idea of deciding upon the respective merits of any two given systems of medication by *counting how many die and how many recover under the application of each:* there's no "*genius*" and no "*practical tact*" wanted for an "*addition sum*," and *they* are all men of genius of course. Will these sages inform us how else than by cautious employment of the numerical method the point at issue between Sir W. Burnett and the other individuals referred to can be set at rest? "We pause for a reply."

* This statement is not accurate as regards Sir William Burnett. He was and is an advocate for bloodletting in certain cases of yellow fever, and often advised and practised it; but he never was an indiscriminate bleeder, and never issued any order enforcing it. —Ed.

Dr. Locock, the author of the article Infantile Gastric Remittent Fever, writes concerning it as follows: "There is apparently a striking confirmation of the modern doctrines of Broussais as to the nature of fever in the acknowledged *cause* of this infantile disease. The most prominent symptoms are referred to the mucous lining of the stomach and intestine; an acute or a protracted form of fever is the *result;* and with an improved condition of the alimentary canal, the febrile paroxysms are mitigated and gradually disappear." Here a certain affection of the gastro-enteric mucous membrane is pronounced to be not only the original cause, but to furnish a measure of the intensity, of the group of symptoms which it has been the custom to term infantile remittent fever; in fact to bear the same relation to his so-called primary fever, as inflammation of the lung to the fever of pneumonia; and all this is most perfectly correct. But in acknowledging the accuracy of Dr. Locock's pathology, we wish he had gone a step further, and denied to the malady the term "fever." The interpolation "gastric" is a concession no doubt; but even this should have been "gastro-enteritic."

The sketch of the disease is practical and to the purpose.

Hectic Fever has fallen to the lot of Dr. Christison. "True hectic," according to Dr. Christison, "occurs only in connexion with serious organic alterations of structure, and seldom [?], unless when suppuration exists." In the same page it is stated that "hectic like other fevers is obscure in the beginning, and can scarcely be distinguished from the febrile state . . . . which attends some chronic internal inflammations, and chronic visceral derangements of structure." Why, we had thought from sentence the first that hectic *was* the fever attending chronic visceral derangements of structure! The attempt made in this article to describe hectic fever as a distinct entity, to make out the conditions of the various functions during its existence to be dependencies upon it, instead of, as common observation and experience prove them to be, coincident effects of the original organic lesion, or results of secondary changes of structure developed during the progress of this, is, we humbly submit, altogether a misconception. To what purpose have such men as Louis, Andral, and Carswell laboured, if their close inferences respecting the organic causes of functional aberration are thus to be set at nought? To what purpose has the science of medicine made advance, if our students are to be still taught these antiquated notions, characteristic of an era when pathological anatomy was yet unknown? Among the "symptoms" of "hectic fever" here enumerated, is "crookedness of the nails." It might be too critical to remark that, in strict language, such a phenomenon, if it existed, ought to be denominated not a symptom but a sign; but it is important to state that the notion itself has been shown to be fallacious by the investigations of M. Vernois, the first that were ever undertaken upon correct principles in regard to this point.

Smallpox. This article is by Dr. Gregory, and gives a clear, well-arranged, and neatly-condensed account of the existing state of knowledge respecting this important disease, and the allied subjects of inoculation and vaccination.

The two subjoined tables are extremely valuable.

*Table exhibiting the comparative Mortality in the several Varieties of Normal and Abnormal Smallpox at the Smallpox Hospital during the Epidemic of* 1838.

| | UNPROTECTED. | | VACCINATED. | |
|---|---|---|---|---|
| NORMAL SMALLPOX. | Admitted. | Died. | Admitted. | Died. |
| *Confluent* ..... ... .... | 295 | 149 | 56 | 21 |
| *Semi-confluent* ..... | 78 | 8 | 42 | 4 |
| *Distinct* ..... | 19 | 0 | 20 | 0 |
| Total Normal ..... | 392 | 157* | 118 | 25 |
| ABNORMAL SMALLPOX. | | | | |
| *Confluent modified* ..... | 2 | 0 | 38 | 4 |
| *Semi-confluent modified* ... | 1 | 0 | 28 | 1 |
| *Varicelloid* ..... | 1 | 0 | 114 | 1 |
| Total Abnormal ... | 4 | 0 | 180 | 0 |
| Grand Total ..... | 396 | 157 | 298 | 31† |

* Of these there died of fever and superadded erysipelas 14.
† ,, ,, disease . 10.

*Table exhibiting the Mortality of Smallpox at different Ages and under different circumstances, as displayed at the Smallpox Hospital in the Epidemic of* 1838.

| | UNVACCINATED. | | VACCINATED. | |
|---|---|---|---|---|
| Ages. | Admitted. | Died. | Admitted. | Died. |
| Under 5 | 42 | 20 | 0 | 0 |
| 5 ... 9 | 37 | 11 | 5 | 0 |
| 10 ... 14 | 30 | 8 | 25 | 0 |
| 15 ... 19 | 104 | 32 | 90 | 6 |
| 20 ... 24 | 115 | 50 | 106 | 16 |
| 25 ... 30 | 45 | 23 | 55 | 8 |
| 31 ... 35 | 12 | 7 | 13 | 1 |
| above 35 | 11 | 6 | 4 | 0 |
| Total... | 396 | 157 | 298 | 31 |

It appears from the first of these tables that during the epidemic of 1838, the total mortality among those admitted was 27·1 per cent.; among the unprotected 39·6 per cent.; among the vaccinated 10·4 per cent.

The only omission of any consequence which we have detected in the essay, occurs under the head of mortality; no notice is taken of the original and invaluable researches of Mr. Farr (Brit. Med. Almanack, 1838,) into the law of death and recovery in different stages of the disease. We have, however, to acknowledge an obligation to Dr. Gregory for the table transcribed beneath, which, as he observes, "points out how little importance can be attached to the doctrine of critical days in smallpox."

Of 168 cases of smallpox there died on the

| First Week. Days. | Cases. | Second Week. Days. | Cases. | Third Week. Days. | Cases. | Fourth Week and later. Days. | Cases. |
|---|---|---|---|---|---|---|---|
| 3d | 1 | 8th | 27 | 15th | 7 | 22d | 3 |
| 4th | 5 | 9th | 15 | 16th | 5 | 23d | 1 |
| 5th | 10 | 10th | 14 | 17th | 3 | 24th | 3 |
| 6th | 5 | 11th | 16 | 18th | 3 | 25th | 1 |
| 7th | 11 | 12th | 11 | 19th | 1 | 27th | 1 |
| | — | 13th | 11 | 20th | 2 | 28th | 1 |
| | 32 | 14th | 5 | | — | 29th | 1 |
| | | | — | | 21 | 31st | 1 |
| | | | 99 | | | 32d | 1 |
| | | | | | | 35th | 1 |
| | | | | | | 38th | 2 |
| | | | | | | | — |
| | | | | | | | 16 |

Dr. Gregory exhibits himself, to a certain extent, the apologist of inoculation. The culminating objection to inoculation is, as all are aware, that it perpetuates and multiplies the sources of contagion. Certain considerations adduced by Dr. Gregory, "must," he believes, "convince any unprejudiced mind that the argument against inoculation, drawn from its supposed tendency to augment and multiply the force of contagion, is not so forcible as the opponents of contagion invariably allege." One of these considerations is that "in 1838, when inoculation was unknown in London, the admissions into the Smallpox Hospital exceeded those of 1781, when inoculation was universally practised; both being years of epidemic prevalence." As it stands, this proposition is valueless; Dr. Gregory should at least have shown that the increase was out of proportion with that of the population.

"Whether inoculation," observes Dr. Gregory in conclusion, "be ever destined again to occupy the thoughts of men, and to cooperate with vaccination in the general design of mitigating the severity of smallpox, is a question which at the present time it would be certainly premature and perhaps unnecessary to consider." (p. 321.)

The influence said to be exercised by atmospheric constitution over the powers of resistance to variola is regarded by Dr. Gregory as indubitably real: "the number of persons attacked during epidemic seasons, who had successfully resisted smallpox contagion *communibus annis*, offers an argument in favour of the position which to our minds is irresistible."

In respect of recurrence to the cow for primary lymph, Dr. Gregory is "bound to acknowledge that the Smallpox Hospital of London changed their old stock of lymph for more recent matter in 1837, and that a marked improvement was perceived in the resulting vesicles. The local inflammation was more severe; the constitutional symptoms were more violent; the virus was more energetic; the most minute incision took effect, and the lymph given out on the ninth and tenth day was still in an active state."

The phenomena of vaccination are well described, and very useful hints given respecting the mode of practising and conducting the operation.

Revaccination may, according to Dr. Gregory, "be recommended for

its safety, even if it be much less serviceable than the Germans contend for. We have sufficient facts before us to state with confidence that it need never be recommended prior to the tenth year of life, and that the age best fitted for it is from the period of puberty to that of confirmed manhood."

Measles and Scarlatina are described by Dr. G. Burrowes. There is little in these descriptions calling for particular notice. "The season of the year" is said to have "a more important influence on the issue of measles than of scarlatina or variola;" a position plausible enough, but requiring numerical demonstration, which is not furnished by Dr. Burrowes.

Under the head "*anatomical characters,*" Dr. Burrowes, as do all the contributors to the volume, enumerates every change of structure discovered in the bodies of persons dying with the particular disease under consideration. This proceeding evinces very curious misapprehension of the proper meaning of the term *anatomical character,* which in reality applies to the especial organic characteristic, and to this alone, of any given malady. Thus, gray or red hepatization is an anatomical character of pneumonia, because the discovery of such lesion on the dead body proves that that disease has existed during life; but softening of the mucous membrane of the stomach is not an anatomical character of, though it is frequently found in subjects dying with pneumonia, simply because it is a change observed in the victims of all varieties of disease. According to the system pursued in this volume, (we give a ludicrous but really a correct illustration, the more forcibly to convey our meaning,) if a man having stricture of the urethra is thrown from his horse and breaks his neck, thickening of the walls of the urethra and diminution of its caliber should be enumerated among the "anatomical characters" of fracture of the spine. It is so uncommon for a number of medical writers to display such perfect unanimity, that we cannot help fancying the individual authors are not justly chargeable with the error pointed out, but that its prevalence is the result of a general order from head-quarters.

Puerperal Fevers. By Dr. Locock. This is a valuable article, and will be examined elsewhere in conjunction with another upon the same subject appearing in the sixth volume of the collection.

Diseases of the Skin. Dr. Schedel has described these affections with perspicuity and conciseness.

Since the satisfactory discovery of the person of the *acarus scabiei,* and of its "local habitation" at the distal point of the furrow communicating with the vesicle, the great therapeutical problem has been how to annihilate the animal with the greatest certitude and dispatch. M. Albin-Gras has made several experiments in search of a sufficiently murderous drug, and finds that the acarus "lives sixteen hours in vapour of burnt sulphur; three hours in water; two hours in olive oil; one hour in the acetate of lead; twenty minutes in vinegar and spirits of wine; twelve minutes in a solution of sulphuret of potash; and only from four to six minutes in a solution of hydriodate of potash." The latter is evidently the *acaricide par excellence,* and probably the best local appli-

cation; half a drachm of the salt may be mixed with an ounce of lard. We say probably, because it does not appear a necessary consequence that the substance which proves most rapidly destructive of the insect's life when acting on it directly, shall be the most active in attacking it in its habitation. And as M. Schedel observes, "it remains to be explained [although the serosity of the vesicle will not produce the disease and the acarus immediately does,] why the itch is so easily caught by only touching the hand of a person infected with it, for it is difficult to extract the insect furrowed under the cuticle."

Vol. II. The second volume of this work is prefaced by some general observations on the Anatomy, Physiology, and Pathology of the Nervous System, from the pen of Dr. Bennett. These are as comprehensive as the space assigned to them would admit; but this space is so small, that no power of condensation would have enabled the writer to do justice to his subject. There is a physiological inaccuracy which we notice the more particularly because it occurs in the statement of a doctrine of considerable importance, which has afforded more than sufficient matter for dispute and altercation, and with reference to which, therefore, it is essential that everything should be laid down as clearly as possible. The view given by Dr. Bennett of the "excito-motory function" of Dr. Marshall Hall, would convey the notion that in the various instances of this kind of action, the *impression*, though distinct from sensation, is made on the extremity of a sensatory nerve; and that the *motory impulse*, though unconnected with volition, is propagated along the course of an ordinary motor nerve. Now this is not Dr. Hall's doctrine. His doctrine is that, independently altogether of the sensatory and motory tracts of the spinal cord, there is, wrapped up in the same sheath and centrically situated with regard to them, a distinct organ devoted to the excito-motory functions; and that, altogether independent of the sensatory nerves connected with the posterior columns and the motor nerves connected with the anterior columns, there are certain *incident nerves* enveloped in the same neurilemma with the sensatory, by which impressions are conveyed to the excito-motory centre, and certain *reflex nerves* enveloped in the same neurilemma with the motory, by which impulses resulting from such impressions are conveyed to the muscles and there cause contraction.

The general pathological views developed by Dr. Bennett are based on the hydraulic principle maintained by Kellie, that the absolute quantity of blood contained in the cerebral vessels is always the same, however much it may vary in the other parts of the vascular system; but that the relative proportion of blood in the arteries and veins of the brain, as well as the degree of pressure exerted by it, are subject to frequent and rapid variations. Of the general truth of this principle, and of its applicability to pathology, we have no doubt; but we think our author is carrying its application a great deal too far, when he assumes that all derangements of the cerebral functions, the cause of which cannot be explained by structural lesions detected after death, are to be referred to cerebral congestion. (p. 9.)

We admit that we are in no better condition directly to disprove this opinion than Dr. Bennett is to establish it. Facts are wanting; but

where this is the case, we are justified in resorting to the most probable analogies. Now, there is every reason to believe that various disturbances of function may occur in a portion of a nerve quite independently of disease of the brain or spinal cord, as neuralgia and paralysis; and it will not be denied that, in many such cases, no derangement of vascular action or lesion of structure can be discovered by the nicest examination; but here the hydraulic principle does not apply, the free tract of the nerve being quite differently related to atmospheric pressure from the inclosed mass of the encephalon;* we are therefore led to admit the possibility of deranged nervous function independently of vascular congestion; and if in one portion of nervous matter, why not in another? If, for example, in the radial or anterior tibial nerve, why not in the brain or spinal cord?

The truth is, we know literally nothing of those molecular changes from which the healthy actions of the nervous system most probably result; and while we continue thus ignorant, we must not hope to arrive at any satisfactory explanation of those morbid actions of the same system which leave no visible traces behind them.

The article on Inflammation of the Brain, by Dr. Hope, has great merit, and evidently embodies the results of much reading and of careful and extensive personal observation. He dwells at considerable length on the precursory symptoms of inflammation of the brain, a subject of vast importance, but of which the investigation is beset with theoretical and practical difficulties.

Dr. Hope distributes his observations under the following heads: 1. Cerebral determination and congestion. 2. Cerebral congestion from debility and inanition. 3. Cerebral irritation, determination, and congestion in infants and children.

A preternatural flow of blood to the brain in consequence of increased arterial action is denominated "cerebral determination." When this exceeds a certain point, the blood, not being carried off by the veins as rapidly as it is introduced by the arteries, accumulates in the latter vessels and produces the state called "active cerebral congestion." When an accumulation of blood takes place in the brain, not from increased arterial propulsion, but from deficient contractility in the capillaries, or from a mechanical impediment to the return of venous blood from the head, the morbid state induced is called "passive congestion," and its seat is principally in the veins and sinuses.

Dr. Hope adopts the hydraulic principle just now adverted to as the groundwork of his reasoning on the derangements of the cerebral circulation, and states that he entertains no doubt of its general accuracy, though he thinks it probable that future experiment will prove the brain to be capable of admitting a small addition to its ordinary quantity of blood. In this we concur, and believe that the principle in question may be safely applied in all reasoning on morbid changes in the circulation within the head: we object to it only when brought in hypothetically to

* We use the word *encephalon* in accordance with the ordinary practice of medical writers, although the proper word is *encephalos;* the former is in reality an accusative improperly used for the nominative. Vide Blaucardi Lexicon ed. Kühn, or Hooper's Dictionary by Grant.

explain cases in which we have no distinct evidence of any change in the circulation, and where the phenomena, for anything we know, may have no immediate dependence on vascular action.

Dr. Hope thinks it may be stated in general terms:

"1. That the symptoms of active determination are those of excitement or exaltation of the cerebral functions; since there is increased arterial action, yet not congestion enough to obstruct the circulation and occasion the opposite train of symptoms. 2. That the symptoms of active or arterial congestion are those of depression of the cerebral functions; because, here the disease has proceeded to the extent of occasioning an obstruction to the circulation through the brain. 3. That the symptoms of passive or venous congestion are likewise those of depression of the cerebral functions; because, here also, there exists an obstruction to the circulation. Now the three states in question are often the precursors both of inflammation of the brain in any of its forms, and of apoplexy and palsy; nor can we always determine in individual instances, why they conduce to the one rather than to the other; but we think it true, as a general rule, that increased determination, implying augmented arterial action, is equally productive of inflammation and of apoplexy and palsy; while congestion, especially the passive form, is more allied to apoplexy and palsy than to inflammation. Sometimes the nature of the exciting cause determines whether the premonitory symptoms shall issue in inflammation or in apoplexy: thus, stooping may occasion apoplexy, while mental excitement may induce inflammation. So formidable are these consequences, whether of one kind or the other, that the states conducing to them must be looked upon as serious maladies; and it is important to the student to know that, in practice, he will encounter an incomparably greater number of 'determinations' and 'congestions,'—in other words, of precursory symptoms,—than of actual inflammations, apoplexies, and palsies." (pp. 11-2.)

With respect to the author's ensuing remarks on the symptoms of these states, as well as on the semeiology of cerebral diseases in general, we think the practical reader will agree with us in regarding them as too explicit and minute to be valid in the present state of our knowledge. At the same time, as they appear to be the fruit of much personal addiction to the subject, they are entitled to a careful perusal.

The description of actual inflammation of the brain and its membranes is very complete and masterly. In treating of meningitis the author enquires whether the arachnoid or the pia mater be the more frequent seat of inflammation; and concludes, in opposition to the opinions of Lallemand, Parent-Duchatelet, and Martinet, that the pia mater is most frequently affected. He states that he adopts this opinion not only from extensive observations of his own, but from an analysis of the cases recorded by several distinguished pathologists; and he agrees with the majority of English writers that we have no means of distinguishing the symptoms of arachnitis from those of inflammation of the pia mater, and that it is therefore expedient to treat of inflammation of these membranes under the common name of *meningitis*. The question whether the symptoms of meningitis can be distinguished from those of cerebritis is discussed by Dr. Hope with great judgment. He admits that meningitis cannot be supposed to exist without exciting inflammation or irritation of the surface of the brain, because the membranes and the contiguous substance of the brain are immediately supplied by the same blood-vessels, which, ramifying and subdividing with extreme minuteness in the membranes, penetrate the cerebral substance in every

direction. This arrangement, as observed by M. Georget, constitutes an exception to the ordinary manner in which blood-vessels enter the substance of organs; for these in general being more or less spongy and areolar, the vessels penetrate them by trunks and branches, and the whole of their vascular system exists in their interior: but the brain is not spongy and areolar; it contains no cellular substance; and presents therefore a necessary peculiarity in the distribution of its blood-vessels. Dr. Hope observes that the connexion between inflammation of the membranes and inflammation or sympathetic irritation of the adjacent cerebral surface, which is implied in such a distribution of the blood-vessels is strongly corroborated by morbid anatomy, and no less so by the symptoms of disease, since the lesions of the intellectual, sensitive, and voluntary powers which accompany meningitis argue a disturbance of the functions of the cerebrum itself. Is it then possible to distinguish meningitis from cerebritis during life? Many eminent writers, among whom are Abercrombrie and Georget, believe that it is not possible. Dr. Hope's observations and dissections have led him to a view of the subject which appears to us so just that we shall give it in his own words:

"When we place, on the one hand, meningitis with the least possible degree of inflammation of the surface of the brain, and on the other, cerebritis not implicating the membranes, the difference between the symptoms is so marked, that the diseases can scarcely fail to be distinguished from each other by a discerning practitioner. But when the two affections coexist, the one will so far modify the other, as in a great measure to neutralize the characteristic symptoms of each. Yet the compound or intermediate character of the symptoms in such cases will sometimes indicate even the double affection, and a predominance of the one or the other may occasionally be inferred from the preponderance of its particular symptoms. We are far, however, from supposing that these latter distinctions can be formed with certainty. The utmost length to which it is possible to go, is to establish more or less strong probabilities. Entertaining these views, we shall treat meningitis and cerebritis as distinct affections, and endeavour to point out under each the manner in which it is modified by the coexistence of the other. In this way, the compound affection which is of far more frequent occurrence than either in the independent form, will, we think, be rendered more intelligible than if it were treated separately as a distinct variety. In this view we are countenanced by an analogy derived from the lungs. Pleurisy and peripneumony are always treated of separately; whereas pleuro-peripneumony is not." (p. 29.)

The precepts for the treatment of the various forms of encephalic inflammation are such as are sanctioned by the best experience. The writer seems to place more reliance on mercury in acute inflammation of the brain, than our own observation has led us to do. We quite agree with him, however, on the expediency of using it early: he observes with truth that the fear of its acting injuriously as a stimulant, which is unfounded, frequently prevents its employment till the occurrence of effusion or disorganization has rendered the probability of benefit from its use extremely small.*

* We believe that the fear of the stimulating effects of mercury in acute inflammatory diseases is generally without foundation. When we consider that inflammation is accompanied either by *diminished* or *vitiated* secretion, while the effect of the mercurial erethism is to *increase the natural secretion* throughout the system, we are led to infer that the mercurial erethism is much more likely to supersede than to augment inflammatory excitement.

His estimate of the success of treatment in the chronic forms of cerebral inflammation appears to be somewhat higher than general experience will warrant. He says,

"Great perseverance is often requisite to effect a complete cure of these chronic affections. If the practitioner proceed, unwearied and undaunted for many consecutive months, his efforts will not unfrequently be crowned with success; but if his patience fail after a few weeks, and he be tempted from the apparently low state of the patient to put him on full diet, failure and disappointment will commonly be the reward of his want of firmness." (p. 61.)

We believe that the protracted duration which gives scope for perseverance affords of itself almost the only ground of hope; for in the few cases in which we have had the good fortune to witness a favorable issue, this has appeared to us to have been referrible to the gradual wearing out of the disease much more than to the influence of remedies.

The subject of Hydrocephalus is treated in an able and practical manner by Dr. Bennett. We find no ground of dispute with him and no deficiency which any observation of ours could supply. Pass we therefore to the article Apoplexy, by the same gentleman.

In theorizing on the proximate cause of apoplexy Dr. Bennett's reasoning is in conformity with those views of the peculiarities of the cerebral circulation already several times alluded to, and we need not here reiterate the grounds on which we believe that he has carried the application of those views too far. In speaking of the treatment of apoplexy, Dr. Bennett recommends stimulants in the asthenic form more decidedly than is usual with writers of the present day.

We confess that, in our inmost mind, we have often questioned whether the diversity of treatment founded on the old distinction of apoplexy into sanguineous and serous,—false as that distinction was,—may not have led to better success in practice than the almost uniformly antiphlogistic course pursued of late years. That apoplexy, in its purest and most intense form, may exist without effusion, or any visible change in the brain or its vessels, is we conceive sufficiently proved by the researches of Abercrombie and others; and we think that the recognition of this fact should lead us carefully to reconsider the principles on which we have been accustomed to treat this disease. In the asthenic form, with a feeble pulse and a collapsed state of the system, we can see no reason why stimulants might not be generally beneficial; whether with Dr. Bennett we regard such state as one of cerebral congestion from debility of the capillaries, or whether we ascribe it, as we think may with more probability be done, to a molecular change in the nervous mass, the nature of which is inscrutable in the present state of our knowledge.

The article Insanity, by Dr. Prichard, contains a useful digest of the opinions elsewhere stated more at large by that justly distinguished author. The subject has so recently occupied our attention, that it would be superfluous to recur to it at present.

The article on Delirium Tremens, by Dr. Bennett, condenses the greater part of what is known on the subject: we think, however, the writer has made a serious omission in passing over entirely without notice,

the opinion of Broussais that delirium tremens is symptomatic of gastro-enteritis. This opinion, however, untenable as an exclusive one, has been so modified by Dr. Stokes of Dublin, as to assume an aspect of sufficient probability to entitle it to serious consideration. This pathologist believes that where the disease supervenes on a debauch, the case is in effect one of gastritis, accompanied and entirely represented by a cerebral affection; but that where it arises from the sudden abstraction of the accustomed stimulus, it is to be regarded as a purely nervous disorder. This view of Dr. Stokes's appears to have been well supported by dissections and the results of treatment.* However this may be, the distinction between the two forms of delirium tremens,—that succeeding to a debauch, and that arising from privations of an accustomed stimulus,—is beginning to be generally admitted by intelligent practitioners as an important guide to the treatment, and as such it is properly insisted on by Dr. Bennett.

The succeeding chapter, on CEPHALALGIA, is by the same writer. He distinguishes headach into the following forms:

"The *congestive*, from congestion occasioned by increased or diminished vital action of the heart and blood-vessels. 2. The *inflammatory*, from inflammation of the membranes or substance of the brain. 3. The *sympathetic*, from disorder of the digestive, biliary, uterine, urinary, and other organs. 4. The *organic*, from structural change of the bones of the cranium, or the membranes, or substance of the brain. 5. The *neuralgic*, from affection of the nerves distributed to the integuments. 6. The *metastatic*, from the metastasis of disorders. 7. The *intermittent*, occurring at stated periods." (p. 155.)

We have no objection to this classification of headachs, nor do we doubt the existence of that termed *congestive;* but we cannot at all agree with our author when he says "it is very probable that every species of headach, except the organic and neuralgic, depends upon a greater or less degree of congestion of the vessels of the brain."

The chapter on EPILEPSY, also by Dr. Bennet, is a good and practical one. It is much to be deplored, however, that the everlasting "cerebral congestion" comes in once more as an indispensable condition to the epileptic paroxysm. Dr. Bennett admits the hereditary character of the disease, in which we fully agree with him; and we think that the only rational mode of accounting for it is by the supposition of a transmissible peculiarity of organization.†

The article on CATALEPSY AND ALLIED AFFECTIONS, also by Dr. Bennett,

* See Brit. and For. Med. Rev., No. XVIII, p. 477.

† On this and parallel subjects, we think the observations of Dr. Holland, published in his "Medical Notes and Reflections," are highly worthy of attention. The fact that inveterate epilepsy is sometimes met with in persons otherwise perfectly healthy, appears strongly to favour the notion of a peculiarity of structure. We have at present under our care a youth about eighteen years of age, who presents a remarkable example of this. He has been affected with severe and frequent attacks of epilepsy for the last eight years. In all other respects he is in the most perfect health. He is of a robust and powerful frame without plethora, and all his functions are well and vigorously performed. We can assign no cause for his affliction but that it is in his family. His sister, who died at the age of nine years, was epileptic and idiotic, and a brother, who is now living, is epileptic and maniacal. Neither of his parents is subject to any cerebral disease, but his father's mother was epileptic.

is a short one, but long enough, nevertheless, to afford space for certain hints concerning "cerebral congestion," whereat we do not wish to conceal our affliction. Otherwise it contains nothing requiring comment.

The article on Spinal Irritations is from the same hand. Although we think that more than enough has been said of late about spinal irritation, considered as a distinct and peculiar disease, we quite concur in the following remarks of Dr. Bennett:

"In all cases of neuralgia, rheumatism, and hysteria, the spine should be examined; while perhaps there is scarcely a functional disorder to which the young female is liable, which may not occasionally be found connected with spinal irritation. We have often had an opportunity of observing the manner in which numerous disorders have been traced to this source, and feel assured that if practitioners in general would pay greater attention to this complication, many of the extraordinary and anomalous cases which are at present the cause of great embarassment in practice, might terminate in the speedy relief of the patient, and increased credit of the physician." (p. 188.)

In truth we continually meet with cases which are pronounced "anomalous," but which so far from being really so, are simple examples of the well-known pathological law that the effects of irritation at the origin of a nerve may be felt principally at its distal extremity.

On the nature of the morbid state of the spinal cord in the affection under consideration, Dr. Bennett remarks:

"As spinal irritation uncomplicated with other disease rarely terminates fatally, the exact anatomical characters of this affection are unknown. There can be little doubt, however, that in the majority of cases the symptoms are referrible to a state of congestion of the spinal cord or its investing membranes. Ludwig and J. P. Frank have alluded to the effects of spinal congestion and the anatomical circumstances which favour its occurrence. The latter, in particular, has pointed out the absence of valves in the spinal vessels, together with their peculiar distribution on the surface of the cord. Their anatomical structure and arrangement render them peculiarly liable to congestion, as the venous blood must ascend in opposition to gravity. They are also equally pressed on by the cerebro-spinal fluid in a state of health, and any cause which tends to increase or diminish its normal quantity, may readily be conceived to produce venous congestion. Hence derangements in the menstrual function, and the various causes which have been mentioned frequently occasion dorsal and lumbar pains, with other symptoms of spinal irritation. The vessels being principally superficial, unless very much dilated, occasion only partial pressure, and consequently increased action followed by the principal phenomena of this affection, viz. neuralgic pains. That motion is not so commonly increased or diminished, may be attributed to the relation of the cord with the osseous walls which surround it, as pointed out by Ollivier, for the anterior columns are almost immediately applied to the bone, while the posterior are five or six lines distant from it. Independent of any positive evidence, therefore, it may be said that the theory of congestion is fully capable of explaining the phenomena, and is the morbid condition which, of all others, we should expect to follow the known causes of the disorder. On the other hand, we cannot with some authors suppose it to be chronic inflammation, for the changeable nature of the affection and its sudden appearance and disappearance are opposed to such an opinion. With regard to the spinal tenderness, it has been well pointed out by Mr. Locock, (Edin. Med. and Surg. Journal, No. cxxxvi.) that 'an inspection of the vertebral column in an anatomical subject will show at once how impossible it is to press the cord or the nerves going from it in the slightest degree.' We cannot think with him, however, that 'tenderness of the spinal marrow is a

sign of little value,' as numerous cases prove that there is a connexion between the situation of the local tenderness and other symptoms, while local treatment has dissipated the disorder, when remedies directed to the removal of its more remote effects have failed." (p. 187.)

With respect to the pain excited by pressure on the spine, it should be remembered that, although the cord is evidently exempted from the immediate effects of the pressure, it is contained in an elastic osseous and cartilaginous tube, the parts of which are moveable on each other, and surrounded by a fluid, through the medium of which vibrations may be easily communicated. We need be at no loss, therefore, to account for the effect of external pressure when the cord is morbidly irritable. We may remark also that a slight tap will often excite more uneasiness than a considerable pressure more slowly applied, which, we apprehend, is owing to the vibration thus occasioned. We had some time ago a female patient in whom we could at will produce a sensation like that of an electric shock, followed by immediate syncope, by striking with the fingers on any part of the spine.

Dr. Bennett mentions, as the most frequent causes of spinal irritation, "uterine disorder; exposure to cold and moisture; dyspepsia, worms, and other sources of irritation in the alimentary canal; affections of the liver, mental emotions; erysipelatous, rheumatic, and eruptive fevers; local injuries, &c." There is one cause which we would specify, because we have observed it to be frequent, namely protracted leucorrhœa, the source of innumerable diseases in women, and one which is too often overlooked or neglected.

The articles on Spinal Meningitis and Myelitis by Dr. Bennett, though brief, are judiciously written; and valuable, as forming some addition to the small number of treatises on these subjects which are accessible to the mere English reader. The subject of Hydrorachis succeeds, and is well handled by Dr. Bennett. A notice of Spinal Apoplexy, by the same writer, contains what little there is to say upon the subject.

The article on Chorea, by Dr. Theophilus Thompson, is an excellent one. The ordinary nosography of chorea as it now presents itself is followed by a short account of the singular modifications of it which have been observed at different times from the first outbreak of the dancing mania. These are divided by the writer into three classes: 1. Those consisting chiefly in energetic and often measured actions of the muscles under the influence of a morbidly excited will, as the dances of St. John and St. Vitus; tarantism; and the leaping ague of Scotland. 2. Those in which the movements are systematic but involuntary, as malleation;* rotation; and propulsion, forwards or backwards. 3. The convulsive disorder which has occasionally prevailed as an epidemic in times of religious fanaticism, and which is usually accompanied with irregular exacerbations and remissions.

Our author observes that the phenomena of the last class have been well described by Dr. Robertson, as they occurred among a sect of en-

* Striking of the knees with the hands as with a hammer.

thusiasts in Tennessee and Kentucky in the year 1800, (Inaug. Essay on Chorea St. Viti, 1805.) He mentions a similar affection as occurring at Cambuslang in the year 1742. Here the movements which were at first voluntary became spasmodic, the muscles of the neck and upper extremities were convulsed, and the sufferers were thrown down and agitated with motions like those of a fish on land. (p. 208.) The occasion of these performances was no other than the famous "Cambuslang awakening," the impression of which was long felt among the peasantry of the south of Scotland, and which is even now frequently talked of.

Some extraordinary developments of blended chorea and mania have at different times resulted from the supposed machinations of infernal spirits or their human agents. The curious reader may find some cases of this kind ludicrously exaggerated by superstitious credulity in Cotton Mather's Ecclesiastical History of New England.*

Dr. Thompson's remarks on the morbid anatomy and immediate seat of chorea are, we think, extremely good, and we regret that our space will not allow us to insert them. The general indications in the treatment of chorea are stated to be: 1, To ascertain whether there be any congestion or irritation of the cerebro-spinal axis, and if there be, to relieve it by moderate local depletion; 2, To act freely on the bowels by suitable purgatives; and 3, To administer remedies calculated to invigorate the frame, diminish nervous susceptibility, and increase the energy of the digestive function. (p. 213.) The particular means of fulfilling these indications are explained with judgment and precision.

The succeeding article on Hysteria is by the same writer. His description of the irregular forms of the disease is scarcely so full as we could have wished, and we think he might with advantage have dilated on some points of diagnosis. We may here, in passing, notice one circumstance not usually mentioned by writers, but which it is important to attend to in practice, namely, that hysterical pain in the abdomen and head is sometimes accompanied with an excited pulse and a hot skin. In one hysterical patient we have witnessed extreme tenderness of the abdomen, with a quick and full pulse, a hot skin, and frequent vomiting. In the same patient hysteria sometimes mimicked phrenitis, yet in both instances the physiognomy and manner of hysteria were so strongly marked as to leave no doubt of the real nature of the case on the mind of an attentive observer. This lady, however, when from home, more than once fell into the hands of practitioners who, misled by the symptoms, bled her, with great aggravation of the evils. Dr. Thompson is, we believe, quite correct in affirming that the cerebral excitement of hysteria is often mistaken for phrenitis, to the great prejudice of the patient. Acceleration of the pulse accompanying hysterical attacks arises, we apprehend, from an irritable state of the heart, and may be distinguished from febrile or inflammatory excitement by its not being uniform, but varying with slight changes in the state of the patient's mind, and other circumstances which would not affect the pulse of fever or inflammation.

* Magnalia Christi Americana; or the Ecclesiastical History of New England, &c.—London. Fol. 1702. Bk. vi. Ch. 7. Four children in particular were "arrested by a very stupendous *Witchcraft.*"

There is perhaps no malady in the diagnosis of which we may derive more important aid from physiognomy than hysteria. Medical physiognomy is indeed in many diseases a source of diagnosis which seldom fails the practitioner who is intimately versed in it; and we believe that much of that exquisite tact in the discrimination of disease which distinguishes some practitioners, and which others can never attain, depends on the vivid perceptions of an eye and ear habitually familiar with the lineaments, the tones, and the gestures of disease.

On the whole, this article is a very able one. The directions for the treatment of hysteria are both copious and judicious, and the concluding paragraph on female education contains much truth happily expressed.

In the articles Tetanus and Hydrophobia, Dr. Bennett has given a good exposition of what is known concerning these diseases. We may remark, however, that we are extremely sceptical as to the cases of recovery from hydrophobia referred to by the writer; and we feel ourselves compelled to state that the very first of the cases alleged, in page 260, to have been cured by bloodletting, was not an example of the disease in question, and was not even adduced as such by Dr. Innes, by whom it is recorded. It is entitled, "An inflammation of the stomach, with hydrophobia and other uncommon symptoms." The disease is not referred to the bite of any animal, and the hydrophobia is only mentioned as a symptom. (Edin. Med. Essays and Obs., vol. i. p. 227; Ed. 1752.)

Dr. Theophilus Thompson has done great justice to the subject of Neuralgia, and that of Paralysis is ably treated by Dr. Bennett. We have nothing particular to say on any of these heads, nor on that of Barbiers, a short notice of which is given by Dr. Bennett.

The volume closes with three articles on Inflammation of the Eye, on Amaurosis, and on Otitis: the two former by Dr. Taylor, the last by Dr. Bennett. These we omit to notice because we have recently treated so largely on this class of diseases.

We trust we shall not be accused of a carping disposition if, before concluding, we point out some literary faults which detract from the general merit of this volume, and for which we hold the editor more responsible than the authors of the articles. "A *mutual* relation to *each other*," (p. 1,) and "*post-mortem* appearances found *after death*," are grievous tautologies. "The practitioner should not be *unwarily scared*," (p. 15), is a queer expression; and "suddenly-or-otherwise-injudiciously-checked morbid discharges" is considerably more in the genius of the German than of the English language. Many examples of equally bad writing might be adduced, and the defects of the English, we are sorry to say, are by no means compensated by the excellence of the Greek. At p. 231, the tetanic distortion of the body to one side is designated as "*pleurosthotonos*, from πλευροσθεν sideways." There is no such word as πλευροσθεν, and *pleurosthotonos*, though commonly enough used, is a barbarism; it should be *pleurothotonos*, from πλευροθεν, sideways. At p. 281, χωφος should be κωφος. In the same page, *ageustia* is derived "from α priv. and γευστος taste;" but γευστος is an adjective, and signifies capable of being tasted, i. e. having a flavour. But we hate the

snarling part of our duty, and will therefore note no more minor faults. The volume taken altogether is highly creditable to those engaged in its production; many parts of it are written with great ability, and we have no hesitation in heartily recommending it as a valuable contribution to practical medicine.

Vol. III. In this volume are described diseases of the respiratory organs and passages, and of the heart.

The subjects of Bronchial and Pulmonary Diseases are treated of with considerable fulness and with the acknowledged precision of that physician, by Dr. C. B. Williams. However, as the different articles are scarcely more than reprints of a series of lectures already placed before the public in the Medical Gazette, it is unnecessary to undertake any very close examination of them on the present occasion.

Another instance of defective nomenclature strikes us in turning over these essays: Why does Dr. Williams speak of tracheitis *or* croup, when he himself labours to prove, what no one who has seen the disease will question, that this is not tracheitis in the ordinary acceptation of such term? We might add, in *any* acceptation; for, admitting that foundation exists for the notion advocated by Mr. Ryland and himself, that the cellular membrane as well as the mucous is engaged in the inflammatory action, there is besides this a nervous or spasmodic element in the disease, which separates it effectually from the whole class of maladies distinguished by the suffix *itis*. Now the importance of the condition of the laryngeal muscles is, practically speaking, extreme; for this is, in rare instances, the efficient cause of death. A case observed by Lobstein demonstrates this: the patient indubitably had true croup, for he had expectorated pseudo-membrane, yet, as dissection disclosed, not the least trace of lesion of any kind existed in the air-passages at the time of death.

Dr. Williams includes among the characters of an asthenic form of the disease, *extension* of the albuminous exudation to the throat and fauces. He neglects to notice the very important opinion, originally stated by Bretonneau, and subsequently espoused by numerous observers of eminence, that croup is frequently *preceded* by pseudo-membranous inflammation of the pharynx: M. Guersent estimates at nineteen out of twenty the number of cases of croup in the infant originating in this manner. We are perfectly aware that this statement will be received by many practitioners with stubborn incredulity; but of persons thus meeting the alleged fact, it may not be improper to enquire, in how many cases they are prepared to prove they have examined the fauces with the necessary care at the outset of the complaint.

The experience of Mr. Porter, M. Hervez de Chegoin, and others shows that the morbid exudation may *commence* in the bronchi, and thence spread upwards. Dr. Williams might also have supplied his readers with more accurate information respecting the frequency with which the plastic formation arises secondarily or otherwise in those tubes, than the vague intimation that it occurs "in some instances." M. Guersent publishes the following analysis of 150 cases by MM. Houssenot and Bretonneau:

| | |
|---|---|
| False membrane not extending below the bronchi . . . | 78 |
| ,, ,, seated in the bronchi . . . . . | 42 |
| ,, ,, in larynx or trachea, the state of the bronchi not being mentioned . . . . . | 30 |
| | 150 |

The history of croup in the adult surely merited separate consideration, though Dr. Williams has not acted as if he thought so.

"It is not necessay," says Dr. Williams, "to discuss the question of the propriety of resorting to tracheotomy in croup, as it has been decisively negatived by Dr. Cheyne, Mr. Porter, and others of the *best authorities*." These eternal authorities! How shall truth ever force her way, while the dicta of these lawgivers suffice to suppress even discussion? But even assuming the question to be one of authority; who shall compare the respect due, on the one hand, to the assertion of MM. Bretonneau and Trousseau, who have performed the operation ninety-eight times, and on the other to that of Dr. Cheyne, who probably never cut into the trachea in his life? Now Bretonneau and Trousseau tell us to operate, and affirm that doing so affords in many cases the only hope of rescuing the patient from imminent death. "Authority" is therefore, we suspect, rather against Dr. Williams. But let us consider the question as one of facts and not of names. The facts, then, are that of 140* cases of croup in which the operation has of late years been performed by different French surgeons, 28 [20 per cent.] have terminated by recovery. Is this a small share of recoveries, when, as Dr. Williams reports from Double, the mortality in the present day† is nearly one half [50 per cent.] of the whole numbers attacked? formerly, when the treatment of the disease was less understood, it amounted to nearly four fifths." [80 per cent.] But further, let it be remembered that the vast majority of these cases were in an utterly hopeless state when the knife was used. What proportion of cases, we would ask, that have fallen into the utterly hopeless category, would recover independently of tracheotomy? Numerical precision cannot in the present state of our knowledge be given to any inference on this point; but we fear did this exist it would prove the deaths to be something very like 10 per cent. of the cases. It is probable that the main cause of failure in unfortunate cases is that the operation is habitually delayed too long; this is rendered indeed almost certain by the fact that while Bretonneau and Trousseau, who performed it at an earlier period than others, can boast of 25·4 per cent. of recoveries, their brethren can exhibit no more favorable result than 9·5 per cent. Another cause of failure is to be found in the existence of pneumonia, marked and extensive, at the period the section of the trachea was made.

Is the result of the operation for hernia performed *in extremis* vastly more favorable than from existing facts the operation for croup is proved to be?

* These numbers are taken from a table given in the article Bronchotomy, by Mr. Wells, in the Cyclopædia of Practical Surgery, vol. i., p. 513.

† Double's book was written before Bretonneau's performances had drawn the attention of observers to opening the trachea.

Influenza. This is written by Dr. Theophilus Thompson. Under the head of "nature of the disease," the author having observed upon the rarity with which uncomplicated influenza destroys life, states that in the rare instances when such event has occurred, the mucous membrane of the larynx and bronchi has been found of a deep red colour, flakes of lymph sometimes observed on the chordæ vocales and in the ventricles, the trachea found injected and lined with glassy-looking mucus, the lungs overcharged with sero-mucous fluid, engorged and even consolidated in a limited extent of their substance. His views of the essence of the malady appear in the following paragraphs: "The danger in extreme cases of influenza appears to arise from an excess of mucus preventing the due arterialization of the blood. The difficulty and rapidity of the respiration are, however, out of all proportion to the quantity of secretion, or even to the amount of inflammation, and the dyspnœa is sometimes intermittent; and this circumstance cannot easily be explained except by supposing that the cause of the disease must operate, by producing an impression on the vital energy of the lungs, analogous to that occasioned by cutting the nervus vagus; and we may reasonably conjecture that influenza depends on an influence exerted on the nervous system, especially on that part of it having most relation to the bronchial mucous membrane, tending to elicit any latent predisposition to disease, and modified in its character by varieties of constitution as well as by peculiarities of climate and other external conditions. . . . . This opinion derives support from the liability, so frequent in this complaint, to derangement in a great variety of organs, as well as from the occasional occurrence of inflammation of the spinal cord, and of inflammation or other affections of the brain. The effects of such a shock thus communicated to the nervous system may be expected to develope themselves in the weakest organ and to vary according to collateral circumstances. Thus, in the same epidemic, one patient will suffer from meningitis, another from enteritis, a third from rheumatic affections. If a sudden increase of temperature succeed to frost or snow, pneumonia will frequently be found associated with the complaint, whilst exposure to fatigue and mental anxiety will increase the liability to erysipelatous complications." (p. 207.)

Respecting the degree of severity of the English epidemic of 1836-7, Dr. Thompson states that "the deaths, as far as could be ascertained, were about two per cent. of the number attacked, a proportion corresponding with that deduced by Ozanam from a calculation of the mortality of all the recorded instances of epidemic catarrh." And he deduces from his own historical sketch that, "since medical records have become available, influenza has prevailed on an average once in ten years, and has proved the most destructive of epidemics." We presume that Dr. Thompson means that the influenza has, in the gross, destroyed most lives; as it is impossible he could have intended to term an epidemic cutting off two per cent. of those seized the "most destructive" of that class of maladies.

The point mooted in the following sentences is not unworthy of consideration:

"There is also reason to believe that a modified condition of the atmosphere may remain for years after the prevalence of the disease, and occasion a liability to affections of a similar character, to which the term *influenzoid* might be ap-

plied. For eight years after the prevalence of influenza at Lyons, this was found to be the case: 1300 deaths out of 10,096 occuring during that period being attributed to catarrhal or mucous fevers. We are inclined to believe that a similar condition has existed in this country since 1833, affections of the bronchi and fauces having been unusually prevalent, associated with severe muscular pains and unusual depression of strength." (p. 207.)

Dr. Thompson seems much too regardless of the importance of accurate bibliographical references; what is the value of an intimation that certain cases are related in "Rust's Mag. and Lond. Med. Gaz.," without even their author's name being mentioned? Does Dr. Thompson mean to "laugh at the beards" of those who might feel inclined to enquire into his accuracy as a reporter of the opinions of others?

The essay on Asphyxia, by Dr. Carpenter, contains a very complete account of all that is actually, or is believed to be, established of the practice respecting that condition. The article is written with great clearness and precision, and the subject generally treated with much comprehensiveness; but as it professes to contain nothing particularly novel, we shall content ourselves with recommending its perusal to our readers.

Dr. Joy has contributed a very excellent essay on Diseases of the Heart, exhibiting correct acquaintance with recent additions to our knowledge of this class of maladies. We have rarely met with a monograph having stronger claims to attentive perusal. Nevertheless, we cannot agree with all Dr. Joy's opinions. Thus, he quotes with apparent commendation the estimate of Bouillaud, that "at least one half of all the cases of acute rheumatism occurring in practice, or what is ordinarily called rheumatic fever, are accompanied in some part of their course by an inflammation either of the internal or external lining of the heart, or both;" whereas we are persuaded, and we know that many close observers unite in this persuasion, the assumed proportion is very materially too high. Further, M. Bouillaud maintains, as our readers are aware, that the inflammation of the membranes of the heart commonly proceeds *pari passu* with the articular affection, instead of, as originally stated by English writers, arising in consequence of metastasis from the joints to the heart. Dr. Joy thinks M. Bouillaud underrates the frequency of the latter mode of development; a correction in the justness of which we are the less disposed to coincide, because we find Dr. Joy seeking an *à priori* argument in its favour in the similarity of the tissues engaged in the inflammatory process in both situations. Why should not this similitude be as fair a reason for their suffering *simultaneously?* In fact, we long since heard M. Bouillaud assert that it was such, and affirm that the identity of structure would suffice to show that both orders of tissue *must* frequently be affected together;* a mode of prejudging the question which first led us to conceive a doubt, justified and confirmed by subsequent experience, of M. Bouillaud's perfect accuracy in relation to the point already referred to. The truth is that identity or non-identity of

* So are the arachnoid and tunica vaginalis both composed of the same species of tissue; yet M. Bouillaud will hardly affirm that individuals with meningitis are especially liable to have inflammatory hydrocele.

tissue has nothing to do with the matter; the question is one to be decided by direct observation alone.

We are pleased to observe that Dr. Joy has given M. Louis his due position among the successful investigators of pericarditis. He it was that first placed its diagnosis within the reach of the ordinarily attentive observer by his just discrimination of the three most important physical signs of pericarditic effusion; unnatural dulness under percussion of the præcordial region, vaulted form of that region, and distance and feebleness of the cardiac sounds. But it has been the habit to forget M. Louis' modest memoir in admiration of the *tomes à prétention* of others.

Here is a nice point in diagnosis which we can readily conceive might become of importance at the bedside:

"An enlarged and feebly-acting heart may be distinguished from the case of profuse pericardial effusion with weak impulse and distant and indistinct sounds, by applying the stethoscope to the supra-clavicular region, where in the latter case, and in it only, the cardiac sounds will be heard with considerable clearness in the course of the carotid and subclavian arteries, indicating that their feebleness in the præcordial region is the result of an obscured, rather than of an actually deficient action." (p. 318.)

We extract a portion of the account of the *bruit de frottement* of pericarditis, as affording a useful practical lesson and a good specimen of Dr. Joy's manner:

"Very early in the course of the disease, as on the second or third day for instance, a faint rubbing or rustling sound (bruit de frottement, or to-and-fro-sound, murmur of ascent and descent, &c.) such as that produced by the friction of silk, paper, or parchment, is frequently audible, accompanying both sounds of the heart. It gradually assumes a louder rougher character, and generally extends eventually over the whole region of the heart, and materially obscures the natural sounds of the organ, though they may still be recognized by applying the stethoscope near the top of the sternum. It has its source in the friction of the opposed surfaces of effused lymph, which, even whilst still very thinly spread and soft, is quite sufficient for its production, as has been fully ascertained by the experiments of Drs. Williams, Clendinning, and Todd. Whether the first stage or that of simple congestion and dryness of the membrane be capable of giving rise to it in a minor degree is still doubtful; the experiments of the gentlemen just named render it, indeed, very improbable that it is ever heard except in those cases where ecchymosis under the pericardium, or some slight traces of coagulable lymph on its polished surface, already exist.

"The rubbing sound occasionally somewhat changes its character, and becomes perfectly similar to the creaking of leather in the sole of a new shoe or saddle (cri de cuir, leather-creak). This was first observed by M. Collin; and though Laennec was latterly sceptical as to its import or reality, it has been since fully confirmed as a valuable sign of pericarditis by Stokes, Reynaud, Watson, Mayne, Bouillaud, Williams, and others. ..... The peculiar sound in question is commonly distinguishable from that connected with valvular disease, as Dr. Stokes many years ago pointed out, by the suddenness of its occurrence, and by the short distance from the cardiac region within which it is audible, as well as by the greater influence of treatment over it. But still there may be considerable difficulty of diagnosis where disease of the valves has preexisted, or where endocarditis springs up simultaneously. The rubbing sound is, however, of a decidedly more superficial and equally diffused character than the bellows-murmur, indicative of disease of the valves and orifices; and is more constantly double, or an accompaniment of both motions of the heart." (pp. 312-3.)

Vol. IV. The fourth volume commences with the diseases of arteries and veins. These are illustrated by Dr. Joy under the heads of functional diseases of the arteries; arteritis; aneurism of the aorta; phlebitis; varicose veins; miscellaneous affections of the veins, &c.

In allusion to *neuralgia* of the arteries, the author expresses himself, we think, in too general terms concerning the distribution of the sympathetic nerve on the coats of these vessels. He says that the seat of the pain is most probably in the minute ramifications of the ganglionic nerves which form a close network around the arterial trunks, and penetrate into the substance of their walls. (p. 1.) Now, the anatomical statement here made is true with respect to the arteries of the head, neck, thorax, abdomen, and organs of generation; but its truth is very doubtful with respect to those of the extremities. It is singular, indeed, that so important a point in physiological anatomy should have been so little the subject of practical enquiry; for although it has been believed by many, and stated as a direct result of observation by some, that the ramifications of the sympathetic accompany the subdivisions of the arteries throughout the body, till they elude the senses by their tenuity, we are not aware that a single anatomist has laid before the public any details of the dissections by which he has been led to such a conclusion.

The subjects of abdominal pulsation, aneurism of the aorta, phlebitis, and other affections of the veins are judiciously handled; the author, however, appears to have derived his materials rather from reading than from personal observation, and consequently these articles afford little scope for criticism. We believe him to be right in withholding his assent from the doctrine now so prevalent that *phlegmasia dolens* is exclusively dependent on phlebitis.

"Many pathologists, however, are still averse, and we think with reason, to ascribing such an extensive influence to the veins in the production of the swelled leg of lying-in women, believing that they are only implicated in common with or even subsequent to several other tissues, more especially the subcutaneous cellular membrane, the inferior surface of the cutis vera, and the superficial nerves; for the swelling here, unlike that induced by the ligature of a vein, most frequently begins in the groin, labium, and thigh, and afterwards spreads downwards, in place of always manifesting itself first in the distal extremity of the limb; and it is accompanied, moreover, by an acute neuralgic tenderness diffused over the whole surface, and not met with in the same degree in cases of pure phlebitis, in which the pain and sensibility to pressure are more localized in the course of the vessels. Again, in phlegmasia dolens, the swelling is not œdematous, but from the quantity of coagulable lymph poured out it is tense and elastic, and when the disease is fully formed, rises immediately after pressure, and cannot be evacuated in almost any degree by puncture or incision; and finally the type of the accompanying fever is very dissimilar to that in indisputable phlebitis, and the rate of mortality is incomparably lower." (p. 29.)

The diseases of the organs of digestion succeed, and, taken a sa whole, are extremely well treated. The following articles are all by Dr. Symonds: stomatitis; gangræna oris; diseases of dentition; glossitis; parotitis; angina; hypertrophy of the tonsils; diseases of the œsophagus; gastritis; organic diseases of the stomach; dyspepsia; gastrorrhœa; inflammation of the duodenum; enteritis; inflammation of the cæcum; diarrhœa; cholera; alvine concretions; hemorrhois; spasmodic stricture of the rectum; colic; lead colic; torpor of the colon; tympanitis; peritonitis;

enteralgia. The diseases of the mesenteric glands, and of the assistant chylopoietic viscera, are commented on by Dr. W. Thomson.

In treating of Stomatitis, Dr. Symonds has done well to notice, after M. Guersent, that an epidemic aphthous fever sometimes prevails in Holland, the most frequent subjects of which are adults, and especially puerperal women. (p. 36.) This disease, though curious, has escaped the attention of the greater part of medical writers, which perhaps has arisen from the small number of medical works published of late years by the practitioners of Holland and Flanders, throughout which countries the malady in question appears to have prevailed time immemorial. It is generally attributed by the authors who have described it to the cold and moist atmosphere of those regions.* We notice the subject because we conceive the study of all epidemic and endemic diseases to be of the highest importance to the etiology of disease in general.

When about to speak of the diseases incident to the process of *dentition* (p. 40), Dr. Symonds very properly premises that this process is one of *development*; a truth too little familiar to pathologists, but which has been placed in a prominent point of view by M. T. G. St. Hilaire, in his excellent work on the Anomalies of Organization. Impressed with the correctness of his notions, we feel convinced that those who refer the various diseases incident to dentition merely to the local irritation in the gums, and the sympathetic disorder of the intestinal or other surfaces, take too limited a view of the subject. Fully admitting the influence of these local causes, we conceive that dentition will also vary in its phenomena, and be more or less favorable in its progress, in proportion as the powers of the constitution are more or less equal to the development of a new set of organs.

In this respect it bears an analogy to puberty. It is true that the relations of general physiology to the theory of medicine are extremely vague in the present state of our knowledge; but they should not on that account be left out of consideration; on the contrary, it should be our ceaseless endeavour to render them more distinct, for herein lies the hope of approximating medicine to the exact sciences.

The subject of Gastritis is introduced by some very good general remarks on the anatomical characters of congestion and inflammation of the gastro-enteric membrane, (pp. 54-7,) and the observations on the physiological relations of the stomach which precede the consideration of *dyspepsia*, (pp. 71-3,) are also well worthy of attention. We quite agree with Dr. Symonds as to the propriety of considering dyspepsia apart from gastritis, admitting at the same time that chronic gastritis constitutes one of the forms of what is called dyspepsia. The following remarks appear to us to be highly judicious:

* The earliest account of it with which we are acquainted is that by Vincent Ketelær, entitled "Commentarius medicus de Aphthis nostratibus, seu Belgarum Sprouw. Auctore Vincentio Ketelær, Med. Doct." 12mo. Ludg. Batav. 1672. Dr. Craigie, in his Practice of Physic, places this work at the head of the bibliographical list relating to *thrush*; but he probably had not seen the work itself, as the exact title is not given, and his own remarks on epidemic thrush refer to it solely as a disease of infancy; whereas Ketelær describes this disease as common to all ages, and both sexes.

"Some authors consider dyspepsia as alway dependent upon an inflammatory or congested state of the mucous membrane of the stomach. This, however, we consider to be a narrow view of the subject, and long observation has led us to believe in the existence of a purely functional disorder of the stomach, that is uncomplicated with any structural alteration or with appreciable permanent disease of the capillary circulation. Of almost every other organ the same remark obtains, certainly of the brain, the lungs, the liver, and the kidneys. But while we maintain that the collective symptoms resulting from chronic gastritis constitute only one form of dyspepsia, we concede the difficulty, in a great many instances, of pronouncing a similar set of symptoms to be independent on such a state of the mucous membrane, and also that great errors in practice are committed every day by overlooking this frequent cause of a disorder, which by many is treated as if it were always functional. Let us endeavour to point out one or two features, more especially characteristic of the cases in which an inflammatory condition prevails. Pain spontaneous, or occurring after food, may depend on mere increase of sensibility. It is often concluded that the pain is not inflammatory if relieved by stimulants and carminatives; but this is not decisive for reasons which will appear when we discuss the treatment, though in the majority of cases of gastritis, the pain would be aggravated by such means. We have found a better test in the effect produced by hot liquids, such as tea or plain water, which seldom fail to aggravate or induce pain in these cases. The existence of tenderness at the epigastrium will confirm this evidence, but cannot be alone relied upon. The state of the tongue used to be thought one of the strongest diagnostic signs, but is liable to great fallacies. Andral, (Clin. Med., t. iv.) and Louis (Gastro-ent. t. ii. p. 64,) have proved that gastritis may coexist with a moist clean tongue of a natural colour; and on the other hand that this organ may be red, papillated, or even aphthous, with a healthy state of the stomach. Still in a large proportion of cases, such alterations of the natural appearance of the tongue as we have enumerated among the symptoms are observable, and should at all events lead us to suspect the disease. The state of the skin is an important help to us; thus squamous and papular disease, coexisting with stomach disorder, intimates very strongly that the mucous membrane is inflamed. The relief afforded by antiphlogistic means affords some useful hints; but it must be valued only in connexion with other signs. We may remark, however, that relief ensuing upon iced drinks is more decisive than when produced by local depletion, for the latter will sometimes mitigate a purely nervous gastralgia. The nature of the matters vomited is a valuable indication. For example, it is improbable that a large quantity of mucus should be secreted, unless the membrane had been previously in a state of plethora." (pp. 59-60.)

The subjects of Enteritis and Cholera are well handled; but as we have nothing particular to say relating to them we pass them by.

Dr. Symonds mentions two cases of Non-malignant Stricture of the Colon presenting varieties not hitherto described by authors. In one of them the stricture was situated in the arch of the colon, and appeared to have been caused by a folding inwards of nearly half an inch of the intestines, with hypertrophy of the mucous and sub-mucous tissue of the part, originally caused, as the author thinks probable, by a small invagination which became permanent by adhesive inflammation. In the other case the stricture, which barely admitted the point of the little finger, was seated just above the termination of the colon in the rectum, and was caused by hypertrophy of the adipose tissue under the serous coat. (p. 123.) We have not space to give the descriptions in full.

The article on Hemorrhois is a good and practical one. With respect to simple hemorrhage from the rectum the writer remarks:

"A frequent cause, though often overlooked, is a small vascular point on the surface of the mucous membrane, from which a minute artery throws a jet of blood every time the bowel is evacuated. This may be unfelt, and the hemorrhage may occur daily, unknown to the patient or his medical adviser, till general anæmia awakens a suspicion of the true nature of the disease." (p. 126.)

We are surprised that Dr. Symonds having made this remark should not have noticed that the hemorrhage which accompanies internal piles is usually not venous, as might be expected, but arterial. This fact has been stated by Sir B. Brodie, and we have had frequent occasion to verify it. We agree with Dr. Symonds that hemorrhage from rupture of the vein, forming a hemorrhoidal tumour, is of unfrequent occurrence. Indeed, according to our observation, the hemorrhage, when venous, is for the most part trifling, and consists in a mere exudation; when it is at all profuse it is arterial, and proceeds from within the rectum.

We are not aware of any point which calls for particular notice in the other essays by Dr. Symonds, of which we have enumerated the titles, unless, indeed, it be one relative to the treatment of peritonitis. The writer states that he has tried the experiment of placing the patient in a warm bath sufficiently long and shallow for him to lie extended with the tumid abdomen rising above the level of the water, and pouring cold water upon the abdomen. He tells us that the relief of pain has been striking even where the disease was too far advanced to admit of cure. (p. 145.) We know not what benefit may be derived from the application of cold to the abdomen while the blood is drawn to the surface of the rest of the body by the action of heat and moisture; but under ordinary circumstances we are no advocates for the external application of cold in abdominal inflammation, the few trials we have made of it having been so discouraging as to prevent their repetition.

The article on Diseases of the Mesenteric Glands is admirable. Dr. William Thomson has here borrowed largely (with due acknowledgment) from the valuable article *Carreau*, by M. Guersent, in the Dictionnaire de Médecine, and has added what was profitable from the writings of British pathologists. He discriminates properly between simple inflammatory enlargement and tubercular disease of the mesenteric glands; very distinct cases which some eminent practitioners have lately confounded in a manner to us unaccountable.

The same author treats of the Diseases of the Liver and Biliary Organs; and, as the article has been republished in an enlarged form, we have noticed it elsewhere.

The Diseases of the Pancreas and Spleen succeed, and are descanted on by the same writer as lucidly as the unfrequency of disease in the former organ and our ignorance of the functions of the latter would admit.

The Diseases of the Urinary Organs are illustrated by Dr. Christison, and his name affords sufficient assurance that the articles

relating to them must be written with ability. The opinions of the writer here expressed on that part of the subject which has lately occupied the most prominent place in the discussions of pathologists, granular disease of the kidney, are identical with those contained in his recent work, which we reviewed in a late number; and as the whole class of urinary diseases have frequently occupied our attention in this Journal, we will omit them altogether for the present.

The Diseases of the Uterine System are treated of under the heads of disordered menstruation; hysteralgia; leucorrhœa; inflammation of the uterus; polypus of the uterus; cauliflower excrescence; carcinoma; corroding ulcer of the uterus; other morbid degenerations of its structure; inflammation of the ovary; dropsy of the ovary; structural diseases of the ovary.

The first three of these are commented on by Dr. Ferguson, and the rest by Dr. Simpson. The papers of both these gentlemen are written with good sense and perspicuity, and in the off-hand manner of men who thoroughly understand their subject. With this general commendation we must pass them by, in order that we may take some notice of the next volume.

Vol. V. This volume contains dissertations on hemorrhage, with the allied subject of scurvy; on dropsy; scrofula; bronchocele; rheumatism; gout; and invermination: it concludes with a treatise on the art of prescribing, accompanied by a formulary. The subject of hemorrhage is introduced by some general doctrines, which merge, rather too abruptly perhaps, in the particular consideration of epistaxis; hemoptysis; hematemesis; hemorrhage from the intestines; hematuria; and hemorrhage from the uterus. These articles are by Dr. George Burrows, and are all good. We think, however, he has committed a mistake in that portion of the paper on uterine hemorrhage which related to the gravid state. "Uterine hemorrhage," he says, "may occur during pregnancy: this accident may happen in the early or in the more advanced stage of utero-gestation. When uterine hemorrhage occurs at the early period of pregnancy it is occasioned by the partial separation of the placenta from the uterus, and the probability of arresting the hemorrhage and preventing abortion will depend upon the extent to which the detachment of the placenta has proceeded." (p. 56.)

The assertion that hemorrhage occurring early in gestation is occasioned (exclusively we are led to infer) by partial separation of the placenta, seems to us not justified either by experience or authority. On the contrary, it is chiefly in the early stages that we meet with hemorrhage between the uterus and the epichorion; occasionally in the substance of the placenta; and, more rarely, between the membranes of the ovum.*

The article on Scurvy, by Dr. Budd, is ably written, and contains nearly all that is known on the subject.

* M. A. C. Baudelocque states that effusion of blood between the uterus and epichorion "is very common in the early periods of gestation, and that the greater part of abortions at these periods are attributable to no other cause." Traité des Hémorrhagies Internes de l'Uterus. (p. 35.) Ed. Bruxelles, 1832.

The succeeding articles, on Dropsy, are by Dr. Watson, and do credit to his judgment. That on the general doctrines of dropsy is particularly valuable, and the acute and febrile forms of the disorder are therein placed in an intimate, and we believe a true relation to disease of the kidneys. The observations of the writer on that variety which has been called acute anasarca, as well as those on dropsy following scarlet fever, are worthy of careful perusal, and we regret that our limits will not allow us to transcribe them. Dr. Watson's view of both cases is based on the supposition that the functions of the skin being primarily deranged, those of the kidney become so in consequence of the close relation which renders the latter complemental of the former; and that albuminous urine and the other conditions of renal dropsy are hence induced. (pp. 131-34.)

The subjects of Chronic Hydrocephalus, Thoracic Dropsy, and Abdominal Dropsy, are well and practically discussed. We are happy to find the writer boldly advocating paracentesis in those cases of chronic hydrocephalus to which it is applicable. This operation seems to us to have met with unmerited proscription from a number of writers. If we examine the cases on record in which it has been tried, we shall find that, in about one half, it is stated to have been successful. Now, making allowance for the fallacy of medical testimony in wonderful cases, which we are fully inclined to do, we will estimate the successful cases at one third, and ask if any other remedy in this disease has been equally fortunate? We might ask also if paracentesis has been as successful in dropsy of either of the other great cavities as in that of the head?

The chapter on Scrofula, by Dr. Shapter, is well written, and gives a copious and accurate view of the subject. It is chiefly a compilation, but this is not derogatory from its merits, since there have been no recent additions to our knowledge which could justify the introduction of new speculations.

On Bronchocele, all that is needful is said by Dr. Rowland. The point of chief interest with respect to this disease is its endemic cause. Dr. Rowland concludes, we believe justly, that it is yet to be discovered. To Mr. M'Clelland's view of the origin of bronchocele, so fully noticed in a former number of this Journal, (vol. VIII. p. 103,) in the use of water passing through limestone rocks, he objects: 1, That it is met with abundantly in districts where there is no limestone, as in the Vallais. 2, That it is absent in many places where limestone abounds. 3, That it prevails in districts where the waters are free from any calcareous impregnation. 4, That a strict adherence to the use of distilled water does not ward off the disease where it is endemic. He nevertheless admits that the use of calcareous waters strongly predisposes to the disease. He thinks also that other causes have the same tendency, though few in an equal degree; and that some influence, hitherto unknown, is necessary to the development of the disease.

The subjects of the remaining papers in this volume are not of a kind to be entered into profitably in a general notice like the present.

Rheumatism and Gout are well illustrated by Dr. W. Budd. With reference to the latter affection he is inclined to a humoral pathology, and congratulates himself in a note at the end of the article on the coincidence, in many points, of his own views with those propounded by Dr. Holland in his Medical Notes and Reflections, respecting the nature of gout as a disease of the blood, and the mode of operation of colchicum. These views, as we have elsewhere stated, we consider as worthy of particular attention, and as affording a very enticing subject of experimental enquiry to the iatrochemist.

The article on Worms, by Dr. Arthur Farre, is an exceedingly well-written and complete essay; we wish, however, that the author had not perpetrated, nor the editor admitted, such a word as verminology, (p. 257.) Why displace the ordinary term *helminthology* for so nasty-sounding a hybrid? The reader will not expect that we should here enter into the natural history of the entozoa, or the symptoms and treatment of the diseases occasioned by their presence.

The concluding essay, on the Art of Prescribing, by Dr. Joy, contains a good view of the operation of the different classes of medicines and of the rules to be observed in their administration: the formulæ also are well selected.

In taking leave of that portion of the "Library" which relates to the practice of physic, we have much pleasure in saying that, although we have found some fault, and might have found more if we had been ill-naturedly disposed, we regard this work as one of great value to the practitioner; and there is one respect in which it is especially so;—namely, in the full and precise directions which are given as to the manner of administering the various remedies recommended; a point on which the greater part of works on the practice of medicine are, in our opinion, extremely deficient. We must, at the same time, give it as our opinion, in justice to the authors, the editors, and the publishers of the Cyclopædia of Practical Medicine, that the present work is, in several respects, inferior to it, and certainly was not required by the profession so long as its predecessor could be procured in the shops; and we think that a more acceptable service would have been rendered to medical readers by the republication of the Cyclopædia, with such modifications and improvements as the lapse of time since its appearance and more experience on the part of its editors might suggest.

Our pained and wearied eyes at the conclusion of our arduous task of examining these volumes too well justify our former reprobation of their mechanical qualities. We repeat, that the long lines, the small type, and the paper neither smooth nor white, seem as if chosen on purpose to deter every reader, except the youthful student, from their perusal; and indicate at once a niggardly spirit and want of taste, which we shall leave to the editor and the publisher to share and appropriate as they best can.

## Art. VII.

*A Letter to Sir Benjamin Brodie, F.R.S., &c., containing a critical enquiry into his 'Lectures illustrative of certain Local Nervous Affections.'* By William Goodlad, m.r.c.s. &c.—*London*, 1840. 8vo, pp. 154.

It must be confessed that, notwithstanding the advance which has been made on the subject, we are still very much in the dark respecting the pathology of what are, unsatisfactorily enough, termed nervous affections. The brain, the spine, the nervous system generally, the uterus, the stomach, and chylopoietic system, are by different individuals regarded as the centre of the symptoms termed nervous, whilst others imagine them to reside in a certain hereditary or acquired constitutional disposition, &c. And the treatment is vague and various in correspondence with these varieties of opinion. Sir Benjamin Brodie performed a valuable service in insisting on the importance of studying "each individual case pathologically, endeavouring to trace the symptoms to their true origin." Those who have endeavoured to systematize these diseases in accordance with any recognized nosological arrangement must have felt the propriety of this direction, however laborious and, in many instances, unsatisfactory its application. For it is a truth of which any reflecting person must be readily convinced, that a careless and immature classification, requiring as knowledge advances to be new modified and changed, is productive of far more trouble than the consideration of individual cases, either quite independently one of the other, or only in such connexion as, without any forced relationship, they will bear. The lectures of Sir Benjamin Brodie, which are reviewed in the volume before us, were reviewed by ourselves in our Seventh Number, and are doubtless well known to the profession generally. The lectures contain a theory of local nervous affections, of what its author terms local hysterical affections, an attempt to explain these pathologically, and directions for general management and surgical or medical treatment, such as have been derived from the author's very extensive experience.

Before proceeding to a consideration of the matter of Mr. Goodlad's volume, we are compelled to state that, although he expresses regret if he should "even in the least degree have deviated from that course and courtesy which a love of truth and his profession dictates," the volume before us is sadly characterized by his misrepresentations of the observations of Sir Benjamin Brodie. We shall not hesitate hereafter to speak in terms of commendation of anything which appears to us to merit praise, or to condemn views against the correctness of which Mr. Goodlad has adduced sufficient evidence; but the same sense of justice compels us, in the most unqualified manner, to condemn the garbled representations which are made of the views of Sir B. Brodie. We are far from wishing to accuse Mr. Goodlad of any intention to make the lectures of Sir B. Brodie appear absolutely absurd; but a severe censure is merited by any one whose incompetence to weigh the value of words, or whose bias against the opinion of another leads him to commit the errors which especially belong to the first few pages of Mr. Goodlad's volume. In illustration of the above remarks we refer, among many others, to p. 6;

and we hesitate not to say that we know no page in any other volume so completely full of truth in the letter and of untruth in the sense as that referred to.

There is too great a disposition on the part probably of everybody to escape from a difficulty in pathology under the cover of a name. Such may be said of the general use of the term hysteria. But in every attempt to classify with greater correctness the diseases commonly termed hysterical, there is great danger that little more may be effected than to change one name which is inappropriate for another almost if not quite as objectionable. Speaking of Sir B. Brodie's employment of the term hysterical, Mr. Goodlad says, "If, as I hope to show, in hands like yours this doctrine leads to false conclusions and dangerous because inert practice, if under this name disorganization creeps on whilst the practitioner is waiting, and if your lectures establish this fact, it requires little more to point out the necessity of confining this term to its legitimate meaning," &c. It is needless for us to make any detailed reference to the lecture referred to. Mr. Goodlad thinks that there is great objection to regarding many of the cases therein mentioned as hysterical, and we in some degree coincide with him in the opinion. A case related by Sir B. Brodie (p. 67 of the Lectures,) as hysterical, in which there had been retention of urine, and a diseased condition of the bladder was found after death, is thus commented on by Mr. Goodlad:

"If this case had been set up as a beacon to warn your pupils against the danger of the doctrine of hysteria, it could scarcely have been more instructive; and it is precisely because it furnishes an alarming proof how, even when united with the greatest intelligence, this theory leads to fatal consequences, that it becomes an imperative duty to examine it. And hence it is that when you say 'there was no sign of mortification but the black colour,' and infer that the colour was not dependent on that cause because there was 'no fætor,' it seems right to remind you that in strangulated hernia there is often 'no fætor,' though the intestine is found 'almost black;' and that where it is so, and especially where 'the coats are easily separated from each other,' no surgeon has ever questioned either that mortification already existed, or that the part was on the verge of mortification." (p. 17.)

But whilst we think with Mr. Goodlad that this case exhibits something more than hysteria, we cannot draw from it all the inferences which he would desire to deduce. We should consider it as either an exception to a general rule, or as a case about which a wrong opinion had been expressed. But as regards the general doctrine and practice recommended in these cases, we must remember that Sir B. Brodie says, "Females who labour under hysterical retention of urine, if left to themselves, *usually recover in a short space of time, sometimes almost suddenly; but if the catheter be employed their recovery may be protracted for an indefinite period.* We may lay it down as a general rule, that in these cases the catheter should not be had recourse to; and the only exceptions to it are in those extreme cases in which actual paralysis has taken place, and the bladder is likely to become diseased, if not artificially relieved." We have marked in italics that part of the above quotation which especially applies to our present subject. Sir B. Brodie observes, when speaking (p. 36, Lectures,) of local pains apart from local disease, "that where there is that state of the general system, whatever it may be, which produces the phenomena of hysteria, it is not uncommon for a particular

joint to be affected with pain and morbid sensibility, such as may lead a superficial observer to believe that it is the seat of some serious local disease, although no such disease in reality exists." Our readers are doubtless acquainted with Sir B. Brodie's endeavour to explain the phenomena of hysteria. Mr. Goodlad observes on the above quotation that "it may be assumed as an axiom that will bear universal application, that wherever there is continued pain, there is increased vascular action somewhere, the discovery of the seat of which becomes the duty of the practitioner." (p. 29.) And he further remarks that in none of the cases mentioned by Sir B. Brodie, was any enquiry made with a view to ascertain whether there existed any cause of irritation at the *origin of the nerves* which were the seat of suffering, expressing at the same time his confident belief that "if the enquiry had been made, an adequate cause would have been discovered." It will thus be evident that the author's object is to explain the phenomena of these cases which are attributed to hysteria on the theory of some morbid condition of the circulation in the spinal cord. And although there may be no great novelty in this view of the subject, we do not think that time will be lost in referring to it for a short time.

Our readers will excuse our passing over much of the volume before us which is merely devoted to an examination of Sir B. Brodie's views, especially as much which is merely theoretical is the subject of criticism. Indeed, our own opinion respecting what are termed nervous diseases is one which is rather eclectic than attached to any particular system; and we find in the endeavours of one pathologist after another to localize these diseases in particular organs or systems sufficient ground to believe that they are all more or less wrong, although each of them may possess a portion of the truth. We have no doubt that Sir B. Brodie's volume contains not a little of this. We know likewise that there is some of it in the volume before us; and if we can find in it anything which is new to many of our readers, or which may refresh the memories of others with views which they have long in various degrees entertained, our purpose will be sufficiently answered.

Mr. Goodlad considers that the sympathies manifested in some of Sir B. Brodie's cases (such, for instance, as the production of pain in the ankle by acidity in the stomach,) are best explained by supposing the cause to exist at the origin of the nerves of the affected parts in the spinal canal. He believes that the ganglionic system is of little use in regulating sympathetic disease. "I shall proceed therefore," he says, "to prove that the different portions of the spinal cord have the power of *creating* disordered action like the brain, and that this may arise in any part of it independent of any other; the effects of which are observed only at the extremities of those nerves connected with it." The first case, which is one of great interest, we must notice at some length. It is that of a female, aged fifty-two, who was violently convulsed in the lower limbs, February 1819. Every treatment which was thought likely to be useful was employed without effect, until, whilst examining the region of the kidneys, "on pressing upon the neighbouring spinous process of the vertebræ, the patient cried out 'there,' and immediately a strong convulsive action commenced, and pursued its usual course." The pressure was repeated, and each time with a repetition of the paroxysm. Leeches were applied,

and the convulsive action ceased. In the following October the convulsions returned, but with much more violence, and were again in the same way relieved. In June 1823, the same happened, the convulsions, however, being much more general and violent, and the spinal affection in a higher part of the column. "The slightest pressure brought on a fresh attack; twenty leeches were again applied, each leech producing a similar effect as it pierced the skin, but effectually relieving her by the time they were filled, after which she obtained several hours' uninterrupted sleep, and had no return. No medicine of any kind was administered." The following year the attacks returned, and were relieved in the same manner. She continued well until 1839, when she suffered a similar attack. In this she died. In making an examination after death—

"The processes of the vertebræ of the back, and the whole of the spinous processes of the vertebræ of the neck were divided.....The cord exhibited in detached portions of its surface very considerable vascularity.....But although some portions of this, the posterior aspect of the dura mater, were very vascular, this was by no means universal, some portions of that membrane presenting a natural appearance. The medulla was then cut across, about the eighth dorsal vertebra, and was raised very carefully from its situation... ... The anterior surface of the dura mater, covering the spinal column, presented one entire vascular mass, and the connexion between it and the membrane lining the canal was more intimate than natural. This great vascularity was most striking and most extensive on the external surface of that membrane, where flocculent masses of a bright scarlet were seen floating from it. In some parts, this outer surface being removed, the dura mater, properly so called, was observed of a paler colour beneath it, whilst at those portions of the surface where the nerves are detached, or at the point of union between the nerve and the spinal cord, to speak more correctly, the vascularity was greatest, and congeries of vessels were seen extending from nerve to nerve on each side..... The dura mater covering the medulla oblongata was ecchymosed, like the conjunctiva after a blow, and was scarcely less vascular.....Returning to that portion of the canal from which the spinal cord had been detached, there were observed distinct patches as if of extravasated blood.....This vascularity was between the theca and bodies of the vertebræ or of the intervertebral substance." (p. 63, et seq.)

Mr. Goodlad remarks,

"It is only when the disease of the spinal column extends by continuous sympathy to the ligaments that pain on motion is experienced, and only when it extends to the origin of the sentient nerves that there is an increase in perception amounting to pain in other parts. Whether the disordered action is confined to the muscles of one part or extends to many depends on the extent of the original cause; but innumerable cases may be related where distant and distinct parts of the column labour under complaint, the intermediate portions being healthy, and where impressions made at the extremity of the nerves connected with one of these portions will be transmitted to and produce disordered feeling at the extremities of the others." (p. 81.)

Different states of disordered feeling are explained by supposing disordered vascular states at the orifice of the nerves. After referring to the opinion of Sir H. Halford, "that tic douloureux of the face arises from irritation of the nerves, occasioned by a portion of dead or carious bone," Mr. Goodlad continues:

"It requires only the admission of this fact and an acknowledgment of what no one will deny, that a similar cause operating near the source of the spinal nerves will produce the same or a like effect, and all the phenomena of what Sir

B. Brodie calls hysteria are accounted for; and we are justified in believing that the disorders in the cases quoted by Sir B. Brodie have this origin, until it is proved by investigation that such a cause does not exist, and unless it can be shown also that there are some symptoms which such a condition will not explain." (p. 83.)

We give the above as Mr. Goodlad's views, but not as reasoning which is to us quite satisfactory. In deciding any such question it is not necessary merely to say that the supposed cause would be sufficient to produce and to afford an explanation of all the symptoms. We must have the evidence of the actual existence of such a state of the spinal cord, and the proof of the connexion between this and the symptoms characterized as hysterical, before we can absolutely determine the question. In the meanwhile, however, our own opinion and that of very many others tend to the belief that such is actually the case. But whatever objections may be made to the views of Sir B. Brodie on the theoretical question, we feel compelled to defer to his judgment when decidedly offering an opinion on a point of practice. And on this point we should refer again and again to Sir B. Brodie's observations on certain heroic modes of treatment which are too often adopted in accordance with the views of spinal irritation or inflammation. Blisters, leeches, various forms of counter-irritation, and remedies calculated to exhaust power are often employed in cases of this character where the proper mode of remedying the morbid state of the spine, even supposing it to have been one of increased vascularity, would have been such general means as are best calculated by means of diet, exercise, &c. to benefit the general health. The following case, which occurred in our own practice, is one out of many which we, in common with every experienced practitioner, could adduce in favour of Sir Benjamin Brodie's practical views.

A lady, very excitable both morally and physically, had been for some time under our care, affected with what is usually termed *spinal irritation*. She benefited under the usual means, and was in very good health, when, in consequence of long-continued fatigue and great anxiety, she was attacked with faintness. To this there succeeded some reaction, great pains in the head and limbs, and some spasmodic motions in them; and on examining the dorsal vertebræ three of them were found acutely painful on the slightest pressure. Here we thought was a case for leeching the spine, which we accordingly proceeded immediately to do. No local benefit was derived from this treatment, but in about two hours afterwards the spasms became of a tetanic character, accompanied with symptoms of exhaustion which we were disposed to attribute to the loss of blood in a state of debility. The progress of this case was marked by the frequent recurrence of tetanic symptoms, which nothing but large opiates would allay; but the amendment which took place in it commenced with the time that medicines and applications to the spine were disregarded, and attention was given, by diet, frictions, gentle exercise, &c., to the state of the general health.

We do not mean to infer from this that Mr. Goodlad's theory is necessarily wrong, but that Sir B. Brodie's remarks on treatment are worthy of much attention when he discourages the free use of means which in many hands might prove very injurious. It is quite consistent with analogy to suppose that a morbid vascular condition of the spine may exist

in many hysteric cases, but which would require any treatment rather than local depletory means for its relief. It appears to us that we may not unfairly infer much respecting the vascular state of the spine, and the most appropriate means of relieving it, from that which we frequently witness in the various tunics composing the eye. No one in these days would look upon leeches, blisters, and remedies of this class, as the best means, under many circumstances, of restoring the distended vessels of these tunics to a healthy state of contractibility. And we must for our parts agree with Sir B. Brodie in the great advantage derived in many cases of the kind alluded to from the use of palliatives, e. g., belladonna plasters, &c. That they should be useful consists with the fairest reasoning and the best experience. It is quite legitimate to suppose that the existence of pain impairs the proper exercise of a function. Take away this pain and aid the system as much as possible in the exercise of its self-restoring power, and the utility of palliatives becomes sufficiently manifest. There is much ingenuity in Mr. Goodlad's reasoning, and we derive from it additional reasons for paying great attention to the condition of the spinal column in all cases of hysteria. But this is now a part of practice which nobody thinks of neglecting. Mr. Goodlad has some good observations on the reasons why appearances after death are not more frequently found in the spinal column, and on the peculiarities of its circulation which favour congestion of its vessels. We quote a part of these :

"A great portion of the medulla spinalis receives its blood from more numerous sources than the brain, each portion or bundle of nervous matter having its distinct nutrient artery sent off from the aorta, and each portion having also its returning veins. Disorders within the spinal canal are not only limited, like those of the brain, by this mode of circulation, but are still further confined, for the ligamentum denticulatum acts like a tentorium, and forms a line of separation by which each portion remains distinct from contiguous ones, either above or below it; and pressure, whether it arises from effusion or congestion, influences one part of the medulla and the nerves arising from it, although not extensive enough to influence other portions of that substance, or the nerves arising from other parts. In like manner one fasciculus of nervous matter suffers from inflammatory action without the others or any of them participating. In other cases inflammatory action, in which one nerve or one set of nerves participates, shall have gone through its stages, producing effusion and consequent pressure and paralysis of those nerves, whilst the circulation in another contiguous circle shall possess greater activity than is natural; and we have paralysis in one part and pain in another; loss of sensation and increased action of muscle in one individual, or loss of muscular power and increased sensibility in another; asthma and epilepsy in one case, asthma and hemiplegia in others." (p. 102.)

The great influence of certain conditions of the spine upon the heart is very ingeniously argued by Mr. Goodlad. Those who have paid any attention to the symptoms commonly ascribed to what is termed spinal irritation, know well how constantly the heart is affected. Mr. Goodlad says that he is now satisfied "that in passive dilatation of the cavity, the heart is to all intents a palsied muscle, and paralysis there, as in all other cases, takes place as a consequence of effusion into the spinal canal." We do not consider that this connexion is at all proved, though at the same time it does not appear unreasonable to attribute some cases of passive dilatation to some cause diminishing the nervous influence which is

transmitted from the spine to the heart. The author's reasonings on this subject lead him to remark, on the importance of paying great attention to the state of the spine in apparent diseases of the heart, and not in these only, but in those of all other viscera; and as our views entirely coincide with his on this subject, we shall be excused in making another extract:

"The knowledge of the existence and the progress of disease in different portions of the spinal medulla, and in which the nerves connected with those parts participate, simplifies the symptoms observed in a great variety of disorders, and gives a precision to practice which can only be obtained, I believe, by this knowledge. Thus, disorders of the uterine organs, and the chain of circumstances, numerous and extensive as they are, which connect these organs with other parts, can only be understood by the admission of corresponding local causes at the origin of nerves connected with them only through the spinal cord. . . The nerves of function in one organ, as the stomach, will labour under the same disordered action as those of the heart, and symptoms of indigestion will exist in such cases long before disorders of the heart are manifested, the latter being called into activity by the extension of the complaint in the spine from a severe cold; by inflammation in the lining membrane of the air-tubes in the lungs, or by cold applied to the surface of the body, and vice versâ. Where such disorder does exist in different portions of the cord and in the nerves arising from it, very slight causes applied to the extremities of one set of nerves, will immediately operate on the organs connected with the other; and hence, on the approach of menstruation, we have not only pain experienced in those organs connected immediately with that function, but inordinate vomiting in one case, violent action of the heart, and violent action of the muscles connected with respiration in another. . . . In most cases of puerperal convulsions, so far as I have witnessed them, previous disorder may be traced, and its existence denoted, by tenderness at the origin of some of those nerves connected with the womb. The act of parturition, and the increased vascular action which is connected with it, are well calculated to give activity to a state of disorder already existing there, although not sufficient to overpower the ordinary functions of any part, without the additional assistance of such a cause." (p. 122.)

The above contains much matter for profitable reflection, and we have found that a mode of reasoning like this has generally appeared to afford us explanations of symptoms which are otherwise not easy to explain. Agreeing with Mr. Goodlad in much which he has advanced, and being ourselves disposed to think that the spinal cord has a most important share in the production of various symptoms termed hysterical, we do not however think that his views of treatment are quite correct. To subdue the inflammatory action which is supposed to exist in the spine, he thinks that caustic issues and rest will often be required, as well as other depletory local treatment; and cases are subjoined illustrative of the propriety of the practice. On this point we shall make a few observations, and with these conclude our remarks on the volume before us, which we do not hesitate to recommend to our readers as well worthy of their attentive perusal.

In a practical point of view, it is very necessary to employ the term *inflammation* in a precise sense. If we observe that which is commonly called inflammation, as it exists in the eye or skin in various states, and in both of which structures it falls more immediately under our observation, we learn that for this condition the most various and opposite treatment is required. At one extreme and in one constitution, local and general depletory measures are called for, and at the other a treatment of directly opposite character; and between these extremes are other conditions

requiring special management. The state of simple hyperæmia of a part is sometimes spoken of as inflammatory, although quite different from it, and having no tendency to the production of effusions such as characterize inflammation. Now, with regard to the cases which have been treated of by Sir B. Brodie and Mr. Goodlad, there is no evidence that inflammation, taken in the strict sense above referred to, is their uniform or common accompaniment. It is true that a case or two may be quoted in which the effects of inflammation have been witnessed after death, and apparently affording a rational explanation of the symptoms which existed during life; and it is further true that occasionally the gradual progress of these cases is such as to allow us to suppose that some disorganization may have taken place at the origin of nerves, or that their functions may be interfered with by deposits resulting from inflammation. But this is not the ordinary course of such cases; the history of them is such as does not justify the supposition of an actual inflammation existing in the spine. The symptoms are frequently intermittent, exist a long while, and are suddenly recovered from, and the completeness of recovery is not such as could be supposed to follow a long inflammatory action going on in the spine. And the treatment which has been found efficacious in the hands of many also militates against the idea of inflammation being a cause. We should be much more inclined to believe that a congested state of the spine existed than an inflammatory state, and such an opinion affords a more credible explanation of symptoms than the supposition of inflammation; and it likewise points out the mode of treatment which we should recommend, and explains its utility. We think that the phenomena of blushing may be usefully considered in relation to the present matter. This congestion is a result of moral and physical weakness, and it is a tolerably constant characteristic of the hysteric class of females. To cure it, we should not leech and blister or enjoin rest, but we should expose abundantly to fresh air, insist on regular and plentiful exercise, with sufficient intervals of perfect rest, feed with a most generous and nutritious diet, and, with but little apprehension in the majority of cases, of failing in effecting a cure. We think that if there be anything wrong in the circulation of the spine in the cases alluded to, it is generally a congested state arising from debility or exhaustion, to be explained by some hereditary peculiarity, some bad habit or mode of education, some series of practices inimical to health, some moral or physical cause which, in by far the majority of cases, produces a condition, the proper remedy for which is the system which a boxer would adopt were he training for the exercise of his calling, modified according to the peculiar circumstances of the case. Daily experience is teaching us the omnipotence of such means in the treatment of various forms of disease; and without meaning to assert that the cases do not occur which require the treatment recommended by Mr. Goodlad, we are well assured that the class of complaints to which Sir B. Brodie has applied the term hysterical are not to be so dealt with.

## Art. VIII.

*Researches, Physiological and Anatomical.* By John Davy, M.D. F.R.S. In Two Volumes; with Plates.—*London*, 1839. 8vo, pp. 455-80.

The style in which these elegant volumes are presented to the public is perhaps alone sufficient to ensure for them a favorable reception, independent of the reputation of their author, and the fact that they contain the whole of his scientific memoirs. But the former of these recommendations is trifling in comparison with the real value of their contents, which include some of the best physiological papers in our language. In collecting these together, from the transactions of the different societies and scientific journals through which they have remained scattered for a series of years, and in now presenting them in an amended form, we feel that Dr. Davy has rendered good service to the public; since, however valuable may be the labours of even the most distinguished observer, they often remain almost inaccessible to the generality of enquirers, when published only in the expensive and ponderous volumes of scientific bodies, which are rarely to be found except in the libraries of the most affluent and of public institutions, and not always in them, as we have frequently had cause to experience.

The *experiments and observations on the Torpedo*, which form the subject of the first paper, and which were communicated to the Royal Society in 1832 and 1834, and subsequently published in the Transactions, are equally interesting to the physiologist and comparative anatomist. Up to the period of these observations "it had not been ascertained," as Dr. Davy remarks, "that the electricity of the torpedo, considering its peculiar influence to be electrical, had either the power of acting electro-chemically, in separating the elements of any compound bodies, or magnetically, either in affecting the needle in the multiplier, or in imparting magnetism to iron; or lastly, of generating or producing heat: points to which my experiments were particularly directed, and with positive and successful results." (p. 3.) The ascertainment of these facts was an important advancement in our knowledge of the phenomena of animal electricity; and, as it appears from the remarks of Dr. Davy, had been looked forward to by his brother Sir Humphry as likely to prove conclusive as to the nature of that astonishing power, whether, as he imagined, it be of a peculiar kind, or, as formerly believed by Mr. Cavendish and recently taught by Dr. Faraday, it is merely another form of common electricity. Upon this question Dr. Davy does not give a decided opinion, but points out distinctly the circumstances in which the electricity of the torpedo resembles the known kinds of electricity, and also those in which it differs from them. Thus it resembles common and voltaic electricity in magnetising and polarizing iron, and in producing chemical changes; but it appears to differ from common electricity in affecting the multiplier and producing these changes more distinctly, while its power of passing through air, and also of producing heat, is considerably less. On the other hand, as compared with voltaic electricity, it acts more feebly on the multiplier, and decomposes water and metallic solutions less readily, but the shock produced by it and its power of magnetising iron are greater. In addition to these, Dr. Davy remarks, "there are other points of difference, . . . in which the metallic

communication was interrupted by a strong solution of salt. In this instance the full power of the fish appeared to pass; water was decomposed, a shock was received, needles were magnetised, and the multiplier was affected. When the same experiment was made on the electricity excited by the small voltaic combination of a single plate of copper and zinc, each less than an inch in length and half an inch in breadth, immersed in an acid, neither water was decomposed nor was the multiplier affected. When it was made on the electricity of the electrical machine by means of a Leyden jar, all the effects were witnessed excepting the motion of the multiplier, and the order of succession of poles in the needles magnetised in the spirals." (p. 45.) Dr. Davy very naturally enquires how these facts are to be explained, but admits that the majority of them are in support of the opinion that animal electricity is identical with the other known forms of this peculiar and astonishing agent.

These experiments naturally lead to a closer examination of the electrical organs themselves, both in regard to the structure of the parts which appear to be most concerned in producing the effects, and also in regard to the differences which these parts exhibit in their formation in the different kinds of electrical fishes; and further also, in what respect these parts differ from the corresponding ones, if any exist, in species closely allied to the electrical fishes, but which have never yet been known to evince any electrical phenomena.

The first portion of this enquiry, the structure of the electrical organs, like almost every other subject connected with animal life, was long ago undertaken by Hunter, and in repeating his dissections, Dr. Davy found that his own observations very closely agreed with those of our great anatomist. Hunter described the electrical organs however as abundantly supplied with blood, but Dr. Davy did not find this to be the case in the recent fish, since the blood-vessels themselves are small, and the quantity of blood sent to the organs is inconsiderable. In accordance with this observation he found also that the specific gravity of the electrical organs, as compared with other parts of the body, is remarkably low. Thus the organs of a full-grown fish had a specific gravity of only 1·026, as compared with water at 1000, while the specific gravity of the abdominal muscles of the same fish was 1·058, and of the dorsal of 1·065. He remarks, however, that the integuments and mucous glands which surround the organs are exceedingly vascular, a circumstance which probably led Hunter to regard the organs as well supplied with blood. From a consideration of all the facts, Dr. Davy thinks it "very difficult to resist the conclusion that the electrical organs of the torpedo are not muscular, but columns formed of tendinous or nervous fibres, distended by thin gelatinous fluid. Their situation too, surrounded by and exposed to the pressure of powerful muscles, shows that if condensation is required for the exercise of the electrical function, they may experience it without possessing any muscular fibres of their own substance. The arrangement of the muscles of the back and of the fins, and of the very powerful cross muscles situated between the under surfaces of the electrical organs, are admirably adapted to compress them." (pp. 33-4.) For this purpose Dr. Davy thinks they are designed.

The nerves supplied to these organs are acknowledged by all anatomists to be exceedingly large. They consist of three distinct pairs, de-

rived in the torpedo entirely from the cerebral structures. This fact deserves especial notice, since this is not found to be the case in the other electrical fishes, in each of which, as we shall find, the nerves are derived from different sources. Hunter had accurately described the origins and trunks of the electric nerves in the torpedo, and Dr. Davy has followed up the investigation of their minute distribution with great care. The description which he gives very closely agrees with that given by Mr. Swan, of corresponding nerves in the common skate, (*raia batis*,) a fish nearly allied, in general structure and habits, to the torpedo. But Dr. Davy has not so accurately identified the individual nerves in the torpedo as has been done by Mr. Swan in the skate. He seems to regard what we believe to be in reality divisions of the fifth and vagus, as *peculiar* electrical nerves; and remarks,—"It is an interesting fact that the gastric nerves are derived from them." (p. 37.) A reference to Mr. Swan's excellent delineation of the nerves of the skate* will materially assist us in identifying those of the electric organs in the torpedo. On comparing the description given of these by Dr. Davy with the description and delineation of those of the skate by Mr. Swan, we entirely agree with Desmoulins and Majendie,† that of the three great trunks which go to the electric organs, the two anterior ones are branches of the third division of the fifth, and the third, or posterior one, of the vagus.

In regard to the connexion of the electrical apparatus with the processes of nutrition, it is remarked by Dr. Davy that,—"In the instance of a fish which I had in my possession alive for many days, and which was frequently excited to give shocks, digestion appeared to have been completely arrested; when it died, a small fish was found in its stomach, much in the same state as when it was swallowed; no portion of it had been dissolved." But although in this instance the function of digestion appears to have been suspended, we cannot agree with him that only "*superfluous* electricity, *when not required for the defence of the animal*," may be directed to this organ to promote digestion. (p. 37.) We should rather ascribe the circumstance above noticed simply to the known principles of animal life, in accordance with which it is found that when the energy of one set of organs is more than usually excited, that of all others is greatly interfered with; as for instance, when the function of the brain is much disturbed in the human subject, digestion, as is well known, is impeded, and often becomes entirely deranged, whether the exhaustion of nervous power be the result of disease, of mental emotions, of violent muscular exertion, or of the stimulus of over-excitement of the generative system. The exhaustion of nervous power in giving shocks was doubtless the cause of the circumstance noticed in the torpedo, the arrested digestion being the result of deficient energy in the whole system, occasioned by over-excitement of one set of organs. But a further observation by Dr. Davy points more directly to the use of the electrical function, as connected with nutrition, a view supported not only by the power which the electrical fishes are now known to possess of decomposing compound bodies, but also by the peculiar habits of the fishes themselves, which, although belonging to different genera, re-

* Illustrations of Comp. Anat. Nervous System. (p. 2, pl. x.)
† Anatomie Compar. des Syst. Nerv.

side either at the bottom of deep and muddy waters, or in dark and shaded places, and but seldom approach the surface. On this Dr. Davy remarks, "It seems probable that as the branchiæ are liberally supplied with twigs of the electrical nerves, there may be some connexion between its respiratory and electrical functions; and I venture to offer the conjecture, that by means of its electricity it may have the power of decomposing water and of supplying itself with air when lying covered with mud or sand, in situations in which it is easy to conceive pure air may be deficient; and in my experiments I have often fancied that I have witnessed something of the kind; after repeated discharges of its electricity, the margin of the pectoral fins has acquired an appearance as if very minute bubbles of air were generated in it and confined." If it be hereafter found that the whole of the electrical fishes possess the same property, the chief use of the organs, and their connexion with the function of nutrition will then be sufficiently indicated; and it will also be evident why the electrical power is so directly under the control of the will of the animal, as it is proved to be by the experiments of Humboldt and others. Humboldt divided a gymnotus in the middle of the body, and found that shocks were given only by the anterior half, which retained its connexion with the brain, although the electrical organs in the anterior as well as the posterior half are supplied with nerves only from the spinal cord. In addition to these circumstances, that of the electrical function being possessed by the torpedo, even in the fœtal state, as found by Dr. Davy, long before it could have any necessity for exercising this function as a means of defence, seems further to indicate that its primary use has reference rather to some organic function connected with the growth of the animal itself, than to its mere defence; and this is still further supported by the fact that the electrical power is more energetic in the young than the adult state.

We must not leave this part of our subject without expressing a hope that opportunities may hereafter be afforded to Dr. Davy of prosecuting similar enquiries on the anatomy and physiology of the other electrical fishes, and thus further assist in elucidating the nature of this most mysterious of all the properties of animal life.

Before passing to the strictly physiological papers in these interesting volumes, we shall briefly examine the most important of those which are chiefly anatomical. The first of these, on the *urinary organs of the amphibia*, is of considerable interest to the comparative anatomist, from the circumstance that the correct anatomy of these parts is yet a matter in dispute. This difference of opinion, which has evidently arisen from insufficient comparative examinations of the urinary organs, not only of the true amphibia, but also the neighbouring classes, the observations of Dr. Davy will probably tend to reconcile. Thus it is disputed whether the true amphibia, the frogs, toads, and salamanders, do really possess a urinary bladder; the vesicle, which in those animals contains a fluid, and is attached to the intestine, being regarded by some as the proper urinary organ, and by others as simply a membranous receptacle or water-bag, in which fluid is collected, as in a reservoir, to supply the aqueous expenditure from the surface of the body of the animal. That it is a true urinary bladder appears to be the opinion of Cuvier, Grant, and many others; while Townson, Dumeril, Altena, and Thomas Bell, believe that

it is simply a membranous receptacle, for the purpose just stated, and that the true urinary bladder is absent. From the observations of Dr. Davy and ourselves on the true amphibia, and of Dr. Davy on the chelonian, saurian, and ophidian reptiles, we are led to the conclusion that the vesicle in question is indeed the true urinary bladder, notwithstanding that the ureters do not terminate in it.

The biauricular structure of the heart in batrachian reptiles, which constitute the subject of the next anatomical paper, was first noticed by Dr. Davy in 1824, but seems to have excited little attention, until the publication of Professor Owen's interesting paper on the same subject in 1834. It is remarkable, however, that even this important fact which, although it escaped the observation of all other anatomists, had previously been observed by our great countryman Hunter, as is proved by a preserved copy of some of his destroyed manuscripts, and had actually been made part of the basis of a classification by him, although the fact itself was even overlooked by Cuvier, and not again noticed by any other anatomist until rediscovered by Dr. Davy. We may refer to this circumstance as a still further proof, if any were wanting, of the necessity for a careful revision and republication of scientific papers during the lifetime of their author. Had this been done with the papers and manuscripts of Hunter, there can scarcely exist a doubt that many valuable facts that are now lost to science would have been preserved, and perhaps have been the foundation of important discoveries, which may yet be many ages before they are brought to light. And many, too, of the most captious enquirers, both in this country and on the continent, who now lay claim to the honour of originality, would have found their labours to a great extent anticipated by those of this giant in anatomical knowledge.

The next paper we shall notice is on *the structure of the ductus communis choledochus, and on the flow of the bile.* In this paper are described some peculiarities of structure which have hitherto escaped the observation of anatomists, even of those who have paid most attention to the structure of the liver and its appendages. We have looked for some notice of this structure in Mr. Kiernan's valuable paper on the liver, but have not been able to discover any indication of his being acquainted with it. Dr. Davy states that after emptying the tube, and washing away the adhering mucus, "the lower part of the ductus communis choledochus and of the pancreatic duct, will be seen provided with a kind of valvular apparatus, composed of delicate angular processes or projections of their inner coat, pointing downwards." (Vol. ii. p. 427.) This structure is sometimes more, and sometimes less developed.

"The processes or valvular projections commonly first make their appearance (tracing the passage downwards from the liver) a little above the place of junction of the pancreatic duct, and are continued to the termination of the common duct in the intestine. In the pancreatic duct they are more limited; they are confined to its mouth, and occasionally to a small portion of the tube adjoining, not more than two lines from its mouth. Their valvular nature is easily shown by the introduction of a probe, directed downwards, towards the intestine, it passes smoothly without obstruction; but in the contrary direction, towards the liver, it is impeded and stopped, arrested by the minute sacculi, which exist at the base of the processes. The use of these processes is implied in their valvular

nature and situation: whilst they do not stop the descent of the secreted fluids, which it is the office of the ducts to discharge; they are well adapted to prevent the pancreatic juice from ascending either into the gall-bladder or the liver, and the bile from flowing or penetrating into the pancreas." (Vol. ii. p. 428.)

This structure had not been noticed even by comparative anatomists, but it has been found by Dr. Davy, on extending his enquiries to the domestic animals, to exist in most of them, in which it closely resembles the corresponding part in the human subject.

The next paper which we feel ourselves called upon to notice is one of considerable pathological interest. It is on the *closure of the chief arteries rising from the arch of the aorta*. It contains the details of a case in which the principal arteries which proceed from the arch of the aorta were gradually closed, without loss of life, and continued so for nearly two years, as was indicated by the entire absence of pulsation in the upper extremities and neck. The death of the patient took place in consequence of a rupture of the aorta near its base, the aorta itself being the seat of an aneurism six inches in height and four in width, which Dr. Davy believes to have been of slow formation. From this case, compared with that of another of aneurism of the same parts, but in which, instead of the dense fibrous coagulum which nearly filled up the cavity of the first, leaving only a passage for the blood on the inferior or concave side of the arch, the inner coat of the artery was thickened, and contained thin plates of bone and depositions of atheromatous matter; Dr. Davy concludes that the great arteries of the upper parts of the body may be tied in operations on the human subject without loss of life. The practicability of this in the rabbit and dog has already been demonstrated by Sir Astley Cooper in the first volume of Guy's Hospital Reports. From a comparison of these cases, Dr. Davy states his belief that the closure of arteries takes place much more frequently than is generally supposed, even in a state of general health.

The next paper, on the *rupture of the heart and aorta*, is of consequence, chiefly from the rarity of well-authenticated cases of this accident; but for its details we must refer to the work itself.

Another of Dr. Davy's anatomical memoirs relates to the *specific gravity of different parts of the body*. The application of specific gravity as a means of detecting disease will probably be of most use with reference to change of structure in the viscera, but although at present it can only be employed as an imperfect means of diagnosis, it may ultimately prove, in the hands of a Laennec or an Andral, highly important, when a sufficient number of careful observations has been made upon which to establish exact general conclusions; since, as Dr. Davy justly remarks, there is probably no organic change in the structure of any part of the body which is not accompanied by a corresponding change in its specific gravity.

The paper *on the age of morbid adhesions and fluid in the pericardium* is of much pathological interest. Dr. Davy remarks that it is a pretty generally received opinion, that the age of these formations may be nearly guessed at by their comparative degrees of strength; but experiments have led him to believe that this opinion is not correct, since "wounds, it is well known, which heal by the *first intention* are often firmly

united in twenty-four hours;" and he remarks that, "in the same space of time, when inflammation has been artificially excited, he has witnessed strong adhesions."

This rapid formation of adhesions the author refers to the coagulated lymph effused from the wound, which quickly becomes "*first viscid*, and afterwards solid." While viscid, and when still transparent, "it has the tenacity of mucus, and admits of being drawn out into fibres and bands, which soon becoming firm and opaque, very well represent the ordinary adhesions of the lungs, and in a very few hours attain their maximum of strength." It is entirely upon this *viscidity* of coagulable lymph, that Dr. Davy believes the formation of these adhesions to depend; a property of lymph which, he remarks, has not before been noticed by authors, who usually explain the occurrence of adhesions without reference to this peculiarity. On the above experiment Dr. Davy founds his opinions, and we think justly, that the age of morbid adhesions cannot always be correctly estimated by their comparative strength.

Respecting the effusion of fluids into the serous cavities, he is again opposed to the commonly received opinion, in believing that it takes place only during the life of the animal. But although the experiments on which this belief is founded are of great value, we do not think they are quite conclusive, inasmuch as they were made only on perfectly healthy animals, and not on those suffering from disease of the parts in which the effusion was expected to take place. Now we are free to admit that effusion may not take place after death in parts which during life are perfectly healthy, but we are disposed to think that, in those which have becomed diseased, and in which only effusion is usually found, when this has commenced during life it may continue for a short period after death. This view is in accordance with the already well-known permeability of animal membranes, and also with a fact noticed by Dr. Davy, that in proportion to the tendency to disorganization in a membrane, such is the facility with which it allows fluids to pass through it.

The *observations on pus* are of a miscellaneous description, but, like the subject last noticed, are of considerable pathological interest. They consist of several distinct enquiries concerning the nature and peculiarities of this fluid. The first of these was to ascertain whether pus contains any admixture of atmospheric air. All the experiments made showed a negative result. The second enquiry was to examine the specific gravity of this fluid, which has usually been stated to be 1030. In this investigation Dr. Davy found as might, *a priori*, have been expected, that the specific gravity of this fluid is variable in different instances. In seven distinct examinations it was found in one so low as 1028, while in another it amounted to 1042. This great difference is attributable to two circumstances, the proportions of the different ingredients, and the variable density of the liquid part. The pus-globules, he believes, are of not less density than the blood-corpuscles, an opinion to which he has been led by direct experiment. In other experiments Dr. Davy found that pus freezes less rapidly than water, and that when thawed it does not regain its original properties. From his experiments on the composition of pus, he has satisfied himself that the analysis of this fluid recently given by Dr. Gueterbock* is correct, and deserving of confidence, it

* De Pure et Granulatione, p. 17; Berol., 1837.

being confirmed generally by the results of his own investigations, although he has followed in these a method of analysis different from that author. Dr. Gueterbock's analysis is as follows:

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| Water | 86·1 |
| Fatty matter soluble only in hot alcohol | 1·6 |
| Substances soluble in cold alcohol, viz. fatty matter and osmazome | 4·3 |
| Substances soluble neither in hot nor cold alcohol, viz. albumen pyina, and the substances of the globules ("globuli et grana puris") | 7·4 |
| Loss | ·6 |
| | 100·0 |

We cannot afford space for an examination of all the remaining papers on detached subjects in these interesting volumes, although many of them are of great value, particularly those on the action of corrosive sublimate, and on that of lime and of tannin on animal and vegetable substances, but shall content ourselves with briefly noticing those on the poisons of some reptiles,—the toad and snakes of Ceylon,—before passing to the consideration of others which have constituted Dr. Davy's favorite enquiries, the temperature of animals and its connexion with the functions of circulation and respiration.

It has long been a popular opinion that the toad secretes a peculiar poison, and dogs have often been said to have suffered from affections of the mouth and fauces after having bitten this reptile. This opinion has been almost universally rejected by naturalists; but Dr. Davy states that his own investigations have led him to the conclusion that the vulgar notion is not without foundation, and that a peculiar fluid is contained in follicles of the skin, more especially about the shoulders and neck of the animal, which he has collected from old and large animals in a quantity sufficient for the purposes of examination. This fluid, he states, is of a more acrid nature than the poison of most venomous snakes, but is less fatal when absorbed into the circulation, and hence he conceives it to be analogous rather to the fluid secreted by the bee and scorpion than to that of the poison of snakes. We cannot subscribe to this opinion. The poison of the insects named is secreted by distinct glands within the body, no way connected with the tegumentary structures, and in our opinion is more analogous to that of the snake, which like it is secreted within the body, than to the fluid from the cutaneous follicles of the toad. The poison of the bee certainly appears to act more locally when absorbed into the system, than that of the snake, at least in the higher vertebrata; but this is not the case in its effects on the invertebrata; to them it is a most deadly poison and is followed by fatal results when injected into the body in a very few minutes, as every one is convinced who has witnessed its effects during the combats which are so well known to take place in the hive. Neither are its effects to be despised when injected even into the human body in sufficient quantity, as many fatal cases are known to have resulted from its introduction.

Dr. Davy seems to think that the poison of the toad may in some way

be connected with the function of the skin and perform a part in the economy of the animal; which certainly is not the case with that of the insect, which must be regarded as an offensive and excretory fluid. He conceives that "it may serve to separate a portion of carbon from the blood, and thus in its function be auxiliary to the function of the lungs." In support of which notion he remarks, "I have found the pulmonary arteries of the toad, each divided into two branches, one of which went to its respective lung, and the other very little smaller, to the cutis between the head and shoulders on each side, and was extensively ramified where the largest follicles were situated, and where there was a plexus of veins of great size, as if intended for a reservoir of blood." (p. 111.) His reasoning, however, appears to us inconclusive: as it is well known that the skin of the batrachia is an important organ of atmospheric respiration, between which and the secretion of poison there does not seem to us to be any connexion.

The poison of animals is a subject that well deserves the closest attention, since its *modus operandi* in most cases is at present involved in the greatest obscurity. At one time its introduction into the living body is immediately followed by the most violent and fatal results, and at another by scarcely any apparent effect, even when the injury in both cases has been inflicted by the same animal. This is fully shown in the account given by Dr. Davy of his experiments on the *poisonous snakes of Ceylon.* These were made to ascertain what are the effects of their poison, and were instituted to smooth the way to determine, if possible, the action of each poison, and furnish data for inferring what are the pure effects of the poison, what of the powers of nature opposing its effects, and what those of the medicines administered. In no subject is discrimination more required than in this mysterious one; and in no one perhaps has less judicious discrimination been used. It has often been taken for granted that the poison of all snakes is similar,—not differing in its kind, but only in its intensity of action; and, agreeably to this assumption, that the medicine useful in one instance must be serviceable in all. And too often, medicines have got into repute as antidotes from being given in slight cases, in which recovery would have taken place without medical treatment,—beneficial changes that were due merely to the preservative powers of the constitution. (Vol. i. p. 132.)

We fully coincide with these observations, which are amply exemplified in the experiments on the snakes of Ceylon. The subject of these experiments were the hooded snake, *naia tripudians;* the carawilla, *trigonocephalus hypnale;* and the tic-polonga, *vipera elegans.* Of these the latter, like the European species of the same genus, is the most venomous, and its bite is usually fatal; that of the hooded snake is fatal in a less degree, and the carawilla but very rarely so. All the experiments showed that the direct effects of the poison of each species were very different. The details of them possess much interest, but they are too long for quotation, and would not well bear abridgment; so that we must refer those interested in the subject to the volumes themselves.

One remarkable circumstance connected with the poisoning of these reptiles is that the most venomous appear to occasion the slightest local inconvenience at the moment of the introduction of the poison, the subject

bitten being often unconscious that he has been wounded. This was noticed by Dr. Davy in his experiments on fowls, and was also noticed in a case of a man bitten by a rattle-snake. In that instance no pain whatever was felt by the wounded party on the introduction of the poison; the effects of which, when they became apparent, were at first trifling and local, although followed by a fatal result in less than twelve hours. Notwithstanding the rapidity with which death takes place in some instances of the bite of venomous reptiles, there are good reasons for believing that in every instance the poison has already entered the general circulation before any ill effects are produced.

On the medical treatment of these injuries Dr. Davy offers no opinion, but merely remarks that oil, both when administered internally and when applied externally, has been supposed to do good in cases of poisoning by the viper, and that arsenic has seemed to do good in some instances of poisoning by the hooded snake. It is probable that there is no specific remedy for any of these injuries, but that, as Dr. Davy suggests, the bite of each species of these reptiles may require a different mode of treatment according to the nature of the poison. The first and most important indication in every instance is to excise the bitten part, and by the application of cupping-glasses and ligatures to prevent as much as possible the poison from being absorbed into the circulation.

In examining the details of these experiments we have been struck with the great similarity of the effects produced by the poison of the tic polonga to those of poisoning by prussic acid. Thus both destroy life with great rapidity, both quickly deprive the muscular fibre of its power of contraction, and both, under certain circumstances, prevent the blood from coagulating. But it is worthy of remark that in this latter respect the two poisons appear to differ or to resemble each other according to the rapidity or the slowness with which they occasion death. Thus when a fatal result is produced by the poison of the snake within a few minutes, the blood is instantly coagulated both in the arteries and veins; but if death be not occasioned quickly, and an hour or more elapse, then the blood remains fluid in the vessels, as in the case of poisoning by prussic acid. This similarity in the principal effects of these poisons has induced us to believe that the employment of powerful diffusible stimuli, as ammonia, &c. both internally and externally, may be attended with benefit in cases of poisoning by the tic polonga, as it is believed to be in cases of poisoning by prussic acid, and by the common viper, a reptile far less deadly than the tic, but which seems to produce the same disposition in the blood to coagulate and become congested in the vessels; as is evidenced by the well-known swelling, lividity, and pain which quickly follow the introduction of its poison.

Before closing these remarks we cannot refrain from noticing a circumstance which fell under our own observation in respect to the slow effects of prussic acid on the common snake. We were endeavouring to kill a full-grown specimen for the purposes of dissection, by pouring into its mouth about a drachm of Scheele's medicinal prussic acid. After waiting a few minutes for the result we were astonished at finding that the poison had produced no marked effect on the reptile, which still continued very lively. A second dose of more than double the same quantity was again poured into its mouth, but it was not until after several minutes had

elapsed that the poison seemed to begin to exert its deadly influence. A friend has mentioned to us an occurrence of the same kind in regard to the turtle. The explanation is probably to be found in those peculiarities of constitution in reptiles which confer upon them such remarkable power of enduring severe injuries.

The various papers on animal heat contained in these volumes include some of the most interesting portions of Dr. Davy's labours, and have added greatly to our knowledge of this important subject, of which Dr. Davy has proved himself one of the most careful and accurate investigators. His success in these enquiries has evidently depended greatly upon his having followed in all his experiments that mode of proceeding which we are glad to know is now more generally adopted than formerly, and which it is to be regretted has not always been pursued by physiologists: the carefully collecting of a large number of facts at the outset of each investigation, upon which to base his opinions, before any fixed theoretical notions had taken forcible possession of the mind of the investigator, a course which alone enables the enquirer to deduce correct and impartial conclusions. The importance of this method is fully estimated by all who have had the patience to pursue it, as being that alone which will lead to the establishment of a solid reputation, the foundation of which is not to be shaken, however much assailed by plausible criticism or brilliant theories. In pursuing this method Dr. Davy has succeeded in establishing many important truths.

In the *account of some experiments on animal heat*, one of the earliest and most valuable of these papers, are announced the important fact of a difference of temperature between the two sides of the heart, and also between venous and arterial blood. Dr. Crawford had assumed, as the basis of his theory of animal heat, that there is a difference in the capacities of the two kinds of blood for caloric, and an uniformity of temperature of the blood in the two ventricles; the heat of the body, according to his views, being supplied by the arterial blood, which he supposed contained more latent heat than the venous, and which it derived directly from the air during respiration, and liberated again during the conversion of arterial into venous blood. The facts ascertained by Dr. Davy are strongly opposed to this theory. Besides an absolute difference in the temperature of venous and arterial blood appreciable by the thermometer, he found that there is in reality scarcely any difference in the capacities of the two kinds of blood for heat in the latent form. In his most carefully performed experiments on the blood of the sheep, he ascertained that the specific gravity of venous blood was 1051, and its capacity for heat 903, while that of arterial was 1049, and its capacity for heat 913, a difference so trifling as to be totally insufficient for the support of Crawford's theory. These were the numbers considered by Davy as the nearest approximation to the truth, but we are nevertheless inclined to suggest whether there may not be constantly some variation in this respect in venous and arterial blood at different ages, and perhaps also at different seasons of the year; since in another set of experiments on the blood of a lamb four months old, the relative capacities appeared to be as 922 for the venous, and 934 for the arterial blood; and in a third set on a lamb five months old, the specific gravity of venous blood was 1050, and its capacity for heat 852; while the specific gravity of

arterial blood was 1049, and its capacity for heat 839. The difference of temperature in the blood of the two sides of the heart has not been observed by all experimenters. Krimer, Scudamore, and Meyer have noticed the difference of temperature in venous and arterial blood, but the latter author could not observe that of the two sides of the heart. We are ourselves satisfied of the general correctness of Dr. Davy's observations on this subject, but do not think that this difference is so great as has usually been stated. Meyer noticed a difference of from 2¼ to 4½ degrees of Fahrenheit between the blood of the jugular vein and that of the carotid, but yet failed to observe a difference between the two sides of the heart. This circumstance leads us to suspect that there must have been some error in performing the experiment. The exposure of the vein for a few seconds, before ascertaining the temperature of the blood within it, would sufficiently account for the excess of difference between that and the blood of the carotid. We believe Dr. Davy's statement of the amount of this difference is much nearer the truth. The failure of many observers to notice a difference of temperature in the right and left ventricle may perhaps have arisen from a circumstance noticed by Davy in explanation of the discrepancy between his own observations and those of Messrs. Coleman and Cooper—namely, that when the animal has been killed by asphyxia, and there is an accumulation of blood in the right ventricle, he has then observed but little difference in the temperature of the two sides of the heart: a circumstance noticed also by ourselves, and which he explains by supposing that in all cases of impeded respiration the blood returning from the lungs to the left ventricle having been insufficiently or not at all acted upon by the air, little or no development of heat has taken place, while there is usually an accumulation of venous blood in the right ventricle, and the temperature in the two cavities is in consequence nearly similar. We are not so well satisfied with the details of the experiments on the temperature of other parts of the body as with those on the heart, since they appear to savour a little of preconceived notions. The great objection is the length of time that elapsed in making them, which occupied about an hour, during which the body was exposed; and, as the notice of the slight degree of pyrexia which followed evinces, together with the quickened pulse, unpleasant sensation of chilliness and parched mouth, clearly prove the body suffered from its unusual exposure to a detrimental influence. Of the fact of a general difference of temperature of different parts of the body we have not the slightest doubt, both as regards the external parts and the internal organs, although this difference may not be so great as has sometimes been supposed, nor as Dr. Davy's experiment on the temperature of his own body seems to indicate. Experiments of this description, in order to approach to the truth, ought to be made under circumstances as nearly as possible similar to those in which the body is usually placed, which was not the case in the experiment now alluded to. One of the most remarkable circumstances mentioned by Dr. Davy is that respecting the temperature of the brain, as compared with the temperature of *the rectum.* He had noticed this circumstance in some of his earliest and most carefully performed experiments, and, as he well remarks, surprised at the fact, was led to repeat his experiments upon four lambs. The temperature of the air at the time being 68°

Fahrenheit, the mean of the four observations on the brain was 105·18° Fahrenheit, and that of the rectum in the same animals 105·81° Fahrenheit, thus indicating an excess of temperature in the rectum of 0·63d of a degree of Fahrenheit; and apparently confirming the opinion that the source of animal heat is directly connected with the function of the lungs. In one only of these experiments was there any excess of temperature in the brain over that of the rectum, and that excess amounted only to 0·25th of a degree, while the maximum of temperature in the rectum, in one instance, was 1·75° Fahrenheit. Notwithstanding these apparently conclusive results, the fact was questioned by Professor Müller; and Dr. Davy in consequence was led to repeat his experiments in December and January, 1837-38, when the temperature of the air was 44° and 45° Fahrenheit, and again obtained similar results, but with much less difference than in his former experiments. Thus in eight observations, there were two in which the temperature of the brain exceeded that of the rectum, each one degree Fahrenheit; and one in which it was of exactly the same temperature, while in one instance only did the maximum of excess in the rectum amount to so much as one degree. But Dr. Davy considers these experiments as generally confirmatory of his preceding ones. On examining the mean of these experiments we find it is exceedingly trifling, and amounts only to 0·13th of a degree Fahrenheit, which, connected with some observations we ourselves have made on this subject, leads us most decidedly to agree in opinion with Professor Müller, that there has been some error of observation. Nevertheless we agree with Dr. Davy, that this apparent difference marks in a decided manner the effect of cooling influences on the brain; and we are fully satisfied that, "excepting the hands and feet, the brain appears to lose its heat more rapidly than any other part of the body," as is shown in his *observations on the temperature of the human body after death*, to which we shall presently refer.

The *observations on the temperature of man and other animals*, first published in 1826, have been constantly referred to as containing, like all Dr. Davy's papers, much valuable information. The first subject examined is the variation of temperature to which the human body is liable in passing from a temperate to a tropical climate, and in descending from a hilly country, where the temperature of the air is moderate, to a low one, where the diurnal variation is very considerable; and next the temperature of different races of men and kinds of animals. On each of these subjects Dr. Davy has succeeded in establishing new facts, which have since been further confirmed by other observers. It is to these observations that we are indebted for our knowledge that the human body acquires an increase of temperature in passing from the temperate to the torrid zone, a circumstance which, although naturally to be expected, was left in doubt by the previous observations of Dr. Franklin on the temperature of his own body. The observations before us were commenced on the 10th of March at noon, in latitude N. 9·42°, when the atmosphere was 78° Fahrenheit. The mean temperature of seven individuals in good health was then 98·85°. We have deduced this from the details given in the paper. On the 21st of the same month, in latitude N. 0·12° at noon, with the sun apparently vertical, the sky clear, a fresh breeze blowing, and the temperature of the air 79·5° Fahrenheit, that of the

same individuals was 99·21° Fahrenheit. On the 4th of April, in latitude S. 23·43, at between twelve and one o'clock in the afternoon, the weather very fine and temperature of the atmosphere 80° Fahrenheit, the mean of that of the same individuals was 99·67° Fahrenheit; and lastly, on the 5th of May, in latitude S. 35·22° Fahrenheit, with the weather damp and cool, the temperature of the air only 60° Fahrenheit, and when a general sensation of chilliness was felt, it had sunk to 98·28° Fahrenheit. No continued series of observations have yet been published on the variation which the temperature of the body undergoes in low states of the atmosphere, so that as yet we have but insufficient means of judging of the amount of this variation in a state of good general health. Observations similar to the above were made on the change of temperature experienced in passing from a high mountainous district with a comparatively low temperature of the air to a low one with a high temperature, with the same general results as those above stated. Thus in passing from Kandy, in Ceylon, which is elevated about 1500 feet above the level of the sea, to Trincomalee, which is about fifty miles distant from any mountains, and has a mean annual temperature of from ten to fifteen degrees higher than Kandy, the temperature of the body was increased, and on returning to the same place was again diminished. Thus six individuals before leaving Kandy, when the atmosphere was 69° Fahrenheit, had a mean temperature of 98·15°; and in a few days afterwards at Trincomalee, when the air was 82° Fahrenheit, and when they had been leading a sedentary life, it was increased to 99·9° Fahrenheit, a difference of more than a degree and a half. Thus, although it must be borne in mind that in the latter instances the individuals had breakfasted but two hours previously, a circumstance for which some allowance ought to be made, it is evident that they had acquired an actual increase of temperature, which was still further proved by the fact that on their return to the mountainous district, when the mean temperature of the air was the same as before leaving, that of their bodies had subsided again to about 98° Fahrenheit. That this variation of temperature does not depend on the individuals examined belonging to a different race from ourselves, was proved by observations on many individuals of different races and tribes, both at the Cape of Good Hope and Ceylon, and the Isle of France; on the Hottentot, the Negro, the natives of Madagascar and Mozambique; the Caffres, the Vaidas, Singalese, Malays, Sepoys, and half-caste. In none of these, as Dr. Davy remarks, did the temperature vary more than it is found to do among any similar number of Europeans, and consequently the slight differences observed between them could not be attributed to difference of race or habit.

In the observations on the temperature of the different kinds of animals, we observe the same general accuracy and care as in those on man. We shall briefly notice the most interesting of these, remarking, however, that in a few instances we have reason to believe that the amount recorded is rather below the general temperature of the animals. Thus in the domestic cat in this country, when the temperature of the air in September was 60° Fahrenheit, that of the cat was 101° Fahrenheit; and in Kandy when the air was 79°, that of another specimen was 102° Fahrenheit; now we have observed it in this country in the summer, when the temperature of the atmosphere has ranged between 65° and 70° Fahren-

heit, amount to from 102·5° to 103·5° Fahrenheit. Again, the temperature of sheep in Scotland during the summer, is said to vary from 101° to 104° Fahrenheit; at the Cape of Good Hope, in winter, from 103° to 104° Fahrenheit; and at Ceylon, when the air was 78° to 105°. We have ourselves observed a much higher temperature than is here recorded in the sheep of our own country, and that too when the air has been scarcely more than 60° Fahrenheit. The temperature of the common fowl has been given with more correctness. At Edinburgh, in winter, when the air was 40° Fahrenheit, the temperature of a full-grown hen was 108° Fahrenheit; and at Mount Lavinia, in December, when the air was 78° it was found to be 110° Fahrenheit; and in the instance of some chickens in was 111° Fahrenheit. The temperature of the common duck was similar to that of the fowl.

The observations on the different classes of reptiles correspond with those which had previously been made by Martine* and John Hunter;† and also with the more recent and numerous cases of Czermak,‡ Welford,§ and Berthold,‖ in so far as that all of these observers have noticed a higher temperature of body than that of the surrounding medium when this has remained stationary, and the animals have been in a state of activity. Hunter's experiments, made as they appear to have been without reference to these circumstances, seem at first somewhat contradictory. Thus in some instances he found a higher, and in others a lower temperature than that of the surrounding medium; but this discrepancy is cleared up by the experiments of others. The cause of the apparent non-accordance of his facts is distinctly shown on comparing those ascertained by Davy with those of Berthold, on the chelonian reptiles and the true amphibia; and also with those of Czermak and Welford, on the ophidian and saurian reptiles. Thus, even in the higher reptiles, the Chelonia, Davy found in one species, *Testudo Mydas*, a temperature of 84°, when that of the atmosphere was 79·5° Fahrenheit; and in another individual of this species, when the air was 80° Fahrenheit, a temperature of 88·5° Fahrenheit; and in a third which had been caught on the day previous, a temperature of only 85°, when that of the air was 86°. These differences, we have no doubt, arose, as in some of Hunter's experiments, from the individuals having been removed from a medium much colder than themselves, or from their being exposed to an atmosphere, the temperature of which was either rising or sinking rapidly, and in which the low amount of respiration in the animals was insufficient to change the temperature of their bodies so quickly as that of the surrounding air. Berthold's experiments clearly prove this to be the case. In the winter, when the animal was torpid, he found the temperature of *Testudo Græca* equal only to that of the atmosphere, which was about 56° Fahrenheit, but in summer when the animal was active, it had a temperature of 74·18° Fahrenheit, that of the air being 73·62° Fahrenheit. The same individual in winter, when aroused by being placed in a very gradually increased artificial heat, acquired in the same space of time a

* Medical and Philosophical Essays; London, 1740.
† Philosophical Transactions, 1775-78.
‡ Baumgærtner und Ettingshausen, Zeitschrift für Physick; 1824, vol. iii.
§ Annals of Philosophy, vol. ii.
‖ Neue Versuche über die Temperatur der kaltblütigen Thiere; Götting. 1835.

much higher temperature than the air, or a vessel of water placed beside it, the temperature of the turtle being 95° Fahrenheit, and that of the water 83·7 Fahrenheit. Dr. Davy observed a similar difference in the same species when examined at different temperatures. At the Cape of Good Hope when the air was 61° Fahrenheit, that of the turtle was 62·5 Fahrenheit; and at Calumbo, when the air was 80, that of a larger individual of the same species was 87° Fahrenheit. In the true amphibia, a temperature lower than that of the surrounding medium is of very frequent occurrence. This may be attributed to the conjoint influence of two circumstances: first, a deficiency of power to generate heat so rapidly as it passes off from the body by means of evaporation, when the temperature of the surrounding medium is rising quickly; and next, and perhaps chiefly, to the much larger amount of heat, which is abstracted from the body in proportion to the amount of exhalation from the surface, which goes on more quickly in proportion to the increased temperature of the surrounding air.

On the temperature of fishes Dr. Davy has added some new and valuable facts. Previously to his observations it was supposed that the temperature of this class did not amount to more than one or at most two degrees above that of the medium in which the animals exist. Some authors even have denied entirely the existence of an independent temperature in fishes; among these are the distinguished naturalists, Humboldt and Provençal. But Dr. Davy has shown, in accordance with the statements of Martine, Hunter, Broussonet, and others, that not only do they generate an amount of heat above that of the surrounding medium, but that this amount varies in different species. In a shark just captured, and while still alive, when the temperature of the air was 71·75° Fahrenheit, and that of the sea 74·75° Fahrenheit, the temperature of the deep muscles of the back was 77° Fahrenheit. But the most remarkable instance of temperature in fishes is in the Bonito, in which, when the sea from which it was just taken was 80°, the deep muscles of the back had a temperature of 99° Fahrenheit; but the heat which is situated nearer the surface of the body was only 92° Fahrenheit: a difference not easily to be reconciled with Dr. Davy's experiments on some other animals.

On the heat of the Invertebrata, Dr. Davy's observations have fallen a little short of the facts. Although it is subject to vary under the influence of external circumstances, even to a greater extent than in most of the lower vertebrata, it amounts to a degree in some instances scarcely inferior to that of the more perfectly organized animals. We have proofs of this in the whole of the gregarious hymenoptera. The whole of the insect tribes have a temperature above that of the surrounding medium. But this is not shown in the experiments before us, some of which seem to indicate a lower temperature in some species than that of the atmosphere. This error of observation has doubtless arisen from inattention to the circumstance of the temperature of the air having risen during a state of quiescence or inactivity of the insect. Thus the temperature of *Blatta orientalis*, the common cockroach, is stated to have been only 75° Fahrenheit, when that of the air was 83° Fahrenheit. We are confirmed in our opinion of the inaccuracy of this observation, and of its dependence upon the circumstance alluded to, by another ob-

servation which Davy made on the same species, in which he found the temperature of the insect 75° Fahrenheit, when that of the atmosphere was only 74° Fahrenheit. That none of the air-breathing insects have a lower temperature than the medium they inhabit, when this has been subject to but slight variations, or even when this has been changing, and the insects at the same time have continued in a state of great activity, is sufficiently proved by the observations both of Berthold and Newport.* Berthold observed a temperature in the larvæ of caterpillars of the white cabbage butterfly, of from 1·5° to 2·5° Fahrenheit lower than that of the atmosphere, as had formerly been noticed by one of the oldest and most careful enquirers of this country, Martine; and also a high temperature in hymenoptera, and the enquiries of Newport have shown not only the constant development of an independent temperature in all insects in a state of activity; but also that the amount of this varies in the different tribes, being greatest in those which are most active and respire the greatest quantity of air. In the crustacea also, as well as in the mollusca, Dr. Davy has failed to observe an independent temperature, although there are good reasons for believing that it really does exist. Rudolphi observed a temperature of two or more degrees in the common crawfish, *astacus fluviatilis;* Hunter, Spallanzani,† and Gaspard,‡ have noticed it in the air-breathing mollusca; Pfeiffer§ in the bivalves, and Hunter in the common leech.

We have dwelt more especially on these measures of temperature in different animals as given by Dr. Davy, and have compared them with the observations of others, because of their great value as a series, and because as such they have been adopted as a standard of reference by Dr. Edwards in his paper on Animal Heat;‖ and further, because of their immediate connexion with the enquiry concerning its origin.

In the paper on *animal heat*, Dr. Davy formerly gave the results of a few experiments on the very young infant, and expressed an opinion which is opposed to the more recent experiments of Dr. Edwards, but which he has not seen occasion to alter in this republication of his labours. He believes the temperature of the young animal to be higher than that of its parent. In illustration of this he states that the heat of the axilla of a child just born was 98·5° Fahrenheit; that at the expiration of twelve hours it was 99° Fahrenheit, and for three days continued at the same temperature; that in the instance of a weakly infant, an hour after birth, the temperature did not exceed 96° Fahrenheit, while on the following day it had risen to 98·5° Fahrenheit; and that in another infant it was 99° Fahrenheit. Dr. Edwards, on the contrary, believes that the temperature of the very young animal is in every instance lower than that of the parent, and that the human infant does not offer an exception to the rule. Dr. Davy's opinion has been advocated by several physiologists, and seems at first to be supported by the numerous experiments he has made on the temperature of children in temperate and tropical climates. But notwithstanding these the opinion of Dr. Edwards is substantially correct. The children experimented on by Davy, varied in age from

* Philosophical Trans., 1837. † Mém. sur la Réspiration.
‡ Majendie, Journal de Physiol, vol. ii.
§ Naturgeschichte deutscher Land und Süsswasser Mollusken; Weimer, 1825.
‖ Cyclopædia of Practical and Comparative Anatomy, vol. ii.

four to fourteen years, a period in which, as Dr. Edwards has since shown,* the temperature of the child is indeed higher than that of the adult, while that of the very young infant is lower. He believes this difference to amount to so much as two degrees. We do not admit the existence of so great a difference as this, but we are satisfied of the fact, not only that the temperature of the child is lower than that of its parent, but also that the very young infant is incapable of maintaining the heat it is able to generate, when exposed to cold or removed from the warmth of its parent; and this opinion is further confirmed by the well-known fact, that the less mature the infant, the more constantly must heat be supplied to it from external sources, in order to assist its development.

But independently of the age of an animal, its temperature is now known to vary in different states of health. In a pathological point of view, therefore, the ascertainment of the real temperature of the body is a matter of great importance. In the insane, as Dr. Davy states, it has usually been supposed to be lower than in other individuals. It was desirable, therefore, to ascertain whether this opinion is correct, since the practitioner in his treatment of those labouring under this, the most humiliating and distressing of all infirmities, is often obliged to judge of the state of health almost entirely by the few symptoms which present themselves, the patient making no complaint of illness but, on the contrary, when questioned on the subject, pronouncing himself in good health, even when suffering from severe organic disease. Piorry† and others have shown that in fevers, and diseases of the lungs and other structures, the heat of the body may become raised to many degrees above the normal standard. The importance of attention to this subject is well remarked on by Dr. Davy. He believes that an attention to the temperature and pulse will often lead to the detection of disease not before suspected to exist, as was directly proved in a case described by him. The patient, who made no complaint, had a good appetite, and performed his functions naturally, on being found with an excited pulse, and a temperature under the tongue of 104·5° Fahrenheit, was more closely examined, and was then ascertained to be suffering from extensive disease with vomica, tubercles, and ulceration of the intestines and disease of the generative organs, but all unaccompanied by the usual symptoms.

Contrary to the generally received opinion, Dr. Davy found that the temperature of the insane certainly is not lower than that of other individuals. On examining the table which he has given of the temperature of twenty-four individuals, the mean age of whom, between the extremes of thirty-one and sixty-nine, was forty-seven years and about nine months, we find that it is not slightly, as stated by him, but considerably higher than that of other persons. Thus the mean of these twenty-four individuals, examined in winter, on the 17th of January, when the temperature of the atmosphere was 30° Fahrenheit, amounted to 99·16° Fahrenheit, the extremes being 98° and 102°; but in summer, on the 4th of August following, when the air was 68°, the mean of their temperature was 104·06° Fahrenheit, the extremes being 99° and 104·50°. These are valuable and interesting facts, and merit further enquiry.

* Cyclopædia of Practical Anatomy, art. Heat.

† Traité de Diagnostic et de Semeiologie, par P. A. Piorry. 1838.

Another pathological condition to which Dr. Davy has directed his attention, is *the effect of violent exercise on the temperature of the body* in a state of health. The observations are too few to enable us to deduce from them any positive conclusion, but are believed by him to show that during violent exercise the temperature becomes reduced. The observations, it should be remembered, were made in a hot climate, where the functions of the skin are more active than in milder regions.

The temperature of the human body after death is another subject of pathological interest, which we cannot omit to notice. In this paper Dr. Davy has directed attention to facts, which, when reduced to general principles, by means of a large number of carefully repeated observations on the rate of cooling of the body under different circumstances and in different temperatures and hygrometric states of the atmosphere, may hereafter become of great value in medico-legal enquiries. But a vast number of carefully repeated experiments are necessary before this important result can be hoped to be attained. Even when reduced to the precision of certain well-ascertained laws, its employment in difficult cases of medical jurisprudence, as Dr. Davy remarks, can only be truly useful in the hands of the most cautious and discerning. That it will ultimately become so, through the labours of many observers, we have not the slightest doubt, and we feel that much credit is due to him for commencing the enquiry. On examining the details now given we are not a little surprised at the remarkably high temperature that sometimes exists. Thus in one case of rheumatism, examined three hours and a half after death, when the temperature of the room was 86° Fahrenheit, after some of the viscera had been exposed for nearly ten minutes the mercury of a thermometer placed under the left ventricle rose to 113° Fahrenheit, and when in contact with the liver, the lobulus spigelii, to 112° Fahrenheit. In a second subject, examined six hours after death, the thermometer under the left ventricle indicated a temperature of 108° Fahrenheit, and when in contact with the lobulus spigelii 107° Fahrenheit. In these cases the patients were ill but a short time, and died suddenly, and Dr. Davy is decidedly of opinion that this high temperature had been acquired before death. It is interesting to observe that the bodies of those who died of slow fevers did not show so high a temperature. The highest was in those in which disease was most active. These circumstances strongly remind us of the high temperatures given by Piorry, and noticed in a former number of this Journal,* but which we were at that time disposed to receive with some hesitation. The subject merits more extensive examination, as well in regard to the temperature in disease before and after death.

*The experiments and observations on the blood* lead us to the consideration of the important changes which this fluid undergoes in the course of the circulation and when submitted to the process of respiration, as well as to the relation which they bear to the development of animal heat. We have seen in the above notices of Dr. Davy's papers the great difference of amount in the temperature of different animals, the variation it is subject to in different seasons, climates, periods of life, and states of health; and the questions naturally arise, what are the causes

* British and Foreign Med. Review, vol. VI. p. 142.

of this difference, and the circumstances attendant upon it? That the heat of the body is the result of certain changes in the blood is now, we believe, almost universally admitted; but it is still questioned by some whether, as most English physiologists believe, these changes, and the accompanying evolution of heat, are the necessary consequences of a chemical action of the air on the blood, or whether the evolution of heat results directly from the influence of the nervous system? Dr. Davy inclines to the opinion "that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs, in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life." (vol. ii. p. 171.) All the facts noticed in these experiments on the blood go to support this opinion; but we cannot afford space in this article, which has already exceeded the limits we had proposed for it, to examine them.

Dr. Davy's more recent experiments (the results of which differ considerably from those of his earlier ones, in consequence of his employing a more perfect apparatus) confirm those of Magnus, in regard to the presence of free gases in the blood, and the predominance of oxygen in arterial blood, and of carbonic acid in venous. They also show the lungs to be both absorbing and excreting organs.

All the facts noticed by Dr. Davy, and many others to which we could refer, tend to show that the evolution of heat is dependent chiefly on the chemical effect of the process of respiration, and also to explain the cause of the variation in the amount of heat in different animals, states, and periods of life. The highest temperature of body is in birds, and it is this class of animals which have the greatest rapidity of circulation and frequency of respiration, and which endure the privation of air with greater difficulty than any other animals. On the other hand it is in the lowest of the vertebrata, the reptiles and amphibia, which have a slow circulation, and can endure the privation of air for a long time, that there is the lowest temperature of body. Now these extreme conditions of bodily heat occur at stated periods in the same individuals of different tribes of mammalia, the hybernating animals. The hedgehog, the dormouse, and the bat, in their season of activity, greatly resemble birds in all their functions; the rapidity of circulation, frequency of the acts of respiration, and a high temperature of body; while like them they perish quickly when deprived of atmospheric air. But at another period, these same animals closely resemble the reptile in supporting, uninjured, for a long time, the absence of air, in the almost entire cessation of their circulatory and respiratory functions, and in the reduction of their temperature almost to that of the surrounding medium. Similar extreme conditions of life are observed in the same individuals in some of the insecta, both in regard to the activity at one season and to the infrequency at another of the respiratory and circulatory functions, and to a greatly augmented or a scarcely appreciable temperature. A striking instance of this variation in its vital functions is seen in the humble bee, which in its state of greatest activity has a temperature of body scarcely inferior to that of many mammalia, respires a very large proportion of atmospheric air, and perishes as quickly as other animals if deprived of it at that season; but at another time, like the true hybernating verte-

brata, it remains for many weeks in a state of total inactivity, and requires for its support not one thousandth part of the air it deteriorates at another season, and evolves no perceptible heat.*

But notwithstanding the apparent strength of the above facts to support the opinion that animal heat is the direct result of changes induced in the blood through the agency of respiration, we cannot omit to notice that there are certain conditions of the body which at first seem opposed to this conclusion, and to lead to that which regards the evolution of heat as resulting from the direct agency of the nervous system. We have ourselves witnessed cases of paralysis of the whole of the voluntary muscles of the body, including some of those of respiration, in which there existed a very high temperature for many hours before death, and this too when the function of respiration was performed with much difficulty. But in each of these cases the activity of the circulatory organs had been greatly increased, so that a greater quantity of blood had passed through the vessels in a given time; a circumstance sufficient in itself, we think, to account for the increased evolution of heat, whatever might be assigned as the cause of this increased activity of the organs.

But our exhausted limits warn us to close this article. We feel assured that what we have said of Dr. Davy's labours will sufficiently convey the very high estimation in which we hold these volumes, and that it is needless for us again, in formal terms, to express the gratification we have derived from their perusal, or our conviction that they will conduce very much to the interest of science.

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## ART. IX.

*The Anatomy of Suicide.* By FORBES WINSLOW, Member of the Royal College of Surgeons, &c.—*London*, 1840. 8vo, pp. 339.

HAD this work not been rather of a popular than of a professional character, we would have noticed it sooner. The variety of anecdotes which it contains may render it in some degree attractive to the general reader, but there is not much to interest the philosopher or the physician. We will notice briefly the several heads under which Mr. Winslow distributes his subject.

Chap. i. is on the "suicides of the ancients." It is written in a sufficiently entertaining manner, and contains divers instances of suicide among the ancients, and some notice of the opinions and laws of the Greeks and Romans regarding it; but here, as in other parts of the work, the author is much too easy as to the evidence on which his cases are adduced. For example (p. 17) he tell us, with great gravity, that when Empedocles threw himself into the crater of Mount Etna, his fate was not discovered till one of his sandals was ejected from the volcano. Setting aside the conceit of the sandal, Mr. Winslow ought to have known that the story about Empedocles and Mount Etna rests on no sort of testimony, and that the real manner of the philosopher's death is quite uncertain.†

In chap. ii. our author notices and confutes "various writers in defence

* Newport, Philosophical Trans. 1836-7.

† Diogenes Laertius, de vitis Philosophorum, in vitâ Empedoclis.

of suicide," and expresses himself with no great amenity towards David Hume, calling him "the detestable author of this abominable treatise," &c. We dislike many of Hume's opinions just as much as Mr. Winslow can do, and think that his metaphysical powers have been over-rated; but we are not on that account disposed to deny that he was a philosophic, candid, and amiable man. We cannot think it much worth while to argue with the advocates of a crime, from which reason, religion, and instinct alike recoil. Mr. Winslow, however, in chap. iii. exerts himself to prove that suicide is "a crime against God and man," wherein we shall content ourselves with entirely acquiescing. He also argues that suicide is an act of cowardice, not courage, a point which admits of more discussion. Any difficulty which the question may appear to present arises, we think, from not discriminating sufficiently between physical and moral courage. We regard suicide as an act of moral cowardice, and we believe that the degree of physical courage of the individual has very little to do with the matter; for it happens in many other instances besides that of suicide, that an utter subversion of all moral firmness and self-possession causes a suspension of the instinct of self-preservation. This has often been exemplified in the valour of men, and of women too, who have fought desperately because they were desperately frightened; the general sense of fear so overwhelming the moral and intelligent being, that the actual physical causes of danger are made light of, and rashly encountered. An amusing illustration of this is contained in James Hogg's tale of "Basil Lee." The hero performs prodigies of valour, and gets the reputation of being the bravest man in the British army because, at the approach of battle, he is so transported with terror that he has only one idea left, which is a vague though intense conception that everything is to be exterminated; and accordingly, he lays about him with wonderful energy and effect. We believe that suicide might be committed by the bravest man in the world, as well as by the man most deficient in personal courage; but no man of moral courage would commit an act which implies an utter loss of all self-dependence, as well as all dependence upon Providence.

Chap. iv. is "on the influence of certain mental states in inducing the disposition to suicide." There is here a good deal of interesting matter, and a number of cases, some of which would have been more valuable if the source whence they were derived had been stated. Some of the instances, also, are irrelevant. Thus, at p. 74, we are told that the late Mr. Mathews, the comedian, knocked a very little man off a very tall horse, in obedience to a sudden and irresistible impulse, arising from an over-excited state of the brain: and at p. 75, we learn the fate of some teacups which fell victims to a blind propensity to destroy, manifested by a woman in the Asylum at Wakefield. We do not mean to say that such cases have no connexion with the subject of suicide, but that the author has omitted to point out the connexion, and placed them heterogeneously among his instances of suicide from a blind impulse.

In a note at the bottom of p. 101 we are somewhat surprised to find the following words:

"A phenomenon attends the dying moment which we do not recollect to have seen noticed. A man who fell into the water, and who rose several times to the surface, had a consciousness of the hopelessness and awfulness of his

situation; he felt that death was inevitable. With this conviction on his mind he saw presented to him a picture of his past life; the minutest action in which he had been engaged was brought in a kind of tableau before him. Circumstances which had long been forgotten were conjured up from his brain, and he had a bird's eye view of his past career. Possibly this may occur to every person at the moment of dying. The expressions of those placed under such circumstances would indicate as much."

We must observe that the coincidence between the above case and one mentioned in the Confessions of an English Opium Eater is rather singular, and that the generalization of the fact (supposing it to be one) is rather hasty. We cannot here enter into so metaphysical a subject, but we may, in passing, express a doubt whether, under any circumstances of life or death, a created mind could take in the survey of a lifetime in a moment; because this would imply an independence of *time* and of the *succession of ideas*—a condition which we apprehend is peculiar to the Deity.

Chap. v., on "imitative or epidemic suicide," is a short one; and the author has perhaps done well not to enter into the philosophy of a subject which involves some of the most obscure points both of physiology and of morals.

Chap. vi., on "suicide from fascination," is also brief. The conclusion is amusing. In reference to the disposition to suicide by precipitation from a height, the author observes that "persons who are subject to feelings of this character should be advised to avoid ascending elevated places."

Chap. vii. is on "the enthusiasm and mental irritability which, if encouraged, would lead to suicide." The morbid excitability frequently attendant on high development of the mental powers is here illustrated by several anecdotes of distinguished men. Speaking of this with reference to Byron, the author observes, "There can be no doubt that it was but the natural effect of a peculiar condition of nervous function; but instead of endeavouring to subdue the feeling, *he did his best to encourage it.*" The last words, we believe, contain the key to many of the peculiarities of men of genius. The truth is, that men of genius, especially in the imitative arts, are too frequently men of vanity; they love to attract attention, and develope various modes of "excitability," precisely on the same principle that some young ladies scream and faint.

Chap. viii. is on the "physical causes of suicide." These are referred to climate, season, cerebral diseases and injuries, physical suffering, diseases of the digestive organs complicated with melancholia and hypochondriasis, suppressed secretions, various forms of vice and intemperance, insanity, and hereditary disposition. In regard to the effects of season, Mr. Winslow combats the prevailing notion that November is the month in which most suicides are committed, and contends that "in all the European capitals when anything approaching to correct statistical evidence can be procured, the *maximum* of suicide is in the months of June and July; the *minimum* in October and November." (pp. 131-2.) With respect to disease as a predisposing cause of suicide, he insists much on the frequency of chronic disease of the brain, arising from injury or other causes, and cites Dr. Mantell's opinion as to the influence of slight but neglected injuries of the head in causing a disposition to self-destruction.

In reasoning on the influence of abdominal disease in inducing hypochondriasis and melancholy, Mr. Winslow makes the singular observation, "that all impressions arising from the viscera of the abdomen are naturally obscure." (p. 148.) We take leave to doubt this. Are the sensations of hunger, thirst, and nausea obscure sensations, or are the pains of cramp in the stomach and colic obscure pains?

Chapters ix. and x. are on "the moral and physical treatment of the suicidal disposition." They are perhaps the best in the book. Mr. Winslow very properly inculcates the expediency of removing a patient who shows a disposition to self-destruction from the scenes and associations to which he has been accustomed. He dwells, with equal truth, on the benefits which arise from cultivating a love of nature and an interest in the affairs, and a sympathy in the misfortunes of our fellow-beings. The contemplative frame of mind engendered by the intense feeling of natural beauty is often mistaken by the vulgar for melancholy; but we believe it to be in reality one of the surest preservatives against that state. We never knew a genuine disciple of Wordsworth who was a melancholy man. Again, it may seem paradoxical to maintain that an active participation in the misfortunes of others can win us from our own sorrows, and restore cheerfulness to the desponding mind. Yet such is the fact. We gain wisdom and strength by comparing ourselves with others, and our destiny with theirs. We find them bearing up against the very evils that we are sinking under; we see them sinking under evils which to us appear trivial: thus strength springs even from reciprocal weakness, and endurance from the interchange of affliction. Grief is indolent, benevolence is active; and in our successful exertions to overcome the difficulties or assuage the miseries of others, we often discover how much we have been wanting to ourselves, and find an apparently overwhelming evil resolved into our own want of fortitude and activity.

In chapter xi. our author argues the question whether suicide be the result of insanity? His general answer is in the affirmative, and we think he is right. It appears to us that there are two widely different states which singly or combined may conduce to suicide. The one is a state of *perverted instinct*, in which a blind propensity to self-destruction supersedes the instinct of self-preservation: a state parallel to that in which a mother destroys her child without being able to assign any cause for it but an irresistible impulse. The other is a state of moral depression, caused by the consciousness of evils which are either in themselves of dreadful magnitude, or which the mind of the individual is too feeble to bear. In this state the sufferer, though he still fears death, and perhaps trembles at the unknown futurity into which he is about to plunge, still dreads nothing so much as his present anguish, and thinking that any change must be for the better, voluntarily puts a period to his earthly existence.

Now it will not be disputed that the first of these states, that of perversion of the most powerful instinct of nature, constitutes a form of mania. With respect to the second state, it should be remembered that the evil which is thought intolerable, and to which death itself is preferred, is usually one which a vigorous and well-poised mind would soon shake off; and that very few evils are insupportable if viewed in a just

light and met in a proper spirit: the very disposition, therefore, to regard any of the ordinary evils of life as utterly unendurable implies either a perversion of ideas as to the fact, or a very enfeebled condition of the moral powers, either of which is sufficient to constitute melancholy. When to these considerations we add, what is truly stated by our author, that in a great majority of cases a careful enquiry into the previous conduct of the suicide would afford strong indications of insanity, we think we are justified in assuming the general position that suicide is the result of madness. We entirely assent also to the opinion of Mr. Winslow, expressed in another chapter, that in the few cases which may be doubtful, the unhappy individual should have the benefit of the doubt, and that the verdict *felo de se* should never be returned.

Chap. xii. treats of "suicide in connexion with medical jurisprudence." It contains most of the information to be found in the books on legal medicine; the subject of suicide by poisoning is, however, scarcely alluded to.

Chap. xiii. is on the "statistics of suicide." It contains much information and numerous tables, derived from the reports of the Statistical Society of London, the work of M. Guerry on the Moral Statistics of France, the paper by M. Prevost in the Bibliothèque Universelle, on Suicide in the Canton of Geneva, and other sources. These documents being well known we need not here attempt any condensation of their contents, which is the less needful with respect to the memoir of M. Prevost, inasmuch as an analysis of it has been given in the second volume of this journal, from which Mr. Winslow has copied his statement* verbatim without acknowledgment. He does, however, express in his preface a general obligation to certain books, and to this journal among the rest.

The two remaining chapters, one treating of the appearances presented after death in the bodies of suicides, and the other containing a collection of singular cases, do not call for any special notice.

Considered as a literary production Mr. Winslow's book is not of an elevated order. The style is diffuse and often incorrect. Many passages are feeble both in thought and expression, and some are altogether devoid of meaning. Thus at p. 64 the author talks of a young gentleman "of high natural and acquired attainments." In these few words we have a *bull* and a pleonasm: how can an *attainment* be *natural?* and if not, how can it be otherwise than *acquired?* At p. 334, he says, "the end of punishment is to prevent the criminal from doing further injury, as well as to *induce* others *from* committing similar offences." And so of many other passages. The general reader will, however, we dare say, pronounce a more favorable judgment on the Anatomy of Suicide; which is certainly more deserving of perusal than Mr. Winslow's former work, entitled Physic and Physicians, the blunders of which were inconceivable.

* Vide British and Foreign Med. Rev., vol. II., p. 277.

## Art. X.

*Die Organischen Knochen-Krankheiten.* Ein Lehrbuch von Dr. A. L. Richter.—*Berlin,* 1839. 8vo, pp. 208.

*The Organic Diseases of the Bones.* A Manual by Dr. A. L. Richter.—*Berlin,* 1839.

The volume whose title is prefixed to the present article is the production of an esteemed and experienced surgeon of Germany, and constitutes a very fair summary or compendium of the present state of our knowledge in the department of surgery to which it relates. The whole subject of organic affections of the osseous system is examined and discussed in a candid and judicious strain; and although the author obtains his materials chiefly from the labours of others, he is yet manifestly not to be regarded as a mere compiler. We do not, it is true, rise from the perusal of the work with any conviction that the author has himself added much to what was before ascertained, with respect to the class of diseases therein discussed. We do not, however, think that on this account its utility is much diminished. It is highly meritorious to have industriously sought out the published results of others, and to have candidly examined the amount of their value and practical importance. In this view of the case, we regard the construction, in a well-arranged and readable style, of a manual like the present, embracing all that is of moment upon any given subject, as a decidedly *useful* undertaking. On this account then, but more particularly as a large class of practitioners, most of those indeed unconnected with large hospitals, obtain from their own observation but little acquaintance with diseases of the bones, and this little only from the treatment of isolated cases, we think that a few of our pages will not be ill employed in furnishing a brief analysis of some of the more interesting matter contained in the volume before us.

The principle upon which our author rests, and from which he never swerves throughout the work, consists in the recognition of a pathological analogy between diseases of the bones and those of the other textures. That our knowledge of the former did not for a long time, maintain equal pace with that of the latter was owing, he states, to the circumstance of its being the custom to regard bone rather as a mere aggregation of lifeless matter, than as living structure, participating in the general properties of the organization. In this state of things, any parallelism between disease occurring in bone and that developing itself elsewhere could hardly be attempted. The older writers would include the most dissimilar pathological conditions, in the employment of the same terms; and the expressions "caries," "exostosis," and "spina ventosa," were used in the most vague and arbitrary manner; a proceeding, of course, which gave rise to a treatment equally irrational and empirical. Petit, whose work appeared in 1705, was the first to institute a comparison between disease occurring in bone and that taking place in the soft tissues; and, although he made no new classification of the forms of disease, a foundation was yet laid for a future scientific advance in this department. The highly interesting researches of Haller, Duhamel, Detlef, Troja, Köler, and others, on the physiology of bone

could not but exert a favorable influence upon the labours of those engaged in the study of its diseases; although a long time elapsed before a very general application of such researches was made to the consideration of morbid alterations. The appearance of Boyer's work, at the commencement of the present century, marked another stage in the progress of knowledge in this branch of surgery; and it for some time remained the principal source whence other writers drew their materials. Within the last twenty years, however, a much more comprehensive acquaintance with the subject has been obtained, chiefly through the exertions of Scarpa, Béclard, Müller, Cruveilhier, Dupuytren, and others; and at the present day it is very generally allowed that the osseous structure may undergo just the same morbid changes as the rest of the tissues, and that any peculiarities seeming to exist therein are to be regarded not as something without parallel in the soft parts, but as modifications dependent upon the low measure of vitality present in bone. This, as we have before stated, is the fundamental principle with which Dr. Richter sets out, and the one which he steadily maintains in the progress of the work.

The whole matter is brought before the reader in twenty-one brief chapters, the pathological classification being for the most part such as we approve. In each division, the morbid alteration examined is regarded under five heads: the general characters of a disease being first given, then the diagnosis, next its etiology, then the prognosis, and lastly the treatment. The whole is then wound up by a copious bibliographical reference. Our author, after introducing the subject generally, enters, in the first chapter, into the discussion of inflammation of the bony structure, following, in this respect, the common example of pathological writers, in awarding the first place to this important lesion of the organization.

*Inflammation* of the osseous structure most commonly originates in the medullary and spongy texture, owing to its higher vitality, gradually involving, however, the denser lamellated tissue. The rise and progress of an attack is perfectly analogous to what obtains in the corresponding affection of other symptoms. Accordingly, inflammation of bone admits of the customary division into acute and chronic. In its general consequences, the latter is the more serious form, owing to its being nearly always more or less associated with some extensive constitutional derangement. The bony tissue in children and young persons, possessing a richer endowment of nutrient blood-vessels than what occurs in more advanced life, is thereby rendered at this period more liable to the affection. The symptoms are such as ordinarily characterize inflammation in other parts, modified only by the lower vitality of the involved structure. It is to be remarked, however, that after an attack has fairly established itself, the pain is at times to be recognized rather in the neighbouring joint than in the immediate seat of the evil. The causes of the affection may exist in the habit of body, or they may be formed by some lesion from without. Syphilis, scrofula, gout, or rheumatism being present in the system, predispose greatly to an attack. Owing to the extent of the constitutional derangement in many cases, and to the difficulty of acting by treatment immediately upon the affected structure, the prognosis is often unfavorable. A successful issue is to be sought

entirely from the employment of measures rationally calculated on the one hand to subdue local excitement, and on the other to improve the general health. All vaunted specifics, in vogue with our fathers, must be cautiously avoided. In a short chapter upon Periostitis, we are told that it seldom or never presents an acute character, except when the causes have been either mechanical violence, some metastasis, or bad treatment of the chronic form.

Suppuration may arise in the osseous structure as a consequence of inflammation, and generally under the same conditions that mark its development in other parts. It is often confounded, but erroneously, with caries, or ulceration of the bony tissue; the processes, however, are just as distinct in the present instance as elsewhere, the one constituting essentially a formative effort, and the other being in its nature destructive. As in the soft parts, the two processes may coexist or may run the one into the other, but there is no identity. Abscesses in bone, when detected during life, have nearly always been found in the tibia, near the extremities. The signs of an invasion of this sort are, for a long time, somewhat obscure; swelling of the affected part comes slowly on, but little or no pain upon pressure is experienced until after some time has elapsed. The neighbouring joint, in the cases hitherto observed, has not participated in the morbid condition. Purulent matter in these instances often forms also beneath the periosteum, owing to its becoming involved in the attack. Sometimes there has been noticed after death a considerable deposit of bony matter, easily separated from the subjacent shaft, from the intervention of the periosteum between it and the adventitious formation. The locality of the purulent accumulation is primarily in the centre of the bone. It is an affection which authors have sometimes confounded with exostosis. It is also one of those to which the stupid and unmeaning term "spina ventosa" has been applied. Dr. Richter, in his account of the treatment of this morbid state, seems to expect little good from anything but amputation or the use of the trepan; and even where the case is encountered in its early stage, he does not think that much advantage is likely to be obtained from any attempt to subdue the affection by a milder proceeding; an opinion that may be true, but nevertheless, judging from analogy, one we think that should not be regarded as decisive of the point. As unquestionably a chronic inflammation or something analogous most commonly, if not invariably, precedes the formation of matter, we should always be disposed, in a suspected case of this kind, to attempt to subdue such inflammation by the means that are often found efficacious in corresponding affections of the other tissues; and this, in the expectation that, afterwards, the absorbents might remove any slight purulent deposit.

*Caries* is defined to be a process in bone analogous to ulceration in the soft parts, a definition, we are informed, only established in modern times. It has been stated, however, that such a comparison is as old as Galen; and certainly, we ourselves remember to have met with almost the very words of the definition in Duverney's treatise, published in the middle of the last century. It is an affection that occurs principally in the spongy structure of bone, although there is no portion of the osseous system which possesses an immunity. In all its character and relations, the analogy between this disease and ulceration in other tissues is maintained. It

may form the primary affection, or it may constitute the issue of some other. The same complications and constitutional modifications that affect the presence of ulceration elsewhere obtain also in the present instance. Hence, we have phagedænic and fungous caries, syphilitic and scrofulous, and so on. The causes of ulceration of bone are also of a character generally like those which excite a similar affection in the soft parts. Any local injury capable of producing inflammation of bone may give rise to caries; or it may depend chiefly upon some great constitutional disorder, especially the state induced by the abuse of mercury in syphilis. When nature brings about a cure of this lesion, it is either by the occurrence of a healthful suppuration, or the complete death of the carious portion, which is afterwards got rid of by exfoliation. In attempting a cure by the aid of art, we must seek then to imitate nature in her proceeding, so far as is practicable. Dr. Richter, after recommending strict attention to the general health, seems, in our opinion, when dealing with the local affection, to attach too much value to topical stimulants and to the actual cautery as a means of converting the ulcerated into dead bone. For many reasons, which our space will not allow us to adduce, we much prefer the more general practice recommended by British surgeons, especially by Mr. Syme, the removal of the carious portions of bone by the knife, in those cases where any violent proceeding of the kind seems indicated. Much interesting matter is found in the sections treating of caries in the individual bones, but our limits do not allow us to go into its examination.

*Necrosis* of bone corresponds to gangrene of the soft parts, just as caries does to ulceration. It forms the lesion to which the term "caries sicca" was formerly applied in contradistinction to "caries humida," the proper caries. The great comparative frequency of gangrene in the osseous system obviously results from its possessing a much lower vital activity than that which is found in the other systems; and it is in consequence of this being more especially the case with the shafts of the long bones that they so frequently become the subject of extensive or even complete necrosis. In the event of destruction of any part of the bony tissue, the reproductive energy of all the surrounding parts becomes exceedingly active; and this furnishes the reason why in many forms of organic disease the supervention of actual necrosis becomes a beneficial occurrence; whilst, on the other hand, the development of caries near the seat of necrosis is a most unfavorable circumstance. The discussion of this division of the subject occupies a considerable space in the volume before us, and the death of the part, its exfoliation, and the regeneration (as forming the three successive stages of cases of this kind) are each in succession examined and discussed. As the precise process whereby the regeneration of the entire shaft of one of the cylindrical bones occasionally occurs has been differently explained by pathologists, we will give the author's account of the same in his own words:

"Total necrosis is followed by a productive inflammation in all the tissues surrounding the affected bone, including the undestroyed periosteum. A plastic gelatinous effusion takes place from these, and at the bony extremities that have escaped necrosis; within the effused mass vessels are developed; and hereupon the various stages of ossification are successively observed; for, after it

has become harder and more cartilaginous, it gains ever more in density and firmness through the deposit of bony earth which occurs at innumerable points, and this deposit is often very considerable, even while the sequestrum remains in the limb. At the inner mouth of the fistulous communications, no deposit of bony matter takes place, and an opening remains which allows access to the sequestrum, and exhibits a *cloaca* which only later undergoes obliteration, to which the contraction of the newly-formed cylinder, after the removal of the dead portion, certainly contributes very much. It is difficult to determine whether or not such newly-formed bone be again furnished with medullary tissue." (p. 92.)

*Mollities ossium* results from a diminished proportion of calcareous matter in the bones, arising for the most part in childhood and in old age. Taking place in the early period of life, it is of the nature of an arrest, so to speak, of the proper development of the bony texture; and its occurrence in old age constitutes a kind of retrogression in the organization. In the former case, osseous matter is insufficiently deposited in the *matrices* of the structure; in the latter, it becomes removed by interstitial absorption. But little is known of the determining causes of this affection, whether it presents itself as rickets in childhood, or occur in more advanced life. A damp climate is commonly regarded as conducive to the disease. Dr. Richter states that on this account it is unusually prevalent in our own country, and that it has obtained in consequence the designation of the "English disease," a national distinction of which we were previously aware, but of the justice of which we very much doubt. Associated with this affection is always observed extensive disorder in all the functions concerned in assimilation. A deposit of lime is often noticed in the urine. The vertebral column is generally the first to give way so as to exhibit deformity. The rickets of childhood seldom cripples to the same extent as the corresponding affection in more advanced life. Nearly all the cases recorded of very extraordinary disfigurement have been of adults. The prognosis in children is often very favorable. An improvement in the digestive organs is usually followed by a hardening of the bones; the induced deformity, however, seldom evincing much improvement until the attainment of puberty. Measures calculated to improve the functions of organic life generally are alone conducive to a cure. The utility of specifics in this affection is quite exploded.

In this brief account, we have hitherto followed our author somewhat in detail through the better half of his work, as the topics handled have possessed more practical interest than those of which he treats in the sequel. In our remaining notice, we can do little more than allude to the particular lesions, in the examination of which Dr. Richter forms distinct chapters. These consist chiefly of certain pathological changes of structure, of whose causation and successful treatment but little that is satisfactory is understood. The review of them, however, is interesting as still further exhibiting the entire analogy, especially in disease, subsisting between the osseous and the other systems.

*Osteosclerosis*, or hardening of bone, is exactly the opposite of the last considered affection, and is owing to a predominance of the earthy matter. It may be regarded as the analogue of induration in other textures. *Osteoporosis*, or porosity of bone, is formed by a loosening of the tissue, and results not from any absorption of the mass, but purely

from the presence of an expansive activity. *Osteopsathyrosis* is a term employed to designate fragility or brittleness of bone, a state induced by defective nutrition and often prevalent in old age, though no period of life is exempt from its occurrence. A sort of atrophy of the osseous texture marked by a diminution of volume, sometimes takes place and is technically designated *Osteonabrosis*. Of these various changes in the structure but little explanation is to be afforded; they form peculiar lesions of the nutritive function, like in many respects to what occurs in the soft parts, and as little, or indeed less, amenable to treatment.

*Morbid growths* upon the surface and within the structure of bone are not of unfrequent occurrence. These vary in their characters according to the state of the constitution with which they are associated, and upon which they often depend. Thus syphilis and gout often lay the foundation of such affections. They may present themselves with characters of a mild or of a malignant nature, just as obtains with such diseases in the soft parts. Simple tumours of bony matter sometimes arise on the surface, called exostoses; and sometimes special formations, such as medullary fungus, the fibrous or the fleshy tumour maintaining in all respects a very close resemblance, in their apparent origin and progress, to such affections in the rest of the system. The tuberculous diathesis, as also the disposition to melanosis, allows no immunity to the bony structure; and even morbid conditions, such as dropsy and the formation of cysts, prevail here at times as elsewhere. Indeed, the further our knowledge advances in the morbid anatomy of the osseous structure, the more complete seems the unity in the essential characters of all the components of the organization; since it is now pretty well made out that even those parts formerly regarded as little more than lifeless supports to the true organism themselves participate in all the general properties of the entire system.

We cannot dismiss the subject without again recording our approbation of the manner in which Dr. Richter has executed his performance. Throughout the volume the style is free and plain, the arrangement is judicious, and, as it seems to us, a due relation is maintained between the amount of facts stated and the extent of their explanations, a rare quality with writers, especially in dealing with subjects of practical medicine. Indeed, we are much disposed to regard this manual as the best of its kind, as it furnishes systematically, in a very brief compass, almost everything of interest or value that has been ascertained in the department. We think that any one having a taste for such pursuits, would render a service to our English medical literature by translating the work into our own language. It would be found, we are sure, a most useful hand-book for students; and we are equally certain, that there are few practitioners who by its perusal would not either add to their prior stock of information, or find that they had profitably renewed or refreshed that which they had previously obtained.

## Art. XI.

*The Sanative Influence of Climate; with an account of the best places of resort for Invalids in England, the South of Europe, &c.* By Sir James Clark, Bart., M.D. F.R.S., Physician in ordinary to the Queen and to the Prince Albert. Third Edition.—*London,* 1841. *post* 8vo, pp. 377.

For the new edition before us this work, we are informed in the advertisement, has been almost entirely rewritten, and whatever appeared not directly to the purpose omitted, in order to make room for the consideration of several new subjects and the introduction of notices of some places not previously described. The work as it now stands thus presents the quality—rare in a new edition—of being actually less in size and price though even more abounding in information than its predecessors.

The peculiar excellence which at once stamped the character of this work when it first appeared twelve years ago was the philosophical and discriminating manner in which climate was viewed as a remedy. That climate exerts a powerful influence over health and disease is a fact which had been long known and generally recognized; but it must be confessed that its application as a remedy in disease had too often been made without discrimination both as regards the climate selected and the stage at which the disease was sought to be benefited by the change. Change of climate had too often been resorted to as a last resource, or it had been misapplied in cases wherein it would otherwise have been capable of yielding essential service. Patients who really might have derived much benefit from change of climate have too often been sent abroad without proper directions regarding the situation most suited to their complaints, and altogether uninstructed respecting various circumstances, a due attention to which is necessary to give full effect to the best selected climate.

In illustration of the indiscriminate manner in which the choice of a climate used to be made we may instance Montpelier. This place is so associated in the minds of the public of this country with everything that is salutiferous in climate, that the very name is frequently employed as an epithet to any place considered more than usually healthy. In accordance with this notion, Montpelier used to be the place to which consumptive invalids wishing to try the effects of change of climate were almost as a matter of course first directed. The celebrity of the Medical School of Montpelier, says Sir James Clark, had probably a considerable share in giving rise to the character which this place obtained for the benignity of its climate,—*olim Cous nunc Monspeliensis.* But whatever may have been the merits of its medical school, it will be easy to show that the climate little deserved the reputation which it long enjoyed as a residence for the consumptive.

Consumption is the disease in which it has been generally supposed that the beneficial effects of climate are chiefly evinced; but far from this being a correct view of the matter, our author remarks that were the character of climate as a remedy to be estimated by its effects on consumption, it would be justly valued at a very low rate. In dyspepsia, and disorders of the digestive organs generally, and in the nervous affec-

tions and distressing mental feelings which so often accompany these; in asthma, in bronchial diseases, in scrofula, and in rheumatism, the beneficial effects of climate are far more strongly evinced than they are in consumption. In cases also of general delicacy in childhood and youth, in the climacteric disease, in convalescence, and in disordered health from various causes, as from a residence in hot climates, change of climate is a valuable remedial agent.

The statistical reports on the sickness, mortality, and invaliding among the British troops in almost all quarters of the globe show that there is no immunity from consumption in any climate, and on this objections to the value of climate as a remedy have been grounded; but Sir James Clark justly observes that when an invalid is sent abroad for his health, he goes, by the direction of his physician, to the climate best suited to his particular case, and at the most favorable season of the year. Moreover, he goes prepared to avail himself of all the advantages of his new situation, and to avoid as far as possible its disadvantages. Hence the influence of any climate upon such an invalid must be estimated very differently from the influence of the same climate on the permanent inhabitants, or on our troops who are resident in it at all seasons, and are exposed to all its prejudicial influences for years. "The great lesson," continues our author, "which the army medical reports teach in regard to consumption is this, that as it is a prevalent and fatal disease in all climates and among all nations, our attention should be chiefly directed, not to a state of disease which is incurable by climate or any other means, but to the prevention and cure of the disordered state of health which constitutes the real cause of consumption. When change of climate is judiciously employed as a remedy for this constitutional disorder which precedes consumption, there will no longer remain any doubts of its beneficial influence, and what a single change of climate does not effect, a succession of changes will often be found to accomplish."

As to the mode of operation of change of climate, our author takes particular pains to show that it is by no means possessed of any specific quality by virtue of which it directly cures the disease. "Let it not be imagined," he says, "that change of climate, however powerful as a remedy, can be considered as at all peculiar in its mode of action, or as justifying on the part either of the physician or patient the neglect of those precautions which are requisite to ensure the proper action of other remedies." Among other collateral advantages attending a residence in a mild climate may be mentioned the very important one, that the patient is permitted to enjoy frequent exercise in the open air, an advantage for which there is no substitute.

In the present edition, the order of the two parts into which the work is divided has been reversed, so that the account of the principal diseases benefited by a mild climate now stands first, and after it comes the consideration of the general physical characters and sanative influence of the climates of the various places which have been recommended for the resort of invalids.

The disorders of the digestive organs are classed under three heads, viz. inflammatory or gastritic dyspepsia; irritable or nervous dyspepsia; and atonic dyspepsia, or that form of the complaint which depends chiefly on a loss of tone. This arrangement is made in reference to the

kind of climate which the different forms of the disease require. The patient with gastritic dyspepsia should not, for example, go to Nice nor the south-east of France. In cases of this kind, the south-west of France or Devonshire is preferable, and Rome and Pisa are the best places in Italy. On the other hand, in atonic dyspepsia, in which languor and sluggishness of the system as well as of the digestive organs prevail with lowness of spirits and hypochondriasis, Nice is to be preferred to all the other places mentioned; and Naples will generally agree better than Rome or Pisa; while the south-west of France and Devonshire, and all similar climates, would be injurious. In the nervous form of dyspepsia a climate of a medium character is the best, and the choice should be regulated according as there is a disposition to the gastritic or the atonic form.

The selection of a residence even in the same place, our author further says, is not a matter of indifference to very sensitive invalids. One will feel himself better in an elevated situation, another in a lower and more sheltered one. But dyspeptic patients, who pass the winter in Italy, need not in general be limited to one place. Although the climate most suited to the particular character of their complaint should be selected as their head-quarters, they may visit, during the season, the principal cities in the south of Italy; and if this is done with judgment, the successive changes may prove beneficial to their health. Generally speaking, Rome will be the best residence in Italy in gastritic dyspepsia, especially during the spring; Nice the best climate in the purer cases of atonic dyspepsia.

*Pulmonary consumption.* Under this head, Sir James Clark dwells particularly on the constitutional disorder which precedes, and which he considers the essential predisposing cause of the formation of tubercles in the lungs. This disorder, our readers are aware, he calls TUBERCULOUS CACHEXY, and he considers it in general the only curable stage of consumption. "The attention," he says, "is still too exclusively directed to the pulmonary disease, and too little notice taken of the constitutional affection; although it is only on the proper treatment of the latter that we can rest our hopes of success, all efforts to cure the former having been as yet comparatively of little avail."

*Choice of climate for tuberculous cachexy.* With regard to the climates of the south of France and of Italy, Sir James Clark observes, that for consumptive invalids in whom there exists much sensibility to harsh and keen winds, and more especially, if immediate vicinity to the sea coast is known to disagree, Rome or Pisa is the best situation for a winter residence. When, on the contrary, the patient labours under a languid, feeble circulation, with a relaxed habit, and a disposition to congestion or hemorrhage rather than to inflammation, and more especially when the sea-air is known by experience to agree, Nice deserves the preference. In cases complicated with gastritic dyspepsia, however, Nice is an improper residence, its climate being decidedly inimical to such a state. The climate of Hyères may be considered as nearly similar to that of Nice. So great is the influence of such a morbid condition of stomach in modifying all other diseases, that Sir James Clark considers it alone sufficient to claim for itself the chief consideration in deciding upon the particular climate.

The climate which of all others the author considers best suited to consumptive patients generally is that of Madeira. There the patient may reside during the whole year, and thus avoid the inconveniences and even risks attending a long journey, to which consumptive patients who pass the winter in Italy are necessarily exposed when leaving it on the approach of summer. The summer climate of the whole shores and islands of the Mediterranean is unsuited to consumptive invalids.

The winter climates in England most favorable to consumptive patients are those of Torquay, Undercliff, Penzance, Clifton, and Hastings. The choice among these places will depend upon the nature of the case, and especially upon the condition of the digestive organs. For persons of an inflammatory constitution, with a disposition to gastritic dyspepsia, Torquay will form the best residence, while it will as decidedly disagree with persons of a very relaxed habit, and subject to copious secretions from the mucous membranes or to atonic dyspepsia. Such patients will bear the climate of Torquay for a very short time only. What has been said of Torquay applies with equal force to Penzance and the other parts of the Land's End. Undercliff, Hastings, and Clifton will form preferable residences in the constitutions referred to.

With respect to the length of time requisite for a consumptive invalid to pass in a mild climate, in order to overcome the disposition to the disease, our author observes that no general rule can be given. When the measure is had recourse to for the removal of the disordered health which precedes tuberculous cachexy, a single winter will be of great benefit and possibly all that may be necessary. When tuberculous cachexy is established, and still more when there is reason to suspect the presence of tubercles in the lungs, several years may be requisite, and in some cases it may be necessary to reside permanently in a mild climate.

In place of sending consumptive patients to pass the winter in a foreign climate, it has been proposed to keep them at home in rooms maintained at a regulated temperature. "With the advocates of such a measure," our author justly remarks, "the state of the lungs appears to be the only consideration; but without improving the general health by exercise in the open air, all our measures directed to the local disease will be of little avail; the removal of the constitutional disorder can alone afford the patient a hope of recovery." Where circumstances, however, preclude the possibility of changing the climate, and the patient is quite unfit to bear exposure to the external air in this country, confinement to apartments properly and equally heated is the best measure which can be adopted. The invalid who has passed some months in an artificial climate established within doors, should, when he ventures out for the first time, do so with proper precautions. The respirator will prove a valuable protection to him in the first instance and for some time afterwards, on any sudden or considerable fall of temperature.

*Bronchial affections.* In no class of complaints is the beneficial action of change of air and climate more speedily manifested than in irritation of the organs of respiration. In the slighter bronchial affections, a change to a very short distance only has often a remarkable effect; coughs ceasing in the course of a few days, which had resisted medical treatment for many weeks. But in protracted cases the disease assumes

a more fixed character, and requires a thorough change of climate to produce much effect on it. When the patient is at the same time suffering from dyspepsia, a very frequent complication in bronchial diseases, this should be remedied, as far as possible, before he leaves his own country, otherwise the change, so far from proving beneficial, may be injurious to him. The skin will also require particular attention, as it is seldom in a healthy condition in persons who have long laboured under bronchial irritation.

With respect to the best winter residence, Sir James Clark found Rome agree more decidedly with such patients than any other place on the continent; and he repeatedly had occasion to compare its influence with that of other climates upon the same patients. The climate of Rome is not, however, so beneficial, when the disease is accompanied with copious expectoration and a relaxed state of the system, as that of Nice; but in the dry tracheal and bronchial affections, the climate of Rome, and also that of Pisa, is preferable. Rome has several obvious advantages over the other residences on the continent for patients labouring under bronchial irritation. It is little liable to high winds, the air is soft, and the surrounding country well adapted for riding—the best exercise for such patients. With the exception of cases in which there is a copious expectoration and a relaxed state of the system, the climate of Madeira proves very beneficial and is preferable, our author thinks, to any part of the continent. In this country Torquay is the best climate in the dry bronchial irritations; for those with copious expectoration, a relaxed condition of the system, or an atonic state of the digestive organs, Undercliff and Clifton afford better climates.

*Asthma.* In no disease, perhaps, is the effect of change of climate so conspicuous as in asthma. Taking the disease generally, it may be stated that a removal to a warmer climate is highly beneficial; but the degree of relief will depend greatly upon the climate being suited to the particular case. We must not, therefore, says our author, prescribe for a name, but take into account the whole pathological condition of the patient, in order that we may be enabled to fix upon the climate best suited to his case. The following forms of asthma our author points out as requiring attention in prescribing change of air or climate: pure nervous asthma; humid asthma; cardiac asthma.

*Rheumatism.* A residence for some time in a mild climate proves of the greatest benefit in chronic rheumatism. Nice and Rome are the best climates on the continent, according to our author's experience.

*Delicacy of constitution, &c.* In cases of delicacy in childhood, a residence for some time in the south of Europe has appeared to our author particularly useful. Rome and Nice he found the best winter residences for children: the former when the digestive organs were in an irritable state, the latter when there was a torpid, languid state of the constitution. About the age of puberty it frequently happens that the health declines, and sometimes tuberculous disease shows itself for the first time. In such cases a temporary residence in a mild climate is of great service. As age advances also, and the system begins to feel the weight of years, a milder climate proves highly beneficial in arresting premature decay. Persons just returned to Europe, and whose constitutions have suffered by a long residence in a tropical climate, will find great advantage in spending one

or more winters in the south of Europe, before finally settling in this country. There are various other states of impaired health in which change of air and climate prove very beneficial. Indeed it would be difficult, says our author, to name the chronic complaint or the disordered state of health which would not admit of being ameliorated by the judicious adoption of such a measure. In convalescence from fevers and other acute diseases, no remedy is so effectual in promoting the recovery of the invalid as a well-timed change of air. In short, the author continues, change of air is to the young and delicate the best and often the only admissible tonic.

To the observations on the dietetical and therapeutical measures necessary to aid the operation of climate we would particularly call attention. They are sketches merely, but they bear the mark of the master's hand. Our limited space prevents us from doing more than noticing those on the treatment of dyspepsia in children. This is a disease which so frequently undermines the future health, that it is of the utmost importance that correct views should be entertained respecting it. Change of climate, though when properly applied of the utmost advantage to the child, should not be adopted till the morbid state of the digestive organs is in some degree corrected; and wherever a child goes, this, says our author, should receive constant attention. For although the general health may be much improved by change of air, or climate, the improvement will not be permanent unless the congestion and irritation of the digestive organs, in which the disorder had its origin, and on which its continuance depends, are removed. The most important means of correcting this state is the regulation of the diet, which of course must be varied according to the age of the child and the degree of congestion and irritation which exists. Generally speaking, the diet should be of the blandest quality, more especially in children of an excitable constitution. When the tongue is red, the skin hot at night, with thirst, milk and farinaceous food should constitute almost the sole nourishment. As the irritation abates, a little mild animal food every second day is allowable. For children of a more torpid character of constitution, who have little disposition to fever, when the tongue is loaded and all the functions languid, a more exciting diet may be permitted. The warm bath and friction will be beneficial in all cases, more especially in the languid constitutions just alluded to. The great objects in the treatment should be, to regulate the diet according to the sensibility and power of the digestive organs, to promote an active state of the circulation on the surface and extremities, with a view to remove the congestion and irritation of the internal organs, and to impart tone to the system. This irritation of the digestive organs influences every function of the body, and without its removal, all remedies directed to the improvement of the general health will produce only a partial and evanescent effect.

In alluding to the medical treatment of such cases, Sir James Clark animadverts on the inconsiderate routine practice generally followed. Active mercurial purgatives, he says, an exciting diet of animal food, not unfrequently repeated several times a day, with the addition of porter or wine, or both, and this followed by steel and other tonics, constitute generally, in this country, the treatment of scrofulous children. Such a mode of treatment, he continues, is at total variance with the gastro-

duodenal irritation and hepatic congestion, which are present in a greater or less degree in all cases of scrofula. Besides this, a complete want of success attends it in practice, whilst striking benefit is derived from an opposite plan of treatment.

Part Second, *on Climates*, is appropriately introduced with observations on ventilation and unhealthy residences, and directions for invalids making a change of climate. The climates of England, of France, of Nice, of Italy, of Malta, of the Atlantic, are successively treated of; and in an appendix the climates of the Southern Hemisphere.

The mild region of England admits of being divided into four districts or groups of climate; that of the south coast, comprehending the tract of coast between Hastings and Portland Island; the south-west coast, from the latter point to Cornwall; the district of the Land's End; the western group, comprehending the places along the borders of the Bristol channel, and estuary of the Severn. Each of these regions has some peculiar characteristic features, both as regards physical and medical qualities.

*London.* A notice of the climate of London is first given as a point of comparison, and for other reasons. There are some peculiarities in the climate of London owing chiefly to artificial circumstances. The mean annual temperature of London is about 1½° above that of the environs, but this difference of temperature is very unequally distributed throughout the year and throughout the day. The excess of the city temperature is greater in winter than in summer, and belongs in *strictness* to the nights. The heat of the day in London indeed according to Howard, falls, on a mean of years, about a third of a degree short of that in the open plain. The temperature of London, also, has not such an extensive range as that of the environs, and it acquires and loses its heat more slowly. The benefit so often experienced by delicate invalids on coming from the country to London in the winter and spring, is no doubt, our author remarks, owing to the qualities of the climate above enumerated. It is during the night that the climate possesses the greatest advantages for the sensitive invalid; in addition to its warmth and dryness the atmosphere is then in its purest state.

*South coast.* Were we to rest contented with the result of the mean annual temperature, we should find very little difference between that of the south coast and of London. But when we descend to particulars, we observe that there does exist a considerable difference in their temperature, arising from the mode of its distribution. It is because the higher degree of the temperature of London and the interior of the island in summer compensates for the lower degree in the winter, that the climate of these places appears to equal that of the south coast. The mean temperature of the latter, however, during the winter months, is from one to two degrees above that of London. In steadiness of climate, as deduced from the variation of temperature between successive days, the south coast does not appear to possess any very remarkable superiority over London.

More rain falls on the south coast than at London, the ratio being, as nearly as could be ascertained, as 30 to 25; but the quantity varies considerably at different parts. Of the places on this coast frequented by in-

valids, Hastings, Brighton, and Undercliff may be considered as having respectively peculiar climates. That of Hastings is mild and soft, whilst that of Brighton is dry, elastic, and bracing. Undercliff has somewhat of an intermediate character: it is evidently one of the warmest climates in Great Britain, and most eligible for a large class of invalids.

*South-western coast.* The south coast of Devon has a winter temperature nearly two degrees higher than that of the coast of Sussex and Hampshire, and from three to four higher than that of London. The principal places adapted for invalids on the south-west coast are, proceeding from west to east, Salcombe, Torquay, Dawlish, Exmouth, Salterton, and Sidmouth. The general character of the climate of the south-west coast is mild, soft, and humid. Torquay is that which possesses the advantages of the south-western climate in the highest degree. Torquay is drier than the other places mentioned, and almost entirely free from fogs.

The influence of the south-western climate on disease may be anticipated, our author observes, in a great degree from its physical characters, which are mild but rather humid, consequently soothing, but rather relaxing. In one class of complaints, it is therefore calculated to prove decidedly beneficial; in another, of an opposite character, equally injurious. Pulmonary diseases are those in which the climate has been considered especially beneficial. But for the reasons already pointed out, our author remarks, the benefit to be derived from this climate will depend upon its being applied to the proper cases. In chronic inflammatory affections of the throat, trachea, and bronchi, attended with a dry cough, or with little expectoration, decided benefit may be expected. But when there exists in such cases a relaxed state of the mucous surface, with copious expectoration, especially when occurring in a languid relaxed constitution, the disease is more likely to be aggravated than diminished by a residence on the south-west coast. From this statement will be understood the character of the more serious diseases of the chest, which are likely to be relieved by this climate.

In gastritic dyspepsia it is serviceable; likewise in dysmenorrhœa and the various nervous symptoms consequent upon it. On the other hand, this climate certainly exerts an unfavorable influence on atonic dyspepsia, in all nervous complaints arising from relaxation or want of tone of the nervous system; and on persons subject to menorrhagia and leucorrhœa; and in all diseases of the mucous membranes attended with relaxation and discharge.

What may be the real estimation in which the climate of Devonshire ought to be held in consumptive complaints, and what may be its absolute effect upon these, Sir James Clark has much difficulty in saying: but this much he ventures to advance, that as the invalid will be exposed to less rigorous cold and for a shorter season, will have more hours of fine weather, and consequently more exercise in the open air, he gives himself a better chance by passing the winter here than in the more northern parts of the island. To compare it also, in this respect, with the mild climates of the south of Europe is no easy task. In the south, the invalid has finer days, a drier air, and more constant weather; but the transitions of temperature, though less frequent, are more considerable.

In the night, invalids are often exposed to severer cold in the south of Europe than here.

The climate of the south coast of Cornwall in its general characters, as also in its influence on disease, resembles closely that of the south coast of Devon, and has also long been resorted to by pulmonary invalids. The temperature of Penzance is remarkably equable both throughout the year and throughout the day. The daily range of temperature at Penzance is little more than half that of the south of Europe, though it is more than that of Madeira. Though the mean temperature of the whole twenty-four hours at Penzance is considerably lower than that of the south of Europe, yet during the night through the winter its extreme minimum temperature is seldom so low. Thus the whole advantage of Penzance, it is remarked, as compared with the south of Europe appears to occur in the winter and during the night. The disadvantages which attach to the climate of the Land's End generally, in point of humidity and liability to violent and frequent gales of wind, are such as, in a great measure, to neutralize the superiority which it possesses over the other climates of England, in mildness and equability of temperature.

*West of England.* The vicinity of Bristol and Clifton appears to be the mildest and driest climate in the west of England, and consequently the best winter residence in that part of the country for invalids. Compared with the south and south-west coasts, the spring is the period of the year during which this climate appears to the greatest advantage. This season is warmer here than on the south coast, with the exception of Undercliff, whilst it is equal to the warmer parts of the south-west coast. When the climate of Clifton is compared more closely with that of Devonshire, it may be characterized as drier and more bracing than the latter, and as more exciting to most consumptive patients, and to those labouring under irritable affections of the bronchial membrane. For such cases the softer and more humid air of Devon will be found more soothing while for invalids whose constitutions have suffered from long-continued derangement of the digestive organs, or a congested state of the mucous membranes with copious secretion, and also for young scrofulous persons, and those of relaxed habits of body generally, Clifton will prove a preferable climate. The advantages of Clifton as a residence for the invalid are not limited to the winter: it and its neighbourhood afford also a very favorable summer climate.

*Island of Bute.* Of all places in Scotland the island of Bute possesses the best climate for invalids. The temperature there never falls low during winter, nor rises high in summer; so that its yearly range is comparatively limited. The climate of Bute may be characterized as mild and equable but rather humid. It resembles in character that of the south coast of England and France, and of the Channel Islands, though considerably less warm than any of them. As a winter residence for invalids, it holds out considerable advantages to the inhabitants of Scotland, but to that class of invalids only for whom a soft equable but rather humid atmosphere is indicated.

*Cove of Cork.* This appears to possess one of the mildest climates in Britain; being inferior in point of temperature to Penzance only during the winter months, and to the same place and Torquay only during the

spring. In point of dryness, Cove is rather low in the table of comparison. In its general characters of climate and the influence of this on disease, Cove corresponds with the south-west of England and other similar climates.

*The Channel Islands.* The climate of these islands has a close resemblance to that of the south-west coast of England, and especially to Penzance. There are the same equable temperature, the same soft humid atmosphere, and the same liability to high winds during the winter and cold north-east winds in the spring, which characterize the latter place. So close is the affinity of these climates, and so similar their influence on disease, that the remarks which have been made on the south-west of Devonshire and the Land's End, as residences for invalids, are perfectly applicable to the Channel Islands.

*South-west of France.* The southern provinces of France, as regards climate, admit of being classed under two divisions, namely, the south-eastern and the south-western. The climate of the latter resembles in its general qualities that of the south-west of England, being soft, relaxing, and rather humid. Generally speaking, the climate of the south-west of France will be found useful in chronic inflammatory affections of the mucous membranes accompanied with little secretion, as in chronic bronchitis not attended by much expectoration, or difficulty of breathing, and in similar morbid states of the larynx and trachea. It will be equally proper in dry scaly eruptions of the skin; in dysmenorrhœa; in certain kinds of headach, especially those induced or exasperated by sharp north-east winds; and in high morbid sensibility in general, when accompanied with that habit of body which the ancients called *strictum*. On the other hand, the same diseases occurring in relaxed habits, in which there is a disposition to copious secretion, will be aggravated by this climate.

*Pau.* The principal place in this district is Pau. There are several circumstances in the climate of Pau which render it a favorable residence for a certain class of invalids. The atmosphere is generally dry and the weather fine, and there are neither fogs nor piercing winds; the characteristic quality of the climate, however, is the comparative mildness of its spring, and exemption from cold winds—circumstances which render this place favorable in chronic affections of the larynx, trachea, and bronchi. In gastritic dyspepsia, Dr. Playfair has found it beneficial, and he has seen it useful in a few cases of asthma. With delicate children also, he found the climate agree well, especially when they removed to the mountains during the summer. Upon the whole, Pau appears to be the most desirable winter residence in the south-west of France for invalids labouring under chronic affections of the mucous membranes.

Invalids labouring under or subject to attacks of rheumatism should avoid Pau. In bronchial diseases also, when accompanied with much general relaxation of the system, and with copious expectoration and with dyspnœa, the climate will not in general prove beneficial; and Dr. Playfair considers it too changeable in consumptive diseases.

*South-east of France.* Various places in the south-east of France have been at different times recommended as affording a good winter climate for consumptive patients; but nothing can be more unaccountable, Sir

James Clark observes, than how such advice ever came to be given, as the experience of later years is in complete opposition to it, and the general and leading characters of the climate show that there never was the least reason to sanction it. The general character of this climate is dry, hot, and irritating. Its temperature throughout the year and the day is distributed with great irregularity, and the range is much wider than in our own climate. The temperature, however, remains more steady from day to day than our own; but its changes, though less frequent, are more sudden and extensive. Although decidedly improper for consumptive patients, and for those labouring under irritation of the mucous membranes of the stomach, larynx, or trachea, this climate, our author says, may prove useful to invalids of a different class. On persons of a torpid or relaxed habit of body, and of a gloomy, desponding cast of mind, with whom a moist relaxing atmosphere disagrees, the keen, bracing, dry air of Provence and its brilliant skies will often produce a beneficial effect. In some cases of chronic intermittent fevers, also, it proves very favorable. In the following places these distinctive characters of climate prevail more or less, but none can be considered as exempt from them, viz. Montpelier, Marseilles, Hyères. The latter of these places is the least exceptionable residence in Provence for the pulmonary invalid.

*Nice.* In the physical qualities of its climate Nice possesses some advantages over the neighbouring countries of Provence and Italy, inasmuch as it may be said to be free from the sirocco of the latter and protected from the mistral or cold northerly wind of the former.

In consumption, the disease with which the climate of Nice has been chiefly associated in the minds of medical men in this country, little benefit, our author fears, is to be expected from it. But in chronic bronchitis, with copious expectoration, which often simulates phthisis, very salutary effects are produced by a residence in this place. In gout and chronic rheumatism the climate is favorable. Its advantages are also great in scrofulous complaints. On children, the climate generally exerts a very favorable influence if attention be paid to their diet. In the large class of hypochondriacal and nervous symptoms which often originate in dyspeptic complaints, Nice is beneficial. The cases of dyspepsia most benefited are those accompanied with a torpid relaxed state of the system, with little epigastric tenderness, or any of those symptoms which denote an inflamed or very irritable state of the mucous membrane of the stomach: where the latter state prevails, Nice will decidedly disagree. In all cases where there is great relaxation and torpor of the constitution, the climate of Nice is extremely useful. In stating its general influence on the animal economy, Sir James Clark says that the climate of Nice is warm, exhilarating, and exciting, but upon the whole irritating, more especially during the spring, at least to highly sensitive constitutions.

Of the climates of Italy, those of Genoa and Florence have not much to recommend them.

*Pisa.* This place has long had the reputation of possessing one of the most favorable climates in Italy for consumptive patients. Its climate is genial, but rather oppressive and damp. It is softer than that of Nice, but not so warm; less soft but less oppressive than that of Rome. For invalids who are almost confined to the house, or whose power of taking

exercise is much limited, Pisa offers advantages over both Rome and Nice.

*Rome.* The climate of Rome is mild and soft, but rather relaxing and oppressive. In regard to its physical qualities, Sir James Clark considers the climate of Rome one of the best in Italy. One peculiarity of it deserving notice is the stillness of its atmosphere; high winds being comparatively of rare occurrence. This quality of calmness, our author remarks, is valuable in a winter climate for pulmonary diseases, and to invalids generally, as it admits of their taking exercise in the open air at a much lower temperature than they could otherwise do. To patients labouring under bronchial irritation, wind is peculiarly hurtful.

Among the diseases benefited by a residence at Rome is consumption. In the early stages of this affection Sir James Clark has generally found the climate favorable. In bronchial affections, he says, the climate is very generally beneficial, especially in cases where there prevails great irritability of the bronchial membrane, with much sensibility to harsh cold winds. In chronic bronchitis, more especially when of the dry irritable kind, or complicated with irritation of the digestive organs, a residence at Rome produced the best effects; and in cases of this kind Sir James Clark considers it the most favorable residence on the continent. Chronic rheumatism is generally much relieved.

With persons disposed to apoplexy, or who have already suffered from paralytic affections, and valetudinarians of a nervous melancholic temperament, or subject to mental despondency, the climate of Rome, our author says, does not agree; nor is it proper for persons disposed to hemorrhagic diseases, or those who have suffered from intermittent fever.

*Naples.* In its general character the climate of Naples resembles that of Nice more than any other. Of this place as a residence for invalids, it is remarked that consumptive patients should certainly not be sent there. The qualities of its climate sufficiently mark it as a very unsuitable residence for this class of invalids; to the list of its defects must be added that of its topographical position which affords no proper places for exercise without such exposure as would prove highly injurious to delicate invalids. For chronic rheumatism, the climate is certainly inferior to that of Nice and Rome. Naples is however well suited, Sir James Clark remarks, as a winter residence for those who are labouring under general debility and deranged health without any marked local disease, and who require mental amusement and recreation rather than a mild equable climate.

*Malta.* No place in the south of Europe, Dr. Liddell thinks, can compete with Malta for a mild and bracing air in November, December, and part of January; and during the other winter and spring months, he thinks it is equal to any of them.

*Madeira.* When we take into consideration, says our author, the mildness of the winter and the coolness of the summer, together with the remarkable equality of the temperature during the day and night, as well as throughout the year, we may safely conclude that the climate of Madeira is the finest in the northern hemisphere. There is no place on the continent of Europe with which Sir James Clark is acquainted, where the pulmonary invalid could reside with so much advantage during the

whole year as in Madeira. Although in his account of the climate of Madeira the author has confined himself to its influence on consumption, he thinks there can be no doubt of its being highly beneficial in several other diseases, more especially scrofula and bronchial affections. Were the accommodations for strangers at Teneriffe, and the means of communication between it and Madeira more frequent, many invalids, Sir James Clark thinks, might benefit greatly by passing the winter partly at Funchal and partly at Santa Cruz. Orotava is cooler but not so dry as Santa Cruz, and as a residence for invalids possesses many advantages over it.

*The Azores.* The Azores, situated in the centre of the northern Atlantic, nearly equidistant from the pole and the equator, and surrounded on all sides by a vast extent of ocean, present a climate purely oceanic, and afford one of the best examples of a mild, humid, equable climate to be met with in the northern hemisphere. It is slightly colder and more humid than Madeira, but probably even more equable. In diseases in which a soft soothing climate is indicated, that of the Azores will prove beneficial; thus in gastritic or inflammatory dyspepsia, and in bronchial irritation, accompanied with little secretion, it is indicated; on the other hand, in a relaxed state of the system, in those morbid conditions of the mucous membranes attended with copious discharges, and in an enfeebled state of the digestive organs, it will decidedly disagree.

Comparing Madeira, the Canaries, and the Azores, says our author, we find a gradual transition from the humid, soft, equable climate of the Azores, of which the mountains are covered to the very summit with evergreens, to that of arid, rocky Teneriffe, where the want of rain during the greater part of the year renders much of the island dry and sterile. Madeira presents an intermediate climate; less humid than the Azores, and less dry than Teneriffe during the winter, it has the advantage of a cooler summer than either.

*Bermuda.* The climate of Bermuda is variable and windy during the winter, and hot and oppressive in the summer. Compared with Madeira, which lies in the same parallel of latitude, the climate will be found much more unequal. The temperature of the two places during the winter may be much the same, but there is a wide difference in that of summer. The coolness of this season at Madeira forms a striking contrast with the oppressive heat of Bermuda. With so few advantages in point of climate, our author remarks, the Bermudas are not likely to become the resort of invalids from this country.

*The Bahama Islands* are not well calculated for the generality of invalids. The climate is not suited for consumptive patients on account of the rapid changes of temperature and the prevalence of winds often of a dry cold character. At the same time persons for whose cases a warm climate is indicated, Sir James Clark says, may pass the winter in the Bahamas safely; and residents in the West Indies might derive considerable benefit by a change to these islands for a few months during this season.

*West Indies.* The climate of the West Indies appears to be prejudicial in consumption, but it has been found to exert a favorable influence as a prophylactic. It is a remarkable fact, confirmed to Sir James Clark by Drs. Arnold and Musgrave, that persons obviously predisposed to con-

sumption are rarely attacked by the indigenous fever. The cases of pulmonary consumption in which the climate of the West Indies promises advantage, says our author, are very few, and their character scarcely ascertained; while those in which it produces mischief are numerous and generally well marked. Of persons predisposed to the disease, a certain proportion are likely to be benefited by the climate; but the nature of the constitution should be well considered before it is recommended even as a prophylactic. The affections of the chest most likely to derive benefit from a residence in the West Indies are chronic diseases of the bronchial membrane in persons in whom the digestive organs are in a sound state. In stomach complaints the West Indies are very generally unfavorable. Neither has the climate been found favorable in chronic rheumatism. Calculous disease and scrofula are rare in the West Indies.

Jamaica from its size differs from the other islands in presenting more variety of climate. Of all the small West India islands, St. Kitt's as a high and Barbadoes as a low island appear to deserve the preference. But a more advantageous and better plan for the invalid than residing in any one island would be, according to our author, to cruise among or make short visits to the different islands. St. Kitt's from its situation among a group of islands is well calculated for the head-quarters of an invalid having such a plan in view.

In the Appendix, besides notices of the climate of the *Cape of Good Hope*, *Australia*, and *New Zealand*, there are some useful observations on the proper application of mineral waters as adjuvants to climate in the cure of some of the principal diseases treated of in the work. The whole is concluded with a more copious and elaborate set of comparative meteorological tables than are to be found anywhere else.

Everything in the work before us being so concisely yet so clearly stated, our jottings of the author's practical conclusions have been sufficient for the foregoing notice, the preparation of which has been to us at once an easy and an agreeable task. From the length of time the last edition has been out of print, and considering the interest which attaches to the subject, our readers will not consider as too much the space we have occupied.

In attestation of the soundness of the author's conclusions, no observation of ours is necessary. Their importance will be at once perceived by the reader. The work is one which, with great truth may be said, ought to be studied by every physician. From the perusal of it we have derived the greatest pleasure, and we can with equal truth add instruction. The principles inculcated are of the highest importance to mankind, and are advocated with all the ardour of one who pleads for truth. It is, to conclude, one of those works, too rare indeed, in which philosophy is attended by simplicity, and in which perfect elegance reigns throughout without the slightest affectation of ornament.

## Art. XII.

*De la Nécessité des Etudes Pratiques en Médecine Légale, &c.* Par H. L. Bayard, Docteur en Médecine, &c.—*Paris*, 1840. 8vo, pp. 30.
*On the Necessity of Practical Instruction in Medical Jurisprudence.* By Dr. Bayard.—*Paris*, 1840.

Dr. Bayard's attention has been called to the defective method of instruction in medical jurisprudence adopted in France, by the evidence given on the recent remarkable trials of Peytel and Lafarge. He has here published his reflections in a small pamphlet. We are induced to notice his essay because if, as he alleges, his remarks apply so strongly to the system of instruction followed in the French medical schools, they apply with much greater force to that adopted in our own.

In the author's opinion at least two thirds of the medical profession in France are compelled to execute the duties of medical witnesses, not from their want of professional knowledge, but from that knowledge never having been properly directed to the acquisition of medico-legal facts. In France, as in England, legal medicine is made a subject of purely *theoretical* instruction for a short period towards the close of a pupil's studies. The science has not, like the other branches taught at the Faculty, its corresponding *practical* course. Anatomy, midwifery, chemistry, and botany have their practical systems of instruction, but medical jurisprudence is without this important auxiliary.

Where instruction is thus purely theoretical the professor, however accomplished, can do little more than point out the course of study to his pupils, and quote to them the contradictory opinions of authors on different subjects. In treating of hanging and drowning, for example, he is confined to a mere verbal description—it is out of his power to procure subjects for illustration.

"The *practical* knowledge acquired by the pupils is thus confined to a few notes taken during the course, or to the reading of some treatise on the subject. It is with this kind of preparation that they undergo a nominal examination." (p. 9.)

The young practitioner thus left to himself happens to be present at an accident, and he is required to make a medical report on the state of the wounded man. He finds himself here entirely at a loss: he may have arrested the hemorrhage and dressed the wound, but he has probably omitted to notice many minute points connected with the injury which are absolutely necessary in drawing up a report. Again, where a person is found dead from wounds, nothing but practical medico-legal knowledge can prevent a surgeon from falling into numerous errors, which may ultimately have the effect of defeating the ends of justice.

The author properly observes that it is not merely defective instruction which leads to such bad results, but the law itself throws obstacles in the way of those who are desirous of perfecting their knowledge of the subject.

Criminal investigations involving medico-legal questions are conducted in strict privacy. No one is allowed to be present except one or two

judicial authorities and the medical inspectors regularly appointed. Thus a vast source of information is cut off from the profession; and in any similar case those who are thus excluded are incapable of acting with the requisite attention to legal forms. Dr. Bayard considers also that the poor remuneration made to medical witnesses in France is one cause of the indifference with which the subject is treated. Again, those who rank high in the profession as physicians or surgeons consider themselves thereby qualified to act as medical jurists. By them—and how many are there in our own country who indulge in this idea?—the study of the subject is regarded as superfluous; for they are satisfied with being masters of the sciences on which it is based. We quite agree with the author, and there are on record in both countries many trials which will bear out his assertion, that a man may be a very accomplished physician or surgeon, and at the same time but a very indifferent medical jurist.

So long as coroner's inquests are conducted in the imperfect manner in which they are at present, and so long as the subjects on which they are held are either not inspected at all, or inspected by one incompetent to the duty, medical jurisprudence must remain in England a purely *theoretical* study. The coroner is an officer not chosen by any test of ability, or by reference to his knowledge either of medicine or law; he has a direct interest in holding as many inquests as possible, so that many are held unnecessarily, others slovenly conducted; and in dispensing with post-mortem examinations, if a lawyer, he does not perceive the necessity for detaining the jury by such a form, and if a medical practitioner, he is apt to think his own knowledge sufficient: thus assuming the character of judge and witness. It would be difficult to say how society is benefited by such a system. The greater number of well-informed lawyers look with contempt on the coroner's court, and think if it were suppressed to-morrow society would be none the worse. There are few cases of importance requiring a coroner's interference which do not also come before a magistrate, and the investigation is here conducted in a clear and satisfactory manner.

Until coroners are selected upon other principles than at present, and until post-mortem inspections of all subjects of inquisitions are as much as possible thrown open for the benefit of science, there can be no hope of medical jurisprudence flourishing *practically* in England, nor can it be expected that sound medical evidence should be forthcoming when it is called for by the authorities of the law.

In France it is customary to appoint a certain number of physicians to each court, who act as referees in medico-legal investigations, and take the name of "*experts.*" The author finds fault with this arrangement, because there is no guarantee for these "experts" being well-informed in legal medicine. From what we know of English practice, however, we should consider an arrangement of this kind far superior to that which is now in use in this country, since among those chosen it is likely that at least some would have the ability to act. In England, under the coroner's inquest, the selection of a medical practitioner for conducting a medico-legal enquiry appears to rest in general with the beadle of a parish! and the rule followed by this functionary is commonly that

of selecting the medical man who happens to live *nearest* to the spot where a dead body may be found. We apprehend M. Bayard would find much more reason to be dissatisfied with the English system than with that to which he objects.

In the French provinces, according to the author, the state of ignorance on these matters is much greater than in the capital. Mistakes are frequently occurring which can only be removed by an appeal to the more experienced medico-legal authorities of the capital. In proof of this view the Peytel case is brought forward, but not in our judgment very appropriately. The medical evidence here established that the prisoner murdered his wife by shooting her. Of this there could be but little doubt; but the evidence is objected to because the chest of the deceased was not properly examined for the marks of drowning, the prisoner having alleged in defence that he found his wife lying dead, with her face in water, although not entirely submerged. This is charged as an offence of omission against the medical witnesses, but their evidence, corroborated as it was by circumstances, brought the crime of murder clearly home to the prisoner, and we do not therefore see that the author has any ground to cavil, *ex post facto*, at the omission of what was really a subservient point in the enquiry.

Then comes the well-known case of Lafarge, which the author very properly calls the mysterious *drama* of Glandier. The evidence of the provincial witnesses is objected to, because before the analysis they did not seal up the vessels containing the suspected poisonous liquids, but as they afterwards discovered no poison, the objection is more technical than real. They are then condemned for presumptuous vanity in having framed a negative opinion when they had operated only on a portion of the liquids; the answer to which is, that although they might have gone further, there is no reasonable ground to suppose that by the *ordinarily recognized processes* for detecting arsenic with judicial certainty, they would have discovered any traces of that poison. The author looks upon Orfila as having vindicated the dignity of medical science from the disgrace thus brought upon it; but we must take leave to say, although we agree that the prisoner was properly convicted and punished for the crime, that we should be very sorry to see the chemical evidence relied on as a precedent in future trials. Unless arsenic be more unequivocally detected in a dead body than in our judgment it was in this case, the dignity of science will be best consulted by a witness declaring that there was insufficient chemical evidence of the presence of poison. Nevertheless, all who have read the proceedings of this singular case, including the account of the decoction of the body of the deceased in the open air, and before the court in which the trial was going on, must think with the author that it was truly a mysterious drama. Although M. Bayard may be right in his opinion as to the deficiencies of the French provincial practitioners, we think he has not been happy in his selection of illustrations to support it.

## Art. XIII.

1. *Practical Observations on Distortions of the Spine, Chest, and Limbs; together with remarks on Paralytic and other Diseases connected with Impaired or Defective Motion.* By W. T. Ward, f.l.s. &c.—*London*, 1840. 8vo, pp. 202.

2. *Spinal Curvature, its Consequences and its Cure; illustrated by the History of Thirty-three Cases, successfully treated.* By J. B. Serny, m.d. &c.—*London*, 1840. 8vo, pp. 90.

3. *Practical Remarks on the Causes, Nature, and Treatment of Deformities of the Spine, Chest, and Limbs, Muscular Weakness, Weak Joints, Muscular Contractions, and Stiff Joints, containing the results of the author's experience, and showing the advantages derived from the modes of treatment which he has recently introduced. With illustrative Plates and Cases.* By J. Amesbury, Surgeon, &c.—*London*, 1840. 4to, pp. 192.

4. *On a new Operation for the Cure of Lateral Curvature of the Spine, with Remarks on the Causes and Nature of that Disease.* By Frederic C. Skey, f.r.s. &c. &c.—*London*, 1841. 8vo, pp. 50.

5. *Spinal Diseases; with an improved plan of Cure, including what are commonly called Nervous Complaints, and numerous examples from upwards of 150 Cases.* By John Henry Robertson, m.d. &c.—*Glasgow*, 1841. 8vo, pp. 160.

6. *The Cause and Treatment of Curvature of the Spine, and Diseases of the Vertebral column.* By E. W. Tuson, f.r.s. f.l.s. &c.—*London*, 1841. 8vo, pp. 283.

7. *Spinal Affections. A popular Lecture on Disorders and Diseases of the Spine.* By H. C. Roods, Surgeon.—*London*, 1841. 12mo, pp. 57.

8. *Série de Mémoires sur les Difformités du Système osseux. Avec Planches.* Par le Dr. Jules Guerin.—*Paris*, 1838-39-40. 8vo, pp. 128, 79, 86, 44, 44, 53.

*Series of Memoirs on Deformities of the Bony System*, &c. By Jules Guerin, m.d.—*Paris*, 1838-40.

It was our intention, in collecting the numerous volumes now before us, to give a complete exposition of the very interesting and important subjects to which they refer, especially of the three principal forms of spinal affection: disease of the bones; simple or lateral curvature; and what is commonly termed spinal irritation. For reasons, with which we need not trouble our readers, we must for the present defer this more systematic article, and content ourselves with a short notice of the individual works on our table, gleaning from each, as far as our limits will allow, what strikes us as most useful in reference to the subject to which we shall almost exclusively confine our attention, *Lateral Curvature of the Spine.*

In regard to the treatment of this affection generally, we will only remark that while there are certain general principles which ought always to be borne in mind, there are none which do not often require modi-

fication in the attempt to apply them to particular cases. Experience will teach every one as it has taught us this truth. And, accordingly in the treatises before us, some of the authors recommend recumbence as all important, others condemn it, yet instances of cure are related under each mode of management. From facts of this kind it is evident that there are various modes of curing the diseases in question. The more important points to be borne in mind are: the replacement of the vertebræ either by direct permanent or occasional pressure upon them; the removing from them, by recumbency or other means, such force as tends to keep them in an improper relation one to another; the maintenance, if possible, of this due relation by support which, without itself producing ill effects, shall prevent the necessity of confinement to one particular position; and, most of all, the strengthening of the muscles and ligaments which act on the vertebræ by direct means, such as frictions, shampooing, palpation, exercises of various kinds, and by the employment of such general remedies, whether of a medicinal or dietetic kind, as shall best support the health of the system.

I. The volume of Dr. Serny is one of the last published, and probably the most valueless of the lot. It contains cases of various kinds of distorted spines, treated on the plan of the late Dr. Harrison. We do not wish to find fault with this plan, but as Dr. Harrison's book is still in existence we see no necessity for Dr. Serny's production. For those of our readers who may not be acquainted with Dr. Harrison's method, we quote from the volume before us the following:

"The usual mode of cure was commenced by directing the back to be well rubbed daily for half an hour. Compression was then made upon the incurvated spine to encourage its return towards a straight line; a padded shield was then placed upon the right side, extending from the axilla over the brim of the pelvis, and confining it there with stays. A considerable vacancy being in this way established between the person and the shield, the back-bone was observed to move sensibly towards the latter." (p. 33.)

Recumbency constitutes an essential part of Dr. Serny's treatment, while the means to preserve the general health of the system, and to invigorate the muscles in particular, are too much overlooked.

II. The work of Mr. Amesbury is a very voluminous quarto, with an abundance of pictures of crooked backs and limbs, made as straight as they ought to be. The book itself is a sadly meager performance. What is well known about the affections of the spine of which it professes to treat is repeated; but when we seek for information and explanations of the mechanical means which Mr. Amesbury professes to have found of so much advantage, we are referred to this and that patent instrument, the construction of which is not explained, because, forsooth, it may be badly copied. We can give no sanction to this sort of mystification. Genuine professional feelings require a man to tell to his brethren all he knows which can be to their advantage; not to stop short exactly at that point at which his pecuniary profit and their information disagree. It is pleasant, however, to see that Mr. Amesbury has an entire persuasion of the excellence of his own motives, and if this were any measure of truth we should congratulate him thereupon. He very properly insists on the fact, that a treater of spinal deformities should be a surgeon possessed of correct mechanical knowledge; that the mechanist distinct from the sur-

geon, or the latter without a knowledge of mechanics, is alike unfitted to manage properly cases of this kind. We quote the following remarks as useful hints:

"There are three principles of chief importance to be borne in mind, in the management of all curvatures of the spine, which have not resulted from destruction of part of the spinal bones, or of the substances which lie between them, viz. to bring the bones into their natural position; to sustain them in their natural position by the help of adequate support as long as it may be necessary; and third, to increase the tone and restore and maintain the balance of power in the muscles, especially those connected with the bones of the spine, so that when the natural figure of the bones is restored, the spine may be maintained in its proper direction, by the action of the muscles, without artificial aid." (p. 14.)

It is useless for us to attempt to enter into Mr. Amesbury's plan of treatment. He makes use of certain instruments which, it is asserted, support the spine, whilst at the same time they do not require the observance of a recumbent posture. Whether this be the case or not we cannot determine, as Mr. Amesbury has kept us in complete ignorance of the only means of so doing. We conclude our notice of this book by the following remarks on the mode of *measuring for shoes* and the principles on which this should be done. They appear to us likely to be useful, as, if attended to, they are calculated to prevent the many painful and injurious effects of ill-made shoes:

"Both feet should be measured, partly in the erect and partly in the sitting posture . . . . The first measure should be taken with the person standing upon the measure. The measure round the toes should be taken in the standing posture, with the foot bearing upon the ground. The first gives the extreme length of the foot when the arch has yielded under the influence of the weight; the second gives the spreading of the toes. The third and fourth measures (that is, on a level with the ball of the great toe and about an inch behind this) round the foot, should be taken when the foot is lifted from the ground; all the other measures to be made according to the usual rules which shoemakers are accustomed to follow. The object of this mode of measuring is to cause the shoe to support the metatarsal bones, and to ensure sufficient room for the toes. The support of the shoe should be felt by the wearer, *above the toes and not upon them. The toes should have room to move freely and not be pressed upon by the shoes at any part.* When these rules are duly acted upon by observant persons who have acquired experience in this mode of measuring, the shoes give no more inconvenience to the wearer than a glove, and at the same time gives considerable support to the foot, which is preserved in its natural symmetry and beauty, free from corns and bunions." (p. 191.)

III. We have derived more pleasure from the perusal of Mr. Ward's book than from either of those already referred to; and we will now, with it for our guide, proceed to notice a little more fully than we have hitherto done, several matters of practical importance relating to the general subject of these volumes.

As to the causes of these affections we may enumerate, in the first rank, all those which produce debility, whatever these may be; all causes which tend to encourage a deviation of the spinal column from its normal direction, such as modes of standing or sitting, &c. unequal length of the lower limbs, the use of ill-made stays, pads or dresses which require awkward postures, shortsightedness, inequality of power in the lower limbs, &c. It is needless at the present time to notice the opinion that

any curvature of the spine is necessarily associated with disease of its bones. Mr. Ward gives the following table :

"In 282 cases of spinal curvature that have fallen under my observation, and of which I have taken notes, the following has been the proportion in which they have occurred :

| | |
|---|---|
| Of curvature to the right side without disease . . . . . | 230 |
| Of curvature to the left side without disease . . . . . | 10 |
| Of posterior curvature unaccompanied with disease . . . | 9 |
| Of posterior curvature with disease . . . . . | 30 |
| Of that which I denominate incurvation, i.e. projection of the lumbar vertebræ within the pelvis . . . . . | 3." |

The same author makes some very rational observations on the power of muscles and the mode of increasing it :

"The comparative power of muscular parts," he says, "depends, 1. On the state of the functions of respiration and circulation, and that increased strength is a consequence of increased vascularity and circulation of blood in a part; and *vice versa*, a want of tone and power of a deficient supply of it. 2. On the degree of exercise or frequency with which they are called into action. 3. On the mental energy or power of solution exerted on them. 4. That the most effectual means of increasing muscular strength is by the frequent exercise of the power itself, and consequently, the preservation of the healthy actions of those functions by which it is influenced. 5. That the muscular parts have a constant tendency to contract, by which they adapt themselves to the state of the limb or parts to which they are attached." (p. 13.)

The bearing of the above principles on the treatment which Mr. Ward recommends in these cases will be sufficiently obvious. And first of all, early in the invasion of the disorder, the diet must be well attended to; the food to be good, plain, simple, nutritious. In young people especially, amusement is recommended to be blended with all the curative means :

"All causes which assist in laying the foundation of the disorder should be avoided; amongst others may be enumerated that of giving strength to one side of the body at the expense of the other, which is done by obliging children to use the right hand exclusively on all occasions, lest they should become left-handed; it is more desirable to increase the power of the arm defective in strength by its more frequent use. . . . In some slight cases, where I have thought the deviation has been caused by a weakness in one of the ankles, I have recommended the game of hop-scotch, and this has been followed by an evident improvement." (p. 40.)

Mr. Ward points out the necessity of restoring the balance of power between the muscles which have been contracted and those which are in a state of extension. He divides these means into passive and active: the former being all those external means which have the effect of increasing muscular power, such as friction, shampooing, percussion, confinement to a particular position, galvanism, electricity, &c.; under the head of active, the excitement of the muscles by volition or that of muscular exercise. It is unnecessary to enlarge upon the modes of employing and the effect of these various means. Mr. Ward's views respecting recumbency agree much with our own, i.e., that although useful it should not be exclusively relied on, because of the atonic state which the muscles thereby acquire. With recumbency, therefore, must be associated the means alluded to.

"I have witnessed," it is said, "many cases where friction alone has been unsuccessfully employed for a considerable length of time; and others where

the inclined plane has been depended on solely, without the other measures being prosecuted at the same time, in which a combined plan of percussion and strong muscular exercise assisted by a recumbent posture has afterwards been attended with complete success. By the union of these means, the cure can be effected in a much shorter time. It would appear that another advantage is gained of great importance with regard to the general health. I have observed that when the recumbent position alone has been relied on, that general ill health and dyspepsia are very often present, sometimes to a distressing degree. This does not occur if recourse be had to occasional action and rest; on the contrary, this regular exercise of the body tends to strengthen the powers of digestion, &c." (p. 47.)

Mr. Ward does not believe that children "grow out of" curvatures, as is sometimes said, unless, indeed, there be an entire change in the habit of life. It should be well remembered that the complaint has a constant tendency to increase unless means are taken to counteract it. The time at which, under the treatment above recommended, some amendment may be looked for depends much on the extent of the deformity.

"The average rate at which improvement takes place, presuming the curvature to be of the extent of an inch and a half or two inches, is half an inch the first month, and one eighth of an inch every succeeding two months." (p. 52.)

There are some very sensible observations in Mr. Ward's book on certain deformities of the chest, and the means to be employed to remedy them. These means are chiefly such as have been already alluded to; such in fact as increase muscular power. The principal deformity here referred to is generally known by the name chicken-breast. Mr. Ward thus describes his own rational treatment of this affection:

"The method which I have employed with regard to the local means in those cases where the spine has been exempt from disease has been that of placing the intercostal muscles and those connected with the anterior part of the chest on the stretch, by placing the patient in a standing position, with the back against a cylindrical piece of wood, and the arms extended backwards. By this means an extension of the pectoral muscles is produced, and they are then brought into full action upon the ribs, as well as the muscles of the abdomen, which are the opponents of them. . . . While in this position the patient is desired to take deep inspirations. I direct manipulation and afterwards percussion to be employed for one or two hours during the day, gradually increasing them in force according to the influence produced on the patient. In addition to these means, I usually desire the patient to suspend the body by the arms, and similar modes of exercise, with a view to promote the full action of the pectorales, serrati magni and postici muscles, &c. on the ribs, to produce the greatest possible extent of elevation of the ribs and sternum, and consequent expansion of the chest. . . . . The good effects of this plan of treatment are not confined to the removal of the local disorder, but it is attended with still more important advantages with regard to the state of the general health. It is uniformly found that in proportion as the parts are restored to their natural form, the pulse is diminished in frequency; the respiration becomes fuller and easier, and the action of the digestive organs as well as of the bowels become more regular and natural." (p. 84.)

In the latter part of the volume there is an application of the same principles of treatment to various other affections, to contractions of the limbs, to paralysis, to St. Vitus's dance. We with pleasure refer to these several subjects, and recommend our readers to do the same, regretting that our limits will not allow us to take a longer notice of them.

IV. Mr. Skey's pamphlet is well written, and contains many sensible and useful observations; but we cannot see that he was called upon to

write a book. All the new matter contained in his tractate might have as well occupied a page or two in some weekly journal. He calls attention especially to the fact that lateral curvatures of the spine occur first of all in the lumbar region, and that the dorsal curvature is a mode of compensation for this. We are disposed to concur with him in this view, although unable to determine on the comparative number of cases in which this takes place. He says of this lateral deviation:

"It occupies its general seat in the loins, where it is situated probably in four cases of disease out of five. The lumbar region is by far the most mobile part of the spinal column; and it forms, as it were, a centre of motion to the whole body. So long as the head is neither inclined to the one side or the other, but retains its natural relation to the trunk, its weight, with that of the trunk, is conveyed in an equal degree by each leg to the ground; or, in other words, the axis of gravitation occupies the mesial line of the whole body; but no sooner is the position of the head altered, by being carried towards either shoulder, than the lower extremity of the same side receives the additional weight of the head extending over and beyond the axis, in a ratio increasing with the squares of the distances between the projecting head and the axis [?] and if the extension of the head to one side be accompanied by the chest also, then the weight is no longer supported by the two extremities, but by the one belonging to that side towards which the head is inclined, and the other leg is placed on the ground generally in a state of extension, for the purpose of enlarging the base and adding steadiness to the position. In this attitude, an angle is formed at the loins, at which the double lines of obliquity of the body meet; the upper line extending downwards from the head, and terminating in the lumbar vertebræ; the second extending upwards from the foot, which supports the weight of the body, reaching the loins by the same obliquity of direction. This position becoming permanent is all that is necessary to the ultimate completion of true lateral curvature." (p. 45.)

From this view of the subject it is clear that in all cases of lateral curvature, greater attention than is commonly the case should be given to the loins. And it also follows that the proper treatment must be directed to that part, a circumstance not sufficiently attended to in many cases. Having established the great probability of the curvature in a large number of cases having its commencement in the loins, and the consequent necessity of some of the earliest steps of treatment being directed to these parts, Mr. Skey examines the anatomical condition on which depends the permanence of the curve, and suggests a mode of remedying this condition. He states that the longissimus dorsi and sacro-lumbalis are contracted, as must necessarily be the case from the approximation of their points of attachment.

"In order to appreciate the especial efficiency of these two muscles to confirm the injury done, we must examine their structure, and we shall at once comprehend the difficulty of treating lateral curvature, so long as they retain their integrity, even in their reduced condition. . . . . These muscles are especially tendinous in structure; not indeed like other muscles, in which the muscular fibre terminates abruptly in tendon, which represents the muscle to its insertion, but we have here a layer of tendinous substance of considerable thickness, spread over the surface of the muscle, and into this tendon the muscular fibres are largely inserted. . . . . Divide this cord, and the loins become straight in an exceedingly short period of time; and as the loins present in the large majority of cases of disease the first indication of its presence, and the direct and certain cause of its upper curvature, so it is that the successful treatment of the lumbar occupies the direct path to the cure of the remaining part." (pp. 32-4.)

This division is effected by a "straight bistoury, introduced from the outer side of the muscular pillar, across its extent or cutaneous surface, and with which the surface only of the muscle need be divided." After this the other steps of treatment may be pursued. The main indication for employing this mode of treatment is (when it is rendered unobjectionable by the absence of many well-known causes and conditions of curvature) the tension of the muscles on the concave side of the curve, when an attempt is made to raise the spine. The treatment here recommended by Mr. Skey is also said in his hands to have proved, and still to be proving, very beneficial. Sufficient time has not yet elapsed for any judgment to be safely arrived at respecting the expediency of the operations, of which the above, described by Mr. Skey, is an example. Various muscles have been divided by different surgeons to remedy curvature of the spine. Some have divided muscles high up in the back, which, if Mr. Skey's opinion be correct, must end in failure. But we are not yet in possession of sufficient data to be able to express any safe opinion on the probable result of such operations. But Mr. Skey's proposition, and the reasons in favour of it, well merit the attention of the practical surgeon.

V. We may say of Dr. Robertson's book the same as of Mr. Skey's; it is not a bad book, but it is not needed. It will, however, be useful to those who have read none other on the subject. We are disposed to think that the most useful part of the volume is its author's description of his mode of employing dry cupping (in such cases as he thinks this applicable to a tender spine), and a description which he gives of a substitute for stays. This dry cupping is very much relied on by Dr. Robertson in such cases, whether the spine be curved or not, as there is a cause of irritation at the roots of spinal nerves making itself known by distant symptoms; tenderness on pressure of the spine, or over its transverse processes, being the indication for the employment of the cupping. Having ascertained personally since reading Dr. Robertson's book the powerful local effect which may be produced by cupping, according to the mode described, we think that we shall be doing service by making it more generally known:

"In treating," says Dr. Robertson, "both the serious affections of the back, and those less severe, and depending on irregular muscular action, disturbed nervous action, or pressure upon a nervous twig, the cupping-glasses are the most powerful remedy I have ever employed or seen employed . . . . . I have been in the habit of practising it for upwards of twelve years, in an immense variety of cases; and in the hope that the attention of the profession and of the public may be more directed to it, have given cases illustrative of its efficacy . . . . . With reference to the cases where it is likely to be useful, I should say, that wherever the pain is dull though severe, deep-seated, chronic, not much increased by pressure, or has *refused to yield to ordinary means*, there would I put it in practice; and, if required, on a part of the body where a cup would not sit, or the nearest convenient spot." (pp. 70-1).

The following is Dr. Robertson's description of the most efficacious mode of employing his cupping-glasses. Having dipped the glass in warm water,

"A little piece of folded absorbent paper is dipped into alcohol, brandy, or pyroxilic spirit, keeping the end by which you hold the paper dry. Apply this to the flame of a candle, and let it drop to the bottom of the glass to be used. . . . . The dry portion of the paper adheres to the wetted or damp portion

of the glass; and when the latter is inverted and applied to the skin, the flame remains at the point furthest from the skin, and causes no irritation from its presence. . . . . The method described, though not perhaps the most elegant, is by far the most powerful way of producing sudden determination towards the surface, and alteration of the internal action of a part. I can in almost any instance in a favorable part of the body fill the glass nigh half full of integument and muscle, and in a *few* instances have seen the blood sweat through the pores of a healthy but fine skin." (pp. 72-3.)

The cups employed by Dr. Robertson are peculiar, and well adapted to fit themselves to surfaces, to which glasses of the common form cannot be applied.

"Even the largest of the ordinary cups is in most cases too small. . . . The shape of the mouth too of my glass is quite different. Instead of the usual plain round mouth, similar to that of a tumbler or wine glass, I have it cut out at the sides, so as to make the *mouth* of the glass a segment, more or less small of a large circle, both sides being alike, thus, ⌣. I consider this a great improvement." (p. 76.)

And we think so too. The glasses employed by the author vary in size from six to twenty ounces. They are capable of producing a very powerful effect, and are well worthy of employment in a variety of cases of local pains. The cases related by Dr. Robertson illustrate their efficacy in relieving pains and spasmodic affections, probably having their source at the origin of nerves in the spinal marrow.

We must also notice Dr. Robertson's description of a "simple and efficient back stay," which he has employed in cases requiring support, but which are injured by ordinary stays; this, he says, he has found to answer admirably.

"A piece of well-seasoned ash, one eighth of an inch thick, two and a half inches broad, and fifteen inches long, was steeped in boiling water, actually boiled or submitted to the action of steam for twelve hours. By this means it became perfectly manageable, and would take easily any shape given to it. It was then covered with chamois, and padded one third of an inch thick in front. Three straps of powerful linen were put across it at certain distances, with buckles, and shoulder-straps also with buckles, the highest strap buckled quite loose above the breasts in the female, the other two at distances lower down. The shoulder-straps came over the shoulders, and were buckled to the first strap already fixed, thus keeping the whole support sufficiently high up, and the upper belt out of the way of the mammæ. . . . . Some have laid aside their expensive steel corsets, and taken to this simple affair; and in no single instance, when made to fit, have I had any complaint, either of its inefficacy, of its producing irritation, or even of its inconvenience." (pp. 98-9.)

VI. We wish that we could give praise in proportion to its size and cost, to Mr. Tuson's book. But as may be said of most of the others, two or three pages of a journal would have been the most appropriate vehicle for conveying to the public all the useful matter which it contains. It is true, that we must then have sacrificed numerous pictures of crooked spines and deformed chests, and long histories of cases little worth the reading; that the draughtsman would have been without an opportunity for displaying a very moderate talent for zinc engraving; and, more important than all, that the printer would have had to seek elsewhere the means of exhibiting a very excellent type: but Mr. Tuson would have avoided the publication of a book which scarcely communicates any novel matter which might not have been well compressed into the space occu-

pied by the preface. There is much common-place about the necessity of ascertaining the causes of curvature of the spine before undertaking their treatment; and one is almost led to believe from such a sentence as the following, that the author imagines this to be a discovery:

"I presume that sufficient reasons have been advanced in support of this doctrine, to induce the reader to agree with me on this point; namely, first to ascertain the seat and cause of disease. He will assuredly see further ground for concurrence in this opinion, as I proceed to the consideration of the separate affections to which the spine in so many cases is subjected," &c. (p. 22.)

After this specimen of presumed ignorance on the part of the reader, Mr. Tuson takes great pains to show, what everybody else knows, that a variety of causes may bring about curvature of the spine, and that the treatment must vary in accordance with such causes. It is surely needless to comment on such a matter as this. Mr. Tuson thinks that there is often a softened state of the vertebræ themselves in cases of lateral curvature. He speaks of the benefits derived from proper exercise, food, &c. in the development of bone and other parts; and he tells us that "those of his readers who have observed the growth of youth, must have seen the frame gradually expand when the child has been allowed a proper degree of exercise, with suitable food and clothing, except in the case of some predisposing cause for disease." This is the important mode in which the fact of growth is communicated to Mr. Tuson's "readers." What will happen to the pitiable many who cannot buy this book? But we will, for the information of those who may not be Mr. Tuson's readers, refer to the apparatus employed by him in some cases of curved spine; and which apparatus produces extension of the vertebral column, and affords convenient means for employing certain exercises whilst the recumbent position is observed. Those who wish to employ the instrument recommended, will do well to procure a sight of it, as without diagrams it is difficult to convey a sufficient description of it. The instrument is a couch, on the horizontal framework of which are laid two stuffed cushions, which are acted upon by two springs. The effect of these two springs is to separate the cushions from one another, so that when the patient is lying on the couch, extension may be made both to the upper and lower part of the spine. In order to increase the power of these springs, weights are applied, the action of which may be varied according to the necessity of the case. At the head of the horizontal part of the couch is an exercising framework, with rollers and ropes adapted to various exercises for the patient's arms, and the position of this framework may be altered as necessary. So that "independently of extension, one of the purposes of the couch is to enable the patient to use a variety of exercises whilst in the recumbent position, which will tend to dilate the chest and increase the function of respiration and circulation." There is an abundance of drawings to illustrate the use of this instrument; and there are many other drawings, some well done, some very ill done, showing the healthy state of the spine and its morbid states; but after having reconsidered all the attractions of the volume before us, we are compelled to adhere to the opinion of it above offered.

Some of our readers may think it out of place to criticise the style in which a practical treatise on affections of the spine is written; but we are of a different opinion: the honour and dignity of our profession are concerned in the literary as well as the scientific and practical knowledge of

its members. We should like to know what reception a work written in a style of which the following is a specimen, would receive at the hands of a literary reviewer? or what estimate he would form of the education and taste of a profession, one of whose distinguished members could thus express himself?

"With him [the anatomist] as with the astronomer, the laws of nature are fixed and immutable; and he raises his voice against whatever may tend to derange and pervert their natural order or fundamental principle. Before him nature stands unveiled; he investigates with a peering eye that incomprehensible structure, the human frame; he explores the deep and secret recesses of its hidden mechanism; he knows that in the organization of animal life, there are certain fixed laws, which, in whatever species they may be found, are invariably the same, having the same nature and sympathies, the same vital and physical principles, emanating from the same cause, and destined for the same functions." (Preface, p. xi.)

VII. Mr. Roods' little work scarcely demands notice here. It contains nothing new, and professes not to contain anything. It probably was written for the instruction of the members of some Institution, and it answered the purpose very well.

VIII. M. Guerin has been long known as a writer on orthopædic subjects, and as a clever and energetic practitioner; but there has occasionally been too much of display in his proceedings. He is, however, a man of much scientific knowledge of his subject, and of great experience. His first memoir is devoted to the explanation of a new method of curing lateral deviation of the spine, by what he terms sigmoid extension and flexion. Of this he observes that "it rests on two new principles substituted for two old ones, oblique or perpendicular extension employed instead of parallel extension, and flexion instead of compression." The mode of treatment spoken of is applied to all kinds of articular deformities. We may premise that M. Guerin is a most wordy author, and that the kernel of his compositions bears a very small proportion to its coverings, and is at the same time somewhat difficult to extract. In the present instance, it consists of a drawing with a description of a most elaborately constructed bed for producing the desired effects on the spinal column. Any attempt at a description of the mode of construction of this apparatus would be useless without a drawing of the apparatus itself, for which we must refer our readers to M. Guerin's work. The following quotation from the book itself will explain the principle of M. Guerin's method:

"In a mechanical point of view, what is required to be done in the treatment of lateral deviations of the spine? To restore to its proper form a column which is curved in one or two points of its length. . . . . Instead of the spine, take a curved but flexible column. The proper mode of straightening this is not to pull at its two extremities and in the direction of its length, but to fix the two extremities of the column, applying at the same time the knee to the convex side of it, and at the same time pulling perpendicularly at its extremities, thus producing a curve in an opposite direction, and the individual doing this will not extend merely until the column is straight; experience teaches that, in order to effect a complete and permanent restoration, it is essential to produce an opposite curve to that which existed before, in order to destroy the force which tends to reproduce it when the force has only been so far employed as to produce a straight line. This, which is commonly done to get rid of any kind of flexible curvature, is what I have endeavoured to render practicable for curvatures of the spine. The method which I propose consists in substituting artificial

for pathological curvatures, directly opposed to the last, so as to give to the column the form of an S in a direction exactly opposite to the S which represents the pathological change; in other words, it consists in substituting oblique and perpendicular extension for parallel extension of the spine. Hence the term *sigmoid*."

If we may judge of the efficacy of M. Guerin's method from his commendation of its effects, it is very useful indeed: and a report made upon certain cases of lateral curvature, which were subjected to treatment with the apparatus, appears to justify the inference that there is considerable advantage to be gained from its employment.

From the second memoir of M. Guerin we learn that simulations of the curved spine are means employed to deceive their fellow-creatures, both by medical men and laymen. It seems that this subject has excited a good deal of strong feeling among some orthopædists in France, but it does not appear to us to be of sufficient importance to employ any space upon it.

The contents of the third memoir does not refer to spinal diseases; but we shall nevertheless briefly notice it. It is on wry-neck produced by muscular contraction, and will be new to some of our readers. It is maintained by M. Guerin that the sterno-cleido-mastoid muscle consists of two distinct muscles, the sterno-mastoid and cleído-mastoid; that the functions of these differ, the cleido-mastoid being a motor of the head, the other a muscle of inspiration; that in wry-neck, the sternal portion of the muscle may be the only part which is contracted, and that the section of this portion alone suffices to remove the essential cause of the deformity. It is not maintained that both parts of the muscle may not simultaneously contribute in some cases to the deformity, but that the cause is most frequently such as, M. Guerin mentions, results from his own observations. There appears to be nothing in the mode of operating which requires description; the object being to render the tendon as tense as possible, and then to divide it beneath the skin. In the other treatment of these cases, the passive retraction of other muscles is to be overcome by proper mechanical contrivances.

The fourth and fifth memoirs are on clubfoot, and are among the most valuable of the series. We have, however, so recently devoted an article to the subject of clubfoot that we shall here omit it; remarking only, that in the fifth memoir will be found some very careful descriptions of the anatomical peculiarities of the tarsal bones in various forms of clubfoot, and with which it is very desirable that operators for this deformity should be acquainted. At the same time we cannot omit to remark on the very tedious prolixity of the composition.

The last memoir in the series is on rickets; and although foreign to the main object of this article we shall here briefly notice it, as we may not have another opportunity of referring to M. Guerin's works. This memoir contains an excellent description of the morbid anatomy of the bones in that disease, which we will abridge. In the early stage (*incubation*) the bone is filled with an effusion of sanguinolent matter, which occupies its spongy tissue, its medullary canal, the space between the periosteum and the bone, the concentric lamellæ of the diaphysis, and all parts, whether of the long or of the flat bones, in which the nutritive arteries are distributed. The swelling of the different parts is the result

of this effusion. In the second period (*déformation*), at the same time that the texture of the bone softens, the matter which is deposited in its interstices tends to become organized. This matter is most abundant between the periosteum and the bone, between the medullary membrane and the canal. In the third period (*résolution*) the newly-formed tissue becomes harder and more compact, and is compounded with the old bony structure, which regains its former hardness. This addition of new matter causes the increase in size, which is witnessed in this disease. In an exaggerated degree of the disease (*consomption rachitique*), the interstices of the bones and lamellæ are far separated, and the consistence of the original bone is so far lost that their external layer is often reduced to a thin pellicle. The texture of rachitic bones in the adult, when the disease is completely resolved, is much harder and more compact than in the normal state. In this condition (*eburnation rachitique*) it is no longer possible to distinguish between the original bone and the parts of recent formation.

We have thus shortly noticed the various books before us. From those which treat of deformed spine, a tolerably rational system of treatment may be adduced; and we should rather make a selection from all than attach ourselves exclusively to one. Thus, for example, the advocates of a system which excludes recumbency must, in certain cases where great pain or fatigue attends on any exertion, make recumbency a part at least of their early treatment, until that degree of amendment has been effected which will allow of the adaptation of such well-applied mechanical support as will permit the body to be exercised without the whole weight of its upper part resting on the spine. We can only regret that Mr. Amesbury has not, by a description of his apparatus, put it in our power to judge of its fitness for the professed object. The means referred to and explained by Mr. Ward must be in various degrees applicable to all cases, and with a certainty in many of a favorable issue. We are indebted to Mr. Tuson for the description of his apparatus, which appears to be well adapted to effect extension in the cases in which this is desirable, and which also affords facilities for certain useful exercises. The operation proposed by Mr. Skey may be auxiliary in those cases where the contracted tendon is evidently the cause of deformity, though we do not think that it is quite clear that the muscles referred to by him are the main instruments in keeping up displacement.

The most important practical lesson to be learned from a consideration of the whole subject before us, is that which is derived from a knowledge of the causes of curvatures of the spine. The majority of the more immediate causes would not suffice to produce the disease if their operation was not favoured by a constitutional debility; and this debility is induced in various ways. It is therefore to the prevention of the causes of the deformity that the attention of the medical man should be especially given. The probability of its occurrence in all families, and especially among the female members of them, should ever be borne in mind; and changes, which if allowed to gain ground may become very difficult of cure, may then be anticipated and prevented. More attention than was formerly the case is now given to early physical education; and if this be continued on a wholesome plan, the occurrence of distorted spine must be proportionally rare in future.

## Art. XIV.

*Wahres und Falsches in der sogenannten Wasserheilkunde.* Von Dr. H. Claessen.—*Köln*, 1840. 12mo, pp. 127.

*The Truth and Falsehood of the so-called Cold Water Cure.* By Dr. H. Claessen.—*Cologne*, 1840.

The medicine of all barbarous nations is simple; a few well-known herbs, a few animal preparations make up mostly their whole materia medica. Superstitious observances fill up the place of medical treatment, and in the cure of almost all diseases, fire and water, whose effects are so striking as to impress the most ignorant, play a prominent part. Our own ancestors and the northern nations in general ascribed peculiar virtues to water. Standing on the river's banks they worshipped, they divined with the smooth stones of the stream, and maidens often sat all night at a spring, *waking the well.* Petrarch tells us how, in accordance with an old custom, the banks of the Rhine at Cologne were covered on St. John's eve at sunset, "præclaro et ingenti mulierum agmine,' who with many ceremonies and muttering a charm bathed their arms and hands in the stream. Water dipped from a running stream at twelve o'clock on Christmas night was reputed a cure for colic; that obtained at midnight of St. John's day was deemed a febrifuge. Many non-medicinal springs had a high reputation; some restored the old to youth; and tales were not wanting of how persons cut in pieces, like Æson of old, had emerged whole from their waters. They were visited from afar, and people sojourned near them for the sake of bathing in them and drinking their waters.

Of these wonder-working springs no other memorials now remain but names, such as Holywell in England, Heilbrunn, Heiligenbrunn, &c. in Germany, except in some secluded spots where old customs and old superstitions long linger. In such a place it was (at Gräfenberg, a little village in Silesia) that Vincent Priessnitz, rather more than ten years ago, first employed on animals that practice which has since become so generally adopted in Germany. The fame of his cures spread, and we doubt not that many an old tradition recovered strength as the simple people flocked from the neighbourhood to be healed by the Silesian peasant. Who would then have anticipated that within so short a time the Gräfenberg water-doctor should be talked of from one end of Germany to the other; that more than twenty different institutions should be formed for carrying out his practice; and that his empiricism should be styled a system, and, dignified with a Greek name, afford, under the title of Hydropathy and Hydriatics, materials for a hundred publications on its merits and defects? Such, however, has been the case; and if it has been found possible to induce several hundreds of persons annually to submit to a diet homely and often coarse, to drink some quarts of cold water, and to be dipped or pumped upon more than once daily for weeks together, or to undergo a process which causes most profuse perspiration and makes the skin break out in boils, it is surely worth while to enquire, with Dr. Claessen, what may be the measure of truth or falsehood in a method so little in accordance with our ordinary therapeutic proceedings.

In this publication the author has had two objects: the one, to "check

the unbounded exaggeration of the advocates of hydriatrics, the other to obtain a scientific ground from which to enter upon a practical investigation of this new mode of treating disease." The latter is, however, by no means an easy task, for the whole literature of the subject does not, as Dr. Claessen assures us, contain a single trustworthy and carefully recorded case. Many debatable points therefore are left untouched, and the author contents himself with describing the proceedings of the hydropathists, and explaining their alleged effects in accordance with the common principles of science. Following Dr. Claessen as our guide, we shall endeavour as briefly as possible to explain to our readers the practice, and, as far as we can collect them, the principles of hydropathy.

Priessnitz, the originator of the cold-water cure, does not seem to be by any means chargeable with all the follies and extravagancies which have been perpetrated by his followers. The pretension that all diseases are curable by cold water appears indeed to have been in a great measure the result of a government ordinance which, while it permitted him to use water in the treatment of diseases in any way which he chose, absolutely interdicted the employment of any other remedy whatever. His ingenuity thus put to task, he devised various modes of applying his one means; and, being a man of considerable tact and acuteness, met with great success in his practice. A kind of system of treatment was thus formed, from which all remedies but one were of necessity excluded, and the next step was to represent cold water as a universal panacea, a sort of *elixir vitæ*, and all other means of cure not only as unnecessary, but as being actually hurtful. Too ignorant to explain the reasons which guided him in his practice, Priessnitz abandoned that task to his followers, men who differed little from their master, save that to ignorance they added impudence. At first there was a debate among them whether the principles of allopathy or of homœopathy were those to be adopted in expounding their practice; at length they hit upon a solution of their difficulty in the discovery that both were erroneous, and that the only true medical doctrine was that of hydropathy. As far as we can gather from the account of Dr. Claessen, hydropathy appears to be a sort of eclectic system, or compound of Brunonianism and of that humoral pathology which for centuries has been current among the vulgar.

We are not aware whether any work has been published setting forth the principles of hydropathy, a creed of the water-doctors, but we suspect not, for Dr. Claessen does not allude to any such. We pass over his own remarks upon the action of cold, which, though sensible, are not new, and copy from p. 25 of his book the following scheme of the different modes of employing cold water in an institution for the cold-water cure:

"A. External employment of cold water.
I. General irritation of the skin by cold water.
1. Simple irritation by cold water.
1. *a.* Washing.
2. *b.* Plunging.
3. *c.* Affusion.
4. *d.* Cold baths.
5. *e.* Cold baths after previous sweating.

2. Irritation by cold water, accompanied with mechanical action on the skin.

6. *a.* Cold washing, with brushing the skin.
7. *b.* Shower baths.
8. *c.* Douches.

II. Partial irritation of the skin by cold water.

1. Simple irritation by cold water.

9. *a.* Application of wet compresses.
10. *b.* Partial baths, hip and foot baths.

2. Irritation by cold water, accompanied with mechanical action on the skin.

11. *a.* Water falling in drops upon a part.
12. *b.* Water propelled in a slender stream against a part.
13. *c.* Douches.

B. Internal employment of cold water.

14. *a.* Drinking.
15 *b.* Clysters.
16. *c.* Injections of cold water into the different cavities of the body."

The first four methods of applying cold water require no detailed explanation. The fifth is the great remedy of the water-doctors, though by no means peculiar to them, but a modification of that kind of bath which is used, not only by the Russians, and other nations of Northern Europe, but also by the North American Indians. "They have a cure for some diseases," quaintly says good old Cotton Mather of the Indians, "even a little cave; after they have terribly heated it, a crew of them go and sit there with the priest, looking in the heat and smoke like so many fiends, and then they rush forth on a sudden and plunge into the water: how they escape death instead of getting cured is marvellous." The process in a hydropathic institution, as described by Mr. Lee in Part II. of his useful little work on the Baths of Germany, p. 121, is as follows:*

"Each patient is awakened about five o'clock in the morning by an attendant, by whom the process of *emaillottage*, or wrapping up, is performed. A blanket or woollen covering is first bound firmly round, so as to envelope the whole body, the face only being left exposed; over this are placed one or two feather beds, or eyderdown coverings, and over these again a second blanket is bound round the body, which, thus enveloped, appears to be about twice its natural size. After a time copious perspiration is induced, the window of the room is then thrown open to admit fresh air, and cold water is given to the patient at short intervals to promote the perspiration, and prevent his being weakened by its quantity, which in some instances is so profuse as to soak through all the coverings, and the mattrass of the bed. When the perspiration has continued for the period that is deemed advisable (sometimes as much as two hours) the coverings are all removed except the first blanket; a cloak being thrown over the patient, and slippers placed upon his feet, he descends quickly to the bath; and first dipping his hands and face for a second or two, throws off the blanket and plunges into the water at a temperature varying from nine to twelve degrees of Reaumur, while the perspiration is still streaming from the surface of his body; the duration of the bath is only a few seconds in some instances, in others it extends to five minutes, or even a longer period, brisk motion and friction of the surface being enjoined."

In those cases where the simple *emaillottage* does not produce per-

* The Baths of Central and Southern Germany. By Edwin Lee, Esq. London and Paris, 1841. 12mo, pp. 134.

spiration, Priessnitz wraps the patient up in a sheet wrung out of cold water, which Dr. Claessen (p. 29) asserts to be a never-failing means of exciting the action of the skin. The degree of reaction to be excited is carefully regulated by at first merely sponging the patient with cold water, and gradually proceeding to plunging, effusion, and at length to the use of the cold bath. Our author states that reaction invariably follows the application of the water, consequently that there is no danger of catching cold, but on the contrary the sensation produced by this treatment is most delightful. One superiority of Priessnitz's plan over that adopted in the ordinary Russian baths, is that the patient constantly respires cool air instead of breathing in an intensely hot atmosphere, which necessarily interferes with respiration and occasions great congestion about the head and chest.

Local applications of cold water are likewise extensively employed by the hydropathists, as in the case of wet compresses which are dipped in cold water and worn during the whole day over the seat of any organ, as the liver, heart, or stomach, which may be supposed to be especially affected. It is asserted that patients soon become reconciled to these applications. The other modes of employing cold water locally which are given in the scheme, seem to call for no explanation.

Such is the armamentarium of the Gräfenberg water-doctor and his followers, with which they profess to cure all diseases. That powerful effects are produced by some of their modes of treatment is undeniable, and the testimony of all persons concurs in representing the flow of perspiration produced by a single employment of the cold bath after sweating as enormous, while its use is often persevered in for months. Dr. Claessen tells us that the perspiration under these circumstances often assumes a very peculiar odour which is regarded by the water-doctors as an infallible sign of the elimination of the morbific virus. A very apocryphal tale is related by Mr. Lee, on the authority of one of these gentlemen, of a lady "who had taken sulphur-baths seven years previously, but who had not used sulphur since that period. After she had been pursuing the cold-water cure for some time, her room smelt of sulphur, a bracelet she wore became tarnished, and her linen was stained of a yellowish colour." Much, however, as a stinking perspiration is to be desired, a patient is to consider himself doubly fortunate if his skin should become covered with eruptions, and especially if boils in great number should break out on his person.

"Hydropathy," says Dr. Claessen, "attaches peculiar value to the purulent contents of these boils which are supposed to be made up of the diseased matter. . . . . This unusual commotion of the different fluids does not take place without a deep impression being made on the organism. Congestion of various organs occurs, severe pain is experienced in different parts more especially in such as are diseased, the activity of one or more organs of the senses is stimulated to a morbid and even to a painful degree, with all of which occurrences hydriatrics are familiar under the name of excitement, (aufregung.) In the further course of the treatment, and during the methodical increase of its activity, a regular fever is induced, which according to the statements of the water-doctors is usually followed by the cure of the disease, and to which they have given the name of crisis. It is, they assert, only in exceptional cases that the first appearance of the fever does not prove critical, but even in those instances its more energetic return and the consequent cure of the disease may be

confidently anticipated. It is not unusual for the crisis to be ushered in by stormy and even dangerous symptoms, for the relief of which hydropathy, in accordance with its exclusive tendencies, employs no other remedy than water. After the fortunate termination of the crisis, the patient is looked upon as cured, but it is considered desirable that the derivation of the fluids towards the surface should be gradually diminished, which is done by the employment of the so-called after-cure." (pp. 38-9.)

The internal employment of cold water is, as has been already mentioned, merely subsidiary to its external use, and the copious draughts of that fluid which the patients are directed to take are said to favour the outbreak of perspiration as well as greatly to increase the urinary secretion. Six quarts of water are regarded as a quantity which any one may boldly venture to drink every day, and at the *table d'hôte* water is the only beverage allowed. Not only are wine and all highly-seasoned meats absolutely interdicted, but one of the dogmas of hydropathy is that all food ought to be taken cold rather than hot. For this injunction, one of the water-doctors, appealing, we presume, to the experience of his former days and humbler occupations, assigns the following reason:

"The intestines of pigs who have been fed upon hot mashes are found to be extremely readily torn, and so rotten as to be unfit for making sausages. . . . . It is therefore evident that hot food is improper since it tends to relax and soften the intestines, and that persons should partake of no food but such as is cold, or at least lukewarm." (p. 79.)

Such is a specimen of the practical reasoning of one of the apostles of the water cure. In illustration of their theories we have space only for the following explanation of the action of cold water:

"The curative power of water consists in its decomposing, purifying action. The healing effects of water result from the animosity to all organic life, with which it attempts to destroy it by reducing it to its original atoms." (p. 10.)

Enough, we trust, has already been said to convince our readers that the materials for a correct appreciation of the curative effects of cold water are not to be sought at the hands of hydropathists, whose system we can regard only as a new chapter in the long history of imposture and credulity. To Priessnitz, however, must be allowed the credit of considerable mechanical ingenuity, and of readiness in devising different methods of employing his remedy, from which some useful hints might probably be gleaned and adopted in our hospitals. The modification of the Russian bath which he has hit upon appears to have some advantages, and is undoubtedly a most energetic remedy, though we do not feel altogether that faith in its freedom from danger which is expressed by Dr. Claessen. If the advocates of the water cure should ever publish cases of which they detail the symptoms minutely and carefully, and not content themselves, as at present they do, with the assertion that they have cured so many cases of pneumonia, so many of fever, &c., we may at some future time return to the subject of hydropathy. Should they not do this, the grain of truth will nevertheless be speedily sifted from the bushel of falsehood, while hydropathy will soon be superseded by some new system of, if possible, greater pretensions and greater absurdity.

## Art. XV.

*Mémoire sur l'Opération de la Taille.* Par M. Souberbielle. (*Mémoires de l'Académie Royale de Médecine.* Tome Huitième.)—*Paris*, 1840. 4to, pp. 50.

*Memoir on the Operation of Lithotomy.* By M. Souberbielle. *From the Memoirs of the Royal Academy of Medicine.* Vol. VIII.—*Paris*, 1840.

This paper includes the cases of 50 patients operated upon by M. Souberbielle between the years 1828 and 1834, in 39 of whom the calculus was extracted above the pubes. The high operation for stone has at different periods been revived and practised by some eminent surgeons of our own country, and as often has it again fallen into disuse; it has likewise from time to time obtained its advocates abroad, who have suggested and adopted it as a judicious method of removing calculi in certain cases where the lateral operation presented difficulties and would necessarily be attended with hazard and danger. The subject must, therefore, always be one of considerable interest to the practical surgeon whose experience has taught him to prognosticate and appreciate the difficulties and untoward results incident to what are generally called unfavorable cases, cases which the surgeon would gladly avoid in practice, and which he is only induced to undertake from a sense of duty at the earnest solicitations of his patients. As the experience of M. Souberbielle in the performance of the high operation has doubtless been greater than that of any other living surgeon, and as his practical acquaintance with the perineal section must have enabled him to compare the two methods with sufficient accuracy, we shall owe no apology to our readers for laying before them the result of his practice, together with the reasons which he gives for his preference to the mode of incising the bladder above the pubes. We shall therefore quote the author's own words from the essay appended to the history of the 50 cases, the details of which form the bulk of his communication.

We may first observe that M. Souberbielle opens the bladder by means of the *sonde à dard*, introduced by the urethra and brought out through the linea alba above the pubes. In three cases, where the size of the stone and the close application of the bladder to its surface did not allow the introduction of this instrument, the incision was effected from without with a bistoury which was carried down directly on to the stone. He considers, however, that the *sonde à dard* is by far the best instrument for opening the bladder with precision and accuracy, especially in fat persons and where the bladder is contracted and deeply situated. A syphon catheter is introduced by the urethra immediately after the operation and retained until the wound is healed, generally from fifteen to twenty days. Before removing the catheter he generally tests the soundness of the cicatrice by closing the orifice of the instrument for three or four hours, and allowing the urine to accumulate. This is repeated for a few days. Sometimes the catheter becomes a source of irritation to the patient, or the contractions of the bladder prevent its retention within the viscus. It may in such cases be removed, and the water then escapes

either partially or wholly by the wound; but the removal of the catheter does not give rise to any other inconvenience than that of somewhat retarding the recovery of the patient.

"In children," says M. Souberbielle, "although the situation of the bladder offers nothing unfavorable to the high operation, I yet prefer the lateral section, merely because they are too intractable to be properly secured, and the violent struggles which they make, and which can neither be guarded against nor prevented, expose the peritoneum to the danger of being wounded: again, the care and attention required to maintain a catheter in the urethra tends much to irritate and excite a young patient, since every time he is approached he imagines that the operation is about to be recommenced. With certain exceptions, therefore, depending on age and peculiar circumstances, I practise the high operation; and my preference for it is founded on reason and experience. Indeed by this method calculi may always be extracted without reference to their size, their number, their situation, the condition of the prostate, &c.; the operation is less painful than any other mode of cutting into the bladder, and exposes the patient to the fewest casualties. The skin, the linea alba, the cellular tissue, and the bladder are the only textures implicated in the section; whereas in the perineum we encounter a variety of structures which it is difficult to avoid, and the necessary lesion of which may give rise to hemorrhage, to stercoraceous fistulæ, to infiltration, to impotence, &c; none of these dangers are incurred by cutting into the hypogastrium. The history of the post-mortem examinations of those who have sunk after the high operation goes to prove that the operation is never itself a cause of death, but that the fatal event has been the result of previous organic alteration or disposition to disease, whose origin was clearly not referrible to the operation, which indeed would exercise no other influence upon it than that of hastening its development. On the other hand, hemorrhages, infiltration of urine, wounds of the rectum, laceration of the cellular tissue, are the consequence of casualties to which the perineal operation is liable, and which are frequently attended with a fatal result. It is my conviction that the high operation as practised by myself is not obnoxious to these fatal consequences. .....

"As regards the size of calculi, the cases 8, 20, and 48* describe stones of large dimensions which offered no difficulty in their passage through the wound, but whose extraction through the perineum would have been either very difficult, as in cases 8 and 20, or altogether impossible, as in case 48.†

"The facility afforded to the extraction of the stone arises from the circumstance that the incision may be enlarged with impunity to any necessary extent, as was proved in the operation I performed on M. Delaborde, member of the ancient academy of surgery, from whom, at the age of eighty-two, I removed a calculus weighing half a pound.‡

* Case 8. M. Gravet, æt. seventy. The stone was of an ovoid form with a rough surface, and weighed four ounces six drachms. The cure was complete. Case 20. M. Constant, æt. sixty-five. A very hard stone of a flattened ovoid form, measuring one inch and a half in its small, two inches and a half in its long, diameter, and seven inches and a half in its circumference, weighing five ounces one drachm. Around it, near its centre, was a groove which corresponded to a sort of collar formed by the bladder, by which it was encircled and so firmly retained, that the knife was had recourse to before the extraction could be effected. The patient was well by the eighteenth day.

† M. Levesque, æt. sixty-five. Two stones of nearly equal volume, of an ovoid form flattened laterally, weighing together seven ounces two drachms and a half. These calculi were moulded to each other and occupied the entire cavity of the bladder; it was necessary to detach the walls of the viscus from the surface before the forceps could be introduced. The incision was carried down directly into the calculi as the sonde à dard could not be made use of. The patient recovered.

‡ No mention is made as to whether the patient survived the operation.

"As regards the presence of a numerous collection of stones in the bladder, or what is equivalent, a single stone which breaks into fragments; several of the 40 cases are very conclusive evidence in favour of the high operation, but none perhaps so strong as case 1, where, notwithstanding the peculiar anatomical condition of the bladder and the numerous sacculi and cells which it contained, nearly 300 calculi were extracted without leaving one behind.*

"The cases 6, 12, 20, 39, 43, 48, present remarkable and decisive examples of the capability afforded for the extraction of stones by the high operation which it would have been impossible to lay hold of through a perineal opening, because they were retained in pouches or else firmly grasped by the bladder itself. In case 43 the stone, which was situated in a pouch at the fundus of the bladder, could not have been reached by instruments introduced through the perineum, and the manœuvre by which it was finally dislodged from its situation could not have been carried into effect.

"The attachment of calculi to the internal surface of the bladder is not so rare a circumstance as might be imagined, since we find that in 16 out of the 50 cases the stones were either embraced by the bladder or retained in the base of the viscus or else inclosed in pouches, which latter it was in some instances necessary to cut before the dislodgment could be effected.

"Lastly, case 35, in which a considerable deformity of the pelvis would have rendered the extraction of the stone through the perineum impossible while it interfered but slightly with the high operation, is a remarkable instance of the advantage of the latter method under certain rare conditions." (Memoire, pp 87-90.)

M. Souberbielle next proceeds to refute the objections which have been advanced against the high operation. These objections he enumerates as urinary infiltration, hemorrhage, and wounds of the peritoneum; the first two he maintains are much more likely to occur after the perineal section, while the latter he considers as an accident not liable to be followed by mischievous results. It would appear that infiltration of urine into the pelvic cellular tissue (which he remarks as being a common occurrence after the lower operation), only took place once out of his 39 cases. It was however undoubtedly the cause of the patient's death on the fifth day. Infiltration, he says, may always be avoided by making a clean incision into the bladder and taking care not to disturb or tear through the loose cellular tissue connecting the anterior surface of the viscus with the abdominal walls. Hemorrhage, which he also cites as a frequent cause of death after the ordinary operations of stone, occurred but in one case, and, notwithstanding all the efforts to arrest the flow of blood, continued until the patient sank twenty-four hours after the operation. In this instance, remarks M. Souberbielle, the fatal event depended on the peculiar idiosyncracy of the individual. Lesions of the peritoneum, he further remarks, are not likely to occur if proper precaution be used in incising the bladder; and when such an accident does take place, it probably depends on some peculiar condition or alteration of the structures concerned in the operation. In two of the cases which form the subject of this memoir the cavity of the peritoneum was opened. One of the patients died, but in the opinion of our author his death was in nowise to be ascribed to the injury inflicted on the serous membrane, as no traces of peritonitis were found on the post-mortem inspection.

* This patient died on the seventh day after the operation from serous effusion into the cavity of the tunica arachnoidea.

We have now laid before our readers the pith and substance of M. Souberbielle's memoir, and extracted in a condensed form nearly all which can be useful or interesting to the surgeon. A few remaining remarks contained in the text we shall also have occasion to refer to, in the observations with which we purpose to conclude this article; for as, after an attentive perusal of the paper, we feel by no means induced to alter materially our previous views of the subject, or subscribe to our author's prepossession in favour of the high operation, it will be necessary to analyze and test the statements he has put forth with so much confidence, and ascertain how far they are founded on accuracy of fact and soundness of judgment. We are far from wishing to impugn the skill or under rate the labour and experience of an eminent man, but we feel ourselves justified in saying that our author's partiality for a favorite operation, which he may be said to have made his own, has evidently led him into great mistakes, into no slight perversion of facts, and erroneous conclusions; that the effects of the strong bias he has conceived has induced him, in the first place, to depreciate the success and magnify the difficulties of the lateral section in the practice of his professional brethren; while, again, it has caused him to exalt unduly the advantages and merits of the high operation.

In the early part of his memoir, M. Souberbielle quotes the published statements of M. Civiale in relation to the number of deaths which occurred in the Paris hospitals after the lateral operation of lithotomy; and although he throws discredit on their accuracy, perhaps not without justice, he yet makes use of them as subservient to his own views and persuasions.

"If," he says, "we are to give credit to M. Civiale, 35 patients died out of 61 operated upon in La Charité; out of 96 patients cut at the Hôtel Dieu there were 27 fatal results; at Beaujon, 6 died out of 11; in the Hôpital Necker, 11 out of 14. I am far from pledging myself to the assertions of M. Civiale but I certainly was struck with the great want of success which followed the operation of lithotomy in the hospitals of Paris, and I attribute it to the faulty means employed. I think the result would have been very different if the lateral method of F. Jaques, the lithotome caché of F. Come, or the *high operation* had been practised for the extraction of the calculi." (p. 82.)

Any reference to loose and uncertain documents for the purpose of establishing the average mortality of lithotomy patients in his own country, has been rendered quite unnecessary by the record of 356 cases occurring in Paris and its environs, furnished by M. Dupuytren in his posthumous work on the bilateral section. Of these 61 died, presenting an average mortality of rather more than one in six. In our review of M. Dupuytren's work, (*vide* Br. and For. Med. Rev., vol. II.) we compared the average mortality in France and England; the summing up was greatly in favour of our own surgeons, since the mean ratio of several series of cases recorded at different institutions was one death in eight operations. We then expressed our conviction that the greater success was mainly dependent on the better method of performing the operation in this country, by which many of those casualties which M. Souberbielle so strongly insists upon as incident to the lateral section are avoided and overcome. We must repeat our former assertion that the French lithotomists seem yet unacquainted with the principle which guides the

English surgeon in opening the bladder; at all events they neither follow it in their practice nor express any clear opinion of it in their works.

The principle and very essence of the lateral section, a principle which involves the subsequent safety of the patient, consists in opening the bladder without entering the cavity of the pelvis, thus forming a direct communication between the interior of the bladder and the external perineal wound, at the same time leaving intact the fascia which lines the cavity of the pelvis. The natural adaptation of this fascia serves to separate the cavity of the pelvis from the lower pelvic opening and from the outlets of the body. In the operation of lithotomy it should always form a barrier between the track of the surgeon's knife and the sub-peritoneal cellular tissue, which becomes exposed and laid open by its lesion. Such an incision, we hold, can only be accomplished with certainty and precision by the gorget or more especially by the knife; we believe its accuracy cannot be insured by the lithotome caché, an instrument almost universally adopted by the French operators. Hence the numerous mishaps and untoward events which have induced M. Souberbielle to abandon the lateral operation and adopt another method, which he considers as less dangerous, but which, if we are to judge from the results, has not been attended with a more successful issue. Our author, in comparing the lateral with the high operation, has of course recorded the risks and dangers which he has experienced and witnessed in the performance of the former. We as English reviewers must claim the privilege of debating the vexed question according to our own experience; and we have no hesitation in affirming that those casualties which our author cites as every day occurrences, are in the English practice very rarely met with. Thus wounds of the rectum and stercoraceous fistulæ, impotence or incontinence are scarcely recognized and certainly not feared as the consequences of lithotomy. Urinary infiltration into the subperitoneal cellular tissue, a most fatal occurrence, very seldom takes place, because, except in cases of peculiar difficulty, the integrity of the pelvic fascia is carefully preserved. It must be remembered that one out of M. Souberbielle's thirty-nine patients died in consequence of urinary infiltration beneath the peritoneum.

The liability to hemorrhage is certainly greater in the lateral operation; but provided the patient is carefully watched and attended to, it very seldom leads to a fatal result, because it can be controlled by the judicious application of pressure; whereas in the case of fatal hemorrhage which M. Souberbielle relates, no means to compress the bleeding vessels could be devised, and the patient sunk from loss of blood.

Although wounds of the peritoneum are disregarded by our author, we certainly should not like to expose our patient to the risk of laying open the serous cavity of the belly; and although in the fatal case related by M. Souberbielle, and already referred to in this article, no evidences of peritonitis were found, we are by no means assured that the collapse and subsequent vital depression which frequently follows abdominal wounds, might not in this instance have hastened if it did not give rise to the death of the individual. We consider that the lateral section through the perineum offers one great advantage to the patient which is not obtained in the high operation, inasmuch as it opens the bladder in the most depending part, in the situation of its natural outlet towards

which all the muscular contractions of the viscus tend. It thus ensures a ready passage for the urine to drain away, and facilitates the expulsion of clots of blood, mucous secretions, or perhaps fragments of friable calculi. A stream of water may be easily passed into the bladder through the lower opening, and thus particles of stone may be got rid of whose expulsion could not have been effected in any other way. It likewise renders unnecessary the nuisance of wearing a catheter, the constant presence and irritation of which must, we imagine, in some cases be almost intolerable to the patient, and totally prevent that calmness and quietude which is so essential to his recovery.

It only remains for us to advert to the last point on which we find ourselves at issue with M. Souberbielle, viz., the frequent occurrence of calculi adherent to, impacted in or firmly retained by, the parietes of the bladder. He states (p. 89), "the *fixity* of stones to the walls of the bladder, under which condition the high operation offers so many advantages, is of more frequent occurrence than is generally supposed." Surely the very circumstance that a state of parts said to be so common should hitherto have escaped the attention and comment it deserved goes far to negative the assertion. What is also somewhat remarkable is that M. Souberbielle himself does not appear to have made this notable discovery until he practised the high operation. Then and not till then he is enabled to tell us that out of thirty-nine cases cut above the pubis, sixteen were either so situated, or else so firmly retained by the parietes of the bladder, as would have rendered their extraction by the perineum a matter of the greatest difficulty, nay, that in six it would have been absolutely impossible. We are aware that a stone has sometimes though very rarely become impacted in a pouch formed by the recession of the mucous lining between the bands of the muscular coat. In such instances, either from the absence of symptoms or the difficulty of striking the calculus with a sound, an operation has seldom been attempted, and the fact has been brought to light after death. But with this exception we hardly know of a single instance in which an operator after having fairly opened the bladder has failed to extract the calculus in consequence of its adherence to the walls of the viscus. We believe that M. Souberbielle has mistaken cause and effect; that he made his difficulties himself by incising the bladder through its muscular walls, thereby causing a sudden and violent contraction of the parietes upon their contents, requiring the free use of his fingers and divers other instruments to extricate the stones from the firm grasp which had seized and retained them. We believe that if he had operated by the perineum and opened the bladder without lesion of its muscular coat this difficulty would not have occurred; that in most of the instances he cites the stones would have fallen within the grasp of his forceps, and would have been extracted with the ordinary facility. In the eleven cases when he operated by the lateral section no such obstruction occurred; moreover they all got well.

Of the fifty cases which form the subject of M. Souberbielle's memoir, eleven underwent the lateral operation and they all speedily recovered. It must, however, be observed that the greater number were children and in a condition in which success might fairly be anticipated. Of the remaining 39, 11 died, or 1 in $3\frac{1}{2}$, in round numbers about 4 in 13.

This certainly presents a fearful rate of mortality; but on referring to the table we shall find that many of the cases were highly unfavorable, and on that account, according to our author's views, more particularly adapted for the high section. Many of the patients were advanced in years, in some the calculi were of large size, whilst others had been previously subjected to the manipulations of unsuccessful lithotrity. The result therefore is not to be taken as a criterion of the merits of the operation or as a measure of the success which may be anticipated in the ordinary average of cases.

Although it has been our misfortune to differ materially from M. Souberbielle in many important points, it is far from our intention to represent the high operation as wholly inadmissible or to stigmatise it as mischievous. We should be inclined to give it the preference in some of those cases where the size of the stone and the enlarged and diseased condition of the prostate would lead us to anticipate great difficulty in the extraction with its almost necessary consequence, contusion of the soft parts around the wound and laceration of the cellular tissue and fascia surrounding the prostate and neck of the bladder. Calculus may also be complicated with certain other conditions and varieties under which the bladder may be incised with advantage above the pubes. Our object in this article has been to investigate fairly and impartially the facts and arguments by which M. Souberbielle seeks to justify his preference for the high operation, and we feel quite satisfied in recording our opinion that its general substitution for the ordinary method would not be attended with benefit to the suffering patients; that in the large majority of instances the high operation fails to obviate those evils which indeed are inseparable from unfavorable cases, whilst, on the other hand, it not unfrequently tends to increase the difficulties and give rise to new dangers from which the lateral section is exempt.

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## Art. XVI.

*On the Diseases and Derangements of the Nervous System.* By Marshall Hall, m.d. f.r.s.—*London*, 1841. 8vo, pp. 380.

There are several reasons why we do not enter upon a full examination of this work at present. In the first place, our available space would not allow us to treat the subject of it as fully as we could desire. Secondly, we have already in the present Number devoted an article of considerable length to nervous diseases. Thirdly, the author announces a much larger and more complete work on the subject, which, if not too long delayed, we would prefer waiting for, more particularly as the present volume, although containing some new points, is, in a considerable degree, made up of materials already before the profession in various publications by the author, the greater number of which have been noticed in this Journal. As, however, Dr. Hall has done us the honour to give to our criticisms of his former works a very prominent place in the one before us, devoting the greater part of a somewhat long preface to reclamations from our judgments and opinions, in which he vents most bitter accusations of ignorance, prejudice, wilful misrepresentations, and all

sorts of unworthy motives against the supposed authors of our reviews, we must here spare a few pages to the consideration of the chief points at issue between him and ourselves. Dr. Hall ought to receive this as a compliment at our hands, as he must be aware that we hardly ever take notice of the remonstrances of authors against our judicial awards, when it happens to us, as it must occasionally happen to every independent journalist, to form an estimate of their writings, different from that which they themselves entertain.

I. In our Third Volume pp. 29-39, Dr. Hall's Lectures on the Nervous System and its Diseases were reviewed. It was there pointed out that the class of actions to which he had given the name of *excito-motor* corresponded with that denominated *sympathetic* by Whytt and other physiologists; but it was freely admitted that merit was due to him for "fixing the attention of physiologists upon them, and on the agency of the spinal cord in producing them; and likewise in casting doubts on the doctrine of their dependence on sensation." On this head we should have expressed ourselves more strongly had not Dr. Hall claimed too much, by asserting that the phenomena to which he was directing attention were "quite unknown to physiologists." The question whether or not these actions necessarily involve sensation was left *sub judice;* "whether they really indicate sensation or not," we observed, "can only be decided by observations in cases of injury or disease of the human body," and two cases were referred to in support of the negative. We *never denied* this part of Dr. Hall's doctrines, as he insinuates that we once did (p. xiii.); but it would have been most unphilosophical to have received them as established facts, upon the very insufficient evidence at that time adduced by him. Dr. Hall complained of our award; and we admitted into our next Number (vol. III. p. 577, et seq.,) his own statement of his claims; and what he intended to be a correction of our mistakes regarding them. To these his reviewer appended some remarks, which clearly showed that he had not in the least misapprehended the degree in which that portion of Dr .Hall's doctrines that might then be regarded as admissible, had been previously taught by Physiologists.

II. Dr. Hall's second Memoir was reviewed by us in our Number for April, 1838. We there entered fully into the scientific and historical merits of the question with no other view, we can most conscientiously affirm, than the elucidation of truth. The additional evidence adduced in the interval by Dr. Hall and others, and especially the publication of cases of the class just referred to, induced us at that time to express our full concurrence in the general fact asserted by Dr. Hall, that the class of actions termed *reflex* is dependent upon the spinal cord and its nerves alone, and *does not* involve sensation as a necessary element of its phenomena. We fully enquired into the history of this opinion, and showed that it had been nearly approached in the doctrines propounded by former physiologists; and we concluded with expressing our belief that Dr. Hall's "great merit consists in the harmonious combination of these doctrines into an uniform system, and the explanation of many phenomena which were not formerly regarded as explicable on such principles." (vol. V. p. 537.) To this opinion, in regard to the physiological principles respecting which it was then expressed, we still most firmly adhere; but, as we were not at that time disposed to consider as proved the anato-

mical distinctness of the excito-motor and sensori-volitional systems of nerves, we did not attribute to Dr. Marshall Hall any merit as the discoverer of it. We did not by any means assert, however, that this last doctrine was false, but left the question open to be decided by future evidence. It is to this article that Dr. Hall's most frequent reference is made, as one calculated to do him serious injury in the eyes of the scientific world. Yet he has nowhere entered upon that calm examination of its asserted fallacies, which alone can justly entitle him to assail it with the violence he has employed. We will venture to say that, by the arguments there adduced in favour of a part of his views, the minds of the profession were better prepared to receive the whole at a subsequent period than they would have been if we had not noticed the subject at all; so that the critique was, after all, of service to him rather than otherwise.

At the conclusion of the same Number which contained this review, we inserted the chapter from Prochaska on the Reflex Function (which had only just become known to us,) with the heading "Complete Anticipation of Dr. M. Hall's Doctrine of the Reflex Function." The essential correctness of this statement, we are still prepared to maintain; and we have only to remind our readers, that it refers, *not* to the distinct excito-motor system of nerves, of whose existence we were not at that time convinced,—but to the Reflex Function alone, of which Prochaska had given an account that proved that he well understood it.

III. When additional evidence was adduced in behalf of Dr. Hall's doctrine of the structural distinctness of the excito-motor system, we took the earliest opportunity of recording our adhesion to it, and of making the *amende honorable* to Dr. Hall, which we did in the following words: "As to Dr. Hall's *entire originality* in this part of his doctrines we never expressed the slightest doubt; and we now are quite willing to accord to him the credit of having made a most important advance in nervous physiology, not only in the manner already stated, but by the discovery of a previously unsuspected fact, which, when placed entirely beyond doubt, will take the rank of those established by Sir C. Bell." (vol. VIII. p. 511.)

IV. In a subsequent article (vol. IX. p. 96, &c.) we contrasted Dr. Hall's discovery at greater length with those of Sir C. Bell, and the result appears to have been satisfactory even to him. (Preface, p. xi.) Yet he still pressed on us his own statement of his claims, to which, as before, we gave free admission. (vol. IX. p. 580.)

V. Our last notice (April, 1841,) refers principally to the *vis nervosa*, a question on which we have the misfortune to be still at issue with him.

Dr. Hall maintains that the physiological facts brought to light by him were sufficiently conclusive in respect to the structural distinctness of the system of excito-motor nerves from the sensori-volitional, to render additional evidence unnecessary; and seems to wonder that any physiologist could feel the necessity of *anatomical* confirmation to his views. To this we simply reply that, if such were the case, the great discoveries of Sir C. Bell are comparatively valueless; since they only confirmed—what had often been previously asserted on physiological grounds to the full as satisfactory as those on which Dr. Hall has proceeded—the distinctness of the sensory and motor nerves.

If our readers will refer to the second of the articles just enumerated, they will find (vol. V. p. 438,) that we there stated it as the published opinion of several physiologists, who admitted Dr. Hall's general doctrines on the physiology of reflex actions, that these *did not in themselves* warrant the recognition of a system of nerves *anatomically* distinct; and that the question might be considered an open one, to be decided by further evidence. We have reason to know that, in this view of the *then* state of the case, we expressed not only our own opinion and those of the authors cited, but of several other distinguished physiologists, including two whose opinion in favour of the general value of his discoveries Dr. Hall records (p. 232, note,) with great satisfaction, and whose opinion in *our* behalf we expect that he will hold in equal regard. Dr. Holland introduces the last chapter of his Medical Notes and Reflections, "On the present state of enquiry into the nervous system," with the following passage: "Those who desire to learn the actual state of knowledge in this complicated enquiry, will do well to refer to a series of most able articles in the British and Foreign Medical Review, embracing its relations to all other branches of physiology as well as to the higher and more general principles of inductive science." As the first edition of the work was published between the times of appearance of the Review of April, 1838, and that of January, 1840, it cannot be doubted that Dr. Holland meant to refer to the former and to the preceding articles. Dr. Sharpey also in his public lectures on physiology at University College, delivered in the spring of 1838, expressed opinions exactly conformable to ours.

Dr. Hall more than once taunts us with having changed our opinions as to the truth of his doctrines. Surely such a taunt comes with an ill grace from one whose great source of complaint against others is that they will *not* change theirs; and who has been so continually modifying his own during the last few years, that it would be very easy for us to point out apparent inconsistencies, much more numerous than those for which he attempts to bring us into odium. For example, between the publication of his first and of his second memoirs, he changed his opinion on the subject of the source of the respiratory movements; having in the former considered the medulla oblongata as their primum mobile; and in the latter announced as a new discovery that which was essentially the opinion of Whytt, that they are occasioned by a stimulus conveyed to the nervous centres, chiefly by the par vagum. And, in the volume now before us, he admits that the contractions of the œsophagus are *partly* reflex, (p. 71,) although he formerly maintained that they were independent of the excito-motor system. Now *we* do not consider it a fault in Dr. Hall that he has been induced by new evidence, or by a reconsideration of the old, to modify his first opinions; and we only ask for ourselves, and for other physiologists, the same liberty. And if he finds that they are willing to admit, upon evidence adduced by others, doctrines to which they refused assent when propounded by himself, the more dignified course would certainly be to express gratification in their conversion to the true faith, rather than to upbraid them with their previous incredulity. It cannot be too strongly impressed upon those who are eager in the propagation of what they believe to be novel and important truths, that the same facts will afford to different minds very different degrees of evidence of them. Wherever there is room for diversity of

opinion, the variable constitution of the human mind is such, that diversities *will* be entertained; and we see no more hope of perfect conformity to a general standard in physiology than in religion. We are quite willing to admit that the array of numbers against any particular novel doctrine is no evidence of its falsity; for scarcely was an important truth ever announced that did not at first meet with general opposition. But when the subject is taken up by a number of independent enquirers, experienced in the scientific pursuit of truth, and possessing competent knowledge of its details, their general agreement in one decision does seem to us to indicate, either a fallacy in the original proposition, or a want of evidence sufficiently convincing to those who are acquainted with the general bearings of the question. That one or two individuals of previously unknown character might, from the first, give their assent to the new doctrines, can scarcely be regarded as any testimony in favour of them, or as any merit on their own part; since such assent might easily be obtained from those not versed in the question, by a one-sided statement of it. For ourselves we can only say that, looking back at what we have written on this *quæstio vexata*, there is not a position we have taken which we are not prepared to justify, as the most conformable to the *then* state of physiological knowledge.

We shall now briefly notice the more particular charges against us, contained in Dr. Hall's preface, and scattered through the volume. In regard to the history of the subject, several of the statements which we formerly made are so distorted that it would be difficult to recognize them again; and we shall content ourselves with requesting such of our readers as feel an interest in the discussion to refer to vol. V., pp. 532-57, for our opinion of the degree in which Dr. Hall had been anticipated by Flourens, and to vol. IX., p. 113, for a similar analysis in regard to Prochaska, and to decide for themselves whether we have made the slightest attempt to show that either of these physiologists had made any approach towards the discovery of a *distinct system of excito-motor* nerves, of which Dr. Hall charges us (at least we know of no other writer to whom he can refer, § 253), with attempting to transfer the merit to others. In order to prove our ignorance and malice, he has obtained from M. Flourens the following disavowal: "Votre beau système des nerfs *excitateurs*, *incidens*, et *réfléchis*, vous appartient bien, et comme grand fait spécial et déterminé, et comme vue d'un grand et nouvel ensemble de phénomènes." But in this he is fighting with a shadow entirely of his own evoking; for, from the moment when we were convinced that the doctrine was valid in itself, we have not hesitated to give the full credit of it to Dr. Hall; and not a word can he find in our previous criticisms showing the least tendency to deprive him of its paternity. As to the question how far M. Flourens had anticipated Dr. Hall in the more general doctrines of reflex action, our statements have never been impugned in any other way than by clamorous vituperation. We have given an abstract of what M. Flourens *does* say, and Dr. Hall replies by assertions as to what he *does not* say; and leaves it to be inferred that, because M. Flourens does not express his opinions in Dr. Hall's language, there was nothing common between the two. Surely the fairer way would be to take our assertions, sentence by sentence, if they are incorrect, and to prove their falsity from M. Flourens' own

work. The opinions which we subjoined, as to the probable modification of M. Flourens' doctrines by Sir C. Bell's discoveries, we offered merely as an *opinion;* and there is not a word in M. Flourens' disavowal of the "distinct system," which in the least contradicts it.

The "vis nervosa" question, however, seems to be, in Dr. Hall's mind, the sorest point of the whole litigation between us. We really feel loath to inflict upon our readers any further discussion upon what may seem to be so personal a dispute. But we have found from Dr. Hall's last arguments, what we had all along suspected, that he does not understand the words in the ordinary sense; that, when he is talking of the *vis nervosa* of Haller, he really means something quite different. Referring to our former statement, that if the "*vis nervosa* of Haller be intended, this is alike the means by which volition, emotion, and simple reflex action, produce muscular movement." Dr. Hall remarks, "How does my young critic know this? it may be, or may not be so;" and he frequently elsewhere speaks of volition, emotion, and the vis nervosa, as the three modes by which the nervous system operates upon muscle. Now, *our* assertion is founded upon the simple fact, that Haller's own definition of the term does not admit its employment in any other sense than the one we have assigned to it. No reader of Haller's works can be ignorant that by the *vis nervosa* he everywhere implies that influence originating in the *brain* and spinal cord, which, being propagated along the nerves, causes muscular contraction, whether in obedience to volition, emotion, or any other stimulus. Haller's great object was to separate the idea of this *vis nervosa*, or nervous power, from the *vis insita*, or contractility of the muscle; and whilst, in the case of the heart and alimentary canal, he argues that the contraction is due to the direct excitement of the *vis insita*, he would say that, wherever a muscle is caused to contract by the influence of a nerve, it was by an operation of the *vis nervosa*. We repeat our assertion, then, that if the *vis nervosa* of Haller be intended by Dr. Hall, he has no right to limit it to his excito-motor system of nerves, since it is also the motor influence by which volition and emotion operate upon the muscles. If we are wrong, let Dr. Hall prove our error from Haller's works. His views on the *vis nervosa*, however, are undergoing so constant a change, that he would probably find it more convenient to abandon all reference to Haller, and give to the term a new definition. How, for instance, can the following be stated of a power which is defined as originating in the nervous centres, and as transmitted from them along the peripheral nerves? "I can discover no reason for thinking that the true spinal system differs, either in its essential structure or properties, in any of its parts, from its minute origins in the nervo-cutaneous and nervo-mucous surfaces, in its incident, in its central, in its reflex portions, or in its ultimate distributions in the muscular or nervo-muscular fibres. All is mere difference of *form*, mere morphology. The structure, the animating principle, is the same in each and every part, only modified in the incident, central, and reflected portions. *In every part of this system, the vis nervosa is seated, is operative.*" Again, "nerves are *not* mere *carriers*. A muscular nerve *has* the motor power within itself; so has the spinal marrow; and, as I have recently discovered and proved, so have the incident nerves." "We really know nothing of the matter," adds Dr. H., "except the *facts*, which *experi-*

*ment* and *observation* bring to light, however the biblio-physiologist may dogmatize on the subject." In this last assertion we most heartily concur; and we are quite willing to leave to our readers the decision of the following questions, by which it will be known who keeps closest to facts, and who dogmatizes least, ourselves or Dr. Hall.

1. Is it consistent with *anatomical* facts to assert that the *central* portion of the spinal system of nerves (and, of course, of the system correspondent to it in the invertebrata) differs from the nervous trunks only in *form*? or is it not rather the fact that we universally find the fibres of the incident nerves losing themselves, and those of the motor nerves originating, in a substance chiefly composed of a plexus of blood-vessels?

2. Is it not most consistent with physiological facts to assert that in this central nervo-vascular substance the motor influence originates, and that it may reasonably be regarded as of similar character, whether excited by a mental act of volition or emotion, or by a stimulus conveyed thither from the circumference; rather than to assert that volitions and emotions themselves travel along the motor nerves, and that the motor influence may originate independently of the central organs, and even travel *towards* them?*

In regard to the so-called *retrograde* course of the stimulus along the spinal cord, again, Dr. Hall greatly misrepresents our meaning, which was simply this: We fully admit that a stimulus conveyed to the spinal cord by an incident nerve, between its anterior and posterior extremities, or an irritation of the spinal cord itself in the same situation, may give rise to motion in either or both those parts; but we merely say that, looking at the true spinal cord in the light of a continuous chain of independent ganglia, we do not see that either of these actions is more direct or more retrograde than the other. Every one knows that the desire to evacuate the fæces or urine, caused by an impression conveyed to the lower part of the spinal cord by the afferent nerves, will cause a number of sympathetic or reflex contractions in the muscles of respiration, and in others which derive their motor nerves from the upper part of the column; and to speak of this as an instance of *retrograde* action, separating it from that to which it is most nearly allied, appears a pernicious use of terms, and calculated to lead into error. It *has* led Dr. Hall into what we believe the great error of supposing that a motor influence travels *towards* the centre, in the operation of the spinal system; at least, we can find no other ground for that opinion than facts analogous to this.

Further, we fully admit the importance of the fact in pathology; but we assert that it was well known long before Dr. M. Hall's researches on this subject commenced; as, for example, in the case of traumatic tetanus. (See vol. III, p. 39, and V, p. 539.)

Will Dr. Hall inform us which is the *direct* and which the *retrograde* course of the reflex action in the nervous circle of the radiata; or in the

* Dr. Hall may perhaps point to the well-known fact, that pinching or otherwise stimulating a motor nerve in its course will give rise to muscular contraction, and may thence argue that the *vis nervosa* is entirely independent of the central organ; but he will do well to remember that the same argument would hold in regard to the sensory nerves, and would go to prove that their extremities and the organs at which they terminate are not the real seat of the sensory impressions. For our own explanation of the fact, see vol. V. p. 494, note.

ganglionic column of the articulata? When the idea of a *head*, containing a brain or something analogous to it, is put out of the question (as, in studying the phenomena of the spinal system, we are called upon to do) we can see no reason why the operation of a stimulus applied to the centre of the spinal cord, or to one of the incident nerves of the same part, can be said to more *retrograde* when it sets the arms in motion, than when it affects the legs. And if this be not assumed, the doctrine that a motor impulse travels *towards* the spinal cord will be found, we believe, entirely destitute of support. But so far are we from wishing to dogmatize on the question, that we would prefer leaving the discussion of it to our readers themselves; and when Dr. Hall has adduced more satisfactory evidence on behalf of his view of the case, or can show that any two physiologists of note have embraced his views, we will give them once more our best consideration. And we trust that we have shown that we are not so wedded to error, even when it has been advocated by ourselves, as to be unwilling to receive truth. One excuse, however, we may subjoin, that it is not always easy to *understand* Dr. Hall's opinions. Let our readers judge for themselves from the following paragraph:

"To me it appears that the motor power acts along an incident nerve, as the vibrations of a ray of light or sound pursue their incident course; that the true spinal marrow induces a sort of polarization, and directs this motor force in new but defined sources of double or multiplied refraction or reflexion, inscrutable in their essence, and only to be ascertained by their effects in the destined and definite movements they induce." (Preface, p. xiv.)

No lover of physiological science can read Dr. Hall's present work, notwithstanding all its peculiarities, without feeling the mind introduced to the consideration of numberless questions of the most interesting nature. If the new facts presented are fewer than the author deems them to be, many old and familiar ones are placed in a new and striking light; and some parts of pathology, before vague and obscure, are illustrated with a clearness which is the more satisfactory because it is the evident result of legitimate and laborious research. Who can fail to lament that the just title thus raised by Dr. Hall to the gratitude of the profession and of posterity, should be impaired by countless manifestations of a fretfulness unworthy of the weakest mind; by an impatience of free criticism, carried even to a ridiculous excess; and by such frequent and such round assertions of his own merit as cannot but stamp him with the character of obtrusive and extravagant vanity as long as his book continues to be perused? Nothing less will satisfy Dr. Hall than to be placed at once side by side with Bacon, and Newton, and Herschel, and Haller, and Harvey. If any one presumes to doubt his possessing a mind of this exalted and capacious class, he rebukes the critic as ignorant and malignant; as youthful above all; forgetting that he himself aspired to be a teacher [of diagnosis] before he ceased to be a student, and that, in another part of the volume, he names, as among his reviewers in this Journal, one gentleman, at least, whom he must know to be more advanced in years and of higher professional standing than himself. There is scarcely a chapter of his work which is not disfigured by an

eagerness to show that no former writer on the nervous system knew anything of the views to which his genius has attained; and that all sure nervous pathology must for evermore rest on the basis of his discoveries. Assuredly nothing can be less *Newtonian*, to use Dr. Hall's favorite expression, than these vaunting aspirations.

Engaged in a pursuit of undeniable dignity and importance, and adding useful matter to the stores of physiological and pathological knowledge, one might have hoped that the irritable system of Dr. Hall would have been calmed by the sweet influences of philosophy; but such is far from being the case. In every step of his great and useful investigation, he turns away his eyes from elevated subjects, casts them on earth's pavement, and hurls defiance and wrath and scorn at societies, and reviewers, and private rivals. Blind, apparently, to the high reward which awaits all diligent followers of truth, he bewails in unmanly terms, his exclusion from worldly rewards of honours or money, which, with characteristic candour, he avows that he thinks he has deserved. It is impossible for a man of science to exhibit himself in a more melancholy light than this. Yet such an exhibition of the workings of mere intellectual ambition may usefully show the utter worthlessness of mere intellectual endowment. There is a wisdom, we believe, as well as a happiness, which has its root in the affections and moral qualities; and where it flourishes, the mind expatiates freely and cheerfully beneath its modest shade. In our time this source of wisdom is too little sought. The fruit of knowledge is too often gathered as a mere means of procuring worldly distinction and advantage; and if these do not follow, the blessed fruit itself turns but to bitterness. Hence arise heart-burnings, and jealous controversies, and all that so often makes the minds of scientific men unamiable, and their lives a waste of sour dissatisfaction; and all that so often causes them, when collected into societies, to constitute but a community of angry and stinging insects, whose foibles and whose fierceness console and amuse those who possess no science at all. Even Dr. Hall, aspiring as he does to the fame of Bacon, forgets one at least of the limitations of knowledge set forth by that great and imperfect man in the outset of his work on its proficience and advancement: "that we make application of our knowledge to give ourselves repose and contentment, and not distaste or repining."

The man of science, to be happy, must pursue science with exalted aim; forget that there are Royal societies and books of periodical criticism; and, knowing how small a corner of the curtain that hides all truth from man's gaze can be lifted up by any one hand, should be charitable towards those labouring, like himself, for all time. In this spirit, with unfeigned sorrow we say it, Dr. Hall appears to us to be eminently wanting: and, great as his merits unquestionably are, and praiseworthy and admirable his labours, this defect will detract from his just reputation until, in the course of years, the trifles which trouble the surface of science are swept down the gulf-stream of human events, and the solid deposits alone remain to aid in the formation of the great continent of truth.

## Art. XVII.

1. *Memoir on the Radical Cure of Stuttering by a Surgical Operation.* By J. F. Dieffenbach. Translated from the German by Joseph Travers.—*London*, 1841. 8vo, pp. 27.

2. *Du Bégaiement, &c.* Par le Dr. C. Phillips.—*Paris*, 1841. pp. 63. *On Stammering, &c.* By Dr. C. Phillips.

3. *Stammering and other Imperfections of Speech treated by Surgical Operations on the Throat.* By James Yearsley, m.r.c.s.—*London*, 1841. 8vo, pp. 46.

4. *On Stammering and Squinting, and on the Methods for their removal.* By Edwin Lee, m.r.c.s.—*London*, 1841. 8vo, pp. 88.

The sanguinary operations which have been recently devised and executed, with the view of curing stammering, are one of the greatest outrages upon modern surgery. Although some of them had their origin in legitimate motives, most, we fear, serve but to show what ruthless expedients will be occasionally resorted to for the purpose of acquiring professional fame, however short-lived, and to what extent the ignorant and credulous will become a prey to craft and subtlety. If our indignation was awakened at the barbarous cruelties practised upon dumb animals for the sake of elucidating the truths of physiology, how much more ought it to be when we consider the multitude of our fellow-beings who have suffered themselves to be maimed and mutilated at the instigation of individuals more remarkable for their reckless use of the knife than for the soundness of their medical science!

Any one who calmly and dispassionately studies the phenomena of stammering will be convinced that it is not referrible to the defect of any single part, but, as Sir Charles Bell has pointed out,* to imperfection in the power of combination of the various parts which are essential to produce the simplest articulate sound, and depending on some derangement of the nervous system. Indeed, it is well ascertained that persons who have stammered in the highest degree, have been remarkable for the perfect integrity of conformation and structure of all the organs of voice and speech; while others who have laboured under a faulty or diseased condition of these organs have preserved their articulation unimpaired. There are even several well-authenticated cases upon record of individuals who have been able to discourse fluently, notwithstanding the absence of the tongue.†

In corroboration of the nervous and spasmodic view of the affection in question, we would merely allude to the well-known influence exercised by mental emotions in either aggravating or removing for a time defective utterance, and to the established fact of the efficacy of judicious mental and moral training in accomplishing generally a perfect cure. Every one is familiar with the classical instance of Demosthenes, who, although he stuttered while a youth, became the most distinguished orator of antiquity.

The work of Professor Dieffenbach is entitled to consideration from the eminent position its author has acquired as an operative surgeon. How far his reputation will be enhanced by its publication, so injudi-

* On the Organs of the Human Voice, Phil. Trans. 1832.

† Jussieu in Mém. de l'Académie Royale des Sciences, 1718, and Phil. Trans. 1742.

ciously premature, is a most doubtful matter. The translation by Mr. Joseph Travers is a very creditable performance. The author informs us that after his attention had been directed for a considerable time to the painful condition of the stutterer, he came to the conclusion that the difficulty of pronunciation emanated from the larynx, was communicated to the tongue, and that in order to subdue it the sole means was division of the nervous and muscular agents at the superior part of the base of the last-named organ. He was led to frame the above hypothesis from analogy, that is to say, from witnessing the almost invariably favorable result which followed the division of the muscles of the eye, in what is called nystagmus bulbi.

However ingenious and plausible the above views may appear, we do not believe they will stand the scrutiny of physiological enquiry. In nystagmus bulbi the muscles of the eye are alone affected, and by cutting them across we may no doubt modify their irregular action; but we cannot conceive how a transverse section of the whole muscular substance of the root of the tongue can induce any permanent change on an apparatus so complicated as that required for the production of speech. It is ascertained that the compression of the thorax, the adjustment of the larynx and glottis, the motions of the lips and tongue, and the actions of the pharynx and palate, must all consent before a word be uttered. Now we have been given to understand that experience has demonstrated the fallacy of the theory; for out of thirty-two cases in which the professor has operated, not more than three have derived anything beyond temporary benefit. The marvellous effects stated to have followed the operation in the first instance, are in all likelihood due to the sudden shock given to the nervous system in highly-impressible subjects. We have been told of a well-authenticated case of a German officer who was cured of stammering through a sword-wound on the face, received in a duel.

The Berlin professor has given trial to the three following methods of operating; they have equally for their object total division of the root of the tongue: "I. The transverse horizontal division of the root of the tongue. II. The subcutaneous transverse division, in which the mucous covering of the tongue is left inviolate. III. The horizontal division, with excision of a wedge-shaped portion." (p. 9.) The last method is the one upon which he places most reliance, "as here some shortening of the tongue must necessarily ensue; and forasmuch as the base of the wedge-shaped slice is made from the dorsum of the tongue, elevation of the tip must take place. This method, then, mechanically assists that organ to assume the position insisted on by those teachers who have been most successful in ameliorating this defect." (p. 10.)

The mode of performing the operation, which was done for the first time on the 7th of January last, is thus described:

"The boy sat with his head leaned against the breast of an assistant; the tongue being protruded as far as possible, was grasped on its anterior half with the forceps of Müzeux, being thus compressed laterally, and drawn forward by one assistant. The gentleman against whose breast the boy's head rested retracted the angles of the mouth with a pair of blunt hooks. Grasping now the tongue as near to its root as possible, between the thumb and forefinger of the left hand, I passed the bistoury through it, and divided it completely from below upwards; a strong ligature, passed through the posterior

edge of the wound, served to fix it temporarily, and prevent too great a strain upon the slender band which alone connected the mass of the tongue to it; the anterior lip of the incision was now grasped, and laterally compressed between the modified hare-lip forceps and a wedge-shaped slice excised out of the whole thickness of the tongue. It will be found more convenient to make this second incision from above downward, and with a small straight knife. The posterior edge of the wound was now, by means of the before-mentioned ligature and a sharp double hook, drawn so far forward that the needles with the ligatures could be conveniently passed through it; six strong sutures served to bring the edges of the wound together and to restrain the hemorrhage. To effect the latter object, they must include the whole depth of the wound within their loop. That the hemorrhage was considerable may be imagined from the nature of the operation, which should not be attempted by all persons indiscriminately." (p. 13.)

For the simple transverso-horizontal division of the root of the tongue, the following procedure was adopted:

"The tongue being fixed as in the foregoing case, and its root cut transversely through, six strong sutures were applied, which brought the wound effectually together. As in the preceding cases, the bleeding was considerable at the time, but completely stopped by the application of the sutures." (p. 20.)

Subjoined is the mode of effecting "subcutaneous division of the root of the tongue:"

"The tongue being drawn as much forward as possible, I pushed a curved bistoury through it, as near its root as I could, and cut through its whole muscular thickness, leaving the mucous membrane inviolate; on the withdrawal of the knife the opening appeared only of the breadth of the blade. The substance of the organ was so completely cut through that a slight additional pull with the forceps would probably have torn it off. The blood streamed from the apertures made by the knife as vehemently as from a large artery, whilst at the same time the cavity of the wound became exceedingly distended with the rapidly-flowing blood. This cavity I sought to diminish by introducing a strong suture from behind forwards through the tongue, and with other sutures I closed the openings made by the bistoury on each side." (p. 22.)

The results of the last-mentioned methods of operating were by no means satisfactory.

The few concluding pages contain some details, *in extenuation*, touching the treatment of wounds of the tongue, the removal of the sutures, and the phenomena of cicatrization. The following summary of "contingencies rationally to be feared, and which must be carefully weighed beforehand," more especially when coupled with the fact of the very doubtful success of the operation, will, we think, deter any rational surgeon, albeit endowed with "the temperament of an operator," from again repeating the operation in this country.* "The extent and importance of the operation, the possible danger to life, or loss of the tongue, either through the want of skill in the assistants, who may tear it off when so nearly separated, or through mortification or ulceration of its connecting isthmus." (p. 26.)

The portion of M. Phillips' *brochure* devoted to the subject of stammering occupies twenty-two pages, whereof nearly nine are filled with

* It has been twice performed in London, first by Dr. Franz, afterwards by Mr. Bennet Lucas. We have been informed that the result of Dr. Franz's operation, which was skilfully done in strict accordance to the precepts of Dieffenbach, has proved a failure. The effects of hemorrhage in the first instance were most alarming. For an account of the latter case, the reader is referred to the Provincial Medical and Surgical Journal for May, 1841.

extracts from M. Amussat's communications to the Academies of Sciences and Medicine, and about three with the details of Dieffenbach's plans of operation, leaving a balance of ten for the observations of the author, which evince too controversial a spirit to interest our readers. He modestly tells us, in concluding, "*Cette petite esquisse historique est aussi complète que possible*"! (p. 63.)

M. Amussat, assuming that the cause of stammering resides most frequently in faulty conformation or undue contraction of the genio-glossi muscles, together with shortening, deviation, or deformity of the tongue, recommends and practises for its relief the section of those muscles near their attachment to the tubercle on the inside of the symphysis of the chin. The number of subjects operated upon by him up to the middle of March last were thirty-three, whereof three were females. The results obtained, he assures us, were quite satisfactory; the cessation of the stuttering supervening sometimes directly after the cut, sometimes not before the lapse of a few days. In a certain number the defect altogether disappeared; in others again there was only a slight improvement. In six cases the mere division of the frænum, together with the mucous and fibrous textures lying upon the muscles, sufficed to effect a cure. The following procedure, suggested by M. Bonnet, (Gazette des Hôpitaux, Avril, 1841,) which is but a modification of that of MM. Amussat and Phillips, appears to be the safest mode of accomplishing the section in question; since considerable hemorrhage and inflammation have occasionally followed the other methods described.* According to M. Bonnet, an opening is to be made beneath the chin. You thus avoid all injury of the mucous membrane of the mouth, all effusion of blood into that cavity, and while you completely sever the genio-glossi muscles, you do not interfere with the genio-hyoidean portion, and at the same time obtain all the advantages resulting from subcutaneous incisions, namely, immediate cicatrization of the external wound, without any risk of suppurative inflammation. He directs a puncture to be made along the median line three or four *centimetres* behind the chin. Through this puncture he introduces a blunt *tenotome*, making it penetrate from below upwards, and a little from behind forwards, its cutting edge being turned towards the jaw. When it has arrived just beneath the mucous membrane of the mouth, as is ascertained by passing the left index finger into that cavity, he feels for the little bony tubercle and cuts away from it both right and left, keeping the edge of the instrument always towards the lower jaw. By this precaution you secure the section of the genio-glossi muscles at their bony insertion. The *tenotome* is not withdrawn until the finger again introduced into the mouth enables the operator to feel that its extremity is in contact with the mucous membrane, that the fibrous texture connecting that membrane with the jaw is wholly severed in the mesial line, as recommended by M. Amussat, and that there are no longer any muscular fibres adherent to the above-mentioned tubercle. In this way, the genio-glossi muscles being cut across in their aponeurotic portion, we do not interfere with the layer of cellular texture placed beside them, nor incur any risk of wounding the lingual arteries. There is very little blood lost. The patient can generally speak and move about on the next day, complaining only of a little stiffness in the movements of the mouth.

* Revue Médicale, Mars, 1841, p. 460. Phillips, p. 46.

This operation, unluckily for its author, has failed in other hands. M. Guersent jun. states (Gazette des Hôpitaux, Avril, 1841,) that out of nine cases in which he had tried it, one only was anywise benefited—an issue far from encouraging. Besides, we cannot tell what may be the ulterior consequences of severing these muscles from their bony attachments. We know that they offer the sole resistance to the movements of the tongue backwards; and we also know that in certain operations about the jaw, imminent danger of suffocation has ensued from the tongue being reversed upon the epiglottis and larynx where these had been cut across. It is true that the preservation of the mucous membrane may tend to counteract so grave an accident; still we are of opinion, that from all the evidence we have been able to collect, there is no good ground to sanction the above operation being performed under ordinary circumstances.

Mr. Yearsley informs us in his pamphlet* that nearly 200 persons, affected with every variety and degree of stammering, have passed under his notice. Of these a large proportion presented enlargement of the uvula and tonsils, while many were short-breathed, exhibiting the narrowing of the chest termed chicken-breasted. The only novelty to which he lays claim "is the promulgation of the facts, that, *in the great majority of stammerers, the tonsils and uvula are in a diseased state, and may be removed with advantage, and that these operations may in particular be applied to the relief of stammering and imperfect speech.*" (p. 11.)

According to him, spasmodic closure of the fauces from obliteration of the isthmus, is the chief if not the sole cause of stuttering. The theory of *nervousness* he wholly repudiates, believing it "to be most erroneous, and applicable solely to a few cases where the stammer leaves the patient sometimes for several days together, and then suddenly recurs." (p. 24.) In short, the very pith and marrow of his doctrine may be enunciated in the words of the Mantuan poet, "vox faucibus hæsit."

The most beneficial effects, he assures us, occurred in the most aggravated cases, and those longest operated upon were most advanced to a perfect cure. He excises the tonsils with a knife, and amputates the uvula with a pair of curved scissors. He observes that no accident of any consequence has happened in the performance of these operations, and in only one instance was the bleeding of sufficient importance to require any interference.†

A formidable and suspicious-looking array of cases is annexed in the

* The pamphlet is dedicated to the members of the Westminster Medical Society, "in return for the honour they did the author in inviting him, though not a member, to read a paper to them on the subject of his discovery." This would seem to be a misstatement; for it is declared in a report of a meeting of the above society of the 17th of April last, contained in the Lancet of the 1st of May, that "On referring to the minutes, it was found that Mr. Yearsley had *not* been *invited*, but by an act of courtesy in the society, had been *permitted*, although merely a visitor, to read the paper in question."

† As monkeys are the only animals which possess a uvula exactly resembling that of man, we would suggest to Mr. Yearsley a trial of its excision in them, as a curious matter for speculative investigation. Perhaps it might improve their chattering: who knows but in his hands it might make them talk? The doctors of the Sorbonne were puzzled why apes should not speak, seeing that they possessed all the organs of voice.

form of an appendix, several of which are reported as cured, others as considerably improved, while others remain unchanged. Now, we considered ourselves bound in common justice to Mr. Yearsley to examine some of the individuals above referred to, and for that purpose perambulated various "back slums" of the metropolis, till we arrived at their places of residence. The result of our inquiry, we regret to say, has been truly unsatisfactory. Indeed, we are thoroughly convinced that that gentleman has been most grossly and wantonly imposed upon by the crafty tribe of stutterers; for in some of the reputed cures we were told that the patients stammered as badly, or, if anything, worse, than before the operation. That the diseased condition of the tonsils and uvula, upon which the author lays such stress, may occasionally coincide with stammering, we readily admit; but that it is to be viewed as the cause of that affection, we utterly deny. Every surgeon in extensive practice has met with abundant instances of hypertrophied tonsils unattended with stammering. Nor is amputation of the uvula for impaired utterance a new thing; under that head Mr. Cooper distinctly recommends it in his Surgical Dictionary, when from enlargement "it becomes troublesome in deglutition and speaking." In conclusion, the expressions of "instant benefit" and "immediate relief," which we find pervading many of Mr. Yearsley's reports of cases, induce us to believe that the temporary good which has followed his operations is to be ascribed to nervous shock, and to nothing else. We have merely to add our entire conviction, that neither cutting a slice out of the root of the tongue, nor dividing the muscular bands which attach it to the jaw, nor "cutting the throat," can produce a sure and permanent cure of stammering.

Mr. Lee's slender volume reached us when the foregoing article was already in type. After some prefatory remarks on the voice and speech, the author reproduces the various opinions of Malbouche, Serres, Rullier, Magendie, and Colombat on the theory and cure of stammering, with judicious comments upon each. He then proceeds to detail very fully the different operative methods to which we have above adverted, and which, we trust, will ere long become obsolete in the practice of surgery. Mr. Lee evidently inclines to the division of the genio-glossi muscles as being the operation "in which the success has been the most permanent in bad cases of stammering." (p. 56.) He has, moreover, had the good fortune to see one "lucky hit" of Mr. Yearsley's, where relief from cutting the tonsils and uvula has continued to the present time (two months from the operation), and where the patient "can now speak with facility and without stammering." (p. 51.)

While we heartily concur in the conclusions Mr. Lee has deduced as to the nervous nature of the affection in the majority of cases, we take leave to differ from those which he has drawn as to the surgical treatment. We can, however, recommend his work to such of our readers as are desirous of making themselves master of this new subject, as by much the completest and best yet published on it in this country.

## ART. XVIII.

*Essai sur un Traitement méthodique de quelques Maladies de la Matrice. Injections intra-vaginales et intra-utérines.* Par AUG. VIDAL (DE CASSIS), &c. &c.—*Paris*, 1840. 8vo, pp. 60.

*Essay upon the methodical Treatment of certain Diseases of the Womb by Vaginal and Uterine Injections.* By AUG. VIDAL, &c. &c.—*Paris*, 1840.

FOR the last ten years M. Vidal has strenuously opposed what he conceives to be the abuses committed in amputating the cervix uteri. He has taken great pains to investigate diseases of the uterus, both in his hospital and private practice; and in this brief essay he gives us the views he has formed, and a description of the practice he has found successful. The chief point to which he directs our attention is the employment of intra-uterine injections, which he considers very valuable in certain diseases of the uterus. He is aware that many are of a different opinion; that these injections have often been unsuccessfully employed; and that very serious objections have been urged against their use. He has deliberately reflected upon the arguments against him, and he concludes that the different results of the treatment he advocates in the hands of others, and the different opinions they have formed arise from the following causes: 1st, Some difference in the mode of using the remedy; 2d, Errors in diagnosis; 3d, A mistaken interpretation of the phenomena produced by the injections. M. Vidal first gives a coup d'œil of those uterine diseases which especially require injections, and he carefully describes the exact practice he has adopted in the hospital for women at Lourcine, that others may apply it in the proper way.

Leucorrhœa forms the first subject of the essay. The various local lesions that produce the discharge in this disease are pointed out, as well as the constitutional maladies which aggravate and render the discharge so tedious and difficult of removal, and which require general remedies, as well as local applications to relieve the disease. The anatomical characters of the diseases to which M. Vidal applies his treatment are either, 1st, engorgement; 2d, redness; 3d, ulcerations. The first are either acute or chronic, and the uterus itself is generally affected. Where redness exists the cervix uteri is always more or less enlarged. It either precedes or follows ulceration. If it follows ulceration, irregular spots are found upon the cervix, resembling those which are seen after chancres or pustules. The degree of redness varies under different circumstances. The ulcerations are either simple or granular. The former are very superficial, and exist with or are the consequence of inflammation of the vagina, which often extends to the uterus, and even to the ovaria. Granular ulceration is deeper, and presents the appearance of a suppurating wound. This form of ulceration is much more obstinate than the superficial, and is more common in women who have borne children. The os uteri is generally first affected with it, and "frequently in raising one lip of the os with the forceps an ulceration is detected which was not expected." The discharge arising from the ulceration varies in consistence and colour. Therefore the term *whites*, "fleur

blanche," is not always applicable, for the discharge is often yellowish or greenish, and sometimes purulent and opaque, or at others, mucous and transparent. The more transparent the discharge is, the less serious is the disease. The following is the treatment adopted at Lourcine:

*Vaginal Injections.* The injection consists of a very concentrated decoction of walnut leaves, of the temperature of the room whatever may be the season. The instrument employed is a large clyster syringe. The speculum with two valves is first applied for the purpose of bringing the cervix uteri into view, upon which the stream of injection is thrown with as much force as the assistant can urge the piston. Immediately afterwards a plug of lint is placed upon the cervix, which imbibes the liquid that has remained in the vagina. The liquid thus injected presses upon the cervix and lowers its temperature; it acts also as an astringent. It rarely happens that the injection gives any pain when it is employed; but when the patient is put to bed, pains like colic around the hypogastrium and iliac regions sometimes arise. M. Vidal has frequently observed these pains when the cure has commenced, and so far from fearing them he looks upon them as favorable. These vaginal injections have frequently hastened in a very marked manner the cure of long-standing engorgements, as well as the healing of very obstinate ulcerations. The injections are repeated twice a week. They are omitted during the menstrual period. They are not employed during pregnancy, nor for three months after labour or miscarriage. If the cervix is very red and turgescent, leeches are applied to the anus, and hip baths or entire baths complete the treatment.

*Uterine Injections.* Some discharges resist the vaginal injection, as sometimes there are lesions of the internal surface of the cervix and of the uterus which are not reached by it. In these cases M. Vidal injects the uterus itself; and for the purpose of proving that this practice may be adopted without a risk, a number of experiments were tried, of which a detailed account is given. The mode of throwing injections into the uterus is also described, and especial cautions are given that the rules laid down may be strictly followed. In a former Number* we have noticed the dangers that occasionally arise from injecting the cavity of the uterus, and we therefore deem it proper to state briefly the effects observed by M. Vidal in his extensive practice. He found that in the majority of cases no pain was caused when the injection was used. In two instances only severe pain was caused, and in these the injections were discontinued. In some cases pains like colic occurred about an hour after the injection was used, which quickened the pulse and produced symptoms of reaction which alarmed the practitioner. But the occurrence of these pains was a rare exception, and did not happen once in ten times. In a brief appendix M. Vidal replies to certain objections which have been urged against the treatment he recommends.

* Br. and For. Med. Rev., vol. XI. p. 179.

## PART SECOND.

# Bibliographical Notices.

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Art. I.—*The Physiology of Vision.* By William Mackenzie, m.d., Surgeon Oculist in Scotland in Ordinary to her Majesty the Queen. —*London*, 1841. 8vo, pp. 292.

Since the publication of Dr. Porterfield's admirable Treatise on the Eye, eighty-two years ago, no work on the subject of any consequence has appeared in this country. In Müller's Physiology, translated by Baly, we have, indeed, an excellent summary of the physiology of vision; but a separate work was still a desideratum in our medical literature; at least, a work which, being the production of one who knows mathematics as well as anatomy, might be consulted by the student without the risk of his being seriously led astray by any heterodox statement in either of these departments of science. It is scarcely necessary for us to mention to our readers that the mere mathematician has committed sad blunders from not knowing the structure of the eye, and the anatomist from ignorance of optics. For example, a mathematician tells us that the centre of the spherical surface of the retina and the centre of the lesser spherical surface of the cornea are the same; but were we to draw an eye on this principle, we should have a very odd delineation. An anatomist, on the other hand, will perhaps state that the lens is the principal agent of refraction in the eye, and that consequently when it is lost, as after operation for cataract, the refractive powers of the eye are greatly weakened. Now the cornea, and not the lens, is the principal agent of refraction in the eye, and the power of the lens is insignificant in comparison. But we can assure the student that he will find no such inaccuracies in the volume before us: on the contrary the work will prove to him a very safe and intelligible guide in the study of one of the most interesting of subjects to the contemplative mind. Dr. Mackenzie gives as a motto a quotation from Sir John Herschel, with which, as it points out so admirably the beauty of the study of the organ of vision, we will conclude this notice.

"It is the boast of science to have been able to trace so far the refined contrivances of this most admirable organ; not its shame to find something still concealed from its scrutiny; for however anatomists may differ on points of structure, or physiologists dispute on modes of action, there is that in what we *do* understand of the formation of the eye so similar and yet so infinitely superior to a product of human ingenuity; such thought, such care, such refinement, such advantage taken of the properties of natural agents used as mere instruments for accomplishing a given end, as force upon us a conviction of deliberate choice and premeditated design, more strongly, perhaps, than any single contrivance to be found, whether in art or nature, and render its study an object of the deepest interest."

ART. II.—*A General Outline of the Animal Kingdom, and Manual of Comparative Anatomy.* By THOMAS RYMER JONES, F.L.S., Professor of Comparative Anatomy in King's College, London; Fullerian Professor of Physiology to the Royal Institution, &c. &c. Illustrated by 336 Engravings on Wood.—*London*, 1841. 8vo, pp. 732.

WE have refrained from noticing this work during its somewhat irregularly-periodical issue, because we deemed it better that the whole should be in our hands before we should introduce it to our readers. "Its object," in the words of the author, "is twofold: first, to lay before the naturalist a complete view of the organization and physiological relations of every class of living beings; and, secondly, to offer to the anatomical student a succinct account of the structure and development of the vital organs, through all the modifications they present in the long series of the animal creation." Each class of animals is separately considered; its principal types of conformation being at first sketched, and a detailed account being then given of the structure of the several organs in each of these. The naturalist will here find a rich store of information, in regard to the internal structure of the beings with whose form and classification he concerns himself; on a knowledge of which alone he can safely rely to guide him in his labours. And the physiologist may here trace the development of any system with which he is concerned, through the ascending scale; and may readily acquaint himself with its relations to other systems in each class. The plan is an admirable one, and well calculated to meet the wants of those who desire to gain such a general knowledge of comparative anatomy as may serve them effectually in their more detailed enquiries in either of these allied branches of science.

In regard to the execution of this scheme, we have little but commendation to bestow. Professor Jones has collected his materials from the most authentic sources, judiciously selected those best adapted to his purpose, and arranged and combined them in a manner that exhibits a thorough acquaintance with the principles as well as with the details of his subject. On a few points, however, we do not think that he is quite master of the results of late enquiries, which might have been advantageously introduced. His account of the structure of the polypifera, for example, is encumbered with a redundance of details on some points, in which great simplification has recently been effected; and the classification adopted by him is by no means in accordance with the views of those who have most diligently studied that very interesting group. The embryological details, also, of the development of birds and mammalia, are not modified, as they should have been, in accordance with late enquiries, but follow the imperfect history of some years' date. These are, however, almost the only faults that we have to find with this admirable work; which we can recommend to our readers as adapted, more than any other with which we are acquainted, to furnish them with a competent knowledge of the principal types of conformation encountered in the study of the animal kingdom. The wood engravings are beyond all praise. With the exception of Wilson's Manual of Anatomy, we have never met with a work so richly illustrated; and our only regret is that its price must keep it out of the reach of many who would otherwise have been glad to possess it.

ART. III.—*Transactions of the Entomological Society of London.* Vol. II. Parts III. & IV.—*London*, 1839-40. 4to.

WE are more particularly induced to notice this work, from the circumstance that it contains some interesting communications on the occasional presence of insects or their larvæ as parasites in the human body. We refer more especially to two papers, the one by the Rev. Mr. Jenyns in the third part of this volume, and the other by the Rev. F. W. Hope in the fourth part.

In Mr. Jenyns's paper are detailed the circumstances of a well-authenticated case of the existence of an immense number of larvæ of a dipterous insect, supposed to be the *anthomyia canicularis*, in the alimentary canal of a patient, under the care of Dr. Haviland of Cambridge, in the autumn of 1836. This patient was a clergyman, seventy years of age, and the symptoms of which he complained, previously to the first expulsion of the larvæ from the bowels, as stated by Dr. Haviland were :

"General weakness, loss of appetite, and a disagreeable sensation about the epigastrium, which he (the patient) described as a tremulous motion. These symptoms commenced in the spring of 1836, and it was not till the summer and autumn of that year that the larvæ were observed in the motions. They then passed off in very large quantities on different occasions, the discharge continuing for several months. According to the patient's own statement, the chamber vessel was sometimes half full of these animals, at other times they were mixed with the stools. He thinks that altogether the quantity evacuated must have amounted to several quarts. The larvæ were all nearly of equal size, and, when first passed, quite alive, moving with great activity. The patient is not aware of having voided anything of the kind before." (pp. 152-3.)

Mr. Jenyns remarks that, after the discharge had ceased, the patient's health was improved, but not completely reestablished. He observes also that these larvæ appear to be similar to those in a case recorded by Dr. Bateman,* that they were most likely introduced into the stomach in the state of ova, and had remained there, and in the intestines, until expelled by stool, a supposition highly probable from the fact that, according to Degeer, the *anthomyia* usually reside in the larva state in the ordure of privies, cesspools, and other filthy places, for which their organs of respiration, minutely described by Mr. Jenyns, peculiarly adapt them.

In Mr. Hope's paper are collected together a great number of cases recorded by many of the most distinguished naturalists and physiologists, in which insects or their larvæ have been found in the human body. These are arranged in the form of tables, which appeared in an imperfect state, some time since in the Medical Gazette. The disease occasioned by insects or their larvæ was some years ago designated by the distinguished naturalist the Rev. Mr. Kirby, *scholechiasis*. Mr. Hope proposes to retain this term for the disease only when occasioned by the larvæ of *lepidopterous* insects, and to use that of *canthariasis* when the disease results from the presence of the larva or perfect individuals of coleoptera; and *myacis* when induced by the larva or perfect state of diptera. We certainly do not perceive the advantage of employing these different terms to indicate the same disease, because

* Edinburgh Medical and Surgical Journal, vol. vii. p. 41.

occasioned by a different *species* of insects, since the symptoms of the existence of the parasite will vary, perhaps not so much in consequence of the different kind of animal as of the different part of the body in which it is occasioning irritation, and inducing the disease by its presence. To us it appears more correct to retain the term originally proposed as a kind of *generic* distinction, and to use those now proposed by Mr. Hope, or others that may appear more suitable, as *specific* terms, in describing the *immediate* object occasioning the disease, especially as the symptoms occasioned by all kinds of insects are very similar, and as the species cannot be known until some of the parasites have actually been expelled from the body.

On examining these tables, we have been struck with the circumstance that more than one half of the whole of the cases recorded have been occasioned by the larva of *dipterous* insects, a circumstance worthy of remark, from the fact that these larvæ, from their structure and habits, are best adapted to reside in closed moist localities, and consequently are those most likely to exist as parasites in the body. Much uncertainty prevails as to the manner in which these animals are usually introduced into the cavities of the body, and the more so from their having been found, especially the dipterous larvæ, in organs in which it is hardly possible to conceive they could have obtained an entrance. On inspecting Mr. Hope's tables it will be seen that they have been found in the frontal sinus, in the nose, the bladder and urinary passages, the antrum maxillare, the stomach, and the intestines. Those which have been found in the antrum and frontal sinus have been almost entirely larvæ of the *œstri* and *muscæ*. On the reading of Mr. Hope's paper, we observe from the Proceedings of the Society, that Mr. Bracy Clark expressed his doubts on the occurrence of œstri in the alimentary canal or frontal sinus of man, and also of there being any species of œstrus peculiar to the human subject, as supposed, and named in the two cases of inflammatory tumour described by Mr. Howship, in the Proceedings of the Royal Society ;* the larvæ in those cases being regarded by him as those of the *œstrus bovis*. At a subsequent meeting, however, Mr. Newport related a case which had been communicated to him by Dr. Carter, of a female out-patient of the Canterbury hospital, who after suffering for a long period with severe anomalous pains in the region of the frontal sinus, at length discharged from the nostrils one perfect living larva of an œstrus, and subsequently portions of two others; after which the pains in the head ceased, and the patient recovered. He mentioned also that a species of *geophilus* had once been voided from the stomach of a patient under his own care. Professor Owen also detailed a case, and produced specimens of the larvæ of a dipterous insect, apparently similar to those in Mr. Jenyns's case, which had been sent to him by a medical man in the country, and which had actually been passed from the urethra by the patient, an aged gentleman, in the presence of the medical attendant. Some of these larvæ had been kept alive in urine for two or three days, but Professor Owen was unable to rear the perfect insect.

Mr. Hope suggests that these insects are conveyed into the stomach in the egg, or the early larva state with the food; but this will not explain how they are admitted into such localities as the antrum and frontal

* Vol. iii. p. 181.

sinus, and above all the bladder. That they are taken into the stomach with the food, in the state of ova, is, we think, highly probable; and that being developed in a locality the temperature of which is almost always the same, they not merely suffer no inconvenience from its great amount, but even thrive in that in which, under other circumstances, they would have perished, seems to explain the mystery of their continued growth and residence in the body. We consider the subject of their introduction into such localities as the urinary bladder as meriting the particular attention of the profession. That some animals of this class are occasionally swallowed in the unfiltered water drunk, more especially by the poorer classes of this metropolis, we have not the slightest doubt, since we have ourselves found the water supplied from the reservoirs, in the summer, swarming with living beings; not merely the common infusorial animals which exist in almost all waters, but with living and, in many instances, perfect insects. We may particularize several species we have noticed, among which the little crustaceous animal *cypris conchacea* is exceedingly common, the *notonecta*, or water boatmen, and the *asellus aquaticus* and some *planariæ*. When it is remembered that these may often be swallowed in the water drunk during summer by the poor, the presence of these creatures becomes a matter of consideration in regard to the public health, and leads us to suspect that many complaints, otherwise anomalous in symptoms, may perhaps, on close examination be traced to this cause. That their presence has sometimes been attended by fatal results is clearly shown in Mr. Hope's tables, which we regard as meriting the attention of the profession, and which we trust will incite its members to a closer investigation of this important subject.

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Art. IV.—*The Prescriber's Pharmacopœia: containing all the Medicines in the London Pharmacopœia arranged in classes according to their action, with their composition and doses.* By a Practising Physician.—*London*, 1841. Small 18mo, pp. 126.

The title-page of this little book indicates its general character; and the author's advertisement explains so well his reasons for compiling it and his grounds for thinking it likely to be useful, that we transcribe the greater portion of it:

"As this little work had its origin in a want daily experienced by the compiler in his practice, he believes that it will be useful to a numerous class of practitioners who possess a memory of the same general character as his own. He has always had great difficulty in calling to mind, at will, a number of heterogeneous particulars which had not been originally contemplated in a systematic order, although he might be well acquainted with them individually. This difficulty he has often painfully experienced when attempting to pass in review the various drugs and officinal formulæ suited to fulfil any special indication that presented itself in the treatment of a disease: and he has frequently witnessed, in the practice of others, a barrenness of prescription and a consequent confined range of resources, originating in the same cause, which could not fail to be injurious to the patient and the reputation of the physician. It is expected that the habit of viewing the articles of the Materia Medica in some such order as that adopted in the present compilation, will tend, in some degree at least, to remedy these evils.

"It is further believed that practitioners, from forgetfulness of the officinal preparations, are often led to prescribe extemporaneous formulæ while there

exist better substitutes in the pharmacopœia; and it is hoped that the marshalling together all the various compound forms of medicine under the title of the main ingredient, as in these tables, will at once facilitate and simplify the labours of the prescriber."

We shall be very much mistaken if the author does not find his expectations fulfilled, as we believe that the class of doctors with bad memories comprehends many beyond the pale of the College of Physicians. We extract the first article in the volume to show the manner in which the compiler treats his subject.

ALOE. (A. Spicata. The inspissated juice.)
1. Extractum Aloës purificatum.
*Dose.* Gr. ss–x–xv.
2. Pulvis Aloës compositus.
*Composition.* 3 parts aloes, 1 guaiacum, 2 pulvis cinnam. comp.
*Dose.* Gr. v–ʒj.
*Form.* Powder, pill.
3. Pilulæ Aloës compositæ.
*Comp.* 2 aloes, 1 extract of gentian, ol. carui et syrup. q. s.
*Dose.* Gr. j–x–ʒj.
4. Pilulæ Aloes cum Myrrha.
*Comp.* 2 aloes, 1 saffron, 1 myrrh.
*Dose.* Gr. j–x–xx.
5. Pilulæ Cambogiæ compositæ.
*Comp.* 3 aloes, 2 camboge, 1 ginger, 4 soap.
*Dose.* Gr. iij–x–xx.
6. Pilulæ Sagapeni compositæ.
*Comp.* 16 sagapenum, 1 aloes, syrup of ginger q. s.
*Dose.* Gr. v–x–ʒj.
7. Decoctum Aloës compositum.
*Comp.* Extract of liquorice ʒvij, carbonate of potass ʒj, aloes ʒjss, myrrh ʒjss, saffron ʒjss, water Ojss (boil down to Oj), compound tinct. of cardam. ℥vij.
*Dose.* ʒss–ij.
*Form.* In other fluids, or simple.
*Incompatible.* Acids, salts, &c. (See Carbonate of Potass.)
8. Tinctura Aloës.
*Comp.* Aloes ℥j, extract of liquorice ʒiij, water Ojss, rectified spirit Oss.
*Dose.* ʒij–ʒj.
*Form.* Single, or in other fluids.
9. Tinctura Aloës composita.
*Comp.* Aloes ʒiv, saffron ʒij, tincture of myrrh, Oij.
*Dose.* ʒj–ij.
*Form.* In other liquids, with other purgatives.
10. Vinum Aloës.
*Comp.* Aloes ʒij, canella ʒss, sherry Oij.
*Dose.* ʒss–ij. (Stomachic ʒj–ij).
*Form.* Simple, or in composition.
11. Enema Aloës.
*Comp.* Aloes ℈ij, carbonate of potass gr. xv, decoction of barley Oss.
*Dose.* Oss. (Enema for ascarides, &c.)

We are always so glad of an opportunity of recommending to our readers the non-restraint system of treating the insane, that we cannot resist the pleasure of transcribing the dedication to the Pharmacopœia, containing, as it does, so just a tribute to one of the highest ornaments of our profession.

"To John Conolly, Resident Physician of the Hanwell Asylum, In sincere admiration and grateful acknowledgment of his noble and successful efforts to ameliorate the condition of the poor Lunatic, by the substitution of a rational system of moral discipline for mechanical restraint, and parental care and kindness for violence and brute force, this little work is, on public grounds, inscribed by the Author."

Art. V.—*The American Medical Almanac for* 1841. By J. V. C. Smith, M.D. Vol. III. Continued annually.—*Boston*, 1841. 18mo, pp. 148.

This is a valuable little work, which we really envy America the possession of; as, since Mr. Farr discontinued his admirable Almanac, we have nothing of the same kind in England. Besides the usual contents of an almanac, it possesses all the requisites of a daily memorandum book, and contains many valuable statistical and medical documents. We have, for instance, a full account of the medical institutions of the United States and of Canada; the statistics of Insanity, and Institutions for the Insane, by Dr. Woodward; a manual of auscultation, under the modest title of "Short Sentences," by Dr. Bowditch; Statistics of the Massachusett's Hospital, by Dr. Turner; Statistics of Phrenology; an account of the operation for Strabismus, by Dr. Dix; an account of Dislocations, by Dr. Capen; of Dissection Wounds, by Dr. Lane; list of the Medical Journals, &c. &c. As a specimen of the materials composing this interesting little volume, we select the following, chiefly on account of its brevity:

"*Statistical Account of the most important Surgical Operations performed in Hartford County, Conn., for the last ten years preceding Jan.* 1*st*, 1840. With Remarks by A. Brigham, M.D., Superintendent of the Retreat for the Insane, Hartford, Ct.

"Hartford county, the northern border of which joins Massachusetts, and lies in latitude 42 deg. forms nearly a square, about 30 miles in length north and south, and 25 in width, and is intersected nearly in the middle by the Connecticut river.

"The soil is rich, various, and fertile, and for the most part highly cultivated. A great variety of manufactures are carried on in the county, employing in the cotton and woollen manufacture alone, in 1830, a capital of 600,000 dollars.

"There are 21 towns in the county, and numerous and pleasant villages. Population in 1830, 51,141; in 1840, 55,725. The following are the number of amputations and other important surgical operations which have been performed in this county during the last ten years; and I have no reason to believe they vary much from the number usually required in a population of 50,000. This account, therefore, may serve to undeceive and to benefit young physicians, who expect to obtain considerable income from surgical operations, and consequently neglect to qualify themselves so thoroughly as they should, to perform well the other and every-day duties of their profession, and upon which they must chiefly rely for support.

1. Amputations of the thigh, *ten;* for the following causes—*Five* for compound fractures with injury of joints; *three* for white swellings of the knee joint; *one* for fungus hæmatodes tumour of the leg; and *one* for mortification of the leg. Six recovered, two of the five cases of injury died, also the case fungus hæmatodes and that of mortification.

"2. Amputations of the leg, *four;*—*two* of them for accidents, *one* for frostbite, *one* for fungus hæmatodes. All but the last recovered.

"3. Amputations of arms, *eight*—five lower and three upper;—*six* for accidents, *two* for fungus hæmatodes; two of the accidental and one of the fungus hæmatodes died.

"4. Trephining, *five* cases; all but two died.

"5. Operations for strangulated hernia, *two;* both recovered. No other case has been heard of requiring an operation. Trusses are generally worn by those afflicted with hernia. Hull's most used.

"6. *Aneurism*—but one case requiring an operation. The popliteal artery was the one affected;—the femoral was tied and the man recovered.

"7. *Lithotomy.* No one so far as I have heard has required this operation in this county, for the last ten or even for the last thirty years. Cases of calculi are quite rare in Connecticut.

"8. There have been a few amputations of the breast of females; several operations for *hydrocele;* and a considerable number for the removal of enlarged *tonsils.*

"Though the county is a wealthy one, and comparatively but few poor people in it, yet the surgical operations alluded to have been mostly among the poor. Five of the ten persons who underwent amputation of the thigh were too poor to pay anything, and all but one of those who had the leg amputated. But two of the eight cases of amputated arm were able to pay, and only *one* of those trephined. The cases of hernia occurred in poor people, but the friends paid a little. The case of aneurism was in a town pauper. The whole sum realized from all the above mentioned amputations, trephinings, and operations for aneurisms and hernia is less than 350 dollars.

"There are 76 physicians in the county, most of whom obtain a good support from their professional business; but, as is very evident from the above account, they derive but little income from important surgical operations."

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ART. VI.—*A Winter in the Azores, and a Summer at the Baths of the Furnas.* By JOSEPH BULLAR, M.D., and HENRY BULLAR, of Lincoln's Inn.—*London*, 1841. Two vols. 8vo, pp. 375-390. With two coloured Lithographs and numerous Woodcuts.

WE opened these volumes solely with the view of seeing what the authors had to say of the remedial qualities of the mineral waters of the Azores, and the sanative influence of their climate. We must confess, however, that we did not part with the work until we had perused the whole of it, so cleverly and beautifully is it written, and so full of interesting details respecting places and people, of which and of whom so very little is known in this country. It has not been our fate for many a day to read so amusing and so pleasant a book of travels.

The mineral waters in the valley of the Furnas, in St. Michael's, are of five different kinds: a boiling alkaline water; a hot carbonated water; a mixture of hot alkaline water and cold carbonated chalybeate water; a highly carbonated cold chalybeate water; a tepid carbonated water. The chemical analysis and medicinal uses of these waters are given in an appendix, as is also a complete view of the climate of the islands. This last is singularly equable and mild, and cannot fail to be useful in certain forms of pulmonary and other chronic diseases. The hot baths are also well calculated to benefit persons affected with gout, rheumatism, skin diseases, &c., and they seem admirably suited to reduce the morbidly corpulent to more wieldy dimensions. They are in considerable requisition for this purpose with the natives, who seem to rival our very aldermen in this civic distinction. We cannot resist the temptation of extracting a few paragraphs on this head, which, in sportive language, contain much important truth, and afford a specimen of the charming stile in which these volumes are composed:

"The women of all the classes above the poor keep very much at home; the men of the same ranks are neither active sportsmen nor pedestrians, nor do they indulge in any of the manly games of our own countrymen, or (as far as I

have seen) in any substitute for them. Those in trade conduct it in the most leisurely way, shutting up their shops at two o'clock; and they are as scrupulous in attending to the red-letter days in the almanack as an English schoolboy used to be when such events were more observed than they are now. Few ride on horseback; the majority move along upon the backs of easy-paced asses, sitting sideways on a soft cushion. The inevitable consequence follows. Man's body is alone suited to earning his bread by the sweat of his brow, and he cannot defy that curse without a heavy punishment. If he need not earn his bread himself, he must substitute laborious pleasures: he must work harder than a post-boy, under the name of hunting; or for mere relaxation, encounter such cold and wet, hunger, thirst, and fatigue in deer-stalking or grouse-shooting, as a half-famished North American Indian meets with who has a starving family depending on his success; or he must rise early and work harder than a labourer in toiling over ploughed ground and stubble fields, or through wet turnips and thick grass in pursuit of partridges or hares; or he must walk up and down the same street in the same country town with all the assiduity of a policeman, to the market, or his club, or newsroom: or with his wife, or for his wife; or he must play at bowls or cricket, at gardening or at navigation; if he does none of these things or similar ones, he grows fat, has indigestion, and consults doctors with the vain hope of being enabled to baffle nature with impunity for some little time longer, and after a few years of perpetual uneasy feelings, it is found that his heart is diseased, he becomes dropsical, or loses the use of one half of his body, and is wheeled about in a chair, imbecile in mind as well as in limbs, or he becomes melancholy, and suspicious of his best friends, or by some such winding-up he arrives at the last scene that ends his common-place eventless history.

"Even to an English eye, accustomed as it is to 'The fair round belly with good capon lined,' the prodigality of fat Azoreans is striking. It is generally thought that England has a monopoly of human fat; that the conditions favourable to its inordinate growth, such as easy circumstances, abundance of beef and mutton from rich pastures, duly moistened with strong beers or strong wines, together with a constitutional selfish quietude of disposition and capacious hereditary powers of digestion,—the combined result of many generations of generous livers,—are alone found under our constitution. The Scotch have a 'lean and hungry look, they think too much;' the Irish are too excitable or too poor; the French have, it is true, enormous appetites, but how can they fatten on bread, beans, dried peas, and thin wine? You may travel through their countries and not meet with more exceptions than are enough to prove the rule. I should doubt the existence of much fat in America; the unexampled busymindedness there must prevent all such accumulations. But here the necessary requisites are found; the rich viands and the beer are wanting, but the climate is superior; no extremes; no cold driving the fat man to unusual exertion, nor that other extreme, intense heat, melting him into a finer form; but throughout the year, by night and by day, an equable greenhouse warmth, keeping the body, even when passive, in a genial glow, and enticing it to quietude and repose.

"The Castle of Indolence might have been built here; and he, who when smitten with the delights of leisure, declared, that if he had a son he should do nothing, and be called Nothing-to-do, should have transported him to this island. No speculations are there to vex the genial current of the soul of the indolent man, who would be richer without labouring for it: politics, instead of a daily excitement, are a monthly or quarterly one, softened by time and distance; there is no literature to set men thinking,—midnight-oil is never burnt,—a face sicklied o'er with the pale cast of thought is not, and no foolish, over-careful Azoreans break their sleep with thoughts, their brains with care, their bones with industry.

"They say that men, who when they arrive at the Furnas look like huge hills of flesh, after soaking an hour a day in the very hot water, and encouraging

dissolution and thaw, for an hour or two afterwards, by lying upon a board covered with thick woollen cloaks, with towels wound round their heads and necks, return so slim as to be hardly recognized by their nearest friends: the baths using up their spare materials as a winter's starvation does those of a hybernating dormouse. There are now a few portly individuals, sleek-headed men, and such as sleep o' nights, who seem to be here for this reducing process. As a remedy against obesity these baths may be highly useful, for they are means likely to be employed, as they require no self-denial. Order a sensual man to take hard exercise, little sleep, and less food, and you are sure to be unattended to; but direct him to use a luxury, and he may, in following his old habits, take the advice." (pp. 190-96.)

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Art. VII.—*Bemerkungen über die Weise wie die Oeffnung in dem Schädel nach der Trepanation, oder anderem Knochenverlust, ausgefüllt wird.* Von Dr. G. Vrolik, Professor am Athenæum zu Amsterdam, n. a.—*Amsterdam*, 1837. 4to, pp. 18, Ta. 1.

*Remarks on the manner in which the Opening in the Skull caused by the Trephine, or other loss of Bone, is filled up.* By Dr. G. Vrolik, Professor at the Athenæum at Amsterdam, &c.—*Amsterdam*, 1837.

The extent to which apertures in the bones of the skull are repaired, and their mode of reparation, are confessedly among the most obscure subjects of surgical pathology. The remarkable case by Scarpa* is the only one with which we are acquainted in which the reparation was complete; but neither this, nor the numerous cases recorded in which the repair was partial, throw much light on the mode in which the new bone is produced. In this respect the history of the case related in the present memoir has considerable interest.

A Danish sailor fell from the rigging to the deck of his ship upon his forehead, and was carried senseless to the hospital. The late R. Harmss, the surgeon, found the middle of the frontal bone so broken and depressed, that it appeared impossible to elevate the fractured portions without applying the trephine. This he therefore did in two places just above the fracture, and having raised and removed the depressed bones, the patient's consciousness was presently restored. He went on well for the first four weeks, but then became sleepy and then comatose, and died in the sixth week after the accident. On examination, the author says,

"I found the great irregular space from which the portions of bone were removed, closed up by a peculiar tissue, which I believe must be considered as a product of the proper action of the vessels. On looking at the inner table of the skull, I was easily convinced that the dura mater had been so completely separated from it that even the edges of the bone next to the aperture had been deprived of it. The outer table need scarcely be mentioned; the former loss of bone in the space now filled up proves that the tissue occupying it could not have been formed from the periosteum; a conclusion which might also be drawn from the fact of the edge of the wound being quite deprived of that membrane."

From this evidence of the complete want of connexion between the reparative tissue and both the dura mater and periosteum, Dr. Vrolik concludes that it must have been produced from the edges of the opening

* De Anatome et Pathologia Ossium Comm.

in the bone, and points out its relation to the membranes on either side of it as being similar to that of the originally cartilaginous cranium. In this respect, indeed, the similarity is greater than the author has pointed out; for in the ossification of the fœtal skull, there is not first formed a true cartilaginous groundwork for all the flat bones; but, in the growth and ossification of each, the cartilage is formed just precedently to the formation of bone; and thus, as Muscher has shown, the border of each growing bone is, as it were, fringed with cartilage. And the same has happened in the case here described; little processes of bone have shot from all the borders of the aperture in the skull into the new cartilaginous tissue by which it is nearly filled up. But besides this growth of bone from the edges, the plate represents several small osseous patches formed, either in the very middle of the new cartilaginous tissue, or just by the borders of the new bone, which, as the author has well observed, exactly resemble, both in their form and their arrangement, the additional nuclei of bone which are commonly produced in such numbers in the expanded fontanelles of hydrocephalic heads, and of which more perfect examples are found in the ordinary ossa Wormiana.

On the whole, the case here related affords very strong evidence of the similarity between the process of repair of losses of substance in the bones of the skull, and that of their original growth. In both, the growth of bone appears to take place from the edges of that already existing, and by the development and increase of osseous *insular* nuclei in the tissue intermediate between the bones. In both also there is remarkably exhibited that increase by the shooting out of bony rays, of which the most familiar example is presented by the fœtal parietal bone. In the present case, as well as in all the others that have been related, the production of bone by the edges of the trephine-hole, though taking place in exactly the same manner, is far less abundant than that from the edges of the merely fractured portions.

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Art. VIII.—*Popular Cyclopædia of Natural Science—Vegetable Physiology.* Published by the Society for the Promotion of Popular Instruction.—*London*, 1841. 8vo, pp. 295.

We know nothing of, and indeed never heard of, the Society by which this small volume is said to be published; but from which we are taught to expect, from the prospectus, nine or ten more volumes of the same general character as the present, to appear quarterly. We can however say conscientiously, after an examination of the first part now before us, that if the subsequent volumes are only nearly as good as this, the Society will, by their labours, have conferred a great benefit on the rising generation. We know not when we have met with a work more clearly and pleasantly written, more full of good matter with so little rubbish of any kind, or one better adapted to fulfil the object for which it is intended. We agree in every word of the following statement prefixed to the work by the Society, and cordially recommend the treatise to the notice of all our readers, who on their own account, or that of their sons or daughters, may be interested in the study of vegetable physiology:

"The 'Society for the Promotion of Popular Instruction' have much pleasure in offering to the public the following treatise on vegetable physiology, as

the first-published portion of their 'Popular Cyclopædia of Natural Science;' exhibiting, as it does, the successful carrying-out of the principles set forth in the Prospectus of the series; and, at the same time, completely harmonizing with the general objects of the society. They feel assured that it will be found sufficiently simple in its character, and clear in its explanations, to be regarded as an elementary treatise, adapted to those who have no previous knowledge of the subject; whilst its systematic arrangement, and the scientific value of the principles laid down in it, render it an excellent introduction to more comprehensive works on the same subject. The general reader, who seeks no more than entertainment or recreation, will find it in this volume, in the copious illustrative facts and interesting collateral information, with which it abounds; whilst to the agriculturist, the gardener, and the domestic economist, it supplies principles and practical applications of great importance."

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ART. IX.—*A Treatise on the Physiological and Moral Management of Infancy.* By ANDREW COMBE, M.D. &c. Second Edition.—*Edinb.* 1841. 8vo, pp. 380.

THE very high character given by us of this work on the appearance of the first edition, scarcely a year since, (see Br. and For. Med. Rev., vol. X., p. 241 and 514,) has been triumphantly confirmed by the profession and the public, as is proved by the demand for a new impression after so short a period. The volume was originally so complete that little room was left for improvement, and we believe this edition is substantially the same as the former. We feel it a duty to recommend Dr. Combe's Treatise in the most earnest manner to all our readers, and through them to all fathers and mothers, as the best guide extant for the physical and moral management of infants.

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ART. X.—*Traité sur l'Hydrocephale Intérne.* Par G. VROLIK, Professeur à l'Athénée d'Amsterdam, &c.—*Amsterdam,* 1839. 4to, pp. 11. Pl. 3.

*Treatise on Internal Hydrocephalus.* By G. VROLIK, Professor at the Atheneum at Amsterdam, &c.—*Amsterdam,* 1839. 4to, pp. 11, with 3 Plates.

THIS brief memoir contains several interesting observations. The author believes that such an hydrocephalus as can be referred to an arrest of normal development is very rare; and, maintaining his opinion chiefly by the fact of the frequent occurrence of false membranes limiting the effusion, holds that the great majority of cases have an inflammatory origin. He remarks on the differences of form assumed by the head according to the situations in which effusion has taken place. When the fluid collects at once in all the ventricles, the expansion of the skull is uniform; but he possesses the skull of a girl which is very much larger on the left side of the base than on the right, in consequence of a circumscribed dropsy of the middle horn of the left ventricle. And again, he has sometimes observed, that when the effusion is limited to the lateral ventricles, or to them and the third, then the expansion is confined to the bones above the occipital, and the latter is scarcely at all increased in size.

Our own observations, as well we believe as Dr. Foville's, are con-

firmatory of the statements of Dr. Vrolik; and we can add that peculiar forms are sometimes presented by hydrocephalic heads, in consequence of unnatural closure of the sutures preventing the expansion of particular parts of the skull. An illustration of this is now before us, in the skull of a subject that died at about twelve years old; the sagittal suture is completely closed, and the head, expanded in every direction except the transverse, has assumed a singularly elongated form, with a very high elevated but narrow forehead.

Professor Vrolik remarks also on the fact that the more slowly the effusion takes place and the head expands, the less danger is there of loss either of life or of intellect; for in these cases it is that the brain, though it changes its form, retains its structure quite unaltered. He has the skeleton of a man who died hydrocephalic at the age of sixty. He was only four feet four inches high, having been extremely rickety; but his skull measures 1·85 feet in circumference, and 1·35 feet from the nose over the vertex to the foramen magnum. The other dimensions are well proportioned to these, yet during life the individual had exhibited a cultivated mind and a highly-developed intellect.

The author fully agrees in the opinion of Gall respecting the *unfolding* of the convolutions in hydrocephalus; and points out that the fact was known by Hernauld, who described it, though he could not explain it, in the "Histoire de l'Académie Royale des Sciences, 1740," p. 375. Hitherto, however, the fact has not been well illustrated; and therefore, to supply this deficiency, the author annexes three engravings from two cases of cerebral expansion or unfolding which he met with in 1812, and all of which show in a remarkable manner the almost complete obliteration of all appearance of convolutions or irregularities on the surface of the brain. At the same time, however thin the layer of cerebral substance thus expanded, he observes that every portion of it showed its normally distinct constitution of white and gray matter, a fact which, with the others more commonly noticed, can leave no doubt that the change which the brain undergoes in simple hydrocephalus is one of form only, and that both its structures and (if the effusion have taken place slowly) its functions may remain unaltered. All these skulls moreover present examples of unequal expansion, in consequence of the unequal accumulation of the fluid in the several ventricular cavities.

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Art. XI.—*Ueber Tuberkulose als die gewohnlichste Ursache des Hydrocephalus Acutus.* Von Dr. F. Schweninger.—*Regensburg*, 1839. 8vo, pp. 88.

*On Tubercular Degeneration as the most frequent cause of acute Hydrocephalus.* By Dr. Schweninger.—*Ratisbon*, 1839.

In this unpretending but valuable pamphlet Dr. Schweninger has embodied the results of the post-mortem examinations of all the fatal cases of hydrocephalus which occurred at Ratisbon between the years 1835 and 1839. The observations are twenty in number, and the morbid appearances are detailed with much care and minuteness; but unfortunately the author has not been able to give equally accurate

descriptions of the course of the disease, since the patients seldom came under his notice during their lifetime.

In every instance tubercles were found in one or more internal organs: in four cases they existed in all the visceral cavities; in ten in the chest and abdomen; in five in the chest alone; and in one only they were confined to the abdomen. The different organs may be arranged in the following order according to the frequency of the tubercular deposit in them: bronchial glands, 17 times; lungs, 15; spleen, 10; mesenteric glands, 7; liver, 7; brain, 4; pleura, peritoneum, stomach, and kidneys, once in each. In five instances, too, the author met with that granular condition of the arachnoid membrane to which the name of meningitis tuberculosa has been applied by some pathologists. From this commonly received opinion, however, Dr. Schweninger dissents, apparently without being aware that the same view of the subject had been taken by Rouchoux in an article published in the Journal Hebdomadaire for 1835. He regards this deposit as being, like the so-called glandulæ pacchioni, a simple exudation analogous to those granular false membranes which are sometimes found on the pleura or peritoneum in cases where no organs present the slightest trace of tubercle.

From his observations he deduces the following results: 1st. That the meningitis which accompanies hydrocephalus does not differ in its anatomical characters in any important respect from meningitis when simple or when complicated with other morbid conditions. 2d. That the meningitis accompanying hydrocephalus does, however, differ from simple meningitis or from that associated with other morbid conditions, in being combined with other simultaneous pathological changes. 3d. That meningitis, though a frequent complication of hydrocephalus, is not the cause of the disease. 4th. This meningitis complicating hydrocephalus is itself only the result of the more or less general deposit of tubercle. 5th. The hydrocephalus is in all the above cases the result of tubercle, and there is great probability in the supposition that the greater number of cases of hydrocephalus may be ascribed to this cause.

In conclusion he endeavours to show that there is nothing in the supposition that infantile hydrocephalus consists in effusion consequent on tubercular degeneration, contrary to the analogies of tubercular disease in general. In that malady nothing is more common than to find abnormal collections of fluid the product either of secretion or of excretion. Œdema of the feet and general anasarca are both by no means unusual in the advanced stages of phthisis. A much more abundant collection of fluid in the ventricles and under the arachnoid is met with in phthisical subjects, than in those who have died of most other diseases. hydropericardium and œdema of the lungs, membranes of the intestines, &c., are common in tubercular patients. The copious diarrhœas and profuse perspirations of phthisical patients are instances of preternatural excretions of fluid, incidental to that disease. The occasional occurrence of serous effusion into the ventricles after the suspension of the diarrhœa in these cases is another circumstance which speaks for the existence of a common cause for both conditions, and is regarded by the author as favouring his supposition of the frequent production of hydrocephalus as a consequence of preexisting tubercular disease.

ART. XII.—*The Transactions of the Provincial Medical and Surgical Association.—London*, 1841. Vol. IX. 8vo, pp. 583.

THE present annual volume of this flourishing Society consists, as usual, of four Parts, besides the proceedings of the association at the place of meeting for the year, Southampton, viz.: 1, Retrospective Addresses in Surgery and Medicine; 2, Papers on Medical Topography; 3, Original Essays and Cases; 4, Infirmary Reports and Statistics. In the First Part we have addresses by Mr. Dodd of Chichester, and Dr. Scott of Liverpool. In the Second Part, a memoir on the Medical topography of Shrewsbury and its neighbourhood, by Dr. Ogier Ward of Shrewsbury, and another (which belongs of right to Part V.) on the Statistics of rheumatism, by Dr. Lyon of Manchester. In the Third Part are—Observations on injuries of the skull and brain, by Mr. Banner of Liverpool; On moveable cartilaginous bodies in synovial and serous cavities, by Dr. Macartney of Dublin; A case of amputation of the lower jaw, by Mr. Dodd of Chichester; Cases of abscesses within the pelvis after labour, by Mr. Wainewright of Liverpool; Cases illustrating the pathology of biliary concretions, by Mr. Mallett of Bolton-le-Moors; and remarks on the influence of the depressing passions, by Dr. Walker of Huddersfield. In the Fourth Part we have—A report of the out-patients at the Birmingham Town Infirmary, by Mr. Ryland; A report of the cases at the Birmingham Eye Infirmary, by Mr. Middlemore; and Suggestions for a form of register for hospitals, by Dr. Cowan of Reading. We hope to return to this volume next quarter. Our limited space will only permit us at present to make one or two brief remarks on its contents which are fully as interesting as usual. The addresses of Mr. Dodd and of Dr. Scott are admirable productions and of considerable extent, the former occupying eighty-four and the latter ninety-six pages. Together they constitute a most valuable digest of the progress of medicine and surgery during the preceding year, and claim the especial notice of all members of the profession, being the only records of the kind published in this country. The perusal of Mr. Dodd's paper will leave no doubt in the mind of any one of the impolicy of the resolution passed at the Southampton meeting, of not having an annual address in surgery as well as in medicine. The argument contained in the two following propositions—1, That this address is full of matter of great importance to the practical provincial surgeon not to be obtained by him elsewhere; 2, That no *physician* in England could compose such an address—is, we think, conclusive in favour of there being an annual address in surgery; and we trust the mistake committed by the association last year at Southampton will be remedied this year at York. We are, indeed, of opinion that it would be highly expedient to subdivide the subject of these annual addresses still further, so as, in place of only one or two, to have even *three* annually, viz. one on Medicine, one on Surgery, and one on Anatomy and Physiology. By this means we should have every department of medical science more thoroughly explored, and its annual progress more clearly presented, while the individual addresses being shorter and delivered at different times during the session of the Association, would be listened to with more satisfaction and profit than can be expected under the present arrangements. The high character of the retrospective reviews and of the topographical and

statistical memoirs contained in the present volume, especially Dr. Ward's *Shrewsbury*, adds force to our previous conviction that it is to the publication of papers of this sort that the transactions of the association should be principally devoted. There exist many other channels for communicating to the profession histories of cases and short essays of a scientific or practical nature; but it is only by an association of this kind, which has plenty of funds at its disposal, that long annual retrospects or topographical and statistical memoirs, comprising expensive tables, maps, and illustrative plates, can be given to the public. We will even go so far as to say that the association should confine itself to the printing of papers of this kind and reports on special subjects undertaken at its instance, essays that have obtained prizes from it, &c. &c. Into this category will come, of course, such memoirs as the admirable one of Mr. Ceeley, on Vaccination, in the last volume, the publication of which at the expense of the association is infinitely creditable to the council. We wish we could have added that the same liberality had been extended to Dr. Davidson and his excellent prize Essay, and that he had not been forced, by the rejection of the proposition to print it in the volume of Transactions, its natural *habitat*, to seek a place for it in the pages of a private journal.

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Art. XIII.—*Rambles in Europe in* 1839, *with Sketches of Prominent Surgeons, Physicians, Medical Schools, Hospitals, Literary Personages, &c.* By William Gibson, m.d., Professor of Surgery in the University of Pennsylvania, &c.—*Philadelphia*, 1841. 8vo, pp. 309.

We almost wish, for once, that we of this Journal were, in reality, what our ingenious but rather nervous friend, Dr. Marshall Hall, in the preface to his last work, calls us, "boys," in order that we might be able to give an impartial notice of Dr. Gibson's amusing little volume. But really our rambler has done us the honour to sketch among his "prominent surgeons and physicians" so many of us, that we know not where to look for a reviewer who might be supposed reasonably free from bias. Nay more, if Dr. Gibson has not tainted the very fountain-head of our critical justice with "sweet, sweet poison," he has, at least, done enough to make its award in his own case suspicious, by pronouncing of our Journal that "of all the foreign reviews it is incomparably the best, being conducted on the fairest and most liberal principles," &c. We must, therefore, content ourselves with giving such a cold and meager notice of it, as even Dr. Hall himself might almost be brought to believe impartial, simply recommending our readers to judge for themselves of its merits, after telling them that it contains graphic portraits from the life of almost every medical man of eminence in London, Paris, Dublin, Edinburgh, and the English provincial cities. We think we may safely add, without any risk of misinterpretation of our motives, that the "Rambles" are incontrovertibly the production of the best-natured of travellers. Dr. Gibson seems to be naturally of such a fortunate temperament, as to relish only what is commendable, passing over, with the happy instinct of an Epicurean, all that is either evil or disagreeable. Consequently, his pictures are almost all *couleur de rose*, without a single trace of that sable tincture which, we are ashamed to say, too many of our own countrymen have employed in delineating the men and manners

of America. But this return of cordial kindness and liberality on the part of our transatlantic brethren, is, at once, the best lesson and the severest punishment they can bestow on the detractors of their own great country. It would be manifest injustice to the author of this work not to say, that it is as cleverly as it is kindly written, and that the members of the profession of his own country and ours, will find in its perusal not a little of that innocent and happy dissipation from the toils and cares of business, which seems to have predominated in its composition.

After all, we find we cannot close this odd and amusing volume without presenting our readers with a specimen of its style and manner. We dip in at random, merely taking the precaution of going as far as possible from the seat of our government for our illustrations:

I. *Lisfranc of Paris.* "He is a big, burly, narrow-shouldered man, more than six feet high, negligent in dress, awkward in gait, uncouth in manners, and loud and boisterous in discourse. In his lectures he is said to be so unsparing of his brethren, and, in hospital practice, so harsh towards his patients, as to be unpopular with both. I cannot say whether those charges are well founded, but am inclined to believe them exaggerated, inasmuch as I saw nothing, beyond the natural want of polish in the man (increased, I thought, by affectation of wishing to appear worse than he really was), from which I should have drawn such a conclusion. I was seated, with a bevy of young medical friends, beneath the boughs of a wide spreading elm, on a delightful summer morning, the 18th of June, and saw him, for the first time, as he entered the hospital gate, and sauntered slowly along the gravelled walk of the long and wide avenue, leading to the ward containing his female patients. His head, covered with a rusty black and red cap, which, in shape of a tea-cup, stuck like a plaster to the summit of his crown; his long-waisted, scanty, snuff-coloured coat, dangling about his heels, and tapering away to sharpness, like the tail of a kite; his curiously contrived pantaloons, loose and bagging about his hips, and at each stride fluttering to the wind; his long, shovel-shaped shoes, scattering pebbles as he walked from right to left; his arms standing out from his body, like the handle of a pump, conjoined with his outstretched flexible neck, which swung to and fro beneath the pressure of his lengthy and wedge-shaped visage, presented one of the most ludicrous spectacles I ever beheld. He cast an enquiring sidelong glance at our group as he passed, which seemed to indicate, I thought, a belief that we were amusing ourselves at his expense, for he instantly bristled up, and with averted head hurried out of sight. . . . . He called the roll to ascertain that all his *internes* or house pupils were mustered at their posts, and refused to proceed until a delinquent, who was in bed, taking a morning nap, was brought to the scene of action. He then began a clinical discourse, explaining the general nature of the diseases before him, waxing warmer and warmer as he proceeded, and gradually raising his stentorian voice until its tones seemed to shake the foundations of the old building and startle the very rafters above our heads; whilst he, peering and scanning from right to left the looks of his auditors, with great self-satisfaction, seemed to enquire into the effect his sesquipedalian words and thundering sentences may have produced upon their minds." (p. 79.)

II. *Dr. Graves of Dublin.* "I quickly perceived a certain indescribable something in his keen penetrating eye, arrangement of the muscles of the face, and mobility of the tip of his long and delicately-formed aquiline nose, plainly indicative of humour, high spirits, and quizzical propensities, with ample power to subdue or bring them forward at pleasure. And I was not deceived; for of the many little stories I picked up in the land of blunders and bulls, some of his were the best; told so oddly and quaintly, with the true Tipperary accent on his tongue, when he chose to place it there, that I can't to this day help laughing when I think of the queer questions put to some of his hospital patients and the funny dialogues carried on between them, such as 'Patrick, my dear, and how are you to-day? Oh bravely, yer honour, and how's yer honour's

self? Did the pills throuble you last night, my dear? Faix, and they handled me nately. Where is your tongue, my dear? In my mouth, yer honour. And are you maning to bring it out? Troth and I am. Can you breathe, my lad? Av coorse, yer honour, and sure you might see that I can. That's right, my dear, and there's a pinch of snuff for you, Pat. Thank you kindly, and long life to yer honour; troth and yer a rale jantleman of a doctor.' And so he proceeded from bed to bed, and no language can convey the odd mixture of kindness, and humour, and familiarity, and good sense, and soberness, and dexterity with which he managed to draw from the patients the history of their disease, the symptoms, &c. All this and a great deal more was enacted in the Meath Hospital, where I spent a forenoon with him, after partaking, at his house, of a plentiful and most luxurious breakfast, such as would have done honour to the best Virginia housewife, or to the table of Mrs. Randolph herself. After this I saw as much of Dr. Graves as a fortnight in Dublin would allow. That is, I saw him every day, either at his own house, or at my lodgings, or the Meath Hospital, where he may be found, every morning, peeping and prying into every hole and corner of the building, cracking jokes with the patient or pupils, or old women, or poring over intently some medical production, or volume of natural history, or book of travels; for he is very fond of such studies, and took great delight in asking all sorts of questions about our Indians, and lakes, and trees, and prairies, and cataracts, and great rivers, and buffaloes. And, from all I did see of him, I felt justified, I thought, in jumping to the conclusion that he is a man of very extraordinary abilities, sufficient to enable him to master any subject to which he may devote his attention, and to sift thoroughly any medical case and follow out successfully its treatment; but that he is seldom capable for any length of time of devoting himself, soul and body, to any given point or subject; that he is too fond of analogy and of drawing conclusions from solitary facts, so that his listener is always left in doubt as to the certainty of his deductions, however striking and brilliant they may be, as they generally are, from wanting full confidence in his premises." (pp. 15-6.)

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Art. XIV.—*Medizinische Zustände und Forchungen im Reiche der Krankheiten.* Von Dr. Robert Volz.—1839. 8vo, pp. 256.
*Medical Facts and Observations upon Diseases.* By Dr. Robert Volz.

This work, as the title implies, is divided into two parts; and these quite unconnected with each other. In the first Dr. Volz, although disclaiming the character of an historical writer, indulges the reader with "a review of the past state of medicine, as the best means of judging of the present." This he represents as some horrible monster, some dragon of Wantley, "monstrum, informe, ingens," dragging its huge length through the land, whose tail is the "médecine physiologique," and "homopathie" its teeth and claws. He thinks the theory of Broussais may be considered as a sort of moral cholera, which first upraised its hydra head in France. "That country," says he, "accustomed for a quarter of a century to revolutions, wars, and fearful blood-sheddings, had fallen into such a state of congestion, that Broussais became an instrument in the hands of Nature to save his country." Dr. Volz doubts whether Napoleon or Broussais cost their country most blood! As some salvo however to the conscience of the latter, he instances "the rich results of pathological anatomy" with which he has stored our museums.

The second portion of the work before us is practical. In facial neuralgia, the author recommends the internal use of stramonium in decoction. He extols the nitrate of silver in increased cardiac action, passive hemorrhages, and irregular menstruation; and speaks highly of an ointment of the nitrate of lead as very beneficial in excoriated nipples.

PART THIRD.

# Selections from the British and Foreign Journals.

## I. THE FOREIGN JOURNALS.

### ANATOMY AND PHYSIOLOGY.

*On the respective Functions of the Upper and Lower Halves of the Spinal Cord, and their relation to the Extensor and Flexor Muscles of the Limbs.* By Ed. Engelhardt.

If, after cutting off the head of a frog, a wire be pushed slowly from above downwards through the vertebral canal, the two thighs are drawn forcibly to the abdomen, and the feet are made to meet above, and are pressed against the wire; in short, motions of the legs take place which have exactly the appearance of being intended to put away the wire. If the wire be now carried farther into the spinal marrow, the same motions still, for a time, continue to be produced, though less forcibly than before; but, when it has been pushed to about the middle of the vertebral column, they assume all at once a different character, and the legs are no longer drawn up to the abdomen, but are forcibly thrust away from it. The deeper the wire is passed, the more powerful do these movements of extension become; nor do they cease till the very last portion of the spinal marrow is destroyed.

To determine more exactly the spot at which irritation of the spinal marrow ceased to produce flexion, and brought on extension of the legs, I instituted the following experiments on a number of frogs: I cut off the head with scissors; and at the instant the legs were every time drawn up forcibly towards the abdomen, and the feet pushed against the scissors. I then removed the abdominal and thoracic organs, laid the trunk on its back on a smooth board, placed the legs in a half-flexed position, and cut through portions of the spinal marrow at each vertebra successively, proceeding regularly from above downwards. The legs were every time, as soon as the motions produced in them by each section had ceased, again placed in the half-bent posture before another section was made. The result was as follows: from the first to the fourth vertebra, the sections of the cord produced flexions of the thighs and stretching out of the feet over the upper part of the trunk. The more there was cut off from the cord, the weaker were these movements; and, when the sections came to be made at the part between the fourth and fifth vertebra, the legs were no longer drawn up, but movements of extension were produced in them, which increased in force in proportion as the sections were made nearer to the os coccygis. When the whole spinal marrow was thus removed, and portions were at the same time cut off from the three nerves which compose the sacral plexus, the result was, that only movements of extension of the legs were produced; and these continued till the section (carried onwards by degrees) came to be made at the part where the plexus divides into the ischiatic and the crural nerves.

To see whether the anterior limbs were similarly influenced by stimulating particular portions of the spinal marrow, I modified the experiment in the following manner: after cutting off the head, (by which the fore-legs were thrown into violent contractions,) I removed the abdominal and thoracic organs, without injuring the walls of the chest or the muscles of the shoulder

attached to them. I then divided the vertebral column between the eighth vertebra and the sacral vertebra, and pushed a wire from below into the vertebral canal. The result was, that, while the lower half of the spinal marrow was being destroyed, the anterior limbs were extended; but as soon as the wire reached the upper half of the cord they were flexed.

To test the influence of division of the upper and lower halves of the cord respectively on the reflex motions, I repeated the above experiments on several other frogs; and after replacing the legs in the half-bent position, as soon as the motions produced in them by the division of each vertebra and the cord within it had ceased, I stimulated the toes of one of the feet by pinching them with forceps. I now observed, that so long as the cord was not cut away by the successive sections, so far as the space between the fourth and fifth vertebræ, movements of flexion only were produced in the irritated leg, and in that opposite to it. But if the division were made at *this* spot, then irritation of the toes of one foot no longer produced flexion either in the corresponding or the opposite leg, but in several cases excited extension, which, however, was weaker than when the marrow itself was directly stimulated. As soon as the spinal marrow was divided between the fifth and sixth vertebræ, the reflex motions ceased, though direct stimulus of the marrow even down to the sacral vertebra still produced the same extension of the hind-legs as in the first-mentioned experiments.

Now, as the vertebral column of the frog consists of eight free vertebræ and one sacral, and division of the spinal marrow from the medulla oblongata down to the fourth vertebra produces flexion of the limbs, and from the fifth vertebra to the end of the column produces extension; and since, moreover, the experiments on the alterations of the reflex motions, according as the marrow is divided in its upper or in its lower part, produce a result which exactly agrees with these, it appears to follow that there is a determinate antagonism between the functions of the upper and the lower half respectively.

*Muller's Archiv. Heft* 3. 1841.

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*On the Anastomoses of Nerves.* By A. W. Volkmann.

Under this name the author includes those cases in which the branch of one nerve is connected with the trunk of another, and passes in the latter, not downwards but upwards, and towards the nervous centre; an arrangement similar to that of the terminal loops, but occurring in these cases, not in isolated fibres, but in whole branches.

One loop of this kind exists frequently, if not regularly, between the fourth pair of nerves and the first division of the fifth in the calf. Looking at it with the naked eye, in one case the loop appeared to consist of a single filament; but, on examining it with the microscope, five branches were found to arise from it, of which four received their fibres from the fourth, and only one from the fifth. This last was fully ten times smaller than that part of the loop which was connected with the central portion of the fifth. If, therefore, this portion were considered as coming from the fifth, there would be only one tenth of the fibres going to the periphery, while nine tenths must have been passing to the brain within the sheath of the fourth nerve. For, since the fourth above the loop gave off no branches, there was no opportunity for these fibres to turn round out of it again, and pass to the periphery. The character of the loop here assumed can therefore be doubted only by supposing that the portion, which seemed to come from the fifth, was really a branch passing into it; in which case it must have consisted of fibres of the fourth passing centripetally in the fifth. Such fibres might thus pass to the Gesserian ganglion, and thence go peripherally in some other branch of the fifth pair.

A second loop of the same kind appears to exist pretty generally in mammalia, between the second or third cervical nerve and the accessory. The author has found it in men, horses, dogs, calves, and cats. In the calf, the

anterior branch of the second cervical gives off a considerable branch, which goes to the deep main branch of the accessorius. One portion of its fibres passes within the accessorius, towards the periphery; the other portion, centripetally. Above their union, branches are given off from the accessorius, so that there is an opportunity for the centripetal fibres just mentioned to pass out of it again, and proceed centrifugally; but the microscope shows that they do not do this. Or, again, as the vagus and the accessorius are connected at the ganglion on the former, the fibres in question might go into the vagus, and then proceed peripherally in it; but, in the first place, the author once found the loop where it was evident that the vagus and accessorius only lay close by each other, without mixing fibres; and, in the second place, he was fortunate enough on one occasion to trace a fasciculus of fibres from the loop of the cervical completely into the root of the accessorius without a break. With the microscope and compressorium, he convinced himself that the fibres thus actually passed through the accessorius into the brain. Unless, therefore, it were admitted that the anastomosis between the accessorius and the second cervical connects two portions of the central organ, there remains only the hypothesis, that the fibres of this loop belonged to the accessorius, and that they ran at first centrally in the cervical nerves, but at one of its branches turned round, and proceeded centrifugally. But experiments proved (as will be shown) that at least a portion of the fibres were sensitive, and came from the spinal cord.

A third and similar loop is found between the cervical nerves and the descending branches of the hypoglossal. In man, as well as in dogs, rabbits, sheep, lynxes, calves, and cats, connecting branches of the cervical nerves pass into the descendens noni, and in it some of their fibres proceed peripherally, but a larger portion of them go towards the nervous centre. These last fibres pass on to the tongue,* and it is remarkable that what is called the descendens noni appears to contain only a few fibres from the ninth nerve. Excluding those fibres in the descendens noni, which, as they go to the tongue, cannot be supposed to come from the ninth, there is the highest probability that many of the fibres which certainly do come from the ninth instead of remaining in the descending branch, go through a branch of the uppermost cervical nerves to the spinal cord.

The author believes that he has obtained a proof of this by dissection of the nerves in the sheep. In it the descendens noni, where it comes off, forms a plexus of numerous fibres, which if it be carefully cleaned, and placed on a glass plate beneath the microscope, permits all its fibres to be closely traced. It is plain that, in this case, far more fibres pass to the descendens noni from the trunk of the ninth than it contains below the part where it is connected with the cervical nerves; and it thence follows, that a part of the fibres which pass from the ninth into its descending branch, do not remain in it, but go through the cervical to the cord; and this is the more probable, because these fibres could not again pass out of the cervical nerves to go peripherally, except at a very acute angle; and there are no signs of such an arrangement.

A fourth anastomosis exists between the second and third cervical nerves of the cat. The third, immediately at the origin of the anterior branch, gives off a large branch which first forms the loop already mentioned with the accessorius, but then passes completely into the second cervical nerve, in which a considerable portion of its fibres run backwards to the nervous centre. Between the first and second cervical nerves of the cat, there is also a constant communication, which, however, when closely examined, has rather the appearance of a plexus.

From what has been said, it results that there are nervous loops in which the fibres are placed together, but from which they do not go to be distributed in

* See paragraph 43, in the paper "On the Motor Influences of the Cerebral and Cervical Nerves."

one organ. Such an arrangement must certainly be considered strange; but its strangeness is of the less weight against its reality, from its being exactly analogous to the terminal loops. The union of adjacent fibres (such as occurs in the terminal loops) is in no respect more intelligible than that of the fibres of distinct nerves; and indeed experiments on these larger loops may serve to illustrate the infinitely more delicate terminal loops, to which experimental investigation is inapplicable. Such experiments were therefore performed, and their results follow:

The skin was divided behind and below the transverse process of the atlas of a live calf, and the anterior branch of the second cervical nerve was exposed. It had already divided into two branches, of which one was directed backwards and downwards, the other forwards and downwards. The latter contained in a looser cellular sheath the finer branch, which lower down became free, and was connected with the accessorius. Around this anterior branch, an inch and a quarter below its exit from the vertebral column, a ligature was tied, producing to all appearance at the instant acute pain. A second ligature was then applied below the former, and therefore nearer to the accessorius. This second ligature was at first tied only gently, but the animal gave out manifest signs of pain. After a short pause, the knot was pulled quite tight, and again there was distinct evidence of pain, though the animal seemed to suffer less than when the first ligature, which was nearer to the origin of the cervical nerves, was applied.

The anastomosis between the cervical and the accessorius was exposed in a calf, and was found to consist of two separate filaments. On tying the first pain was produced, but a second ligature tied on it nearer to the accessorius excited no appearance of pain. Three ligatures were then placed one close behind another on the second branch. The first, which was put in the middle of the branch, gave severe pain; the effect of the third, which was applied close to the union with the accessorius, was doubtful, the animal only drew back as if hurt. Supposing pain was produced, it must have been excited through branches of the accessorius, some of which other subsequent experiments proved to contain sensitive fibres.

The loop between the cervical and accessory nerves was exposed in a calf, and with short pauses after each application was tied three times. The first ligature excited pain, so did the second, which was placed nearer the spinal cord; but so did not the third, which was applied nearer to the accessorius than the first was. A fourth ligature was applied on the accessorius itself, and gave distinct pain.

The accessorius was exposed in a calf near its exit from the skull, and tied above its connexion with the second cervical nerve. The animal struggled and bleated during the drawing of the knot; after a pause the nerve was divided above the ligature, but there was no sign of pain. Then the divided nerve was repeatedly irritated at its peripheral end, and distinct indications of suffering were noticed every time.

Neglecting the very slight anomalies that were observed, and which may fairly be referred to the difficulty and tediousness of the operations, these experiments appear clearly to establish, 1st, that the anastomosis connecting the accessorius with the second cervical nerve is sensitive; 2d, that the course of conduction of the nervous influence is twofold, stimuli producing sensation by acting towards the brain as well as towards the spinal cord; 3, the sensitive fibres in the loop that belong to the spinal cord are considerably predominant.

The anastomosis between the second and third cervical nerves in a cat was exposed. A ligature tied in the middle of the loop excited the severest pain. A second ligature applied close to the part where the loop seemed to come out from the second cervical nerve produced no reaction. A third placed close to the third nerve gave acute pain. The same experiment was repeated with the same result in another cat. In a third the loop was divided close to the second

nerve, and on irritating the portion of it nearest to the third nerve, there were some signs of pain, but the animal was too much exhausted to draw any strict deductions from its appearance of suffering.

[Other experiments were tried with the other anastomoses, but the author confesses that the difficulties of tracing such minute fibres as compose these nerves in the living subject, were too great to permit his attaining any definite result. He then enters upon a theoretical consideration of the intentions served by these anastomoses; but into this, as his hypothesis seems very far less valuable than his facts, it does not appear necessary to follow him.]

*Müller's Archiv*, 1840. *Heft* v. *p.* 510.

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*On the Functions of the Roots of the Nerves.* By M. LONGET.

LAST year M. Longet addressed the Royal Academy of Medicine, deducing from certain experiments the inference that the anterior or motor root of the spinal nerves was endowed in a certain degree with the faculty of sensation, and that it owed this faculty, not to its relations with the antero-lateral column of the spinal marrow, but those which it has, by means of the spinal ganglion, with the corresponding posterior root. M. Longet has since repeated and modified these experiments, and finds, after trials upon seventeen dogs, and upon the roots of ten nerves in each dog, (thus making 170 experiments), that, uniformly, the anterior roots and the corresponding columns of the spinal marrow are insensible to every kind of mechanical irritation, while the posterior roots and the posterior spinal columns are always exceedingly sensitive. "But," he adds, "in applying alternately the two poles of a voltaic pile of twenty couples to the two roots under similar circumstances, I excited the most violent convulsions in acting on the anterior roots, while not the slightest trace of convulsions was manifested in experimenting on the posterior roots." In all these experiments the roots were isolated by plates of glass. Whether mechanical irritation, then, or galvanism be employed, the phenomena are so uniform and so evident, that we cannot doubt that the anterior roots are exclusively motor, and the posterior exclusively sensitive.

Galvanism may pass from the anterior column of one side to that of the opposite side, through the medium of the anterior white commissure which unites them; but it is worthy of remark, that it never passes from the posterior to the antero-lateral column through the medium of the posterior corner of gray substance which completely separates these two columns. The gray substance, then, appears to be a bad conductor of electricity.

*Journal des Connaissances Médico-Chirurgicales. Février*, 1841.

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*On the Motor Influences of the Cerebral and Cervical Nerves.*
By A. W. VOLKMANN.

IN experiments on the motor influences of the cerebral nerves, it is necessary to irritate their roots within the skull, because connexions take place between their branches soon after their exit from it, and sometimes even within its cavity. For this purpose the author adopted the plan of making longitudinal sections of the heads of fresh-slain animals, and irritating the nerves within the skull, either mechanically, or with the wires of a galvanic battery of which he could regulate the force. [We need not detail the cautions adopted to shut out all sources of error; the reputation which Volkmann has already earned in this branch of physiology is the best guarantee of the correctness of his experiments, and of the care with which he would draw results from them.]

I. *Of the oculo-motor or third pair of nerves.* 1. In a fresh-slain calf, the orbit was broken into and the muscles of the eye exposed. On irritating the root of the third nerve within the skull, contractions ensued in the levator palpebræ,

rectus superior, rectus internus, rectus externus, obliquus inferior, obliquus superior, and retractor bulbi.* Repeated experiments on several calves, a cat and a sheep, always gave the same results. In the sheep the experiment was performed so as to remove all suspicion that the motions of the obliquus superior and rectus externus were secondary. The orbit was broken into, and the eyeball with all its muscles, except these two, removed; but the irritation of the oculo-motor nerve still excited contractions in them both, though in the obliquus superior much more violently than in the rectus externus.

2. A dog's skull was sawn through longitudinally, the orbit broken into, and all the recti muscles cut away directly after death. On irritating the third nerve, the direction of the pupil was not in the least altered; the eye did not move upwards, but it turned round on its axis just like a wheel on its axle-tree. The upper portion of the sclerotica described a small portion of a circular arc, and approached the outer angle of the eyelids. This experiment was repeated with a slight modification in another eye. The lower eyelid, and a portion of the lower margin of the orbit, were removed from a fresh-slain dog, the exposed obliquus inferior was irritated, and the result was the same; the rotation was like that of a wheel, and as regular as if the visual axis were so fixed as to prevent any other movement of the eye except this of rotation. The experiment was repeated and confirmed in the calf.

3. The healthy eye of a live calf was observed, and Bell's idea that at every wincing the pupil suddenly moves upwards and inwards, was found as little confirmed here as it is by examining the eyes of the rabbit, horse, and cat. A rotatory motion, however, was often noticed; the elliptical iris, with its long diameter, going almost transversely across the eye, moved round the pupil like the beam of a balance round its pivot, so that the posterior (external) end of the ellipse was considerably depressed, and the anterior as much elevated. The obliquus inferior was now exposed and divided, and the balance-like motion of the iris was no longer perceptible. When the conjunctiva was irritated, there was often no motion of the eyeball; sometimes, however, there was a sudden retraction of it, and sometimes a twitching of it upwards.

II. *Fourth pair.* 4. In a fresh-slain calf prepared as before, the fourth nerve was irritated within the skull. The eye turned round on its axis, and the balance-movement of the iris was manifest, but its direction was the reverse of that in exp. 3, the anterior part being depressed and the posterior raised. The pupil was not directed outwards and downwards. In the cat the same phenomena occurred, but in a less degree.

5. The orbit of a fresh-slain animal was broken into, and the fourth nerve galvanized. The exposed obliquus superior contracted, but it did so in like manner when the third nerve was irritated (exp. 1.) Endeavours to make other muscles move through the medium of the fourth nerve were uniformly unsuccessful.

III. *Sixth pair.* 6. On irritating this nerve after similar preparation of the head and orbit, violent contractions ensued in the exposed rectus externus, drawing the eye outwards, and producing various other twitchings. After removing this muscle, and irritating the sixth nerve, there ensued twitching movements of the eyeball, and the pupil was drawn into various directions. After removing all the other muscles, the retractor bulbi was found acting as often as the nerve was irritated; and according as one or other of its fasciculi contracted, the pupil was drawn into this or that direction. In no experiment could the inferior oblique be excited through the sixth nerve.

7. In another similar case irritating the sixth nerve excited irregular motions in different directions, and especially a turning of the pupil outwards; and every time the nervewas irritated, the third eyelid was moved from the inner towards the outer angle of the eye.

* "My assistant has traced the branches of the third nerve to these last two muscles. The third in the calf gives a branch to the fourth nerve."

IV. *Fifth pair.* 10*. In a fresh-slain calf, the smaller root of this nerve was galvanized, and violent motions of the jaws ensued. The same result was obtained in other animals. When the great root of this nerve, or the root of any other of the cerebral nerves was irritated, no motions of the jaw took place. The muscles of the lower jaw of a calf were exposed, and irritation of the small portion of the fifth nerve was then seen to produce contractions in the mylo-hyoideus, anterior portion of the digastricus, temporal, masseter, and internal pterygoid. The external pterygoid could never be exposed while the irritability lasted. On irritating the small portion of the fifth, neither the buccinator nor the soft palate was ever moved; the author believes, therefore, that though Bell, on anatomical grounds, attributed to this nerve the motions of the buccinator muscle, and Valentin on similar evidence those of the soft palate, its motor influence is really limited to the muscles enumerated above.

V. *Seventh pair, or facial.* 11. In calves, dogs, sheep, and goats, the root of the facial being exposed and galvanized, distinct contractions occurred in the following muscles: frontalis, buccinator, orbicularis palpebræ, orbicularis oris, part of a muscle moving the nose, and of another moving the angle of the mouth, the attrahens, retrahens, and attollens auris, the posterior belly of the digastricus, the stylo-hyoideus and platysma.

12. The author could never succeed in exposing the muscles of the tympanum so as to see them act, but in his attempts he several times found irritation of the chorda tympani produce contractions of the buccinator.

13. On irritating the portio intermedia Wrisbergi, twitchings ensued in all the muscles of the face and ear, in the same manner as if the great portion of the facial had been irritated.

14. In a great number of experiments, irritation of the facial never produced motions of the tongue. 15. Neither is the soft palate ever moved by the influence of the facial, either in sneezing, as Bidder supposed, or in swallowing, as Valentin held. In the cases in which such motions seem to occur, the movements of the palate are only secondary to those of the tongue. The facial governs the digastricus and stylo-hyoideus; and when these are divided, or their action has ceased, the motions of the soft palate are no longer discernible, nor can they ever be excited, though all the muscles of the face and ears are thrown into active contractions. It is therefore manifest that the branch of the facial, which, under the name of superior vidian, passes to the sphino-palatino ganglion, does not convey any motor filaments to the soft palate.

VI. *Glosso-pharyngeal.* 16. This nerve arises in the calf, dog, cat, and man, by two distinct roots, each of which may be separated into two fasciculi. On the larger root (and often even within the skull) lies the Ehrenritter ganglion. But this does not seem to be really distinct from the petrous ganglion; for often only one ganglion can be found, and when there appear to be two, they are always connected by a tract of ganglionic substance. The separation of the two is therefore accidental; and however varied their apparent arrangement, it may be stated as a general rule, that all the fibres of the glosso-pharyngeal nerve pass through ganglionic matter. It is impossible therefore to compare the two roots of this nerve with those of the spinal nerves, or the Ehrenritter ganglion with the ganglia of the spinal sensitive roots.

17. After numerous unsuccessful attempts to produce motion by irritating this nerve, the author met with a case in which its two roots were separated, not only from the vagus, but from each other by dura mater. Irritation of the larger of these roots produced no effect, but that of the smaller (which in the previous experiments had been overlooked) excited violent motions in the pharynx. On exposing the latter it appeared that the middle constrictor and the stylo-pharyngeus were acting. This result was confirmed on two calves and

* For convenience of reference, the numbers of the paragraphs are marked as in the original paper; a considerable portion on the actions of the muscles of the nerve is here omitted, but is printed in a separate abstract.

two cats, so as fairly to establish, 1st, that the glosso-pharyngeal nerves govern only these muscles; 2d, that no other nerve has any influence on them; and 3d, that only the small root of the nerve is endowed with motor power. In the experiments of Valentin and John Reid, who could find no direct motion produced by irritating the glosso-pharyngeal, its smaller root must have been overlooked, as it was by the author in his first experiments.

VII. *Nervus vagus.* 18. In the dog and sheep a part of the roots of the vagus pass by the ganglion, and are connected with it only by cellular tissue; but in the calf this is not the case; several fibres seem indeed to go behind the ganglion, but on these there are minute irregularities, which when examined with the microscope are found to be true ganglionic matter. All the fibres in the calf therefore pass through ganglionic matter; a fact which is of some importance in relation to the following experiments, and to the physiology of ganglia and gangliated nerves.

19. A fresh calf's head was separated from the trunk and sawn longitudinally, so that the soft palate and upper part of the pharynx could be seen. The medulla oblongata was then drawn aside, and the roots of the nervus accessorius were carefully divided with scissors. No motions took place. The roots of the vagus were then cut, separately and singly from behind forwards; and at almost every section motion took place, either in the soft palate, or in the upper part of the pharynx, or in both at once. The experiment was repeated with every caution on five animals, and always gave the same results.

20. A calf's head was sawn longitudinally, and several roots, supposed to belong to the vagus being irritated, motions were produced in the pharynx and soft palate. Around every such root a silk thread was tied, and when the experiment was finished they were all traced to their origins. By far the greater part belonged to the vagus; one pair only came from the glosso-pharyngeus; but as experiment 17 shows that the latter nerve can excite motion in neither the upper part of the pharynx nor in the soft palate, it follows that the motions that were seen in this case must have depended on the vagus.

21. In another calf's head similarly prepared, the accessorius and glosso-pharyngeus were carefully removed as far as the foramen lacerum. The roots of the vagus were then galvanized, and motions took place in the superior and inferior constrictors of the pharynx, and in the crico-thyroideus, and in all the muscles of the soft palate. Subsequent dissection showed, that this experiment had been correctly performed.

22. Heads of calves, sheep, goats, cats, and dogs, were prepared directly after death, so as to expose the muscles of the larynx, pharynx, and soft palate. The roots of the vagus were then galvanized, and independent contractions ensued in the levator palati, azygos uvulæ, constrictor superior, c. inferior (but never in the c. medius), pharyngo-palatinus, and crico-thyroideus.

23. Different calves, sheep, goats, dogs, and cats were killed, by opening the carotids or abdominal aorta, and the descending branches of the vagus were exposed. Its roots were then irritated within the skull, and the most violent motions of the œsophagus instantly ensued, extending through its whole length, and exactly resembling those of voluntary muscles. The stomach never moved except at its cardiac portion by the communication of the movements of the œsophagus.

24. The larynx viewed from the spine during these experiments often presented manifest motion, but more rarely an alteration in the dimensions of the glottis, than a twitching of the prominent parts of the arytenoid cartilages. When the muscles were exposed the crico-arytenoideus posterius and lateralis were seen to move in the cat and calf, and the latter only in the dog; but the results of these experiments were often confused by the nerves being divided in the process of exposing the muscles; and it was probably on this account that the arytenoid muscles did not contract on irritating either the vagus or the accessorius.

25. After a cat had been killed by a stab in the heart, the vagi were divided

below the superior laryngeal, and the glottis was exposed. Irritation of the superior laryngeal did not move the glottis, but on irritating the lower part of the nerve the glottis was widened, and sometimes the vocal ligaments were stretched. On irritating the superior laryngeal the exposed muscles that contracted were the constrictor faucium superior, the crico-thyroideus, and (in dogs and calves), the thyro-hyoideus. On irritating the lower part of the vagus, the muscles that contracted were the crico-arytenoideus posticus and lateralis.

26. In two young dogs the cerebrum and cerebellum were removed, the glottis being exposed it opened and shut with every effort of respiration. In one, the superior laryngeal nerves were divided, and the motions of the glottis were not in the least affected in consequence. In the other the vagi were divided, so as to destroy the influence of the recurrent, and the glottis remained permanently closed. The same result followed the division of the vagus within the skull in young dogs; but it is probable that this closure of the glottis is the consequence, not of any muscular contraction, but of the pressure of the atmosphere, which, when the muscles are paralyzed, forces the edges of the glottis together at every inspiration.

28. In different animals killed by bleeding, the chest and abdomen were opened, and the roots of the vagus galvanized, but no motion was seen in either stomach, intestines, heart, or trachea; or, at least, the increase of the peristaltic motions of the first was not more than would result from the contact of the air, or from the communication of the movements of the œsophagus.

29. No one of the muscles just mentioned as being influenced by the vagus could be made to contract through any other nerve. In experiments on nearly 100 animals, there were but two which seemed to indicate that the influence ascribed to the vagus belonged to other nerves; in one cat irritation of the accessorius excited motion of the soft palate, and in one calf that of the glosso-pharyngeus excited contractions of the crico-thyroideus, but it is quite possible that these anomalies were the results of error in the experiments.

VIII. *Nervus accessorius.* 30. When this nerve was galvanized within the skull in cats, dogs, goats, calves, and rabbits, violent motions ensued in the neck and shoulder, and when the skin was removed the muscles contracting were seen to be the whole of the sterno-cleido-mastoid and the trapezius; no motion was ever seen in the soft palate, (except in the one cat in No. 29), the pharynx or the larynx, but they were acted on as soon as the vagus was irritated.

31. The heart was exposed in a rabbit killed by hemorrhage; its contractions were but few and distant, and they did not in any degree correspond with the repeated irritation of the accessorius within the skull.

All these results occurred uniformly in a great number of experiments; the muscles of the larynx, pharynx, and soft palate, &c. were always moved when the vagus, but never when the accessorius was irritated; the excitement of the latter produced motions only in the trapezius and sterno-mastoid. Now since it is not at all probable, that in irritating the accessorius certain roots belonging to it were *always* missed, and that in irritating the vagus these same fibres were *always* included, there can, Volkmann thinks, be no doubt that the motor functions he has ascribed to the vagus do really belong to it, and not to the accessorius, and that the latter has no influence on the motions of the palate, fauces, and glottis.

IX. *Hypoglossal Nerve.* 34. This nerve always arises in several bundles of roots that pass through separate holes in the dura mater; and a ganglion being usually seated on one of them, has given rise to the general opinion that this is a nerve of mixed function. The ramus descendens has always been considered a branch of this nerve, but if the mode in which they are connected be examined, it will be found that all the fibres of the former do not pass centripetally into the trunk of the hypoglossal, but that some go on through it centrifugally, and therefore cannot be said to come from it. This is the case in man, calves,

sheep, rabbits, and cats. In the horse, the author once saw that what is commonly called the descendens noni was really a ramus ascendens, receiving no fibres from the hypoglossal, but conveying all the fibres it contained to it. These fibres came from the two upper cervical nerves.—(See further the paper on Anastomoses of Nerves.)

35. In several fresh slain animals the hypoglossal was irritated at its root; motions ensued in the styloglossus, hyoglossus, genio-glossus, lingualis, and (in the calf), the hyo-epiglottis. In rabbits, irritation of either root excited contraction. Experiments were repeatedly made to see whether irritation of this nerve would produce motions of the omo-hyoideus, sterno-hyoideus, and sterno-thyroideus, but only the sterno-hyoideus could be made to move, and this in only two calves and one dog out of all the animals experimented on; facts which, with those mentioned in the preceding paragraph, are sufficient to prove that the hypoglossal gives but a few motor fibres to the descendens noni, and in general none but those for the thyro-hyoideus.

36. Each root of the hypoglossal was separately divided in a calf, and at every division a distinct motion of the tongue occurred. The little root with the ganglion however was overlooked; but in subsequent experiments, on galvinizing it, motion was repeatedly and regularly excited in a small space on the back of the tongue. The same experiment was twice repeated with every caution lest the galvanic action should be too powerful, but the result was in both the same.

X. *The cervical Nerves.* 37. These join the cerebral nerves in two different modes, their fibres pass into them either centrifugally or centripetally. The latter direction is generally disregarded, but repeated microscopic examinations have proved to the author, that branches of cervical nerves do thus pass to parts where they are least expected to be found. (This subject is treated of in a separate paper, on Anastomoses of Nerves.)

38. A sheep having been pithed between the occiput and atlas, the pharynx was exposed, and a galvanic current directed through the cervical part of the cord. Violent contractions ensued in the muscles of the neck, but none in the œsophagus, and this occurred repeatedly with very slight galvanic forces. There was indeed a slight undulatory motion of the œsophagus, such as the contact of the air might cause, but no general muscular action corresponding with the stimulus, or like that produced by irritating the vagus (23.). The same results followed irritation of the cervical nerves themselves where they are leaving the vertebral canal.

39. Irritation of the (motor) roots of the first cervical nerve excited motion in the sterno-hyoideus, sterno-thyroideus, and thyro-hyoideus (which was in one case moved by irritation of the hypoglossal also), but neither the sterno-mastoid nor the pharynx. Irritation of the second nerve excited several muscles in the neck, but none of those just mentioned, nor the œsophagus.

A number of similar experiments all tended to prove negatively that the movements of the pharynx and œsophagus are independent of the cervical nerves and governed by the vagi.

43. The muscles of the tongue were exposed in a fresh-slain sheep, and the descendens noni divided. At the instant of division the genio-hyoideus twitched, and this was repeated at every irritation. The central portion of the nerve was galvanized in several animals, and motions ensued in the hyo-glossus, genio-hyoideus, genio-glossus and lingualis, but it was difficult to say whether any of these, except the first, were moved primarily. Irritation of the root of the first cervical nerve regularly produced a motion of the tongue by which it was curved upwards, the lingualis being set in action.

45. As the general results of these experiments (which however are probably more valuable in their details,) the author gives the following conclusions:

A. All the nerves of the head, except the three of special sense, are motor (in a greater or less degree?).

B. Each muscle of the head (two of those of the eye excepted) receives its

motor power from one nerve only, and therefore the voluntary and automatic motions of these muscles both depend on the same nerve.

c. This was found true of so many animals, that it is probably true of man also.

d. Some muscles of the tongue receive (as Valentin showed the iris does) branches from the cervical as well as from the cerebral nerves, a fact by which the defects of speech in disease of the cord are intelligible.

e. Motor nerves may have ganglia on their roots.

*Müller's Archiv. Heft* v. 1840.

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*On Electrical Currents in the Nerves.* By Professor BISCHOFF of Heidelberg.

THIS paper contains a brief series of experiments performed by Professors Bischoff and Jolly, of which the results corroborate entirely the conclusions drawn by most other physiologists from the facts already known. They establish, first, that even the most delicate galvanometers can detect no current of electricity in the nerves; although they are such bad conductors of electricity, that its passage through them is not discernible even when its force is such as would act on very coarse galvanometers. These two facts together are conclusive against the existence of the supposed natural currents, since if they existed in such bad conductors, it is impossible but that the electrical tension would be sufficient to affect the galvanometer. But on the other hand, it is clearly proved that the nerves themselves are the most delicate of galvanometers, being so irritable to the electrical stimulus that muscular contractions are excited by a current too weak to be detected by the ordinary galvanometer-needle. Other experiments are alluded to which seem to prove that there is no free electricity in either the brain or spinal cord.

*Müller's Archiv. Heft* i. 1841, *p.* 20.

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*On the Circulation in the Infusoria.* By Dr. ERDL of Munich.

THE author in a letter to Professor Müller says, "I have now very often seen and shown to my friends about here a kind of circulation in the infusoria, a phenomenon so remarkable that I cannot but wonder that it is not mentioned by any microscopic observer. I find it most distinctly in the Bursaria vernalis, whose abdomen, you know, appears to be quite full of green globules. Of these globules, those which lie near the periphery of the animal are incessantly moving (whether the animal itself be still or not) in an elliptic current upwards and downwards. In this current three or four globules always lie close by one another and move together with the stream. It has no relation whatever to the vivid ciliary motion that is constantly going on on the outer surface."

*Müller's Archiv. Heft* iii. 1841.

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*On the Functions of the Anterior and Posterior Columns of the Spinal Cord.* By Dr. KUERSCHNER.

THE experiments here related, to establish that the anterior and posterior columns are respectively and exclusively motor and sensitive, are simple and founded on the well-known phenomena of reflex action.

The facts that individual muscles can be voluntarily moved, and that isolated sensitive impressions can be perceived, prove that the nervous principle is conducted along the spinal cord according to the same laws as it is along the nerves themselves. Reflex motions, therefore, cannot take place when the supposed sensitive columns are stimulated at the top of a divided cord, for the sensitive fibres cannot conduct centrifugally, nor can any motions take place in consequence of stimulating those columns unless they contain motor filaments. But in several experiments, chiefly performed on decapitated frogs, no motions what-

ever were excited by stimulating the upper part of the posterior columns, though by the slightest touch of the anterior severe convulsions were produced. The same result was obtained in whatever way the experiment was modified, so that there can be little doubt that there are no fibres in the posterior columns which are capable of conveying impressions (directly) to muscles.

The converse, namely, that there are no fibres in the anterior columns capable of conveying sensitive impressions, was proved in a similar manner. Thus, reflex motions occur only with the assistance of the centripetal nerves; and therefore when the posterior roots of the nerves of one of the legs of a frog are divided, irritation of the skin of that leg should cease to excite motion. And this in reality occurs; whenever the posterior columns, or roots of the nerves of a limb are destroyed, all reflex motion on irritating its skin is put an end to, although irritation of the anterior columns or roots still produces active muscular contractions.

*Muller's Archiv.* 1841. *Heft* i, p. 15.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

*On Croup and its Cure.* By Dr. Grahl of Hamburgh.

Dr. Grahl calls attention to the employment of arm-baths in croup, the use of which has already been advocated by Dr. Dorfmüller in the fifty-first volume of Rust's Magazine. They are indicated in the commencement of the stage of exudation, the existence of which is rendered evident by the difficult respiration. He regards the employment of leeches in this stage as of no avail, and the administration of sulphate of copper and emetics as likely to increase the exudation. His recommendation is that the arms of the patient be placed in a vessel sufficiently deep to admit them to a hand's breadth above the elbow-joint and filled with water as hot as it can be borne. A cloth should now be thrown over the head of the patient, which, falling down around the edges of the bath retains the vapour, and this the patient should be allowed to respire for a quarter of an hour together, repeating it at short intervals. The first application usually induces some degree of moisture in the Schneiderian membrane, and diminishes the dyspnœa. As it is repeated the cough usually loses its hoarse tone and the patient expectorates the exuded lymph. In cases where the symptoms are extremely urgent, calomel in large doses should be given and a blister applied to the throat, but in the great majority of cases the employment of the arm-bath is all that is necessary.

*Zeitschrift für die gesammte Medicin. Band* xvi. *Heft* iv. S. 496.

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*Communication of an Intermittent Fever from a Mother to her Child.* By Dr. Brunzlow, of Brandenburg.

The mother suffered throughout nearly the whole of the last seven months of her pregnancy with her first child from tertian and then from quartan ague. The child was born very weakly, and after some weeks of more or less illness the mother noticed that it appeared much worse on some days than on others. On closely watching it, the author found that every fourth evening the infant became very cold and had its nails quite blue, and that then a considerable degree of heat came over the whole body, and was followed by slight sweating. On the next morning all these symptoms had disappeared, and for the next two days the child remained perfectly well. The evidence of its being actually affected with quartan ague was further strengthened by its being slowly cured by the administration of quinine. *Medicinische Zeitung. Marz* 24, 1841.

*Poisoning of Leeches by the Blood of the Sick.* By Stefano Grandoni.

In a paper containing experiments to prove that portions removed from leeches are not reproduced, the author relates the following interesting case:

In the beginning of May there was brought to the Brescia Hospital for Males a butcher, who had contracted malignant carbuncle in cutting up an ox that had died of that disease. Among the means employed were 120 leeches, which were to be applied in three divisions to his right arm, all of which died on being taken off the skin of the patient. Our chemist being informed of the case, though he did not doubt that the death of the leeches proceeded from the blood which they had sucked and were full of, had several other lively and strong leeches applied, and these also immediately fell off dead. This having occurred in the calamitous time of the cholera, curiosity was excited to know whether leeches would also die from the effects of the blood sucked from cholera patients, and it was found on good information that this had actually occurred in the cholera hospital.

*Commentarij dell' Ateneo di Brescia*, 1837-8.
*Annali Universali di Medicina, Ottobre*, 1839.

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*On the Treatment of Acute Articular Rheumatism by large doses of the Nitrate of Potash.* By M. Aran.

This is a very long paper, detailing the results of twelve cases of acute rheumatism treated by MM. Gendrin and Martin Solon. Of these, ten were males, two females, from the ages of sixteen to forty; the stage of the disease varying from the second to the fifth day, and the previous treatment having been unimportant. The nitrate of potash was administered in some demulcent ptisan, the mean quantity taken by each patient being *an ounce and two scruples per diem*, and twelve ounces throughout the whole duration of the disease. The mean duration of the disease was eight days from the commencement of the treatment and rather more than twelve from the period of invasion. The usual effects were very abundant perspirations; occasionally copious stools, but without colic; and free secretion of urine. The digestive organs were unimpaired in their functions; only one patient vomited; the pulse was depressed; and the pain in the joints rapidly diminished. In three cases complicated by endocarditis or pericarditis, these affections rapidly subsided, though no other remedies were employed. Only two patients suffered a relapse, and they were again cured by the same means.

[This treatment is not new, for Brocklesby and White in this country, towards the close of the last century, gave nitre from ten drachms to two ounces in the twenty-four hours, and spoke highly of its efficacy both in acute and chronic rheumatism. However the mean duration of the disease under its use does not appear to be less than is obtained by other means. Dr. Hope, who trusted to calomel and opium, states the usual duration to be eight or ten days, and we have frequently seen severe cases rapidly cured by colchicum and opium. Bouillaud, who employs free bleeding, gives one or two weeks. Dance, who gave the tartarized antimony freely, forty to sixty days. The nitre is evidently preferable to the two latter.]

*Journal des Connaissances Médico-Chirurgicales. Avril*, 1841.

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*On the Pathology of Plica Polonica.* By Dr. Bidder, of Dorpat.

In a paper on the structure and physiology of the human hair, the author has communicated the following interesting observations on this disease, which he had an opportunity of making, during a residence of several weeks in a district in which it was very prevalent, though not in its severest form.

In none of the cases examined did the matting of the hair extend to the scalp, but for a distance of between half an inch and an inch from it all the hairs,

however matted in the rest of their length, were perfectly normal. It might be imagined indeed, that healthy hair had in these cases begun to grow again, and that in an earlier stage of the disease, the whole had been matted together; but the same appearance having been observed in at least twenty individuals placed under a variety of different circumstances, proves that it is a constant and essential fact. Moreover, since the scalp beneath the matted hair was perfectly healthy, exhibiting neither redness nor swelling, nor increased sensibility, it is impossible to refer the morbid appearances in the hair to disease of its matrix. It must rather be held, that in consequence of a morbid process commencing in a defined part of their constituent fibres, the hairs stick together in bundles of various sizes, and become individually so loosened in their texture, that they split up into smaller and finer portions. The frequent occurrence of such fine hairs, as well as that of the much coarser hair in the plica, can scarcely be otherwise explained.

A fact still more remarkable, and affording a still stronger proof of the vitality of the hair, is a process of separating or shedding which occurs in some cases of plica. The author saw two persons from whom plicæ had lately fallen off spontaneously, without leaving any part of the head bald; and indeed this occurrence is regarded by the people of the district, as a favorable though a rare symptom in the disease. In two others he was able to trace the mode in which the separation takes place. In these, there formed at the limit between the diseased and healthy hair, in the tuft that was matted beyond a certain distance from the scalp, a deep groove which completely encircled the tuft as if a string had been tied round it. It formed a very distinct boundary between the two parts of the tuft, and as it went on, the constriction becoming gradually deeper, a part of the hairs separated, and the whole plica was held by only the remainder of those by which it was originally constituted, and at last it entirely fell off. The whole process bore a remarkable resemblance to that of the casting off of other diseased parts, such as occurs, for example, in gangrene; and the whole series of the phenomena are explicable only on the belief that they depend on an action occurring in the hair itself.

*Müller's Archiv.* 1840. *p.* 546.

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*On Horny Excrescences on the Eyelids.* By Dr. F. A. v. Ammon.

Professor Stromeyer, of Erlangen, had the kindness to send me a drawing of a horny growth, with the following description: "This horn has existed ten weeks, and still goes on growing; six weeks since the excrescences on the nose formed, and these are now ready to fall off. The horny mass does not proceed from a follicle, but was probably secreted by the vascular skin at its base. It gives no annoyance; the closing of both the eyelids is effected only imperfectly from want of tone, and the conjunctiva is somewhat loose. The patient is a woman, seventy-one years old." Thus far Stromeyer.

To this interesting communication I may add the following. I have twice observed horny growths on the eyelids. The cases were as follows: A stocking-maker, forty years old, healthy and strong, had a horny growth, three lines large, on the right upper eyelid, a few lines from its ciliary margin. It had grown gradually, and turned downwards. The patient believed that it came from a wart; and he came to me only because he had lately often struck the growth, and given himself pain. I cut it off with scissors, going rather deep into the healthy skin, and the wound bled considerably for some time. When it had ceased bleeding I touched it with nitrate of silver, and in twelve days it was completely healed. Two years have elapsed, and the disease has not reappeared.

The most accurate examination detected nothing more than that the horn was a dark cartilage-like substance, uniform from its base to its apex, and consisting of several close lamellæ. Where it was connected with the skin, there was seen an intermediate substance like a thickened cutaneous gland.

In the second case, I observed the disease in a woman fifty years old, who had ceased menstruating for some time, and had had warts on many parts of her body successively, which, however, were always removed by the remedies she applied. From a wart on the left upper eyelid, a horny excrescence had gradually formed, which was upwards of four lines high, inclined to one side, and smaller at its apex than at its base. It gave no pain, but it often itched, and when the woman scratched it, it bled. I extirpated it with scissors, and it has not been reproduced.

As far as I know, among the cases of horny excrescences described by authors, there is none of a *cornu cutaneum* of the eyelids.

*F. A. v. Ammon's Monatsschrift für Medicin n. a. Juli*, 1840.

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*Poisoning by Solution of Acetate of Lead.—Lead found in the Urine.*

A young girl of good constitution, driven by despair to suicide, took about an ounce of acetate of lead in solution. She was almost immediately seized with collapse and syncope, and afterwards with vomiting and convulsions. Sugared water, sulphate of magnesia, and sulphate of soda were given, but she died in twenty-five hours. She voided a large quantity of urine, which M. Villeneuve sent to M. Orfila. Carbonized, treated by nitric acid, and submitted to the tests of the salts of lead, this urine afforded a sensible quantity of lead. *Journal des Connaissances Médico-Chirurgicales. Janvier*, 1841.

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*Antimony in the Urine.*

At the sitting of the Royal Academy of Medicine, Dec. 8, 1840, M. Husson stated that he had given a scruple of tartar emetic to a patient affected with pneumonia in twenty-four hours. It produced neither stools nor vomiting, and on the urine being examined by M. Orfila, with Marsh's apparatus, it afforded the antimonial stains in great abundance.

*Archives Générales de Médecine. Janvier*, 1841.

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*On the Employment of Arsenious Acid in Phthisis Pulmonalis.* By M. Trousseau.

M. Trousseau is now making trial of the powers of arsenic in phthisis at the hospital Necker; a means, however, not new, as Dioscorides employed fumigations of the sulphuret of arsenic in the same disease. During some few months M. Trousseau has submitted eight patients to the action of this agent. In four affected with diarrhœa, the disease continued its progress; it was in an advanced stage, and death occurred in the usual manner. In four others, notwithstanding vast caverns, and symptoms which announced approaching death, the symptoms amended under the influence of arsenious fumigations, the general health improved, appetite returned, digestion was good, emaciation checked, and the cough and expectoration diminished.

A sheet of white paper is dipped in a solution composed of one part of arseniate of soda and thirty parts of water. The paper is then made into little cigars of the length of a finger, and the patient is directed to smoke one or even two daily, in such a manner that the fumes may pass into the lung. This is readily accomplished, by the patient inspiring the moment the fumes enter the mouth. This inspiration of arsenious vapour at first causes slight cough, but after some time both cough and expectoration are much diminished.

This means, though it has not as yet cured phthisis, deserves a trial. In one case of chronic catarrh with emphysema, it rapidly removed symptoms of suffocation. Careful experiments should be made with this remedy, for phthisis is a disease hitherto so incurable that anything which affords any probability of effecting permanent good is worthy of investigation.

*Bulletin Général de Thérapeutique. Février* 15 *et* 28, 1841.

*On Scorbutic Pericarditis and Pleuritis, and the Cure by Paracentesis.*
By Dr. Karawagen, of Kronstadt.

The scurvy epidemic which prevailed among the sailors at Kronstadt during the months of May, June, and July in 1839, was particularly marked by the effusions of blood into the cavities of the chest which are known by the names of *pleuritis* and *pericarditis scorbutica*. The common sign of both these effusions, which commonly take place very rapidly, is an extreme difficulty of respiration. In the pericarditis the heart's sounds appear faint and remote, and percussion yields a dull sound over an unnaturally wide extent. In the pleuritis, only a weak respiratory murmur can be heard on the affected side; percussion is dull, and the respiration is short and hurried. There are also present slight fever and most of the ordinary signs of severe scurvy, which indeed always precede by one or more days the more dangerous symptoms of the thoracic effusions. When the latter come on, the condition of the patient is at once one of imminent danger, and the pericardial hemorrhage is rarely survived beyond the third day.

In the examinations of sixty patients who died of scurvy, the author found that thirty had this kind of pericarditis, twenty-two had pleuritis, two pleuritis and pericarditis together, and two peritonitis. In the pericarditis the pericardium appears three times as large as natural. It contains a bloody, dark-red exudation, which may amount to as much as four or five pints; so that it might be suspected that some vessel had been ruptured, though on examination none such can be found. The heart itself appears three times as small as usual; its surface, as well as the lining of the pericardium, is covered with a layer of red mould-like substance that may be scraped off with a knife; and beneath this the substance of the heart is soft and fatty. The endocardium is red, probably from imbibition; the ventricles are greatly compressed, and always empty; the left lung is compressed and bloodless. When there has been pleuritis, ten or twelve pints of bloody fluid are found in the pleuræ, whose surface is similarly covered by a layer of mould-like substance, and is very red and vascular. In peritonitis the appearances are similar, but the quantity of fluid amounted (in the cases examined) to from thirty to thirty-five pints. Once, in a case in which the patient died with symptoms of apoplexy, a similar effusion was found in the cavity of the skull. Sometimes the bones and fibrous tissues were affected with scurvy, and this was especially the case with the ribs, whose ends were found separated from their cartilages, moving on them with a kind of crepitation, and looking as if they had been eaten away by caries.

In despair of any other remedy, the author determined on tapping the serous cavities into which the hemorrhage had taken place. He performed this operation six times: on four patients with pleuritis, and on two with pericarditis; one of the last recovered completely, and all were immediately relieved and had their lives prolonged. The case of the man whose life was saved, and who was the only patient that recovered after pericardial effusion had evidently taken place, is related at length. He appears to have been in extreme peril when the operation was performed; but directly after three pints and a half of fluid had been extracted he felt much relieved, though a quantity of air had passed into the pericardium through the trocar; and he gradually recovered under the use of the ordinary antiscorbutic and some other means. Five months after the operation he might be considered quite well.

*Medicinische Zeitung. December* 23, 1840.

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*On certain Anthelmintics in use in Abyssinia.* By Dr. Aubert.

Dr. Aubert, who has resided some time in Abyssinia, states that the whole medical practice of the natives consists in the employment of three indigenous vegetable anthelmintics called Cusso, Bisenna, and Abbatsjogo; and that tænia is so common among the inhabitants as to be considered almost natural. Bruce remarked that almost all Abyssinians were affected with ascarides; but as the

Abyssinian word applies to either species, and Bruce was not a physician, it is probable he confounded the two, especially as the cusso, which he praised as a remedy against these worms, Dr. Aubert finds used in tænia.

The cusso is the Banksia Abyssinica of Bruce, the Hagenia anthelmintica of Willdenow, one of the species of the genus Saponaria of Linnæus. M. Kunth, an able botanist, has recognized the flowers as a new genus of the Rosaceæ. About two drachms of the flowers in powder are taken in an electuary of honey, and ordinarily in about twenty-four hours the patient passes the worm, but without purging. The whole of the worm is rarely expelled; but the patient follows his usual occupation, and after a time, as the medicine is not unpleasant and does not cause griping or purging, the dose is repeated. Dr. Aubert became a subject of tænia whilst in Abyssinia, and by taking aperient medicine after the cusso, he completely relieved himself of it.

The bisenna is a species of conifera much resembling the Juniperus Virginiana of Linnæus. The pulverized bark is employed, but not so generally as the cusso, as it causes irritation, colic, and in some cases enteritis.

The abbatsjogo resembles the Ixia bulbocodium of Linnæus. It is less employed than either of the former species.

*Bulletin de l'Académie Royale de Medécine. Mars* 15, 1841.

## SURGERY.

*On a new Method of preventing the Entrance of Air into the Chest in the Operation for Empyema.* By M. REYBARD, of Lyons.

IN this proceeding a membranous tube formed of a portion of intestine of a goose is applied by one of its extremities, at the spot selected for puncture, by means of a pellet of diachylon, and communicating by the other with a pig's bladder, the fundus of which gives passage to a trochar, the opening being closed by a ligature. The bladder being folded along the trochar, and the membranous tube firmly supported by the left hand, the trochar is directed along the latter puncture, made in the usual manner, and the trochar withdrawn. Pus runs into the bladder, and gradually distends it. When it ceases to flow, the canula is withdrawn and a ligature applied to the membranous tube, which is cut beyond this point, and the operation is completed by squeezing the diachylon over the wound. The author remarks that this method, with certain modifications, may be used to extract a liquid from any of the large cavities. *Gazette Médicale de Paris*, 3 *Avril*, 1841.

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*Section of the Tendons of the Muscles of the Eye in the Horse; Reunion of the Tendons.* By M. LUCIEN BOYER.

AT the sitting of the Royal Academy of Medicine, January 12th, 1841, M. Boyer submitted to the members specimens of pathological anatomy which proved in the most evident manner that when the muscles of the eye have been completely divided, their extremities are not united in any case to each other, but that their reunion to the globe of the eye is effected by an aponeurosis of new formation, which is always inserted behind the primitive insertion on the sclerotic. They also prove that reunion is very quickly effected, and that the muscle, elongated by the interval between its two portions, becomes almost normal in its action. *Journal des Connaisances Médico-Chir. Fevrier*, 1841.

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*On Tincture of Iodine as a topical Application to Phagedenic Chancres.* By M. RICORD.

AFTER a trial of all the most common topical applications recommended in the treatment of phagedenic chancre, M. Ricord finds them all frequently inefficacious, and from none has he obtained such prompt and happy results as from the tincture of iodine. He has employed this tincture during the last

three months, and almost constantly obtained a prompt modification of the ulcerated surfaces, which soon lose their phagedenic character. This has occurred in many cases. The action of the iodine was particularly evident in a patient who had an open bubo, the ulceration being phagedenic and very extensive. Various means were employed during two months without success, when the tincture of iodine alone so modified the ulcerated surfaces that their extent visibly diminished from day to day. The iodide of potassium had been given in this case without effect, but its use was continued conjointly with the tincture of iodine as a topical application. The cure was perfect in less than a month. *Bulletin Général de Thérapeutique.* 15 *et* 28 *Février*, 1841.

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*Great Hemorrhage after Division of the Genio-glossi Muscles for Stammering.* By M. Guersent, Jun.

M. Guersent performed the above operation at the Children's Hospital on a child twelve years of age. The hemorrhage at the time of the operation was very slight, but the next day it came on to a large amount, and was checked by charpie dipped in a solution of alum. On the third day the hemorrhage recurred, and was checked by the same means. Fourth day, no hemorrhage, cold lotions, ice in the mouth. Fifth day, hemorrhage came on with the greatest violence, but was stopped by the actual cautery. Sixth day, no hemorrhage. Seventh, it recurred, and the red-hot iron was applied seven times without arresting it; compression, by means of pledgets of charpie steeped in a solution of alum checked it after several hours. The compression was continued on the following day, and permanently suppressed the bleeding. The state of this child excited the greatest solicitude. The weakness and pallor indicated such pressing danger that M. Guersent had determined to tie the lingual arteries if the compression had not effected his object.

*Bulletin Général de Thérapeutique. Avril* 15 *et* 30, 1841.

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*New System of Bandaging.* By M. Rigal, of Gaillac.

The object of this memoir, which was read at the Royal Academy of Medicine in November, 1840, is to point out the advantage obtained by the use of strips of indian rubber in the place of common ligatures and bandages, made elastic by means of the same material, in place of the common roller. The author has used them for the last two years, and speaks very highly of their power of maintaining parts in apposition during the most varied movements of those parts. He particularly instances hare-lip, and the operations for the restitution of lost parts. The elastic bandages are also said to be very useful in maintaining oblique fractures of the lower extremities in perfect apposition, opposing a force in constant operation to the irregular action of the displacing muscles. In this way also they may assist in the replacement of parts after tenotomy.

*Revue Médicale. Janvier*, 1841.

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*Ectropion cured by Autoplasty.* By M. Bérard.

M. Bérard presented to the Academy a young butcher whose inferior eyelid, all except the free border, had been destroyed by a malignant pustule. After having arrested the progress of this terrible disease M. Bérard sought to repair the loss of substance and prevent greater depression of the free border of the eyelid already affected by ectropion. A portion of integument was taken from the temporal region and applied just below the inferior eyelid. The superior border of this slip became united to the inferior border of the eyelid, and its inferior border to the superior edge of the malar integuments. The whole was maintained in apposition by simple agglutinatives. Four months after the operation, the wounds are cicatrized, the new eyelid is perfectly vital, and of a tint precisely similar to that ordinarily seen in the situation which it occupies.

*Bulletin de l'Académie Royale de Médecine. Mars* 15, 1841.

*On Excision of the Elbow-joint.* By M. ROUX.

Since 1812 M. Roux has performed excision of the elbow-joint eleven times, the whole of the articulation having been removed in every case. Of these eleven patients three have died; six reaped every benefit which could arise from the operation. The patient operated on in July, 1839, can flex and extend his arm imperfectly, while a few fistulæ still remain in the patient operated on last November. M. Roux formerly practised the operation of Moreau, making an external and internal vertical incision, and one transverse below the olecranon, but now he only makes an external and a transverse incision, the triangular flaps thus formed being quite sufficient when turned back to expose the articulation for removal, while the subsequent union is much more rapid than when an internal incision is also made.

*Bulletin de l'Académie Royale de Médecine. Mars* 31, 1841.

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*On the Impropriety of Dividing the Muscles of the Back in Lateral Curvatures of the Spine.* By M. BOUVIER.

After numerous experiments, M. Bouvier concludes:

1. That the section of the sacro-lumbalis, longissimus dorsi, spino-transverse muscles, &c. is not immediately followed by any diminution of spinal curvature.

2. The changes which the curves undergo during the succeeding mechanical treatment are exactly identical with the changes produced by this treatment alone, when it has not been preceded by the section of the muscles.

3. The space of time necessary to obtain these changes is the same whether we have recourse to orthopedic means alone, or practise also section of the muscles.

4. In a word, dorso-lumbar tenotomy has no kind of influence in remedying lateral deviation of the spine, properly so called.

M. Bouvier further concludes: 1. That the majority of lateral curvatures of the spine are not owing to muscular contraction; and, 2. That etiology, pathological anatomy, and clinical experiments proscribe the section of the muscles of the back in the treatment of these curvature.

*Bulletin de l'Académie Royale de Médecine, Février* 15, 1841.

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*Case of Intestinal Invagination in the Cow, cured by Gastrotomy.* By M. BOULEY.

AT the sitting of the Royal Academy of Medicine, March 16, 1841, M. Bouley related a case of intestinal invagination cured by gastrotomy in a heifer. The animal had suffered from colic, and on the third day the symptoms were such as led the veterinary surgeon to suspect internal strangulation. He therefore introduced his arm into the rectum and felt a tumour in the iliac region, which confirmed his diagnosis. The seventh day the symptoms persisting, he performed gastrotomy, guided by the tumour in the iliac fossa, which he then found to consist of five or six pouches of intestine, agglutinated together by effusion from the peritoneum. The operator broke up their adhesions, and withdrew the invaginated portions of intestine by gentle tractor. These portions were perfectly healthy. The opening in the abdominal parietes was united by a suture, and a perfect cure was expected. It may be questioned whether a similar operation would be successful in the horse, as this animal is more liable to peritonitis than the bovine race.

*Gazette Médicale de Paris. Mars* 20, 1841.

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*On the Improvement of the Defects in the Refractive Power of the Eye, by Myotomy.* By Dr. KUH, of Breslau.

M. Jules Guerin, Mr. Philipps, and the author have all divided the muscles of the eyeball in the hope of remedying short-sightedness by diminishing the amount of the muscular pressure on the exterior of the eyeball, by which its

axis is lengthened. Philipps has divided the inferior oblique, an operation which the author thinks must be perfectly useless. Both he and M. J. Guerin have divided the straight muscles, either the superior and inferior recti, or, in different operations, all the recti of the same eye. The author's first patient, after the division of all the recti muscles, could see to read as well at a distance of nine inches as before the operation he could at a distance of four inches. In the second case, in which the external and internal recti alone were divided the result was null; he could see no better after than before the operation.

*Casper's Wochenschrift. April* 10, 1841.

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*Report of the Lateral Operations for Stone in Naples, with Statistics of Lithotomy in the last Ten Years.* By M. S. de Renzi.

*First series*—32 men and 1 female operated upon in the year 1839.

Among these 33 patients 10 were under 15 years of age, 22 were adults, 1 was aged. In all there was but a single calculus; 24 were operated on and cured, 9 died. Five were cured in 1 month, 8 in 40 days, 5 in 2 months, 6 in 3 months. Hemorrhage occurred in 5 cases. In 10 the most severe symptoms manifested themselves. In 2 there had been a species of "*pourriture d'hôpital.*" Of the 9 who died, 4 had very large stones, which rendered long and painful manipulations necessary. Of these, 2 died on the second day, 1 on the eighth, 1 on the thirteenth, and all with the symptoms of enteritis or cystitis. Of the other 5, in 1 the dilated bladder filled the pelvis, adhering to the soft parts and the psoas muscle, with an effusion of sanious pus in its cavity. Another, aged 48 years, had purulent collections in the bladder. A child died of vermicular enteritis the third day after operation; 2 others survived 9 and 12 days: and both had purulent collections in the kidneys.

*Second series*—14 men, all operated on with success, 5 of whom were under 15, all the others adults. None of them had dangerous symptoms. In two thirds of these cases the stone was of a moderate size, in the other third it was large.

*Statistics of Lithotomy, from* 1821 *to* 1828, *and during* 1839.

| | Men. | Women. | Cured. | Died. | Children. | Adults. | Old Men. |
|---|---|---|---|---|---|---|---|
| 1821 to 1828— | 579 | 17 | 503 | 91 | 306 | 231 | 59 |
| „ 1839— | 46 | 1 | 38 | 9 | 15 | 31 | 1 |
| | 625 | 18 | | | | | |
| | 643 | | 543 | 100 | 321 | 262 | 60 |

*Il Filiatre Sebezio. Agosto,* 1840.

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*On the present mode of Treating the Itch at Berlin.* By Dr. Hauck.

The treatment of itch has lately been made the subject of extensive experimental observation in the Berlin hospitals, and it is satisfactory to learn that a slight modification of what is termed the English method, has been found in every respect superior to any other that was adopted, accomplishing as it does all the desirable objects of curing the disease quickly, certainly, and economically.

The remedy employed was the sulphur and soap liniment of the Prussian Military Pharmacopœia, composed of one part of flowers of sulphur, and two parts of soap mixed with sufficient hot water to make them into a soft ointment. The patients after a warm bath of soap and water had been applied, were placed undressed in a chamber, kept constantly at a temperature of 95° Fah., and well rubbed with the ointment over all the parts where the eruption had appeared three times a day, and then made to sweat profusely by putting them into warm beds. This system was continued for three days and nights; on the morning

of the fourth each patient had a warm bath, and then if not cured, was provided with clean bed and body-linen, and put in a ward of ordinary temperature, in which the suspicious parts were still rubbed with the ointment, and a warm bath taken every other day. In general, no internal medicines were given; but the diet allowed was reduced to a fourth portion, and water only given to drink.

In this manner, with but one short interval, 1981 were treated and cured between September 1839 and February 1840, making the total number of days of treatment 15,890, which gives on the average 8 days and a small fraction for the cure of each patient, and for the expense of each about two dollars. The exact result was, that

| | | | |
|---|---|---|---|
| In 3 days | there were | cured | 42 |
| — 4 | ,, | ,, | 161 |
| — 5 | ,, | ,, | 333 |
| — 6 | ,, | ,, | 376 |
| — 7 | ,, | ,, | 207 |
| more than 7 | ,, | ,, | 859 |

The treatment of these last was prolonged by many circumstances which can hardly cast discredit on the remedies. In many among them the itch was soon cured, but they remained under treatment for the ulcers which had come on from long neglect of it, or were kept in the hospital till there was no chance of the ulcers communicating the disease. Others among them after being cured of the skin-disease had to be treated for other affections, such as ophthalmia, fever, &c.; and others again had their cure delayed by an obstinate refusal to adopt all accessory treatment. And in addition to these causes giving rise to an apparent increase of the length of time necessary for the cure of the disease, there were some others dependent on the management of the hospitals, and other circumstances quite foreign to the treatment adopted, but which, had they not existed, would have permitted the average number of days of treatment to have been stated much lower.

In the whole 15 months there occurred only 8 cases of relapse, less that is than half per cent. of the cases treated; and among these there was, in many, good reason to suspect a fresh infection. The other cases in which there appeared to be a relapse were in fact only examples of eczema resulting from the stimulus of the skin by the sulphur. In no case did the treatment give rise to any general disorder, or to the inflammations and congestions which some have described as resulting from it.

*Medicinische Zeitung. Februar* 10, 1841.

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*Cure of an ununited Fracture of the Humerus by a Seton.*
By M. JOBERT, of Lamballe.

A robust man, about forty-five years old, received in a fall a fracture of the humerus, complicated by a small wound at the seat of the fracture. The arm was placed in an immoveable apparatus during a month. At the end of this time the small wound still suppurated, and there was no trace of consolidation. During two months longer the ordinary apparatus was applied, but did not hasten consolidation. M. Jobert then passed a seton between the two fragments, but instead of leaving it five or six weeks, as Physick and others have done, he only let it remain eight days. A month afterwards consolidation was completed. Experience has shown that in some cases, the seton has left after its passage organized fistulæ incapable of cicatrization, which maintain the mobility of the bone. Sometimes these passages border on portions of bone denuded by the seton. But when the seton is only left during eight days, it irritates the periosteum, and the inflammation of this latter brings the deposit of a sufficient quantity of osseous matter to effect consolidation.

*Gazette Médicale de Paris. 5 Séptembre,* 1840.

*New mode of Treating Orchitis.* By M. Dieulafoy, Surgeon to the Hotel-Dieu at Toulouse.

The following treatment has been employed by this surgeon in more than thirty cases, always with success and without accident: "with the left hand," says he, "I embrace the testicle at its superior part, and thus strongly stretch the scrotum at the inferior part of the organ, and thrust in a very sharp bistoury perpendicularly. Before the instrument is withdrawn it is necessary to enlarge the opening to allow the free exit of fluid. It is necessary to penetrate the tunica vaginalis, or the puncture is without effect. . . . Treated thus the duration of orchitis is from eight to ten days. . . . . I think this is the sole means the surgeon need employ in treating gonorrhœal orchitis. We need not fear to pierce very deeply, and wound the body of the testicle. Galbois has proved that wounds of the body of this organ are not dangerous, and are not followed by any accident."

*Journal de Médecine et de Chirurgie de Toulouse. Juillet*, 1840.

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*Remarkable Case of Bronchotomy from a Bean in the Air-passages.* By M. Pescheux, of Verneuil.

The principal point of interest in this case is that during this operation a small artery in the neighbourhood of the crico-thyroid membrane was divided, and the child was immediately threatened with suffocation, from the effusion of blood into the trachea. The child voided blood by the mouth and nares, ceased to respire, and became cold, when M. Pescheux instantly applied his mouth to the wound, and sucked out the blood from the trachea for three or four minutes, and being fatigued M. Aury took his place. Some instants afterwards they cauterized the wound and with a female catheter passed into the trachea insufflated the lungs. In a few minutes the child revived, its respiration gradually became regular, and after some cold water had been thrown over it, it looked at the surgeons as though nothing extraordinary had happened. Although the bean could not be found, and was not expelled till five days afterwards, the child perfectly recovered. *Gazette Médicale de Paris. 29 Août*, 1840.

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*Removal of a large part of the Rectum affected with Scirrhus.* By Dr. Francesco Rizzoli, of Bologna.

The patient, Paula Porticelli, was about forty-eight years old when the catamenial discharge permanently disappeared. Numerous hemorrhoidal engorgements supervened, with a sense of pain and weight along the internal part of the sacrum, and constipation, which at length became so obstinate that the patient passed thirteen days without alvine evacuation. There was a discolored fetid exudation from the intestine, and the fæces which came away were in the form of small cylinders. On examining the part scirrhous tubercles were observed, and on passing the finger into the rectum, hard and fungous carcinomata projected, which, about three inches and a half up, were disposed circularly, forming a kind of ring, within which the end of the finger could be introduced with difficulty. An operation being determined on, the patient was placed in bed on the left side, with the thighs bent at right angles with the body, and the nates held apart by an assistant. The operator, with a curved bistoury curved forwards, made a semicircular incision on each side of the anus, which met posteriorly at the coccyx and anteriorly at the perineum; and when the integuments and adipose tissue were divided, he detached the inferior portion of the rectum, taking care to preserve the sphincters. The right index-finger was then introduced above the scirrhous ring, and used to draw it down as far as possible; then the inferior portion of the intestine being held to the right and left sides by two assistants with Museaux forceps, and at the same time drawn lower down, Dr. Rizzoli insulated it from the surrounding cellular

tissue by the index-finger of the left hand, except where it is connected with the vagina, in which place the union being more intimate and firmer, it was necessary to detach it, partly with the bistoury and partly with a spatula; the finger was then again passed beyond the scirrhous ring, which being used as a guide, by means of a pair of long forceps the intestine was removed by incising it all round, taking care that the edge of the instrument should reach the sound part. After exploring and removing all the diseased parts that remained behind, the bleeding arteries were secured by ligature and torsion, and the venous hemorrhage arrested by means of plugging, and the proper dressings applied. The operation was succeeded in a few hours by fever, which was relieved on the fourth day by five bleedings, and the catheter had to be applied twice. The dressings were changed on the third day, when the patient was again bled, and castor oil given, which procured a copious evacuation followed by great relief. After a few days granulation commenced, and the alvine and vesical evacuations were regular. The patient was attacked with phlebitis on about the thirtieth day, which appeared in both legs, and which gave way to cataplasms and bandaging. Afterwards, the new portion of intestine having a tendency to contract, mechanical dilatation was applied, and the patient continued to live free from any inconvenience and without further medical treatment. *Bulletino delle Scienze Mediche di Bologna. May and June*, 1840.

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*Notice respecting the Operation for Stammering.*

"To prevent all questions of priority, I hereby inform my colleagues, that I first performed my operation for stammering on the lad Dönau, thirteen years old, on the 7th of January, 1841, and had the honour to present him completely cured at the meeting of the Hufeland Medical Society on the 22d of January, 1841. DIEFFENBACH." *Medicinische Zeitung, Feb.* 3, 1841.

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*On the subcutaneous Division of the Pronator and other Muscles of the Hands and Fingers.* By Dr. P. DOUBOVITSKI.

THE only point of practical importance in this case, which is related at a most unnecessary length by the patient, who, though a doctor, suffered himself to be grossly maltreated for a fracture through the condyles of the humerus, is that it is not safe to divide tendons that run in synovial sheaths. Two out of four of the tendons of his fingers thus divided failed of reunion, and he lost the power of moving the corresponding phalanges. He knows of other cases in which the same untoward result followed similar operations; and he justly observes that when under such circumstances tendons about the feet have seemed to reunite, it may have been that other muscles have performed the actions that properly belonged to them, and that the success of the operation has consisted, not in the restoration of the action of the contracted muscles, but only in the giving to the distorted foot its proper form.

*Annales de la Chirurgie. Février*, 1841.

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*On the Treatment of Pseudarthrosis.* By Drs. WOPPISCH and BAUER.

Two cases are here related for the purpose of pointing out the advantages of a long-neglected method of treating false joints, namely, the rubbing together of the ends of the bones. With the exception of the mode of treatment, the cases present nothing of particular interest. In the first, the portions of a fractured thigh which had not united in fifteen weeks were violently rubbed together for a quarter of an hour; the operation gave great pain, but it was followed by such inflammation that the bones united accurately and firmly after a few weeks more. In the second case the fracture (a compound one of the leg) had not united at the end of the eighth week; the same treatment was adopted with an equally favorable result; (but it may be a question whether patience alone might not have obtained the same end.)

*Medicinische Zeitung. December* 30, 1840.

*Iodine in Opacity of the Cornea.* By Dr. Lohsse.

The case in which this remedy was successfully employed was one of opacity of the cornea consequent on syphilitic ophthalmia, and so considerable as almost completely to destroy vision. The iodine was given internally, and from four to six drops of the following collyrium were let fall into each eye three times a day.

℞. Iodini, gr. j.
Potassii iodidi, gr. ij.
Aq. dest. ʒvj. M.

Afterwards this was exchanged for an ointment consisting of iodine, gr. jss, Iodide of potassium, ℈j, and lard, ʒss, of which a small portion was once or twice a day put between the eyelids. The cure was perfected in three months.

*Medicinische Zeitung.* *Marz* 3, 1841.

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*Researches on the Anatomy of the Aponeuroses and Muscles of the Eye, in relation with the cure of Strabismus.* By M. Bonnet, Surgeon to the Hôtel-Dieu at Lyons.

These researches conduce to the scientific explanation of the persistence of the action of the orbital muscles, after the section of their anterior part in the operation for the cure of strabismus; they show the method to be followed in this operation, and throw some light on the movements of the eye and eyelids, considered in the normal state.

The eye is not in contact with the fatty matter of the orbit, as anatomists have stated; it is separated from it by a fibrous capsule, in which it moves with facility. This capsule, concave and open within, is inserted on the anterior extremity of the optic nerve, around the two posterior thirds of the eye without being in contact with them, and terminates on the eyelids on which it is prolonged. The straight and oblique muscles traverse it to reach the eye and contract, with it, intimate adhesions. They have thus two insertions, the one into the sclerotic, the other to the fibrous capsule, and they cannot move one without transmitting to the other all the movements they execute. This has not been hitherto noticed by anatomists, and we shall trace their operation on the movements of the eye and eyelids.

We know that when one of the muscles of the eye has been cut in the operation for strabismus, the increased action which caused the disease immediately ceases, and the movements attributed to the divided muscle are executed as in the healthy state. The explanation of these effects, to be satisfactory, should apply to any of the muscles of the eye indifferently, for after division of any of them the persistence of their function is observed. This cannot be attributed to any phenomenon, which requires, like cicatrization, a work of many days, for the motions effected by the divided muscles are manifested immediately after their section has been made. The anatomical explanation is founded on the fact that the muscles of the eye are inserted both into the sclerotic and the fibrous capsule, and the first of these insertions alone is cut in the operation for strabismus. The second remains entire; the muscle continues to act on the fibrous capsule, and by this medium transmits to the eye these contractions simply weakened.

This double insertion of the muscles of the eye, and the adhesions of this organ to the fibrous capsule explain, it is true, the persistence of the action of the muscles after their division, and indicate the conditions of this persistence, but they do not lead to the knowledge of the method to be adopted in operating for strabismus. But this knowledge is gained in part at least from the dispositions of a fibrous membrane immediately applied over the whole external surface of the sclerotic. This membrane, altogether distinct from the capsule before described, is confounded with the fibrous sheaths of the muscles, and serves to unite them to each other, forming an immediate layer between the conjunctiva and sclerotic. This must be traversed in the operation, and when by its section

we have reached the lax cellular tissue which unites it to the eye, the stylet glides without obstacle behind the sheath of the muscles, which, with their aponeurosis, can then be divided certainly and entirely. The knowledge of this membrane gives an astonishing facility to the section of the muscles, both in the living and dead subject, and is as important in the operation for strabismus as that of the sheath of the arteries in the ligature of these vessels.

If it is demanded how the beautiful harmony which always exists between the elevation and depression of the eyelids and similar motions of the eye; or what muscle depresses the lower eyelid; questions hitherto unresolved: their phenomena are readily comprehended when we know that the tarsal cartilages are the continuation of the fibrous capsule into which the latter is inserted, and which conveys the motions of the levator and depressor muscles of the eye. These muscles cannot contract without acting at the same time on both eye and eyelids, and the cause of this simultaneous action is simply anatomical, for we cannot move these muscles in the dead body after having denuded their posterior half, but the eyelids move at the same time and in the same relation with the globe of the eye.

*Bulletin Général de Thérapeutique.* 15 et 28 *Février*, 1841.

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*On Congenital Dislocation of the Femur.* By M. BOUVIER.

AT the sitting of the Royal Academy of Medicine, Jan. 26, 1841, M. Bouvier presented two preparations illustrative of the two principal forms of congenital dislocation of the femur. In one the head of the femur is received in a superficial cavity above and behind the cotyloid cavity, which latter is reduced to very small dimensions. The capsule embraces both the new and old cavities, and is strongly applied around the femur, the movements of which it limits, and does not allow the head to be brought opposite the cotyloid cavity. The subject was a female seventy-six years of age, and the dislocation was said to have occurred when she was four or five months old, but the probability appeared that it was congenital.

The second specimen was taken from a woman fifty-three years of age, and in this no point of contact exists between the head of the femur and the ilium, which were attached to each other by the capsular ligament and muscles alone. This disposition of parts occurred on both sides, and we know that it is recognized in congenital dislocations alone, and especially in those which affect both sides. The head of the femur is lower and more behind than is usual in iliac luxations, and is only a few lines before the ischiatic notch, and very near the great sciatic nerve. Entirely covered by capsular ligament, the head of the femur rests on a cellulo-fibrous cushion, which here covers the surface of the ilium, and which is united to the capsular ligament by a sort of lax cellular membrane. The round ligament remains, much elongated, flattened, and partly confounded with the capsule. The head of the femur is almost of its normal size, while scarcely a vestige of the cotyloid cavity remains, and this of a triangular form, as is usual in most similar cases. Several of the muscles have undergone important changes, but none in such a manner as to limit the motions of the limb, all of which, with the exception of extension, were almost unaltered.

*Bulletin de l'Académie Royale de Médecine.* 15 *Février*, 1841.

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*On Compression in the Treatment of Mammary Abscess.*
By MM. TROUSSEAU and CONTOUR.

THIS is a very long paper, illustrated by eight cases, to show the utility of compression of the breast by means of strips of diachylon plaster, an inch broad and several feet in length, carried completely round the body so as to produce regular and methodical compression of the whole breast. The conclusions of

the authors are, 1, that this compression may be employed in all forms of inflammation of the breast in nurses; 2, it will sometimes cure when used at the commencement of the inflammation; 3, during suppuration it will not arrest the progress of the formation of pus, but it immediately relieves the pain; 4, compression should be made twenty-four or forty-eight hours after the abscess has been emptied; 5, under its influence the pain ceases, the walls of the abscess unite, fistulæ are cured, and a few days in general complete recovery. Continued for a time it prevents relapse.

*Journal des Connaissances Médico-Chirurgicales. Février*, 1841.

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*Statistics of Amputation performed in the African Army, in Hospitals and the Field, during the years* 1837-8-9. By Dr. Guyon

The number of amputations performed in the above years (the campaign of Constantine in 1837 excepted,) was 63, namely:

| | |
|---|---|
| Disarticulation of the shoulder-joint | 6 |
| ,, ,, elbow | 2 |
| ,, ,, wrist | 6 |
| ,, ,, knee | 1 |
| ,, partial, of foot | 1 |
| ,, tarso-metatarsal | 1 |
| Amputation of the thigh | 16 |
| ,, ,, leg | 7 |
| ,, ,, arm | 15 |
| ,, ,, fore-arm | 8 |

Of these 63 patients, 46 were cured, 17 died. As, however, four died from circumstances scarcely connected with the amputation, the proportion of deaths may be stated as 1 to 11. This result is much more favorable than that during the siege of Constantine in 1837, for of 10 amputations performed at Médéah, only 1 survived, and of 62 at Blidah, 39 died.

Of the 63 operations referred to above, 44 were performed immediately, 19 secondarily. The former gave 32 cures, 12 deaths; the latter 14 cures, 5 deaths. Thus the proportion of cures after secondary amputation was not less satisfactory than that after immediate.

*Gazette Médicale de Paris. Février* 13, 1841.

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*Operations of Laryngotomy.* By M. Guyon.

In a paper on the operations performed with the French army in Africa, M. Guyon states that laryngotomy was twice performed at Bone by M. Génin in croup, "*angines croupales*," but it did not retard death. M. Vital performed the same operation at Constantine for laryngitis, the symptoms of which ceased after its performance. The wound cicatrized, but the same symptoms returned as before, and the patient died six weeks after he had undergone the operation. On post-mortem examination, the mucous membrane was found tumefied so as to obstruct the opening of the larynx. The same operator, at Algiers, performed the same operation to remove a leech from the larynx, and it was crowned with success. M. Guyon states that he and M. Méardi have seen three cases in which a leech had got into the larynx, but they were removed without a necessity for laryngotomy.

*Gazette Médicale de Paris. Avril* 3, 1841.

## MIDWIFERY.

*Hemorrhage after Delivery arrested by a new Method.* By Dr. HECKING, of Coblenz.

A WEAK and delicately made woman, soon after she had been quickly and easily delivered of twins, began to have more than usual hemorrhage from the uterus. Permanent pressure on the abdomen, with friction over the uterus, and repeated dashing with cold water were employed, but they were all ineffectual, and the hemorrhage grew more rapid. The author therefore introduced his hand into the uterus (after having dipped it in cold water), and doubling his fist endeavoured, first by rubbing the parietes, and then by compressing them against his other hand, which was placed on the walls of the abdomen to bring on contraction of the uterus. But this plan also was altogether useless; the uterus, still remaining distended, was every instant filled with fluid and coagulated blood. After all attempts had proved vain, and when the patient's danger was now extreme, the author took a sponge dipped in cold water, and then squeezed it on the internal surface of the uterus, so as completely to wet it all over, at the same time maintaining the external pressure. As often as the sponge became covered with coagulated blood, it was taken away, washed with cold water, and again introduced as quickly as possible. By this means several times repeated (for in the powerless condition of the patient it was not a difficult thing to do,) the hemorrhage was at last arrested, and the woman's life was saved.

*Casper's Wochenschrift. November* 28, 1840.

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*Hernia of the Uterus, and Cæsarean Operation: Mother and Child saved.* By M. LEDESMA, of Salamanca.

ON the 26th of January, 1839, a female named Ramus, 24 years of age, of good constitution, having had six children, stated that some time before her marriage she had an inguinal enterocele on the right side. When the seventh pregnancy had advanced to the third month, she suddenly perceived an unpleasant dragging sensation at the bottom of the left side of the abdomen; the abdominal tumour disappeared, and she voided some blood by the vagina. Something unusual, hard, and painful on pressure was now discovered in the site of the inguinal hernia. She frequently attempted reduction in vain. Seven weeks afterwards she perceived evident movements. The tumour descended from the bottom and right side of the abdomen on the thigh of that side, distending the right labium pudendi, falling on the pubis and crossing towards the left thigh; it was 22 inches in extent, 25 in circumference at its middle, and $22\frac{1}{2}$ at its junction with the abdomen. It was fluctuating, and on displacing the liquid, a hard body was perceived which appeared moveable in the liquid. The neck of the uterus could not be found in the vagina. At seven months the sounds of the fœtal heart, and the placentar souffle when examined by the stethoscope were perfectly distinct from the superficial position of the uterus. M. Ledesma judged by the souffle that the placenta was inserted to the left and a little anteriorly: the sound was clear and very circumscribed. Attempts at reduction were useless. On the 6th July she experienced the first pains. On the 7th the membranes broke, and the waters were discharged by the vagina. Reduction again attempted in vain. Hysterotomy was practised, with care to avoid the left lateral part of the tumour. A great quantity of blood followed the incision of the uterus. The head of the fœtus was against the cervix, the surgeon seized the feet which were at the fundus and forcibly withdrew the child. The placenta was at the place foreseen. After the uterus was emptied a tepid bath was prescribed, which excited uterine contractions and arrested all hemorrhage. Reduction of the uterus was thought to be impossible, and it was left externally.

July 12. The lochia take the route of the vagina. After some accidents, slight in comparison with the severity of the operation, the patient's health is reestablished. The wound suppurated more than a month.

July 13. The catamenia reappeared.

August 11. Convalescence is complete. The tumour has considerably diminished. The infant was well, but died some time afterwards.

*Gazette Médicale de Paris, 7 Novembre*, 1840.

(*Taken from the Journal de la Société de Médecine Pratique de Montpellier.*)

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*Retroversion of the Unimpregnated Uterus.* By Dr. ALKEN, of Bergheim.

A young woman, twenty-six years of age, accustomed to hard work, found herself suffering one day from difficulty in passing her urine and fæces; and these gradually increased till, after fourteen days, there was complete retention of urine and of fæces. When the author was called in he found the patient with a pale fallen countenance, cold extremities, a small, rapid, jarring pulse, hurried respiration, insatiable thirst for cold water, hiccup, vomiting, &c. The abdomen was very tense, the urinary bladder distended up to the umbilicus, and every movement of the abdomen and the slightest touch extremely painful. Examination detected a complete retroversion of the uterus, so that its vaginal portion was immoveably fixed against the pubes, and its fundus was thrust deep into the pelvis. The patient had been in this state for ten hours. The urine was with difficulty drawn off by the catheter, and after bleeding and the warm bath, an attempt was made to reduce the uterus to its right position by pressing it in opposite directions through the medium of the vagina and the rectum. After the efforts had been continued an hour the uterus returned to its place. The replacement was perfect; but on the following day the retroversion again occurred after some exertion. It was again reduced with much greater facility than before, and by observing the horizontal posture for nearly three weeks, it was prevented from again returning. The patient was watched for several months, and it was clearly determined that at the time of the retroversion the uterus was in the unimpregnated state.

*Casper's Wochenschrift. April* 3, 1841.

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*Extraction of a Foreign Body implanted in the Uterus.* By M. MAISONNEUVE, of the Hospital St. Louis.

THIS patient was thirty years of age on her admission to the hospital, Sept. 14, 1840. At the age of twenty-eight she was in good health, became pregnant, and in the fifth month of gestation states that she miscarried and suffered from severe metro-peritonitis. She was obliged to enter the hospital La Pitié soon after she had begun to leave her room, where the surgeon diagnosticated metritis with hypertrophy of the anterior surface of the uterus. Various means were persevered in without effect, and when she came under the care of M. Maisonneuve her general powers were enfeebled, digestion bad, hectic at night, and she had dull continued pains in the loins and hypogastrium, which latter region was occupied by an irregular hard tumour, slightly painful on pressure, which filled the pelvis and extended into the iliac fossa. The os uteri permitted the entrance of the finger, but the body and neck of the organ were lost in an irregular, hard, and absolutely immoveable mass. Examined by the speculum the vagina appeared to be in the normal state, and the only unusual appearance was abnormal patency of the os uteri, which permitted the surgeon to see something whitish. He passed a stylet to discover the nature of this substance, and was astonished to find that he could circumscribe the unknown object by passing the stylet before and behind it. It adhered to the lips of the os uteri on all sides.

Persuaded that this was a foreign body implanted in the walls of the uterus,

M. Maisonneuve first endeavoured to divide it with scissors but could not. He then placed one beak of a pair of long polypus forceps behind and the other before it, and by careful traction removed, without causing much pain, a piece of wooden stick, 122 millimeters in length, pointed at one extremity and bent at the other. Looking to her account of the case, it appeared highly probable that this stick had been broken in the uterus during criminal efforts to produce abortion, and this opinion has been since confirmed.

The operation was followed by a return of abdominal and lumbar pains, and great febrile reaction, which disappeared in about eight days under general bleeding, baths, and several applications of leeches. But there remained a tumour in the pelvis which probably resulted from chronic adhesions of the uterus, bladder, and rectum, with some of the intestines. However by the 1st of January, 1841, the tumour had greatly diminished and the general health was much improved.

*Gazette Médicale de Paris. Avril* 3, 1841.

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*Experiments on Uterine Injections.* By M. HOURMANN.

1*st series.* Injections of the uterus separated from the body. Eleven experiments were made. Five times the liquid traversed the fallopian tubes and was effused into the peritoneum; in the other six the tubes were not distended, or but slightly.

2*d series.* Injections of the uterus *in situ.* In five experiments, the liquid passed through the tubes.

3*d series.* Injections of the uterus *in situ* some days after delivery. Injections passed with force from large syringes never penetrated the tubes. The mucous membrane of the uterus and tubes was generally tumefied and red. Four of these experiments were made by M. Danyau.

Among the six cases of non-penetration in the first series, three were in females lately delivered. These seven cases, then, show that uterine injections may be employed without danger after accouchement.

*Gazette Médicale de Paris. Février* 20, 1841.

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*On the Microscopic Characters of healthy Milk.* By Pr. Von D'OUTREPONT, of Würzburg.

HE has recently put to the test some of M. Donné's statements with reference to the characteristics of healthy milk, and has arrived at somewhat different conclusions, though he fully confirms Donné's statements with reference to the difference of the corpuscles in the colostrum and milk.

Professor Von D'Outrepont found that in the greater number of instances the peculiar granular bodies of the colostrum (*corps granuleux*) disappeared on the third day after delivery and not on the sixth or tenth as stated by Donné. Even in those cases, however, in which they could still be detected on the tenth or twelfth day, the milk produced no injurious effects on the infant; nor did it indeed in some instances in which the milk retained the characters of colostrum so long as a month after delivery. The milk of a female labouring under severe metro-peritonitis presented the characters of true milk, not of colostrum. That likewise from the left breast of a person whose right breast was in a state of suppuration presented all the characters of healthy milk, though pus-globules were mixed with the milk in the other breast. In two instances where the breasts became inflamed without suppurating, the milk continued to present all characters of the healthy secretion and did not contain any of the granular bodies of the colostrum.

Professor D'Outrepont had the opportunity of examining the milk of a woman who, after having suckled her third child for some months, began to menstruate regularly. During the flow of the menses the child became indis-

posed to suck and suffered from vomiting, but recovered its health immediately on their cessation. During menstruation the milk possessed all the characters of colostrum, while at other times its appearance was precisely that of healthy milk. The secretion from the breasts of a woman who had never been pregnant presented all the peculiarities of colostrum; that contained in the breasts of another woman who never suckled her children, though the glands were always full except during pregnancy, differed in no respect from healthy milk.

*Neue Zeitschrift für Gebertskunde. Bd.* x. *Heft* i.

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

### *On Asphyxia from Carbonic Acid.* By M. Devergie.

In the 11th Number of this Journal, at p. 241, our readers will find the report of a singular case of murder by carbonic acid, recorded by M. Devergie. Some of the medico-legal questions which were raised in that case were of so novel a character, that the author has been induced to reexamine the whole subject. We shall here give an abstract of the results of his observations and experiments.

1. *On the circumstances under which asphyxia by carbonic acid may take place.*

It is commonly supposed that asphyxia by carbonic acid is not likely to occur in those cases, where the source of combustion does not exist in the apartment itself in which an individual may be found apparently dead. This is an error, for it is well known that when two chimneys or the flues of stoves communicate, if a fire be lighted in one room, the vapour may be drawn through the communicating chimney by a downward current into a distant room. The distribution of the vapours of burning fuel will therefore depend greatly upon the state of expansion or contraction in the air of such communicating apartments. This source of asphyxia has been hitherto overlooked. M. D'Arcet has published several cases which show that serious accidents may thus occur without the cause being even suspected by the medical attendant.

2. *What takes place in an apartment in which a charcoal fire has been lighted?*

When the charcoal is kindled, and so long as it burns it absorbs oxygen from the air, producing thereby carbonic acid and carburetted hydrogen: the latter, however, is converted as soon as it is formed into carbonic acid and water. Since the carbonic acid thus produced occupies the same bulk as the oxygen which was required for its production, it follows that the quantity of air in the apartment can undergo no augmentation, if we except the addition to it of a very small portion of carburetted hydrogen that may escape combustion. But, although there is no actual increase in quantity, there is a vast difference of bulk, which is due to the great expansion caused in gases by a rise of temperature. The heat of combustion is such, as probably to give a threefold volume to the air of an apartment; and to cause the upper and lower strata rapidly to change places. This increased bulk must continue during the whole period of combustion; and the dilated air will endeavour to find an outlet through every fissure or opening connected with the apartment. If there be any impediment to this escape, it will exert a pressure in proportion to its state of expansion on all surrounding bodies: a pressure which would materially impede respiration, and accelerate the access of asphyxia.

There is another fact of medico-legal interest. While the combustion lasts, there are two currents of air circulating through the room; the one ascending of heated air, and therefore rich in carbonic acid, the other descending of cold air. It is by means of the ascending current that the carbonic acid produced in combustion is equally diffused through all parts of the chamber. To establish

this point, several experiments were made. Among others two vessels for collecting gases were opened in the apartment, so soon as the combustion had terminated: the one being placed at the top, and the other on the floor. The quantity of carbonic acid collected was perceptibly the same in the two cases, the proportion being about one fiftieth part. *Hence it follows that during the combustion of charcoal, the carbonic acid is equally diffused throughout the space in which it is produced.*

The air of such an apartment is unfit for respiration: 1st, because it is deficient in the quantity of oxygen, necessary for the maintenance of that function; and 2d, because it is contaminated by deleterious gases.

So soon as the combustion is finished, there is a tendency to the production ofan equilibrium in temperature: the expanded air slowly contracts and occupies less bulk. If any air had previously escaped from the apartment through openings, a fresh portion now enters to supply its place, so that when the equilibrium is perfect, the air is less vitiated than it was during combustion. If the apartment were so close that no air could escape, then the pressure of the expanded mass diminishes with the decrease of temperature, and resumes its original condition; but in this case, it is just as much contaminated as it was while the charcoal was burning.

3. *Do the gases formed separate according to their specific gravities after the air has cooled?*

This is a question of material importance in relation to asphyxia, and to illustrate its application, the following supposed case may be taken: Two persons are found asphyxiated in the same chamber; the one may be stretched on the bed, the other lying on the floor. Which of these two has been more powerfully affected by the asphyxiating cause, (the charcoal vapour)? The solution of this question must rest partly on reasoning and partly on experience. After the complete combustion of charcoal, the respirable air becomes specifically lighter: a large quantity of oxygen has been removed from it in the production of carbonic acid; nitrogen now predominates, and this gas is lighter than air. Hence we may affirm that irrespective of the carbonic acid produced, the specific gravity of the air of the chamber has diminished. The specific gravity of carbonic acid is one half greater than that of air (1·5 : 1); but in consequence of the change which the air in the chamber has undergone, the difference becomes still greater. We might therefore infer that when once the air has cooled, or even during the act of cooling, the carbonic acid would tend to fall to the lowest part of the chamber. To this inference may be opposed the experiments of Dalton on the diffusion of gases, which establish that these bodies, notwithstanding the difference in their specific gravities, have a tendency to mix uniformly on contact. M. Devergie was, however, induced to think that under the above-mentioned circumstances, carbonic acid would collect on the floor of an apartment, in a stratum of variable density; and to remove any doubt on the point, the following experiment was performed:

Two vessels proper for collecting gas were placed the one at the highest and the other at the lowest part of a moderately large chamber, in which a quantity of charcoal was burnt. The next day, when the apartment had become thoroughly cooled, the bottles of gas were removed: a quantity of solution of potash was put into each, and they were immediately closed. Two hours afterwards, the solution was placed in a proper vessel connected with a graduated jar, and sulphuric acid added until the potash was saturated. The liquid in the bottle from the floor of the chamber yielded 150 parts of carbonic acid, while the other vessel, which was at the summit of the apartment, gave only 32 parts. A cat placed on the floor uttered painful cries for an hour and a half, and the next morning was found dead.

We must therefore consider it as proved by this experiment, that after combustion has ceased, and while the apartment is cooling, the carbonic acid *separates from the air by its greater specific gravity*, and forms on the floor a stratum,

of which the thickness must vary according to the quantity of fuel consumed. Still the gas does not separate entirely: a portion remains mixed with the air in the upper part of the room, which contains about $\frac{1}{8}$th; while that on the floor contains $\frac{1}{9}$th. It is possible that this phenomenon may be due to gaseous mixtures, taking place in definite proportions, as well as to the effect of the difference of temperature in the different strata of air. The fact is somewhat analogous to the separation of metals, according to their density, in melting certain kinds of allays. The following case is quoted as confirmatory of this view:

A gentleman tried to destroy himself by burning charcoal in his chamber. For this purpose he lighted a brazier and threw himself on the bed. Not finding that he became affected by the vapour, he for the time abandoned his design. Upon mentioning the circumstance to some friends, as if another party had been concerned, he learned from a professional man present that carbonic acid only produced serious effects to those placed on the floor of a chamber. A few days afterwards, this gentleman was found dead in his room, sitting at the foot of his bed, and a candle still burning on a table. It was evident that he had destroyed himself by charcoal vapour.

4. *On the nature of asphyxiating media.*

The only data which exist on this subject are derived from the experiments of Orfila. The analysis of the vapour, produced from charcoal, according to this chemist, varied with the degree of combustion; when this was *slow*, the carbonic acid formed about one seventh of the mixture; when *rapid*, one ninth: these results appear so paradoxical, that M. Devergie places but little reliance on them.

Charcoal vapour, when existing in large quantity, has a bluish tint owing to the smoke mixed with it. This colour the author has found to disappear in twelve hours after the completion of combustion, and he thinks it may become still more rapidly dissipated. This fact, he observes, may be useful as a datum to determine the period at which asphyxia probably commenced in an unknown case. The vapour has likewise a peculiarly disagreeable odour, producing nausea and cephalalgia: it is chiefly perceptible when the charcoal is first ignited, and disappears so soon as the combustion has become very active. It probably has some influence in the production of asphyxia, and to it may be attributed the narcotic effects of charcoal. This charcoal vapour is heavier than atmospheric air; it reddens litmus, gives a white precipitate with lime water, and extinguishes burning substances. In relation to the last character, it is necessary to observe, that in a state of admixture with air, it may produce asphyxia without extinguishing the flame of a lighted candle. Among the experiments performed, a cat and two birds were asphyxiated in an apartment, in which five candles were burning. This vapour never contains sufficient carbonic oxide or carburetted hydrogen to become inflamed by the approach of a candle.

5. *On the quantity of charcoal which is required to be burnt, in order to render a certain quantity of air deleterious.*

Varin showed that animals perished in about three minutes, when plunged into an atmosphere containing about one fifth of its volume of carbonic acid. But it is not necessary for asphyxia to ensue, that the atmosphere in a room, in which charcoal is burning, should acquire such an impregnation as this. There is obviously a great difference between a mixture of free carbonic acid and air, and a mixture containing carbonic acid actually produced at the expense of the oxygen of the air. Thus in the two cases, the air may not contain more than ten per cent. of carbonic acid; but where this has resulted from combustion, the atmosphere will contain seven per cent. less of oxygen and seven per cent. more of nitrogen than in the case where free carbonic acid has been mechanically added to air. On the other hand, Varin proved that the strongest proportion in which carbonic acid could be mixed with air without rendering it in-

jurious to respiration is about two per cent. If only one fourth of the oxygen be removed by combustion, it will contaminate the air with one twentieth of carbonic acid, and render it asphyxiating.

Experiments were then performed to determine how much charcoal was required to be burnt, in order to remove one fourth of the oxygen from a given quantity of air. In these experiments it was necessary to allow for the bulk of the charcoal; the wood from which it was made; the degree to which the charring process had gone on; the quantity of hygrometric water present, and the different proportion of salts entering into its composition. We need not detail these experiments, since, as the author allows, from the circumstances above mentioned, they can only be regarded as remote approximations to the truth. An answer to a question of this kind must therefore be a matter of pure speculation.

6. *How to determine the capacity of a chamber in which asphyxia has taken place.*

This is rather a question for a surveyor. We are directed to multiply the length in feet by the width, and the product by the height, to obtain the measurement in cubic feet. The author lays great stress upon our making an allowance for any want of angularity in cupboards, closets, &c.

7. *How to determine the quantity of charcoal which has been consumed.*

Charcoal in burning leaves on the average about four per cent. or $\frac{1}{25}$th of its weight of ashes. All we have to do is to collect the ashes and to multiply them by 25. Here we are met by the objection, that different kinds of charcoal yield different quantities; and we can test the accuracy of our inference only in those cases where some of the charcoal used happens to remain unconsumed. The author advises us to satisfy ourselves that there were no ashes already in the brazier, prior to the asphyxiating attempt being made.

8. *On the closure of apartments and its influence in the production of asphyxia.*

It is a matter of general belief, that for asphyxia to take place it is necessary that the apartment in which the charcoal is burning should be perfectly closed. That this view is erroneous many cases on record clearly show. The perfect closure of a room is a circumstance highly favorable to the production of asphyxia; but it is not an indispensable condition. In the author's experiments the windows were loosely secured, and the chimney not perfectly closed. Persons have been repeatedly found asphyxiated in apartments, where there was a pretty free communication with the external air, by means either of a half-opened window or flue. We are then justified in affirming that this imperfect closure of outlets is unfavorable to the production of asphyxia, but that it does not render its occurrence impossible.

9. *On the influence of the situation of the person in the occurrence of asphyxia.*

A case has been already related (3), in which it was shown that the asphyxiating atmosphere had not the same intensity throughout. An individual, who escaped this form of death in a first attempt while lying on a bed, fell a victim to it when, on a second occasion, he placed himself on the floor. It has been proved, that during the actual combustion of charcoal, the air is equally impregnated with carbonic acid throughout; but that in the act of cooling this gas fell by its greater specific gravity to the lower part of the chamber. The following points may therefore be considered as settled: 1, That, whatever be the situation of a person, he will perish from asphyxia if the quantity of charcoal consumed was sufficient, *during its combustion*, to render the atmosphere deleterious: 2, If two persons are situated, the one on the floor and the other three or four feet above that level, the latter may not be asphyxiated while the former may perish, provided the quantity of charcoal consumed was insufficient to render the air deleterious during its combustion, but still sufficient to impregnate with carbonic acid during the act of cooling the lowest strata of air.

10. *On the influence of sex, age, and profession (!), on asphyxia.*

All persons are not equally susceptible of the influence of charcoal vapour. Those who consume much of this kind of fuel, for domestic or other purposes, resist its effects better than others. According to the Police Registers of 1834-5, there were in Paris 360 cases of asphyxia by carbonic acid. Out of this number there were nineteen cases of two persons together (men and women), and only one case of two men together. There were only three instances, out of these nineteen cases, in which one of the two persons could be resuscitated, and in each of these the subject was a female. The number of females saved is greater than of males. In 184 cases of asphyxia, occurring during the year 1835, there were eighteen females saved out of seventy-three cases, or about one fourth, and nineteen males out of eighty-three cases, or the proportion of about one fifth.

MM. Marye and Ollivier have quoted cases which lead to a contrary inference, showing that there is a greater power of resistance to the effects of this gas in males than in females. M. Devergie pronounces these to be isolated facts, and prefers his own results.

*Annales d'Hygiène. Jan.* 1840.

[Remarks.—Such is an abstract of M. Devergie's late researches on asphyxia from carbonic acid. Since we last adverted to this subject (No. XI. July 1838), one or two cases have occurred in this metropolis which have given rise to considerable discussion, and it cannot be concealed that there is even now so great a difference of opinion in the profession, that probably no two medical witnesses would agree in their answers to some of the questions proposed by M. Devergie. There are many statements made by the author of this paper, to the correctness of which we cannot assent; and as we should regret to be the means of disseminating views which might give rise to much mischief, if implicitly relied on, we shall here offer a few remarks on some of the most disputable doctrines.

In the second query we find it stated, that the air by expansion, if unable to find a ready outlet, will exert a compressing force on the body, impeding respiration, and accelerating asphyxia. We can understand that phenomena of this kind might ensue, if the man as well as the fuel were confined in a hermetically sealed vessel; but it seems to us impossible to admit, that in ordinary apartments, any such serious effects as the author imagines can take place.

In the third query, M. Devergie assumes from his experiments, that gases of different specific gravities mixed by heat, are capable of separating from each other to a greater or less extent on cooling, so that the heavier gas falls in great proportion on the soil or floor. To us this appears a material question in relation to death by carbonic acid; and the grounds on which such a statement is based require to be closely scrutinized.

The law of the diffusion of gases, now so well known to all chemists, has contributed to remove the old opinion, that carbonic acid from its weight had a tendency to collect and remain on the lowest levels. Gases are now known to mix readily on contact, whatever be their relative densities. Thus carbonic acid will rise into hydrogen, and hydrogen will fall into carbonic acid, although the difference of sp. gr. is as 22 : 1. There is no proof of this mixture of gases taking place in *definite* proportions: on the contrary, they are generally considered to mix in *all* proportions. Again, another fact commonly received by chemists is, that when once mixed, gases do not again separate from each other according to their specific gravities. M. Devergie, while he admits the principle of diffusion, affirms, that where this has been aided by heat, a partial separation takes place on cooling, so that carbonic acid and air, although mixed when heated, separate when cold. The single experiment which he adduces in support of his peculiar view is unsatisfactory. It would be impossible by such a method as that pursued by him to determine, with the least pretension to accuracy or certainty, the relative proportions of carbonic acid when mixed with air. This will be easily proved by trying the experiment with a mixture con-

taining a measured quantity of pure carbonic acid. The case which he adduces is altogether inappropriate as an illustration. How are we to know that, in the first attempt at suicide, when the man was lying on the bed, the air of his apartment became sufficiently impregnated with carbonic acid to destroy him? yet it is here assumed, that in the two attempts there was an equally fatal contamination, and that the deceased only died in the second attempt, because he had placed himself on the floor!

We might stop here; but we are desirous of showing, not merely that M. Devergie's opinion is unsupported by his own experiments and reasoning, but that it is at variance with the most simple facts of almost daily occurrence. If on cooling, carbonic acid separated from air, as alleged by him, it is not to be expected that when the gases are once perfectly cold they would readily mix, nor that the heavy gas would again rise into the air and diffuse itself with a rapidity proportioned to the surface of contact, and yet that these phenomena do actually occur the most simple experiments will show. If a large glass vessel, having a diameter at the mouth of about five inches, with a depth of sixteen inches, be filled with carbonic acid and exposed to the access of air in a quiet and cool apartment, the whole of the gas will disappear from it in the course of a few hours. How is this to be accounted for if M. Devergie's position be true? If his opinion be correct, the carbonic acid should not leave the vessel. Let the capacity of its mouth be five times as great as that above mentioned, the gas will quit the vessel in a *few minutes*, from the greater extent of surface exposed. Now this is precisely the condition of M. Devergie's asphyxiating apartment: there is considerable extent of surface, and yet it is to be supposed according to him that the carbonic acid collects in larger proportion at the bottom of the room than it does at the top. If M. Devergie's statement were well founded, it is obvious, that in a calm state of the atmosphere the air in the streets of a crowded metropolis would be perfectly irrespirable.

In support of the erroneous view which we have just condemned, we sometimes find it stated, that carbonic acid exists in wells, on the floors of cellars and grottoes; and the Grotto del Cane near Naples, is often quoted as an instance where the gas is collected on the soil. But in all these cases it is forgotten that the source is permanent, and that the gas is continually issuing from fissures in the soil, or from decomposed matter.

To our minds, it is not proved satisfactorily that any separation of gases ever takes place, such as that above assumed; and while there is this want of evidence on the one side, there are numerous facts on the other, which render it certain that such a separation does not ensue, at least to any perceptible extent.

In the ninth query we have a practical (and in our view a most erroneous) deduction from the circumstances which we have just been considering. Thus it is assumed as probable, that of two persons in a small chamber where charcoal is burning, the one on the floor may perish, while another three or four feet above that level may escape, owing to this supposed separation of gases. We do not believe that such a case is likely to occur; and if it did take place, the result would probably depend upon whether the individuals were equally near the source of combustion. M. Devergie admits, and indeed proves, that the poisonous gas becomes equally diffused all over the room: that it may be insufficient to destroy life while thus diffused, but that it becomes destructive by slowly falling to the floor, and there forming a concentrated stratum. We have already said enough to show that if this be the case with charcoal vapour, it differs materially in its chemical and physical properties from those known to be possessed by carbonic acid. In our view, most people are killed by carbonic acid during the actual diffusion of the gas, so that the subsequent separation on cooling, even if it took place, would have very little influence in producing a fatal result. Besides, we must protest against the correctness of the inference so generally drawn at coroner's inquests in these cases, that because a person is found lying asphyxiated on a floor, he must therefore have been affected by the

gas while in the recumbent posture. It is possible that he may have been sitting or standing, and have fallen asphyxiated. To analyze the stratum in which his head is found lying, and infer from the quantity of carbonic acid there discovered, that he has been attacked and has died in the recumbent posture, is absurd; since, unless the body be accidentally supported, every asphyxiated subject must be thus found.]

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*On the Signs of Suspension, before and after Death.* By M. Orfila.

At the sitting of the Royal Academy of Medicine, October 6, 1840, M. Orfila read a long memoir on suspension. He passed in review all the characters given by different authors to determine whether suspension had taken place during life or after death. The presence of zoospermata in the urethra is not, according to M. Orfila, any proof that the subject had been suspended during life, for semen may arrive there from other causes, sometimes even in a manner purely mechanical. In two cases of suicide by suspension reported by M. Ollivier, of Angers, there was no trace of injection or congestion either of the scrotum or penis; the latter was remarkably flaccid and the corpus cavernosum very pale. In these cases death had been very rapid. M. Orfila concludes with M. Ollivier that the turgescence of the penis is a mechanical effect of position, and thinks that the state of the vertebral column is a highly important consideration. He made experiments on twenty dead bodies suspended after death at various heights from the ground, and subjected them to different degrees of traction and rotation, but in none was the atlas luxated upon the second cervical vertebra. In one case extravasated blood was found in the vertebral canal; an important fact proving, contrary to the opinion of most writers, that this character is not a proof of suspension during life. The author concludes his memoir with the following propositions:

1. None of the signs given by authors as sufficient to distinguish suspension during life and after death are to be depended upon.

2. When with the signs of asphyxia there is neither ecchymosis, furrow around the neck, fracture, nor luxation, the suspension *may* have taken place during life, but more probably after death.

3. If there is ecchymosis in the skin and cellular tissue about the furrow of the cord, it is most probable that the suspension has been during life.

4. If some of the vertebral ligaments are torn, the hyoid bone or thyroid cartilage fractured, ecchymosis about the neck, fracture of the vertebræ and no other lesions upon the body, suspension may have followed death, or may have been the result of suicide.

5. If with the signs of asphyxia any of the cervical vertebræ are broken, and there are no marks of other violence, the case is probably one of homicide.

6. If there is luxation of the atlas upon the axis we may affirm that suspension has followed death, unless there be caries of one of the vertebræ.

7. The moral and circumstantial evidence is in general more capable of proving whether the suspension has been before or after death.

*Gazette Médicale de Paris. Octobre* 10, 1840.

## ANIMAL CHEMISTRY.

*On the Chemical Analysis of the Blood in its morbid conditions.*
By Dr. Louis Mandl.

The object of this series of papers is to point out the defects and fallacies of the present modes of analyzing the blood, and especially of estimating the *quantities* of its several principal constituents.

1. Three methods have been most commonly employed to separate the

fibrine, but all are fallacious. The first consists in squeezing the clot of blood in linen, so as to get rid of its serum, and then repeatedly washing the remainder. Now the *clot* of blood has by no means always the same characters; some clots are hard and dense, and can only with difficulty be broken up, and these may certainly have all their fibrine, which forms compact stringy masses, retained within the linen. But others are soft and friable, and when these are broken up and squeezed numberless particles of fibrine pass through the fine meshes of the linen, and what remains is but a small portion of what really existed in the blood. Independently therefore of the varying effects of more or less force in pressing the clot, and of the coarseness or fineness of the linen used, it is impossible that by this method, (which was generally employed by M. Denis,) any just idea should be formed of the respective quantities of fibrine in coagula whose physical qualities are different; for in all soft coagula, the fibrine breaks up into pieces small enough to pass through the linen with the serum and colouring matter.

The second method, by stirring the blood, is not less defective. Besides that, even under the best of circumstances, it is not so easy to stir the blood as to get all its fibrine in shreds, here again as in the preceding case the fibrine may be in such a state as to render its separation impossible. In blood mixed with pus for example, it forms in coagulating particles which are either so small that they can hardly be seen, or pieces which stick to the walls of the vessel, or swim in the rest of the blood, and cannot possibly be separated from it. And in many other circumstances a similar condition exists in a less degree, so that this method can be employed only in cases in which the fibrine is disposed to coagulate firmly and in large masses, and has not its tendency to become solid, interfered with by the presence of purulent, or saline, or other substances in excess in the blood.

The method of Berzelius, that of compressing slices of the coagulum in blotting paper, and when they have ceased to give out moisture washing them till they are colourless, is open to different but almost equal objections. In washing the remains of the coagulum one of two things will happen, either the blood-globules that are retained in it will be washed out entire, and then they will carry with them small pieces of the fibrine itself, or else their fluid contents and their colouring matter only will be removed, and their own membranes and those of their nuclei (which cannot be chemically distinguished from fibrine) will remain. In the former case the quantity of fibrine will appear less than it really was; in the latter it will seem more, perhaps much more; nor can any dexterity of manipulation avoid these errors. Besides from one kind of clot, (the firm one of inflammatory blood), it is almost impossible to wash away all the globules without breaking it up; and from others (the softer kinds formed in typhoid fever, &c.) the fibrine separates sometimes in particles almost as small as the globules themselves, and cannot be retained on a filter; numberless particles of it may be detected with the microscope in each portion of water that passes through in the process of washing, and many more when (as is commonly the case) the fibrine is washed, not on a filter, but in a vessel of water.

In whatever method it be obtained, the fibrine is subsequently macerated for a considerable time in water, in which, if the process be contained for twelve or more hours, as it commonly is, decomposition takes place, the water becoming turbid, and the fibrine gelatinous. And after this it has to be dried, an operation requiring great delicacy and judgment to ensure that at the end of it water shall not be still retained in the organic substance which so readily absorbs it.

And after all it is very doubtful whether under the best of circumstances the fibrine extracted from the blood is all that it really contained, for none of the analyses yet published take into account that which in all probability exists in the envelopes and contents of the globules, and which may well amount to more than there is in the *liquor sanguinis*. For several things are quite inexplicable on the supposition that as commonly stated there are only three parts of fibrine in 1000 of blood; as for example, the immense quantities effused in false mem-

branes, amounting in some cases to more than the whole mass of blood is said to contain, and many others.

If it be said that in spite of all these sources of fallacy the analyses of different chemists closely accord; reference need only be made to those analyses, and it will appear that the quantities of fibrine said to exist vary from 0·75 (Berzelius) to 4·96 (Müller) parts in 1000. Besides, these analyses were made on healthy specimens of blood; the difficulties are greatly increased when it is in a morbid state, when the determination of its quantity of fibrine is most important.

2. The composition of the globules is as yet rather uncertain, but the author has little doubt that their contents are in part fibrine, by the coagulation of which the nucleus is formed; and he thinks that their membranes are also fibrine, coloured with hematosine. Now, as already stated, this fibrine within the globules has not in any analysis hitherto published been taken notice of; nor is it right to estimate its quantity from that of the albumen (as it has been supposed to be) which chemists have obtained from the globules: for it is difficult in any case to collect all the globules in a sample of blood; a number of them always float in the serum, and the more the softer the clot is, so that in many cases the serum itself is deep red; and moreover, in the ordinary plans of treatment, the clot is never obtained completely free from serum, and therefore from a greater quantity of albuminous matter than properly belongs to its globules. In short there is not at present any method known by which the globules can be all removed from a portion of blood, and obtained separately from its serum and other principles; and there cannot therefore be any certainty in the calculations of their weight or of the quantity of fibrine or albumen which they contain.

3. The fallacies in the analysis of the serum depend chiefly on the quantity of globules and of portions of fibrine that may be suspended in it. These in the healthy state of the blood are unimportant, but in its diseased states they may give rise to considerable errors.

In the next division of his subject the author treats of the coagulation of the blood, and the formation of the buffy coat, of the chemical nature of which, he rightly says, nothing is at present known. [But under this head we find nothing worthy of being abstracted.]

In the application of his objections, M. Mandl takes two classes of cases, namely, those in which the blood is said to be poor in fibrine, including scurvy, putrid fevers, typhoid fevers, &c. and those in which it is described as rich in globules. For the first the quantity of fibrine will appear small in all cases in which a firm clot does not form, that is, not only when there is really little fibrine, but when that which does exist, after coagulating, does not contract and form a dense hard clot. And this may depend on the presence of pus or other morbid fluids in the blood, or, still more probably, on an excess of alkaline salts. The latter are known to exist in excess in scurvy, and perhaps may be found so in the other diseases in which the blood has been less carefully analysed. At any rate, it is certain that we have at present no means of ascertaining the real quantity of fibrine in a soft clot, and therefore no right to assume a deficiency of it in any cases till the softness of the clot is proved not to depend on some other cause than that deficiency. Similar difficulties attend the appreciation of the true quantity of globules; and hence the diversities of the statements of authors respecting their increase or decrease in different diseases.

The last part of these papers contains general reflections on the application of the chemical researches on the blood to pathology. [But in this also the remarks are trivial. Indeed, the author has evidently much more skill in finding fault than in improving defects. We have ourselves long thought that in the matter of the blood, physiologists have too implicitly received the statements of chemists; and we hope that the brief abstract we have given of M. Mandl's papers, though they do no more than point out the numerous and unavoidable fallacies of these analyses, will engender caution in drawing conclusions from such evidently insecure premises.]

*Archives Générales de Médecine. Decemb.* 1840, *and Jan. and Fevr.* 1841.

## STATISTICS.

*Account of the Petersburgh Foundling Hospital, and Establishment for bringing up Children.* By J. G. KOHL.

THIS establishment was founded by the Empress Catherine in 1770. Its extent was of course at first but limited, and even in 1790 there were only three hundred children in it. But since the commencement of the present century the number of its inmates has very greatly increased, and has varied at different times, from one thousand to five, ten, or even five and twenty thousand. In 1837 there were no less than 25,600 young people in this gigantic establishment, and the number of children admitted into it of late years has been from 1828 to 1829, between 3 and 4000; from 1830 to 1833, between 4 and 5000; and from 1835 to 1837, between 5 and 7000.

The admission of children is perfectly unrestricted; every one that is brought is taken in, and without further ado received into the establishment, and the government, so far from setting a limit to the admissions, has rather taken care to provide for the wants of the charity with extraordinary liberality. The original foundation by Catherine was, in proportion to the present means of the Institution, extremely small; but it has been increased by munificent presents from private individuals, and still larger gifts from Alexander, Paul, and Nicholas; and it is now one of the richest charities in Russia, and has many millions (of rubles) invested in houses. To these sources of income Alexander added the revenue derived from the manufacture of cards, and that from the *Lombard* (or general establishment for pawning, *Mont de Piété*), which, in consequence of the great fluctuations of private property in Petersburg, is an establishment of enormous extent. Thus it is that every year six or seven hundred millions of rubles are available for the Foundling-Hospital, and pass through the hands of its director. The maintenance of the whole establishment now costs 5,200,000 rubles a year; in 1837, two millions was spent in building, and 300,000 in the erection of a church for the inmates.

The principal establishments are at Petersburg and at Gatschina, but their benefits are extended over the whole neighbourhood of Petersburg. In the latter city are the chief buildings for the reception of the children of both sexes, and for the care of them during the first six weeks; after which time, when they can bear removal, they are sent to people in the villages and towns within a circuit of 130 wersts from the city, with whom they remain till they are six years old. At this age the girls are brought back to Petersburg for further education, and the boys are sent for the same purpose to the establishment at Gatschina. When their education is completed, they are all permitted to leave the Institution, and are left perfectly free to follow their own choice of an occupation, or are sent to the business for which, according to their capacities, they have been prepared.

Six or seven hundred nurses are kept at the Institution, and twelve physicians, mostly Germans, officiate in the hospitals attached to it, and have the charge of examining and frequently visiting the children that are sent into the provinces. A lying-in hospital is also attached to the Foundling, and is based on the most liberal principles, every one that wishes being admitted without restriction. Complete secrecy however is maintained over this part of the establishment, and it is open to none but those who are actually attached to it.

At the Foundling the door of the little receiving room is open day and night throughout the year, and an inspectress, with several female assistants, is constantly in attendance. A thick book is kept in which the children, amounting to fifteen or twenty daily, are entered. The only question asked on receiving each of them is, whether it has been baptized, and whether it has a name. If this be the case, the child is entered in its own name; but if not, it is entered with a certain number attached to it. The women come in the greatest numbers at the close of the evening, with their children wrapped in cloths, and their attendance is more numerous in fine than in bad weather, and in summer than

in winter, but most of all in the spring. We were at the Institution at one o'clock in the day, and at that time seven new inmates had been received, whose numbers we saw entered in fresh ink, 2310 to 2317. It not unfrequently happens that when the mother undoes the clothes, she finds her child dead; they are then not received, but are sent to be examined by the police. The living children however are at once received without reference to their origin; they are all baptized and admitted into the true church, and after six weeks are sent into the provinces. A fourth of them however die within these six weeks, and half of those that are sent away die within the next six years, so that only about a third of those that are received survive their sixth year, while that of the children brought up at their own homes fully half survive that time. A great proportion of this heavy mortality depends on the great distances through which they are carried to and from the establishment, for many of them come from remote parts of Russia, (indeed from all parts except those from which the superfluous children are sent to Moscow), and many are half dead before they arrive. In 1836, for example, there arrived on one day a child from Kischeneff, in Bessarabia, and another from Tobolsk, in Siberia, both of which places are about 250 miles from Petersburg. How many therefore must die even before they arrive at their destination! *Casper's Wochenschrift. Marz* 13, 1841.

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## II. THE AMERICAN AND COLONIAL JOURNALS.

*Remarks on Dyspepsia as connected with the Mind.* By A. Flint, M.D., Buffalo.

[The view taken of a large class of dyspeptic diseases by the author of this excellent paper, is so accordant with our own observation and experience, and has so long successfully guided our practice, that we gladly transcribe the greater part of it, and earnestly recommend the attentive study of it to the host of drug-doctors in this country, whose name is "Legion."]

A dyspeptic presents himself to a physician and states his sufferings. The physician inquires concerning his habits, mode of life, &c. He ascertains that he has indulged his appetite for food and drink without much discrimination, and at all periods has neglected to take exercise, &c. He exclaims at once, "it is not surprising that you have dyspepsia," and he recommends him to reform his habits. If the patient follows his advice perhaps he recovers his previous health without difficulty. Shall we then say that dyspepsia generally arises from dietetic errors? This, probably, is the common doctrine; but I am disposed to doubt its general applicability. How many are there who pursue such a course for a great length of time, perhaps during the whole period of life, without becoming dyspeptic, when, as far as we can judge, there is no reason to suppose their immunity is owing to a better constitution or stronger power of endurance! On the other hand, how many with the utmost care and prudence become and continue dyspeptics! It is not to be inferred that irregularities and intemperance are not common causes of this disease. In the poorer classes, it may, perhaps, generally be attributed to these, conjoined with destitution, bad food, sedentary occupations, close or pernicious atmosphere, exposure, &c.; in the rich to the abuse of luxuries, with love of ease. But there is a class who do not want the comforts of life, and who do not indulge in luxurious excesses, and observation shows, that in this class dyspepsia is not only common, but peculiarly obstinate and persistent. This is an important fact in relation to the disease. A poor person, if he be better fed and clothed, his habits and the circumstances about him improved, is restored. A rich man, if he curtails his pleasures, uses more exercise, engages in healthful occupation, may, in general, expect the speedy return of good health and spirits. But the class between these extremes, who are already clothed and fed, who have no excesses to curtail, find, whatever course they may pursue, that to overcome their difficulties, requires not a small degree of care and perseverance. Physical peculiarities of constitution may explain this in some cases, but, in general, it seems to me, that the explanation is to be derived from the connexion of the disease with

the mind. This class, it is to be observed, as a general remark, embraces that portion of mankind, who are disposed more or less to occupations or pursuits which involve, in a greater degree than in the other classes, the exercise of the intellect. Dyspepsia, it has been always observed, is more liable to seize upon those who are thus disposed, and two reasons have been assigned for this preference; viz. 1. The sedentary habits which these pursuits and occupations generally involve. 2. Reasoning from the well-known sympathy which exists between the brain and stomach, to excessive or disproportionate cerebral exercise. There cannot exist a doubt that the former play an important part in the production of, and predisposition to, the disease, but with regard to the latter, so far as I am able to judge from my own observations, it is rarely a cause of the disease, excepting in as far as it involves the former. Among literary men we do not find that they are so liable to the disease who are in the habit of intense, prolonged, or frequently repeated intellectual exertions, although accompanied with much excitement and perturbation, as those whose exertions are of a plodding character; and these generally seem to suffer in the way as some artisans, viz., from the deficiency of muscular exercise, the invigoration of the atmosphere, &c. On the other hand, deficiency of intellectual exertion as a cause of the disease, seems to have escaped observation. I have been led to think that this in certain mental constitutions peculiarly predisposes to dyspepsia. "Mind tends to action," or, to quote the expression of another physiologico-philosopher, exercise or action is a "want" of the intellect. This tendency or want will exist in proportion to the extent of the mind's capacity for exertion, and, like all the instinctive impulses and demands implanted in the human constitution, it must be fulfilled and gratified, or the economy will suffer. If we carefully examine the history of the cases which fall under our observation, we shall find that a large number of them, although in their details or particulars they may differ, are, nevertheless, to be associated, as it regards the causes which have produced and which perpetuate them under this common principle. Nor is it intended to apply these remarks exclusively to those who are pre-eminently intellectual persons. The mental energy may be expended on other than literary and scientific objects, in the performance of any occupation not wholly mechanical, in the fulfilment of the various responsibilities of life; and its obstruction as it regards the latter may be attended with the same results as in the former case. According to this view, the disease under consideration, is consequent to the unnatural condition in which many individuals are placed as it regards the exercise of the various faculties and powers of mind; or, in other words, to a want of correspondence between the mental constitution and extrinsic circumstances. By the term mind, and the expression mental constitution, I would embrace all that appertains to the moral as well as intellectual powers and faculties. My remarks have had more particular reference to the latter, but in many, if not the larger proportion of individuals, the wants of the moral nature, the affections and sentiments, predominate over those which are purely of the intellect, and there is reason to believe that similar results may follow their obstruction or perversions. Indeed, it is probable that instances of the latter are more common than of the former.

It may be said, on the supposition that this explanation of the origin of the disease be correct, why are not its peculiar aberrations the direct effect of causes operating on the mind, without the intervention of the digestive organs. This is not probable in the first place, from the constancy with which they are associated with derangement more or less of these organs, together with their distinctive traits; and in the second place, it is not presumed that all cases of dyspepsia originate in this manner. The successive agents, then, in the development of the disease will be threefold:—

1. The operation of the mental causes.
2. The affection of the digestive apparatus.
3. The reaction of the latter upon the mind, producing those mental aberrations which characterize the disease.

From this doctrine is derived a sufficient explanation of a fact which has been mentioned, viz., that dietetic errors are persisted in often with impunity by those whose strength of constitution and powers of endurance are apparently no greater than of those who suffer.

*Treatment.*—It is well known, that in numerous cases, all the various modes of medical treatment recommended, accomplish but little toward restoring the patient to a healthful condition. The truth is, in the majority of cases, the patience of the physicians is exhausted, by the inefficacy of the remedies prescribed; or the patient, after application to different members of the profession, and experimenting with the thousand and one empirical nostrums, relinquishes all expectation of benefit from the materia medica. But the inquiry arises, if it be true that the disease, in a large number of cases, is to be attributed to causes existing in the mind of its relations, would not the philanthropic physician be able to afford, in many instances, effectual service by suggesting measures which have reference to these, in addition to those appertaining to the materia medica? It is too common for medical advisers to pay but little regard to the mental aberrations peculiar to this disease, thinking that, in the language of Skakspeare, "Therein the patient must minister unto himself." To examine them with attention, and, if possible, to afford relief, would, under any circumstances, be embraced within that philanthropy which should be inseparable from the practice of our profession; but, since they depend upon physical causes, they are to be regarded as morbid symptoms, and fall, legitimately, within the province of the healing art. If physicians were more generally and fully impressed with this view of the subject, perhaps the disease under consideration would become less an opprobrium than it, confessedly, now is.

It may then be stated, as the first important rule in the treatment of the disease, to ascertain fully the kind and degree of mental aberration which exists. To listen with patience and sympathy to all the changes of feeling which the patient is ready to describe, if he receives encouragement to do so, is, in itself, a source of much consolation, and goes far to secure to the medical adviser the possession of the entire confidence of the sufferer. In connexion with this, the mental characteristics of the individual, his habits, education, &c. are to be considered, both as enabling us better to appreciate the nature of the changes which have occurred, to decide upon the remote causes of the disease, and to determine upon the measures to be recommended with a view to restoration.

In the second place, it is important to satisfy the patient that the altered condition of his mind and feelings is symptomatic of a morbid condition of body. This is often so little suspected, that his unhappy state is not described until inquiry is made relative to this point. But, as soon as the patient finds it is suspected as associated with the disease, he gladly becomes communicative. It is truly pleasing to witness the surprise and animation which lights up the sombre countenance of the unfortunate dyspeptic, when he finds that the state of his feelings is anticipated by the physician. He seems to hail it as a favorable omen. If the idea has never been suggested that his unhappy condition is the effect of a disordered body, it furnishes the first occasion for hope; and whether restoration is effected or not, he is enabled to resist and sustain his trials with more fortitude and perseverance.

The next object will be to endeavour to remove the causes which have originated or which maintain the disease. But inasmuch as these are very multifarious, and their different varieties have not been considered, the remarks upon therapeutical principles will of course be very general. Each case, in fact, should form a separate study; but in general terms, the patient should be urged to provide that particular kind of stimulus for the intellectual and moral powers, which he seems to require.

In some instances, the difficulty seems to consist chiefly in the monotony incident to routine duties. Then, the indication is to vary their character, or advise a temporary interruption. In such cases, travelling is highly useful; but, frequently, to be permanently efficacious, it should not be confined within a

narrow sphere of time or space. Especially foreign travel is useful by the increased excitement and interest derived from the comparison of scenery, and the habits, manners, and institutions of other countries. But this unfortunately is a measure which only in a small number of cases can be adopted. Those means alone can be embraced which are accessible at home. These, however, are not few or powerless. Sometimes it may be proper to advise an entire change of occupation and locality, in order to supplant completely old by new associations. This method, which has been found of such utility in mental derangement, would, probably, be not less so in cases of partial alienation, as these cases must be regarded.

When this is not advisable, or practicable, other measures must be adopted to rouse the faculties of the intellect and the moral sentiments. One of these is the commencement of certain branches of study, or some plan of intellectual effort. Those departments which are pursued by means of observations and experiments rather than abstract contemplations are to be preferred. This will of course apply only to those who have leisure, inclination and capacity for such occupations, and to that class who require more especially excitement of the intellectual faculties.

In other cases, the social and moral sentiments are to be operated upon by the formation of new connexions, assuming new responsibilities, and by directing the mind to objects which are calculated to engage the feelings of benevolence and philanthropy; such are politics, the cause of popular education, and the numerous particular plans of every scale of magnitude, tending to the amelioration and improvement of the human race.

The selection of any of these measures, as has been already remarked, will depend upon the combination of circumstances which distinguish the cases individually, and is to be left to the discrimination of the medical adviser. The hearty co-operation of the patient is of course requisite to the prosecution of any plan, and with a view to this the whole subject should be fully discussed and the state of the case frankly stated. One good result will at least accrue from such a course; it will tend to preserve feelings of respect for the character of the medical profession with a class of patients whose experience of it too often leads them to entertain opposite sentiments.

*American Journal of the Medical Sciences. January*, 1841.

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# III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING MAY 30, 1841.)

## ANATOMY AND PHYSIOLOGY.

*On the Relation between Muscular Contractility and the Nervous System.* By JOHN REID, M.D.

THIS short paper contains some very interesting experiments upon this *quæstio vexata* of physiology, devised and executed for the purpose of removing some of the grounds of the objections recently made by Müller, Dr. M. Hall, and others, against the Hallerian doctrine of the independent contractility of muscular fibre. Dr. Reid had long ago ascertained, that the contractility of muscles whose nerves had been divided, would return in frogs after having been exhausted; and this not once only, but several times. He has now established the same fact by experiments on rabbits; and has thus removed the objection made to his former results, on the score of their being afforded by cold-blooded animals. Hence it would appear that the contractility cannot be derived from the nervous system, but must be a *vis insita* of the muscle. It has been urged

by Müller, in support of the neurological doctrine, that muscles whose nerves have been for some time divided lose their contractility; and it has been replied that the disordered state of their nutrition, consequent upon their want of use (which is shown by microscopical examination to change the structure of muscular fibre), was sufficient to account for facts of this kind. Dr. Reid has very ingeniously proved this explanation to be the true one. In a rabbit, whose sciatic nerve had been divided on one side, the contractility of the muscles of the leg was nearly lost at the end of seven weeks; these muscles were found to weigh only 170 grains, whilst those of the sound limb weighed 327 grains, or nearly double; and the tibia and fibula had diminished from 89 grains to 81. A still more decisive experiment was next performed. The spinal nerves were cut across in the lower part of the spinal canal of four frogs, and both posterior extremities were thus insulated from the nervous centres. The muscles of one of the paralysed limbs were daily exercised by a weak galvanic battery, while those of the other limb were allowed to remain quiescent. This was continued for two months; and at the end of that time, the muscles of the exercised limb retained their original size and firmness, and contracted vigorously; while those of the quiescent limb had shrunk to one-half their former bulk, not yet losing, however, their irritability.

Dr. M. Hall has expressed the opinion that the spinal cord is the seat of irritability; grounding this view upon the fact, that, if one limb be paralysed to the influence of volition by dividing the spinal cord, but retain its connexion with as much of the latter as suffices for reflex action, whilst the other is cut off from all communication with the nervous centres,—the muscles of the former will retain their irritability, when those of the latter have lost theirs. Dr. Reid justly remarks upon this, that the muscles of the former limb would be frequently in action by the excitement of contact, &c. during the ordinary movements of the animal; and that thus their nutrition would be maintained, and their irritability preserved, as when differently excited to action in the last experiment; and, further, that the stimulus of a galvanic current in water, the test employed by Dr. Hall, would act upon the former, not only by directly exciting the muscles, but by reflected operation through the spinal nerve, so that it would occasion more powerful contractions in that limb than in the other, on the muscles of which it had only one mode of action. Moreover, the doctrines of Dr. Hall and Müller are totally irreconcileable with Dr. Reid's experiments; so that the original statements of Haller must still be preferred to them.

*Edinburgh Monthly Journal of Medical Science. May*, 1841.

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*On the Order of Succession in which the Vital Actions are arrested in Asphyxia.* By John Reid, M.D., F.R.C.P.E., &c.

Two questions, which had not been satisfactorily replied to by preceding experimenters, are proposed for investigation in this valuable paper; and they are treated with the skill constantly manifested by its author in the elucidation of difficult subjects of physiological enquiry.

The experiments of Drs. Williams and Kay demonstrated, that in Asphyxia the circulation is first brought to a stand by some impediment to its free passage through the lungs; but they did not afford ground for determining whether this impediment results from the cessation of the usual respiratory movements of the chest, which are known to have an important influence on the movement of the blood, or whether it is to be attributed to the arrestment of the usual chemical changes between the blood and atmospheric air. The latter view, upheld by Dr. Alison on general grounds, was also supported by the fact ascertained by him, that, if animals be asphyxiated by breathing an atmosphere of nitrogen, an accumulation of blood on the right side of the heart commences before the respiratory movements have been seriously weakened. Much more conclusive

evidence has been afforded, however, by Dr. Reid's experiments. He fixed a tube with a stop-cock in the trachea, and applied a hemadynamometer to the femoral artery. By shutting the stop-cock, the respiratory process was suspended; and upon inspecting the hemadynamometer, the very unexpected result became apparent, that, when the asphyxia was proceeding to the stage of insensibility, and the attempts at respiration were become few and laboured, and the blood in an exposed artery was quite venous in its character, the pressure upon the walls of the artery was much *greater* than when the animal was breathing freely. Upon applying a similar test to a vein, however, it was found that the pressure was proportionally diminished; whence it became apparent that there was an unusual obstruction to the passage of the venous blood through the systemic capillaries, a fact of much importance in the general theory of the circulation, and also explaining how it is that some amount of blood is generally retained on the left side of the heart in asphyxia. After this period, however, the mercury began to fall steadily, and at last rapidly, in consequence of the diminished force of the heart; and its downward progress was not at all arrested, by causing the animal to breathe nitrogen, a bladder of which gas was screwed to the stop-cock. But if atmospheric air was then admitted, the mercury rose very speedily; showing that it was by the chemical influence of the oxygen that the force of the arterial circulation was restored, and not by the respiratory movements, which had been previously renewed in the nitrogen, without any accelerating influence. These experiments, then, fully confirm the view that the stagnation of blood in the capillaries of the lungs is not due to a mechanical cause, but to the interruption of the chemical processes essential to its movement through the capillaries, by the exclusion of the necessary element. And they add the important fact, confirmatory of the same general doctrine, —that venous blood will not readily pass through the systemic capillaries, the changes to which it should there minister being prevented by the depravation of its own character.

The second question investigated by Dr. Reid relates to the cause of the arrestment of the sensorial functions in Asphyxia. It is well known that Bichat considered this to be due to the circulation of venous blood through the brain; but that Dr. Kay, on the other hand, attempted to prove that it resulted from an absolute deficiency of the circulating fluid. Dr. Reid shows the unsatisfactory nature of the experiments of each of these enquirers; and proves satisfactorily, to our minds at least, that the sensorial functions are greatly impaired, before the supply of blood has sustained any considerable diminution,—in fact, whilst its pressure on the coats of the arteries is greater than ordinary, from the cause just explained; and as, at this time, its character has been almost reversed, we can have little hesitation in assenting to Bichat's opinion of the cause of the phenomenon. "We do not however," says Dr. Reid, "maintain that venous blood exerts any noxious influence upon the functions of the nervous texture; but believe that its effects are solely to be attributed to the want of the proper excitation of this organ; for, when the circulation of arterial blood is renewed, its functions rapidly manifest themselves, provided that this be done within a given time."

"We believe, then, that, in Asphyxia, the order of succession in which the vital processes are arrested is as follows:—The [partly] venous blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the individual becomes unconscious of all external impressions. The functions of the medulla oblongata are enfeebled about the same period that the sensorial functions are arrested, but are not fairly suspended for some time after. Immediately after the sensorial functions are suspended, and the blood has become still more venous, it is transmitted with difficulty through the capillaries of the lungs, and consequently begins to collect in the right side of the heart. A smaller quantity of

blood must now necessarily reach the left side of the heart; and thus diminution of the quantity of blood sent along the arteries, conjoined with its venous character, and the ultimate arrestment of the circulation, being circumstances incompatible with the manifestation of vitality in the other tissues of the body, in general death is sooner or later induced."

*Edinburgh Medical and Surgical Journal. April*, 1841.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

*Jaundice from Non-Elimination, together with remarks on the Pathological Condition, and Chemical Nature of the Bile.* By W. H. Lowe, M.D.

Dr. Lowe rejects the doctrine as purely hypothetical, that when jaundice appears suddenly and without premonitory symptoms, it is due to spasm, or to the opposite condition, paralysis of the gall-ducts. Jaundice from non-elimination may take place, either with the liver in a state of disease, or when there is no appreciable disease in that organ. Of the latter species, Dr. Lowe furnishes a single case of his own, and refers to others. According to Le Canu, three views are entertained by chemists of the condition of the blood of icteric patients. Some believe that it always contains bile: others, that it contains none, but only a particular colouring matter: others adopt a middle view, and think that the blood of such patients, without containing all the constituents of bile, holds its colouring principles.

Dr. Lowe, without attempting to offer an exact analysis of the bile, gives the result of some experiments of his own, "which have been conducted on a somewhat extensive scale." He endeavours to show that the resin of Gmelin, the picromel of Thenard, and the choleic and choloid acids of Demarcay, are "one and the same thing." We must refer our readers to the author's paper, for the experiments on which he grounds this view.

*Edinburgh Medical and Surgical Journal. April*, 1841.

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*On the Diagnosis of Abdominal Inflammations.* By William Griffin, M.D, M.R.C.S.E., Physician to the County of Limerick Infirmary, &c.

[This paper contains some new and striking illustrations of the important practical fact to which Dr. Griffin had previously called attention, viz. that inflammatory, or other affections of the spinal cord, or of its nerves, at their origin, more frequently simulate abdominal and thoracic inflammations, and are more frequently mistaken and maltreated, than is at at all imagined.

We have not room to give the cases, but extract the greater part of the general observations.]

There are three effects very common to inflammation, or disorder in the spinal cord, or at the trunks of its nerves.: 1st, Superficial tenderness, more or less exquisite, and either limited to the integument immediately over or about the affected portion of the cord, or extending thence to the front of the abdomen or thorax, in the direction of the spinal nerves; or occupying the whole cutaneous surface of those parts of the body, which are below the portion of the cord affected. 2dly, Pain either close to the affected portion of the cord, or at the extremities of the nerves, which have their origin there; or in the ganglionic nerves supplying the viscera, which have connexion with that portion of the cord. 3dly, Loss of power evinced in partial or complete palsy of the parts or organs, to which the affected nerves are distributed. These effects often occur simultaneously, but any one of them may also occur independently of the existence of the others, offering very strong evidence that the sensibility of the surface or skin and that which exists in internal organs is dependent upon nerves, which, though sentient, are as distinct from one another as they are from those on which the power of motion depends. Keeping these ordinary effects of disorder of the spinal cord in view, it must appear obvious, when we detect soreness or tenderness on pressure in the region of the liver or spleen, or of the lower abdominal viscera, how important and essential it is for us to

ascertain whether the soreness be superficial or deep-seated, which we can almost always do by careful examination. Again, when pain is complained of in the region of the liver, or middle or lower parts of the abdomen, how necessary is it to ascertain, whether, as in the case of soreness, it be superficial; and, if deep-seated, whether it be merely an affection of the nerves of the part, and probably connected with some affection of the adjoining portion of the spinal cord, or whether the internal organ itself be in a state of acute or chronic inflammation. Finally, if there be oppression in breathing, whether it arises from deficient action in the respiratory nerves, and, as a consequence, imperfect action of the respiratory muscles, or imperfect performance of the process of oxygenation of the blood in the lungs; or from actual inflammation, or organic disease of the mucous membrane, or parenchyma of these organs. Or if there be obstinate constipation of bowels, whether it depends on spasm, or enteric inflammation; or whether purely or deficient power, or partial palsy of the muscular fibres of the intestines.

In determining the diagnosis of abdominal inflammation, where both pain and tenderness on pressure exist, we should always endeavour to ascertain:

1st. Whether there be any pain or tenderness on pressure in the corresponding portion of the spinal column; because, if there be, although it may not absolutely decide whether inflammation be present or not, it is quite sufficient to account for both the pain and tenderness, without assuming the existence of any inflammation.

2d. Whether there be no spinal tenderness or pain, the soreness of the abdomen be superficial or deep-seated, which may be ascertained with tolerable certainty in all cases by an examination directed to that end. And whether, if both superficial and deep-seated, as it usually is, in peritoneal inflammation, gentle, steady pressure with the flat of the hand can be easier borne, than with the points of the fingers. In pain and soreness from affection of the spinal nerves it commonly can be so borne, while in peritonitis every kind of pressure, and even the weight of the bed-clothes, is very distressing.

3d. Whether the boundaries of the pain or soreness extend beyond what the suspected inflammation could produce. Thus, if inflammation of the liver be suspected, and we find the soreness extending to the spine of the ilium, or groin, or to the opposite side of the abdomen to which the liver does not extend, it is obvious, the soreness cannot be attributable to mere disease of that organ. Again, if the whole abdomen be tender to the touch in a case otherwise closely resembling peritonitis, and we find the tenderness is not confined to the abdomen, but extends over the hips and lower extremities, it is obvious, we can attach no importance to the abdominal soreness as a sign of inflammation.

Finally, it should be recollected that constipation may depend on mere loss of power in the intestinal nerves, as well as on spasm, obstruction, or inflammation, as the treatment in each case must necessarily be modified or directed by the supposed cause of this symptom. *Dublin Journal. May*, 1841.

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*Observations on Dropsy of the Pericardium.* By John Mackenzie, Esq., Surgeon.

The diagnosis of hydrops pericardii is still involved in obscurity and uncertainty. The difficulty connected with this subject arises from the rare occurrence of the disease, unassociated with effusion into the pleural cavity, and to the still rarer opportunities of verifying such cases as do occur, by post-mortem examination. Having for several years been attached to a large military hospital in Russia, where chronic inflammation of the pericardium and consequent effusion were frequently met with, and where every facility of examining the bodies of those who died was afforded, I observed that, in those cases where there was co-existing effusion into the pleural cavity, the patients, from the commencement of the disease, could not bear the horizontal posture; and that, where the effusion was confined to the pericardium the patients preferred to be with the head remarkably low, till general effusion took place, and the breathing became laborious: of these, some preferred to sleep on their face, inclining to the left

side, in such a manner as to make the region of the heart the most depending part, whilst others lay on their back. After death, I found in some of these cases that the pericardium occupied nearly the whole of the chest, and contained from six to ten pounds of fluid. In two cases there was not a vestige of the left lung to be found, except a thin layer of cellular substance.

The reason why the horizontal posture was preferred in these cases, appears to be, that the weight of the pericardium was taken off the diaphragm and rested on the spine or the ribs. It is to be observed, that in hydrothorax, when the patient lies with his head low, the water in the chest flows back and presses on the root of the lungs, preventing a free ingress of air, and thereby causing dyspnœa and sense of suffocation; and that in hydrops pericardii this cannot take place, in consequence of the water being inclosed in a firm bag which is bound down in its place by its attachment to the diaphragm. A very singular case of this disease came under my observation, where the patient could lie in all postures equally well, and experienced no inconvenience whatever; yet after death, which was caused by a sudden attack of inflammation of the right lung, the pericardium was found to occupy the whole of the left side of the chest, from the clavicle to the diaphragm, and contained ten pounds of thick brown fluid. *Lancet. April* 17, 1841.

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*New method of employing Iodine in Phthisis.* By A. Leigh, m.b., Jersey.

I direct the patient to apply a sufficient quantity of iodine ointment on the ribs, under both axillæ, and to cover the head with the bed-clothes, to breathe the iodine volatilized by the heat of the axillæ; the ointment produces counter-irritation on the skin where it is applied, and is to be repeated according to circumstances. This method has appeared to me to arrest the progress of the disease. *London Medical Gazette. May* 28, 1841.

## SURGERY.

*On the Employment of Threads of Caoutchouc for Satures.* By Thomas Nunneley, Esq., Surgeon to the Leeds General Eye and Ear Infirmary.

More than twelve months since I was induced to try several substances, in the hope of discovering one which should be free from the objections incident to the ordinary ligatures; this, I think, I have succeeded in, or, at least, I have found one which certainly possesses many advantages over the filamentous material now commonly employed; it is that substance which is now applied to so many purposes in the arts—caoutchouc, drawn out into fine strings; these threads I have hitherto obtained by removing the fibrous envelope of cotton or silk, with which the Indian-rubber itself is surrounded in the elastic web, at present so commonly employed for bandages, not being able to procure in Leeds the elastic strings alone, but in London, or where the fabric is manufactured, there could be no difficulty in obtaining them.

The advantages which it possesses as a ligature are, the much longer time it remains without producing irritation of any kind; being elastic, it holds the divided parts in contact with much less stretching, in a more natural manner, and, at the same time, it keeps up an equal degree of tension; for if the parts swell, the ligature gives way in proportion to the pressure; on the contrary, do they contract, so also does the ligature, and an equable approximation is maintained. From being smooth and unirritating, it excites so little disturbance that a greater number of ligatures may be introduced, and thus the parts may be more accurately adjusted to each other; moreover, by not inducing ulceration, the scars left after its withdrawal are less than those produced by silk, are advantages not to be lost sight of in operations about the face and neck.

I need scarcely add, that the very qualities which render the caoutchouc thread preferable for sutures for wounds, totally unfit it as ligatures for vessels, where a sharp unelastic string is necessary for first dividing the inner coats, and subsequently retaining them in contact. *Lancet. March* 13, 1841.

*Case of Neuralgia Facialis, cured by Operation.* By T. T. LAMBERT, Surgeon, Hull.

E. L., a female, aged sixty-three, had for several years been affected with a most painful condition of the nerves of one side of the face, coming on in paroxysms of the utmost severity, mostly towards night. At various times she had tried most of the usual remedies, without experiencing anything but temporary alleviation. Under these circumstances, as the branches of the facial nerve seemed to be principally affected, the pain shooting across the cheek down towards the lower jaw, and occasionally upwards towards the temple, I proposed the division of the facial nerve in the parotid gland, after its exit from the foramen. To this the poor woman gladly consented, and I proceeded to perform the operation by making an incision betwixt the mastoid process and the lobe of the ear. My next incision brought me in contact with a nerve, which I supposed, from its situation, to be the auriculus magnus nerve, one of the ascending branches of the cervical plexus. On raising this nerve with my forceps, a state of tension seemed to be produced upon all the nerves of the face which were affected with the pain, and the patient exclaimed that was the place from whence all her pain proceeded. Under these circumstances, as the operation for the division of this nerve is trivial compared with the division of the facial, and as it was just at hand, I determined to try the effects of its division; and the result was most satisfactory: the woman passed a delightful night, and enjoyed some good sleep. The next day she had a little pain, and from that time to the present she has been perfectly well, except having, on one occasion, a very trifling degree of pain. Four months have now elapsed since the performance of this operation, and during this interval the patient has been exposed to the intense inclemency of the recent winter. I am not one of those who would pay too much attention to a solitary case; yet, as a single fact, this seems to me to be important, inasmuch as the means employed have effectually cured, for four months at least, and I hope permanently, a very severe case of neuralgic pain.

*London Medical Gazette. March* 12, 1841.

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*On the Section of Muscles in Spinal Curvature.* By ROBERT HUNTER, M.D. Professor of Anatomy, Andersonian University, Glasgow.

I BELIEVE I was the first in this country to perform the subcutaneous section of the dorsal muscles for the cure of lateral curvature of the spine, and as I have now had some experience upon this subject I shall lay the result of my observations briefly before your readers. In no instance has the operation of itself produced a cure; but in all the cases on which I have operated, with one exception, it manifestly placed the patient in a more favorable state for the performance of a cure. The operation itself appears to me to effect no more than to take off, either in part or whole, the power of muscles that are interested in maintaining the curvature, and thus placing the spine in a condition to be more easily influenced by mechanical and physiological causes.

The cases which have been treated by me have all been of long standing, none less than seven years, and some ten, sixteen, and twenty years, and all with considerable torsion and gibbosity, as well as lateral curvature. In fact, they were cases which were either considered absolutely hopeless, or on which medical skill had been exerted for years without the slightest benefit. In some instances the section of the muscles was instantaneously followed by an obvious improvement in the state and appearance of the back; in other instances I could discover no change whatever.

I perform the subcutaneous section of the dorsal muscles at four different places of the back. 1st, I weaken the tension of the deepest seated layer of muscles—that formed by the multifidus spinæ—by dividing the thickest part of that muscle, as it lies comparatively superficially upon the dorsum of the

sacrum, opposite the posterior superior spinous process of the ilium. 2d and 3d, I remove the tension of the middle layer of spinal muscles, that formed by the longissimus dorsi and sacro-lumbalis, by cutting these muscles across, sometimes in the lumbar region and sometimes in the costal region, according to the circumstances of the case; but more frequently in the lumbar region, near the origin of these muscles. 4th, To destroy the tension of the flat and more superficial muscles, I divide these muscles by a longitudinal incision, close to the spinous processes of the vertebræ, at the place where the tension of these muscles appears to be the greatest. In one instance I cut through, with considerable effect, the latissimus dorsi, at the side of the chest, and consequently at some distance from the spine. The muscle crossed the contracted and concave side of the trunk, and appeared to be accessory in huddling in the ribs of that side. When the patient attempted to elongate that side, a cord, as thick as the little finger, was seen stretching from the crest of the ilium to the scapula; as soon as this rigid cord of muscle was cut through, the ribs became less huddled together, and the side could be elongated to a much greater degree, and the spine materially affected.

The cutting of the dorsal muscles is only the first, though an important step in the treatment of spinal deformities. The means that are afterwards employed in conducting such cases to a successful issue are both mechanical and physiological. The first consists in the application of pressure, made in various ways and by various means to assist in the gradual return of the parts to their natural places, and the second, without which the first would be useless and unavailing, consists in infusing power into the muscles which have become weak or dormant from disease, by simply calling these muscles frequently and in various combinations into action. *London Medical Gazette. March* 26, 1841.

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*On the use of Splints in Chorea.* By G. Southam, Surgeon, Salford.

I was led to try the experiment of confining the affected limb in splints, having frequently observed, in those instances where the convulsions had continued in spite of all remedial means, that if the patient's attention be directed to the deranged muscles, as for instance when in the hand and arm, causing him to grasp the sides of a chair, the motions comparatively cease; and this, when frequently repeated, proved a useful auxiliary in removing the disease. Upwards of two years ago a case came under my notice where the convulsions had existed in the hand and arm for six months, though a variety of means had been adopted for their removal: believing them to depend on habit, from the control the patient had over the motions, and his apparently excellent health, I had recourse to the splints. For the first few days the twitchings, from their violence, caused some difficulty in keeping the splints firm, but they soon diminished in force, and in three weeks had entirely disappeared. I have tried the same treatment in four other cases. In three, after attending to the bowels, I applied the splints, and continued them until a cure was established, which occupied in none more than a month. The convulsions were confined to one of the upper extremities, with slight dragging of the leg of the same side; but as the symptoms left the arm, the power over the leg returned. The other case occurred in a youth fifteen years of age. The convulsions were more severe, and rendered it extremely difficult to keep the splints firm with an ordinary bandage: to obviate this inconvenience, I used the apparatus recommended by Velpeau for fractures, with the addition of small splints to keep the limb extended and at rest during the consolidation of the apparatus. This was removed in a week to allow of exercise, and the severity of the convulsions being subdued, a common bandage and splints were again used. Though the convulsions affected both arm and leg, and splints were only applied to the former, the boy had recovered the use of both extremities in six weeks.

*London Medical Gazette. May* 7, 1841.

# PART FOURTH.

# Medical Intelligence.

## TABLE OF MORTALITY FOR LONDON,

FOR THE FOURTH QUARTER OF 1841:

Showing the number of Deaths from all causes registered in twelve weeks, from the 2d January, 1841, to the 27th March, 1841.

| Causes of Death. | Total Deaths. |
|---|---|
| CLASS I. | |
| Smallpox | 588 |
| Measles | 150 |
| Scarlatina | 155 |
| Hooping-cough | 601 |
| Croup | 84 |
| Thrush | 42 |
| Diarrhœa | 61 |
| Dysentery | 16 |
| Cholera | 1 |
| Influenza | 121 |
| Typhus | 313 |
| Erysipelas | 83 |
| Syphilis | 4 |
| Hydrophobia | 0 |
| Total Epidemic, &c. | 2219 |
| CLASS II. | |
| Cephalitis | 156 |
| Hydrocephalus | 403 |
| Apoplexy | 217 |
| Paralysis | 233 |
| Convulsions | 709 |
| Epilepsy | 49 |
| Insanity | 14 |
| Delirium tremens | 12 |
| Dis. of brain, &c. | 138 |
| Total Dis. of Brain, &c. | 1931 |
| CLASS III. | |
| Quinsy | 19 |
| Bronchitis | 289 |
| Pleurisy | 25 |
| Pneumonia | 1214 |
| Hydrothorax | 74 |
| Asthma | 706 |
| Consumption | 1659 |
| Dis. of lungs, &c. | 271 |
| Total Dis. of Lungs, &c. | 4257 |
| CLASS IV. | |
| Pericarditis | 15 |
| Aneurism | 15 |
| Dis. of the heart, &c. | 249 |
| Total Dis. of Heart, &c. | 279 |
| CLASS V. | |
| Teething | 216 |
| Gastritis, enteritis | 199 |
| Peritonitis | 16 |
| Tabes mesenterica | 56 |
| Ascites | 4 |
| Ulceration | 19 |
| Hernia | 32 |
| Colic or ileus | 42 |
| Dis. of stomach, &c. | 72 |
| Hepatitis | 20 |
| Jaundice | 26 |
| Dis. of the liver, &c. | 93 |
| Total Dis. of Stom. &c. | 795 |
| CLASS VI. | |
| Nephritis | 9 |
| Diabetes | 10 |
| Stone | 3 |
| Stricture | 3 |
| Dis. of the kidneys, &c. | 38 |
| Tot. Dis. of Kidneys, &c. | 63 |
| CLASS VII. | |
| Childbed | 127 |
| Ovarian dropsy | 5 |
| Diseases of uterus, &c. | 41 |
| Tot. Dis. of Uterus, &c. | 173 |
| CLASS VIII. | |
| Rheumatism | 37 |
| Diseases of joints, &c. | 44 |
| Total Dis. of Joints, &c. | 81 |
| CLASS IX. | |
| Ulcer | 7 |
| Fistula | 5 |
| Diseases of skin, &c. | 2 |
| Total Dis. of Skin, &c. | 14 |
| CLASS X. | |
| Inflammation | 66 |
| Hemorrhage | 46 |
| Dropsy | 480 |
| Abscess | 42 |
| Mortification | 88 |
| Scrofula | 23 |
| Carcinoma | 78 |
| Tumour | 19 |
| Gout | 23 |
| Atrophy | 81 |
| Debility | 290 |
| Malformations | 13 |
| Sudden deaths | 227 |
| T. Dis. of Uncert. Seat. | 1476 |
| CLASS XI. | |
| Old age or nat. decay | 1182 |
| CLASS XII. | |
| Intemperance | 4 |
| Privation | 17 |
| Violent deaths | 284 |
| Total by Violence, &c. | 305 |
| CLASS XIII. | |
| Causes not specified | 43 |
| T. Deaths from all causes Males 6373, Females 6445 | 12818 |

## A Sketch of the Character of James Hope, M.D., F.R.S., late one of the Physicians to St. George's Hospital, with brief preliminary notices of his Life.

James Hope, the ninth child of Thomas Hope, Esq., of Prestbury Hall, Cheshire, was born at Stockport on the 23d of February, 1801. Very early indications of quickness and intelligence, of general activity, and of a mild, gentle disposition, gave a pleasing promise of future excellence.

Under an inefficient instructor, he was left, until nearly eleven years of age, to pursue in a great degree his own inclinations in reading. His avidity for knowledge, however, led him to turn over his father's books with intense interest, and to select for his perusal not only the productions of fancy and amusement, but also works of solid instruction and even of high genius. When about eight years old, Milton's Paradise Lost engaged his attention, as also Parkes' Chemical Catechism, on which he happened to stumble. He became so engrossed with chemistry as at once to commence a zealous experimentalist, and was not a little mortified, after many trials, to find himself unable to make gunpowder for the supply of his sporting brothers.

From the age of eleven to seventeen and a half, he made steady advances in his classical studies under able masters; and having determined, if possible, to be a first-class man at college, he did not relax his vigorous application until he had mastered the highest Greek authors. During this period he was also storing his mind with general literature, and becoming well acquainted with works of taste.

Having a great desire to practise at the English bar, when the period for his leaving school arrived he was much disappointed to find that his father had neglected to secure him a place at Oxford, and was bent on his entering on mercantile pursuits. This Dr. Hope positively refused to do, and after a year's delay a compromise was made. His father offered him the medical profession, and though he had a strong repugnance to it he consented on condition that he should be permitted to practise in London. Whilst waiting for rooms, he resided at Oxford for a year and a half. His father, tired of this delay, insisted upon his commencing his medical education, and being thwarted in his cherished wish of completing his studies at Oxford, he proceeded to Edinburgh. At that celebrated school of medicine he pursued a laborious and highly successful course of study during five years; graduating with much eclat, and during the latter portion of the time holding, with signal practical advantage to himself and others, the offices of house-physician and house-surgeon at the Royal Infirmary.

From Edinburgh he went to London, with a view more especially to perfect his acquaintance with practical surgery. After diligently spending six months at St. Bartholomew's Hospital, he passed the College of Surgeons, and then commenced the tour of Europe. Attracted by some most favorable opportunities of cultivating auscultation and pathological science in Paris, he tarried in that capital an entire year. Thence he visited the principal cities and other interesting localities of Switzerland and Italy, and returned to England after an absence of two years. In December, 1828, Dr. Hope fixed himself as a physician in Lower Seymour-street, Portman-square.

In 1831 he married Anne, daughter of J. W. Fulton, Esq., of Upper Harley-street, formerly of Calcutta. One child (a son) resulting from that happy union survives his lamented father. The same eventful year was marked by his election as senior physician to the Marylebone Infirmary, which office he held three years, only relinquishing it on his appointment as assistant physician at St. George's Hospital. That laborious field of exertion proved overwhelming. In the course of five years he saw 20,000, and took notes of 15,000 out-patients which he had there attended.

In 1839, on the resignation of Dr. Chambers, he was chosen physician to that

hospital; but his exhausting duties in his former capacity had broken down the powers of his constitution. Phthisis supervened. At the age of forty, and on the 13th of May last, this amiable and accomplished physician departed from our world. In 1832 Dr. Hope was made a fellow of the Royal Society. In the succeeding years he received diplomas from several foreign societies, and in 1840 the College of Physicians elected him a fellow of their body.

By his will he directed that a post-mortem examination should be made, and that the persons employed should be at liberty to make any use of the morbid appearances they might think fit for the benefit of science, assigning as his reason that a physician should set a good example. The examination was accordingly made under the direction of Drs. Latham and Watson, who during his long illness had attended him with an unremitted attention, a sympathy, and a kindness, honorable alike to themselves and to their profession.

We proceed to give an outline of Dr. Hope's character, as viewed in its intellectual, moral, and professional aspects.

Dr. Hope's intellectual qualities were of a high order. A quick perception, a ready and singularly retentive memory, clearness, vigour, and comprehensiveness of thought, were happily associated with a sound and discriminating judgment, great decision, and a modest independence of mind.

From an early period Dr. Hope evinced a power of giving close and continuous attention to any subject in which he was engaged. Hence the impressions made upon his mind were singularly clear, and his recollections proportionally vivid and accurate.

*Decision of mind* was strikingly characteristic, especially as viewed in connexion with that modest opinion of himself which accompanied him through life.

The *vigorous activity* of Dr. Hope's mind rendered occupation always agreeable. He loved information for its own sake, and scarcely less for the pleasure he found in its pursuit; and having proposed to himself a high standard of attainment, he was never satisfied with any degree of excellence short of that which he believed himself able to reach.

*Comprehensiveness* was equally an attribute of Dr. Hope's mind. With an aptitude for observing the details of science he combined an admirable facility in classifying his facts, estimating their absolute and relative value, and cautiously deducing from them general principles.

He was also distinguished by *logical acumen*. Being himself a fair and close reasoner, never descending to sophistry or equivocation, he readily detected any fallacy in argument; while his thorough acquaintance with the laws of evidence led him to require the most complete proof of which the subject in question was susceptible.

*Versatility* moreover was happily associated in Dr. Hope's mental constitution, with his power of abstraction. Such was the elasticity of his mind that he could immediately rise, as with a bound, from the gravest to the most cheerful subject. During an extended classical education, and while studying the highest Greek authors, he was equally distinguished on the play-ground as the champion of athletic games; and subsequently, when zealously devoting himself to professional studies, his natural talent for drawing was so happily cultivated in the short intervals of relaxation as to enable him to copy, amongst others, a small landscape of Vanderveld with a spirit and fidelity worthy of an accomplished artist. This production of his pencil has a place in the valuable collection of the Lord President Hope.

Having thus cursorily noticed the particular features of Dr. Hope's intellectual character, it is but right to advert to the *singularly happy proportion* which subsisted between its several faculties, of which no one seemed to overpower or eclipse another. On the contrary, each appeared to sustain and invigorate the rest; and the harmonious working of the mind remarkably corresponded with the symmetrical arrangement of all its parts. Unity of design and consistency of action pervaded Dr. Hope's proceedings. His plans were wisely conceived, and as judiciously matured. At the fitting season they were carried

into effect with determined perseverance, however obstructed by difficulties or delayed by disappointments. Nor could he be induced to swerve from his fixed purpose by the lure of pecuniary advantage or the prospect of an ephemeral fame.

In its *moral* aspect Dr. Hope's character appears to great advantage.

The mild benignity of his countenance, blended as it was with an expression of acute intelligence, gave a true indication of the liveliness of his feelings and the kindness of his heart. His benevolence indeed would have frequently led him into extremes had he not, in the exercise of a wise self-discipline, accustomed his feelings to the due control of his judgment.

That Dr. Hope was ambitious cannot be denied; but his ambition was of the highest kind. He was ambitious of excellence, and was far more anxious *to be* good and great than to be thought so; and such was the noble elevation of his character that if undeserved distinctions had been within his reach he would have rejected them with disdain. To rise by legitimate means was doubtless his desire, but to rise in appearance only, or by the depression of others, was as foreign to the generosity of his nature as to his established principles of rectitude and honour.

Simplicity, ingenuousness, and magnanimity were strikingly manifested in his spirit and demeanour.

In the early stages of his career he was accustomed to view human nature in too favorable a light, an error not unfrequently attaching to young persons of high moral worth; and even when, by painful experience, he gained a deeper insight into the selfishness, double-mindedness, and envy which, alas! too often lurk even under a fair and polished exterior, he still endeavoured to put the kindest construction which truth would admit on human motives and actions. The tortuous policy of other men produced no change in him; his integrity sustained him. The directness and stability of his principles never allowed him to turn aside from the plain path of honour, even to secure the most important or favorite object. He knew himself too well to imagine that any advantage unworthily obtained could yield him true or lasting satisfaction.

Yet such was the kindness of his spirit, and so exquisitely keen his sense of fairness and justice, that the ungenerous treatment which he sometimes received even from those from whom he had deserved better things, pierced him to the quick. Yet under such trying circumstances his magnanimity ever rose superior to all other considerations, and forbade him to retaliate on the offending party.

Dr. Hope was moreover strictly conscientious. An inflexible regard to truth and justice pervaded his whole conduct. He considered himself as constantly under the eye of Omniscience, and as responsible for the right improvement of his time, talents, property, and influence. In short, he was a Christian, in the highest sense of the term, not from education merely, but after full and deliberate investigation. The momentous subject of religion demanded, he was aware, his deepest attention. Accordingly, he brought to its examination the same concentration of effort, the same patient scrutiny, the same logical acuteness, the same determination to believe nothing without conclusive evidence, which had proved availing in all his scientific researches. The result was a firm and abiding conviction of the Divine origin of Christianity, as revealed in the sacred records. Thus convinced, he unhesitatingly bowed to the authority of inspiration, and implicitly received its holy doctrines as the foundation of his highest hopes, and its holy precepts as the invariable rule of his conduct. Nor is it superfluous to add, that his confidence in the religion of the Gospel remained unshaken to the last. It proved his highest solace in affliction, and his support in death.

The *professional* character of Dr. Hope strikingly exemplified the practical influence of the valuable qualities already specified.

The profession of physic was not in the first instance his own choice. On the contrary, he had entertained a strong repugnance to it. But having at length resolved to be a physician, he immediately brought all the energies of his mind to bear upon the one point, of thoroughly qualifying himself for an

elevated position in the science and practice of medicine. His decision of character led him at once to impose proper limits on his favorite studies in general literature and the fine arts; in which, too, he had made considerable proficiency. He soon, however, had occasion to apply his skill as a draughtsman and colourist to the illustration of interesting subjects in pathology; and thus, by the skill and fidelity of his pencil, as well as by his close observation of disease, he not only contributed largely to the advancement of science, but also laid a deep and broad foundation for future eminence and usefulness. All the drawings in his plates of morbid anatomy were executed by his own hand.

Nature also seemed to have peculiarly qualified Dr. Hope for auscultation. His hearing, as well as his vision, was remarkably acute. With an exquisite ear for music he could distinguish the slightest differences in sound, and could distinctly perceive sounds which were so faint as to be inaudible to most other persons. This important faculty, aided by a long-continued habit of closely observing the *general* indications of disease, eventually raised him to the highest distinction as an auscultator.

With such endowments and such principles of action, it will be readily supposed that Dr. Hope's career of academical studies must have given a high promise of professional excellence; and such was the fact. The same studious habits were uniformly continued during the whole period of actual practice. He was constantly accumulating facts and educing principles. On settling in London, having only *one* private acquaintance, his professional efforts for a few years were attended with scarcely any pecuniary advantage; yet was he never disheartened, but persevered with cheerful alacrity and untiring diligence in adding to his stores of practical information; investigating doubtful points; submitting theories to the test of rigid induction; and more especially, proceeding steadily with his two great works on the Diseases of the Heart, and on Morbid Anatomy. When the first of these was published, about nine years since, the whole profession united in commendation of its excellence; and, in the enlarged and improved form in which the author was fortunately enabled to reproduce it, in a third edition, two years before his death, it is now universally acknowledged to be the best book on the subject in any language. Dr. Hope became at once a high authority in that class of maladies. His brethren and the public flocked to him; and had he not prematurely broken down under the pressure of excessive labour, an ample fortune and the highest professional elevation, would in all probability have been his reward.

Dr. Hope's demeanor in the sick chamber was peculiarly appropriate. A kind, attentive interest, combined with a calm, well-sustained investigation of the case, secured his patient's confidence and regard.

As a lecturer, Dr. Hope was highly prized. A familiar acquaintance with his subject, and an evident desire to impart to his pupils, in the clearest and most impressive manner, the extensive information he possessed, were advantageously associated with a grateful fluency of diction, and considerable power of lively illustration. A high-toned morality, and an almost affectionate interest in the welfare of his class, inspired and obtained their high respect and attachment.

It is most gratifying to record that the celebrity thus acquired was borne with perfect equanimity. Dr. Hope continued the same modest, ingenuous, benevolent character as he had ever been. No assumption of consequence, no harshness of manner, no unkindness to junior practitioners or to the sick, whether rich or poor, ever derogated from his well-earned fame.

Dr. Hope's career was indeed a short one. At the age of forty, in the very zenith of prosperity, in the midst of honours and usefulness, with a rapidly increasing fortune, and in the full enjoymont of domestic happiness, he was called to resign them all. But futurity was not a blank to him; he had a hope full of immortality.

Our limits have necessarily excluded almost every kind of proof and illustration. Both, however, may be expected from the rich and ample materials which are already in a course of preparation for a more extended memoir.

### Establishment of Hospitals in China.

In an early number of this Journal, (vol. IV. p. 568,) we noticed the establishment of Medical Missions in China; and we are much gratified to learn that they have been most successful in their double capacity of providing for the temporal and spiritual wants of the singular people on the borders of whose vast country they are planted. One of the most zealous and enlightened of these excellent missionaries, the Rev. Peter Parker, M.D. of the United States, is now in England for the purpose of raising "a permanent fund for the support of the 'Medical Missionary Society in China,' for the maintenance of the hospitals already established, and for the founding of others at every accessible and eligible part of China." The efforts to benefit the Chinese in this way in modern times are, we are told, briefly these. Alexander Pierson, Esq. surgeon to the Honorable East India Company, introduced successfully the art of vaccination in 1805; this has since extended widely through the empire. Dr. Livingston and Rev. Dr. Morrison opened an infirmary for the poor Chinese at Macao in 1820, which was sustained for some time, and alleviated much suffering. In 1827, T. R. Colledge, Esq. surgeon to the Honorable East India Company, opened his Eye Infirmary at Macao, and, during the three years of its continuance, afforded relief to no less than 4000 patients, among whom were persons in different ranks, and from various parts of the empire, from whom he received many and unequivocal tokens of gratitude. The Ophthalmic Hospital at Canton was opened by the Rev. P. Parker, M.D., October, 1835, and the General Hospital at Macao, in July, 1838. Up to the 17th June, 1840, these institutions had received upwards of 8000 patients, embracing every variety of disease. It was after long effort that a place was found for an hospital; and when at length a suitable building was rented, and previous notice had been given, the first day no patients ventured to come; the second, a solitary female affected with glaucoma came; the third day, half a dozen; and soon they came in crowds. Since then patients from all parts of the empire have availed themselves of the benefits of the hospital; persons of all ranks, military, naval, and civil officers, the Nanhæhêer, or district magistrate, the custom-house officer, salt inspectors, provincial judges, provincial treasurer, a Tartar general, governors of provinces, Commissioner Lin himself, and even a member of the imperial family.

The following extract from the same pamphlet which has supplied us with the above information respecting hospitals in China,* and which we recommend to the notice of our readers, points out, in a few words, the nature of the noble cause in which this gentleman is engaged, and which cannot fail to commend itself to every philanthropist.

"That the union of the art of healing with that of teaching, in the missionary of modern times, is as important as in the early ages of Christianity, is no longer doubtful. The experiment has been made, and succeeds. Healing by miraculous agency was employed at the commencement of the Christian era, chiefly as other preternatural powers were to establish the divinity of Christianity. A still further object was to exhibit the beneficial spirit of the gospel. The age of miracles and the occasion for them ceased together; but the *spirit* of the gospel is the same in every age. Healing the sick, opening the eyes of the blind, and the ears of the deaf, and causing the tongue of the dumb to speak, and the lame to walk, by natural and scientific means, is not less calculated in the nature of things, to conciliate favour, and to demonstrate the disinterested and benevolent genius of Christianity now, than it was eighteen centuries ago. Though the practice of medicine and surgery among western nations is founded upon *science*, yet, to an uncivilized and superstitious nation, it has much of the appearance of a superhuman power, which may lawfully subserve a good end, if the truth of the case be distinctly stated, and their credulity be not imposed upon. The gratuitous practice of medicine and surgery, founding hospitals

* "Statements respecting Hospitals in China," by the Rev. Peter Parker, M.D.—London, 1841.

and infirmaries, confer a direct and great good upon suffering humanity. This species of charity is peculiarly needed in China. To sustain the hospitals already established in the empire, and to multiply them as the way is prepared, and in them to train up Chinese youth, to extend the blessings beyond the limits to which the policy of the government restricts foreigners, and to give a correct and scientific practice of medicine and surgery to an empire which exceeds in territory and population any other nation, is of itself a grand enterprise. The undertaking so great, is also *practicable*. Had the object no claims beyond those already alluded to, it would be deemed sufficient in respect to any christianised country; but in relation to China, they are the subordinate claims, compared with still higher ones, to which they conduct. In exhibiting the utility and importance of this object, let it not be supposed that any other is displaced. It is not to be lost sight of for a moment, that *Divine truth* is the great agent through which our ultimate aim is to be gained. While by the needle of the oculist the light may be poured upon the eye long dark, by the surgeon's knife the useless limb amputated, and by the physician's skill even the malignant disease may be cured, nothing short of a higher power can, in a moral sense, remove the film from the eye, clarify the spiritual vision, and heal the malady of sin. At present, however, it is but to a limited degree that the higher means can be employed; but to exhibit the spirit and the fruits of the gospel, Providence has remarkably opened the way; and it is fondly hoped that at no distant day, the Chinese will regard these benevolent offices as such, and permit us to publish and enforce the *precepts* of Christianity. After several years' experience and residence in China, the firm belief is expressed, that in the present state of the Chinese, who are prejudiced against foreigners, as avaricious and barbarous, and possessed of no redeeming qualities, there is no method so directly adapted to remove false impressions, and to convince them of the true character of Christian men and the Christian religion, as by the plan adopted by the Medical Missionary Society in China."

We earnestly commend the sacred cause in which Dr. Parker and his brethren are engaged, to the attention of all whose position and means enable them to promote it. Independently of the richer fruits of their heroic labours, we look forward, with confidence, to great benefits which medicine may expect from them, in the observation of new forms of disease, and in the discovery of new therapeutic means in the natural productions of this vast and unknown country, and amid the mountains of pharmaceutic rubbish which have been accumulating in their traditionary and written records, from periods anterior to all occidental history. We shall even hope for no inconsiderable addition to our stock of oriental medicine, from the work which Dr. Parker is now preparing for the press, founded on his observation and practice in China.

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## BOOKS RECEIVED FOR REVIEW.

BRITISH.

1. Memoir on the Radical Cure of Stuttering by a Surgical Operation. By J. F. Dieffenbach. Translated from the German by Joseph Travers, late House-surgeon to St. Bartholomew's Hospital.—London, 1841. 8vo, pp. 26. Plates. 3s.

2. Guy's Hospital Reports. No. XII., April, 1841. 6s.

3. Elements of Medicine. Vol. II. On Morbid Poisons. By Robert Williams, M.D., Senior Physician to St. Thomas's Hospital.—Lond. 1841. 8vo, pp. 686. 18s.

4. The Anatomy of the Arteries of the Human Body. By Richard Quain and Joseph Maclise. Part VI. 8vo and folio. April, 1840. 12s.

5. The Baths of Central and Southern Germany, considered with reference to their remedial efficacy in Chronic Disease. By E. Lee, Esq.—London and Paris, 1841. 8vo, pp. 139. 3s. 6d.

6. A Complete Practical Treatise on Venereal Diseases, and their immediate and remote Consequences; including observations on certain Affections of the Uterus attended with Discharges. By William Acton.—London, 1841. 8vo, pp. 410. With an Atlas of Plates. 1*l.* 11s. 6d.

7. A Medical Guide to Nice; containing

every information necessary to the Invalid and Resident Stranger. By W. Farr, M.D. —London, 1841. 8vo, pp. 177. 5s. 6d.

8. On the Cure of Strabismus. By Thomas Elliott. (From the Edinburgh Journal.) 8vo, pp. 27.

9. Jaundice from Non-Elimination. By W. Lowe, M.D. (From the Edinburgh Journal.) 8vo, pp. 19.

10. Report of the Lunatic Department of the Edinburgh Charity Workhouse for 1836-7-8. By John Smith, M.D. 8vo, pp. 12.

11. Report of the Saughton Hall Lunatic Asylum. Nov. 1840. 8vo, pp. 12.

12. Stammering and other Imperfections of Speech treated by Surgical Operations on the Throat, &c. By J. Yearsley, M.R.C.S.—London, 1841. 8vo, pp. 46. 1s.

13. The Philosophy of Mystery. By W. C. Dendy, F.L.S.—London, 1841. 8vo, pp. 443. 12s.

14. The Madras Quarterly Medical Journal. Edited by Samuel Rogers. Vol. II.—Madras, 1840. 8vo, pp. 462.

15. Hints for Invalids about to visit Naples; being a sketch of the Medical Topography of that city. Also, an account of the Mineral Waters of the Bay of Naples. By J. C. Cox, M.D., F.L.S.— London, 1841. 8vo, pp. 190. 7s. 6d.

16. Popular Cyclopædia of Natural Science. Vegetable Physiology.—London, 1841. 8vo, pp. 295.

17. Researches on Operative Midwifery, &c. By Fleetwood Churchill, M.D.—Dublin, 1841. 8vo, pp. 325. With plates. 14s.

18. On the Minute Structure and Movements of Voluntary Muscle. By William Bowman, Esq. (From the Philosophical Transactions.)—London, 1840.

19. The Transactions of the Provincial Medical and Surgical Association. Vol. IX.—London, 1841. 8vo, pp. 583. 21s.

20. Spinal Affections. A Popular Lecture on Disorders and Diseases of the Spine. By H. C. Roods, M.C.S.L.—London, 1841. 12mo, pp. 57. 2s.

21. The Philosophy of Death; or a General Medical and Statistical Treatise on the Nature and Causes of Human Mortality. By John Reid, Licentiate of the Faculty of Physicians and Surgeons of Glasgow.—London, 1841. 8vo, pp. 381. 6s. 6d.

22. Report of the Cases attended at the Birmingham Eye Infirmary during the years 1838-9. By R. Middlemore, Esq. (From the Transactions of the Provincial Association.)—Worcester, 1841. 8vo, pp. 20.

23. Rambles in Europe in 1839; with sketches of prominent Surgeons, Physicians, Medical Schools, &c. By W. Gibson, M.D., Professor of Surgery in the University of Pennsylvania, &c.—Philadelphia, 1841. 8vo, pp. 309.

24. The Sanative Influence of Climate; with an account of the best Places of Resort for Invalids, in England, the South of Europe, &c. By Sir James Clark, Bart., M.D., F.R.S., Physician to the Queen. 3d edit.—Lond., 1841. 8vo, pp. 377. 10s. 6d.

25. Eleventh Annual Report of the Belfast District Asylum for Lunatic Poor.— Belfast, 1841. 8vo, pp. 26.

26. A General Outline of the Animal Kingdom, and Manual of Comparative Anatomy. By T. Rymer Jones, Professor of Comparative Anatomy in King's College. With 336 engravings.—London, 1841. 8vo, pp. 732. 1*l.* 18s.

27. Report upon the Mortality of Lunatics. By W. Farr, Esq. (From the Journal of the Statistical Society.)—London, 1841. 8vo, pp. 17.

28. A Treatise on Pyrosis Idiopathica, or Water Brash, as contrasted with certain forms of Indigestion, &c. By Thomas West, M.D.—Lond. 1841. 8vo, pp. 108. 5s.

29. Observations on the Surgical Pathology and Treatment of Aneurism; being the substance of a course of lectures on that disease. Part I. By W. H. Porter, A.M., Surgeon to the Meath Hospital, Dublin. (No date.) 8vo, pp. 214. 6s.

30. On certain Physiological Inferences which may be drawn from the study of the Nerves of the Eyeball. By W. P. Alison, M.D., F.R.S.E. (From the Transactions of the Royal Society of Edinburgh.)—Edinburgh, 1841. 4to, pp. 20.

31. An Essay on the Chemical, Botanical, Physical, and Parturient Properties of the Secale Cornutum. By T. H. Wardleworth, surgeon.—London, 1840. 12mo, pp. 69.

32. The Physiology of Vision. By William Mackenzie, M.D., Lecturer on the Eye in the University of Glasgow.—London, 1841. 8vo, pp. 292. 10s. 6d.

33. Odontography; or, a Treatise on the Comparative Anatomy of the Teeth. By Richard Owen, F.R.S. Part II., with 50 plates.—London, 1841. Royal 8vo. 31s. 6d.

34. On Stammering and Squinting, and on the Methods for their Removal. By Edwin Lee, M.R.C.S., &c.—London, 1841. 8vo, pp. 88. 3s.

35. Phrenology consistent with Science and Revelation. By Charles Cowan, M.D., &c. 12mo, pp. 55.—Reading and London, 1841. 2s. 6d.

36. An Inquiry concerning the Diseases of the Brain, the Spinal Cord, and the Nerves. By Amariah Brigham, M.D.— New York, 1840. 8vo, pp. 327.

THE

# BRITISH AND FOREIGN MEDICAL REVIEW,

FOR OCTOBER, 1841.

## PART FIRST.

## Analytical and Critical Reviews.

### Art. I.

1. *On the Typhus Fever which occurred at Philadelphia in the spring and summer of* 1836; *illustrated by Clinical Observations at the Philadelphia Hospital; showing the distinction between this form of disease and Dothinenteritis or the Typhoid Fever with alteration of the follicles of the small intestine.* By W. W. Gerhard, m.d., one of the Physicians to the Hospital. (*American Journal of the Medical Sciences.* Nos. 38 and 40, February and August, 1837.)

2. *Considérations sur la Fièvre Typhoide, &c. &c.—Du Typhus Fever et de la Fièvre Typhoide d'Angleterre.* Par M. Valleix, m.d. (*Archives Générales de Méd.* Janv. Fevr. Octob. et Novemb.)—*Paris,* 1839. 8vo.

*On the Typhoid and Typhus Fever of France and England.* By M. Valleix, m.d.—*Paris,* 1839.

3. *Report on the Epidemic Fever of Edinburgh. An Account of the Symptoms and Treatment.* By William Henderson, f.r.c.ph. &c.

*Analysis and Details of Forty-seven Inspections after Death.* By John Reid, f.r.c.ph., &c. (*Edinburgh Medical and Surgical Journal,* Vol. LII. pp. 429-462.—*Edin.* 1839.

4. *Mémoire sur le Fièvre Typhoide, sur les diverses Formes qu'elle peut présenter, et sur le Traitement qui lui est applicable,* (*Mémoire honoré d'une médaille d'or par la Société Médicale de Toulouse.*) Par J. B. Delaroque, Médecin de l'Hôpital Necker, &c.—*Paris,* 1839. 8vo, pp. 235.

*Essay on Typhoid Fever, its different Forms and mode of Treatment, &c.* By J. B. Delaroque, Physician to the Necker Hospital.—*Paris,* 1839.

5. *Der Abdominal Typhus.* Monographische Skizze von Dr. F. Cramer.—*Cassel,* 1840. 8vo, pp. 128.

*On Abdominal Typhus.* By F. Cramer, m.d.—*Cassel,* 1840.

6. *The Articles "General Doctrines of Fever" and "Continued Fever."* By Dr. Christison, (*Library of Medicine,* vol. I. pp. 113-188.)—*London,* 1840.

7. *Lectures on the Morbid Anatomy of the Serous and Mucous Membranes.* By Thomas Hodgkin, m.d. (Vol. II. Lectures xxii. and xxiii.)—*London*, 1840.

8. *Some Considerations on the Nature and Pathology of Typhus and Typhoid Fever, applied to the solution of the question of the Identity or Non-identity of the Two Diseases.* By A. P. Stewart, m.d. (Read before the Parisian Medical Society on the 16th and 23d of April, 1840.)—*Edinburgh Medical and Surgical Journal*, Vol. LIV., pp. 289-339. October, 1840.

9. *Observations on Typhus, submitted to the Faculty of Physicians and Surgeons of Glasgow.* By Andrew Anderson, m.d. &c.—*Glasgow*, 1840. 8vo, pp. 66.

10. *Traité de l'Entérite Folliculeuse* (*Fièvre Typhoide*). Par C. P. Forget, Professeur de Clinique Médicale de la Faculté de Strasbourg, &c. &c.—*Paris*, 1841. 8vo, pp. 856.

*Treatise on Follicular Enteritis* (*Typhoid Fever*). By C. P. Forget, m.d., Professor of Clinical Medicine to the Faculty of Strasburg, &c. &c.—*Paris*, 1841.

11. *Recherches Anatomiques, Pathologiques et Thérapeutiques sur la Maladie connue sous les noms de Fièvre typhoïde, putride, adynamique, ataxique, bilieuse, muqueuse, gastro-entérite, entérite folliculeuse, dothinentérie, &c., comparée avec les maladies aigues les plus ordinaires.* Par P. C. A. Louis, m.d., Médecin de l'Hôtel-Dieu, Président perpétuel de la Société Médicale d'Observation, &c.—*Paris*, 1841. 2 tomes, 8vo, pp. 542, 523.

*Anatomical, Pathological, and Therapeutical Researches on the Disease known under the names of Typhoid Fever, &c.* By P. C. A. Louis, Physician to the Hôtel-Dieu, Perpetual President of the Medical Society of Observation, &c.—*Paris*, 1841.

In our last Number we presented the readers of this Journal with a tolerably close analysis of the volumes of M. Louis upon Continued Fever, and in the course of this announced our intention of pursuing the subject, and more especially of examining the much-litigated question of the identity or non-identity of the disease, as it exists in France and in our own country. Accordingly we proceed to examine with as much fulness as we may reasonably venture on, the state of facts and opinions, as displayed in some of the most recent works published on both sides of the channel. We shall endeavour to render the basis of comparison still more complete by a brief enquiry into the history of the malady observed in other continental countries, and in the United States. Having submitted, as we pass, the work of each author to the criticism apparently warranted by its pretensions, we shall draw our conclusions from the whole mass of evidence brought before the reader.

One of the authors whose names appear at the head of this article, an enthusiastic advocate of the doctrine ascribing to the lesion of Peyer's glands all the phenomena of typhoid fever, commences his bulky tome with an elaborate enquiry into the opinions respecting the nature and essence of the malady entertained by our forefathers. We shall not ac-

company the writer, M. Forget, seriatim through the details wherefrom he considers himself justified in concluding that all historical evidence goes to prove the acknowledgment from the time of Hippocrates to the present day of three fundamental propositions in the doctrine of fever: namely, the identity of the different forms of continued fever; their inflammatory character; their seat in the intestine. Our limited space prevents us from undertaking this task in its full extent; but it will not be beyond our means, nor irrelevant to the purpose of the present enquiry, to take a rapid survey of the more important passages adduced by this ingenious ransacker of bygone literature, as susceptible of the interpretation he has found for the febrile doctrine of antiquity. It must, in justice to M. Forget, be admitted that his quotations are correct, and if it be fair to build upon particular passages general doctrines, to argue from the literal acceptation of a phrase, while the *animus* of its author throughout the pages in which it occurs is wholly lost sight of, M. Forget may claim the honour due to a faithful historical investigator. It is true that Hippocrates has said "febres vertiginosæ, et cum tenuis intestini morbo, et sine hoc, perniciem intentant," but, as even M. Forget shrinks from regarding these words as more than the accidental expression of a notion vaguely conceived and enunciated without conviction, it is needless for us to set about proving what is self-evident. Baillou, no doubt, asserts his belief that "almost all fevers (with the exception of some symptomatic ones) have their cause in the mesentery," and declares that "the surface of the intestines is inflamed;"* but these opinions are far from pervading his works. Willis notes the frequency of dysentery, as he terms it, during continued fever, speaks of the formation of small ulcers in the intestine, and compares this internal pustulation to the external of variola, a parallel for the honour of originating which, superficial and fallacious as it is, contemporary French authors have contended. The passages wherein the illusory nature of common belief respecting the "malignity" of fever is vigorously stigmatised by Sydenham, are probably too well known by English readers to require particular citation. Sydenham did not recognize any connexion between continued fever and intestinal lesion, but in ascribing "malignity" to intense inflammation he appears to M. Forget to lend aid—truly, most garbled aid—to the establishment of the thesis we have stated. Chirac observed, as he alleges, redness, inflammation, and livid spots in the stomach and intestines of subjects dying of fever "before the fourth day." Baglivi professes notions of the same stamp as Baillou. Frederic Hoffman states that "putrid synochus, ardent, bilious, inflammatory fever, are accompanied with inflammation of the stomach and intestines." Huxham speaks of certain symptoms as "generally originating in inflammation or mortification of the intestines." If Pringle's theory ascribed "the continuation of fever to early inflammation of some part of the brain or nervous system," he also speaks of the intestines as being "more particularly subject to inflammations." Roederer and Wagler, in their celebrated work *De Morbo Mucoso*, carefully describe the condition of Peyer's patches, and ascribe the fatal result in fever to abdominal inflammation and gangrene or to engorgement of the lungs. Sarcone, ob-

* De Urinar. Hypostasi.

serving at Naples, maintained the alimentary canal and the entire abdominal system to be the parts, next to the blood, most constantly affected; his countryman, the celebrated Cotugno, observed enlargement of the mesenteric glands. Cullen is appealed to by M. Forget as upholding the doctrine of the identity of all "essential fevers," though his nosographical arrangements tended to keep alive the idea that his notions were of a very different kind. Thus he speaks of synochus and typhus as being produced by the same causes, and therefore varieties of each other; looks upon typhus as a genus comprehending several species, a great number of which acknowledge no specific difference, and appear to be merely varieties depending upon accidental circumstances. Stoll's testimony to the existence of gastro-intestinal lesion in fever is distinct and unquestionable; but he regards this as the result of irritation induced by saburral matter circulating in the bowel. The doctrine of essentiality taught by Pinel, and his division of continued fever into species, is denounced by M. Forget as having thrown the science back. Now we mean not to defend either branch of this doctrine, for we believe both to have been totally erroneous; but we deny that their promulgation changed the prospects of the science in the least, for the simple reason that Cullen and others of the authors cited by M. Forget preached a theory fundamentally the same. But M. Forget evidently knows something more of Pinel than he does of Cullen. Pass we next to one of the most zealous labourers, produced by the positive tendencies of the age, in the discovery of the organic cause of disease—Prost, the real originator of the gastro-enteritic theory of fever. Prost wrote in 1804: "I had made at least 150 examinations of the bodies of persons dying of ataxic fever, without being able to discover anything particular in the brain; but I had *always* seen inflammation of the intestinal mucous membrane with or without excoriation. . . . . Mucous, gastric, ataxic, adynamic fevers have their seat in the mucous membrane of the intestines; they result from divers morbid changes in that membrane."* In 1813 Petit and Serres described their "entero-mesenteric fever," defined with tolerable clearness the lesions of the patches of Peyer and mesenteric glands, distinguishing them from common inflammation; they taught that this morbid condition precedes the development of fever, and that the danger of this latter is always proportional to the degree of the former. The year 1816 was marked by the advent of Broussaisism. How completely that doctrine was anticipated, as regards its founder, is manifest from the passage just quoted from Prost. Prost sank to the grave unhonoured and unthought of; Broussais was almost deified at the very outset of his career: the cause is plain; the latter cried aloud till he was heard, the former contented himself with a modest statement of what he believed himself to have observed; the one wrote with the energy of conviction, the other with the feebleness of indifference. The result exhibited once more the triumph of *manner*, realizing even in matters of science the saying of Chateaubriand: "*C'est par le style qu'on vit.*"

There cannot, we imagine, exist a particle of doubt in the mind of any one acquainted with the general doctrines of the authors referred to, that those doctrines are grievously misinterpreted by M. Forget. Indeed,

* Médecine éclairée par l'ouverture des corps, t. i. p. viii.

to English readers the perversion must be so apparent, that for their sakes we should not have considered it necessary to proclaim it; but as in France the historical manifesto of the Strasburg Professor is likely to be triumphantly appealed to by the Broussaisian school, it is incumbent upon us to enter a protest against its justness. We have no inclination to contest M. Forget's right to close his volume with the "*Cecy est un livre de bonne foy*" of Montaigne; but, if so, it is clear that with *bonne foy* alone a man cannot write history. However, the truth is, that what the general theories held by these departed worthies may have been, is, comparatively speaking, matter of little moment: the important point is to determine what the just inference is from the facts they observed and registered; the history of things, not of opinions. Now an examination of the entire of these works, especially the English, and exclusive of all those produced from the time of Prost downwards, shows that the true version of their authors' experience in respect of M. Forget's three propositions is this: continued fever is sometimes attended with disease of the intestine; fever is commonly attended with, if it do not necessarily involve the presence of, inflammation; the different forms of continued fever are all probably referrible to a single species. These notions are very different from those putatively gleaned from antiquity by M. Forget, and condensed by him into the terse dogmata—seat in the intestines, inflammatory nature, identity; but they are precisely such positions as the latitudinarian interpretation of a hot and ingenious partisan might most easily convert into those put forth by this writer.

The opuscule of M. Delaroque, although "got up in an extreme hurry" for presentation to the Medical Society of Toulouse, will, the author dares hope, satisfy the desires of "practitioners." We feel abundant gratification in being able to reassure one suffering under such modest misgivings as to the perfection of his doings: it will more than satisfy them, it will enrapture them, if by "practitioners" we understand, with M. Delaroque, that host of "good easy men," who ask neither for logic nor philosophy, but cry aloud for a drug for each malady, and fancy themselves practitioners, when some culler of simples answers *eccola!* To these persons M. Delaroque's pages will be an invaluable gift, pages announcing in purgatives a panacea for typhoid fever. Whether drastics deserve this character or not, however, it is not our purpose to enquire; and had the author contented himself with developing such asseveration, his labours would not have been brought under notice in the present article. But no, the alleged fact is not enough, a theory of the disease must be found; M. Delaroque having worked marvels in therapeutics, covets the fame of a pathologist. Accordingly we are taught that typhoid fever is the mere "product of the action of decomposed fluids on the alimentary canal," which are presumed, having first stagnated at the end of the ileum, to enter the circulation. Here the organic origin of the disease is set aside, and the saburral doctrine of Stoll restored; a doctrine "dating from so early a period," (what will the historic penetration of M. Forget say to this?) "that it is to be found in Hippocrates, and in all the authors of antiquity." Besides, adds M. Delaroque, "it is the only theory accordant with the daily results of treatment." The only theory accordant with the results of *my* treatment—*hinc illæ lachrymæ*; we suspect, in truth, the sweet voice of

antiquity might have sung in vain, had not M. Delaroque gained a certain Parisian notoriety by treating typhoid fever with purgatives in the morning, purgatives at noon, purgatives in the evening, and purgatives in the morning again.

Possessed of a very superficial acquaintance with the malady he describes,* M. Delaroque unhesitatingly gives forth the law upon every possible point, and sneers at those who have profoundly laboured in its investigation. All this is the natural, and almost the necessary, consequence of the attempt to found a theory of a disease upon presumed results of treatment. Compare M. Bouillaud, who affirms that the disease *must* be a violent inflammation, because he *cures* it by extracting almost all the blood in the body: M. Delaroque, who is equally positive the whole malady *must* consist in the presence of saburræ, because by submitting the small intestines to a process of perpetual scouring he *cures* the disease: and lastly M. Piedagnel, who (if he were to follow in the same strain) has stronger reasons than either for declaring the malady *must* be no malady at all, inasmuch as he *cures* more patients than any one by doing nothing at all.†

Let us now return to the volumes of Louis, the main doctrines of which may be enunciated as follows: 1. The different species of continued fever admitted by Pinel are all referrible to a single malady, the typhoid affection. 2. Their anatomical character is a special lesion of the patches of Peyer, and of the mesenteric glands, a lesion inseparable from its existence, and one which developes itself in a more regular and fixed manner than the anatomical character of any of the phlegmasiæ. 3. This lesion is developed before any one of the other organic changes that attend the disease, except probably an altered condition of the blood. 4. It is specific, differing in its characters from those of ordinary inflammation. 5. The general tract of the gastro-intestinal surface is not necessarily implicated. 6. The disease is attended with marked proneness to membranous ulceration. 7. The typhoid affection is not strictly assimilable to the exanthemata. 8. It occurs but once in a lifetime. 9. It is not observed in individuals who have passed a certain age, about fifty. 10. It originates spontaneously and in contagion.

The first point we have to enquire into is, whether the doctrines here maintained are borne out by general Parisian experience. Respecting the fundamental identity of the different species of "essential fever" established by Pinel (and these answer to the different forms of continued fever with or without organic complication admitted in this country), no doubt is now hazarded by any of the more observant physicians of Paris. M. Chomel, M. Bouillaud, and all others who have given public expression to their experience, agree in regarding the class of essential fevers as a single malady, varying accidentally in its symp-

* M. Delaroque leaves acquaintance with the anatomical history of the disease to such persons as MM. Louis, Andral, and Chomel, "*who have occasion to make many post-mortem examinations.*" He, of course, can have no such opportunity; live but the purgative system awhile, and the dissection of a patient with typhoid fever will be matter of tradition.

† Of sixty cases allowed by M. Piedagnel to run their own course, three only terminated fatally, the lowest ratio of mortality ever obtained in Paris.

tomatic manifestations, but under all circumstances equally characterized by the existence of the glandular lesion of the small intestine, and capable of being referred thereto during life in spite of any degree of variableness of those manifestations ever actually witnessed.

Some discrepancy of opinion prevails, as has already appeared in our former article (No. XXIII. p. 22), regarding the invariable and necessary presence of the special lesion of Peyer's glands in subjects labouring under the continued fever of Paris. The argument of M. Dalmas, and the species of refutation adopted by M. Valleix, have already been mentioned. It is not easy to detect sophistry in the reasoning of the latter; it is difficult even to assign just cause for not adhering to it altogether, yet it has been by some esteemed rather more ingenious than solid, and that an *arrière-doute* leaves the mind unsatisfied by its perusal cannot be denied. And this doubt is strengthened by the fact, that Andral concludes from his experience, that "in the pyrexiæ constituting the different morbid groups designated by the title of essential fevers, certain lesions are very frequently, ninety-eight times in every hundred about," [but only this] "detected in the digestive tube;" he believes, however, that "when continued fever is connected with the intestinal exanthema, the latter originates at the outset of the disease." M. Chomel, as previously mentioned, has not encountered a single case of typhoid fever within the last five years unattended with the Peyerian lesion, yet in respect of general assertion, he conceives himself warranted in going no further than the affirmation, that "if it be not constant in the strict acceptation of the term, its total absence is extremely rare." M. Rostan has within the last year taught that continued fever does in rare instances exist in Paris without the Peyerian disease.

A serious objection of the converse kind has been raised to the doctrine; it has been affirmed, that lesions undistinguishable from those developed as attendants on typhoid fever, occur in other diseases; among acute, in scarlatina, epidemic cholera, and yellow fever; among chronic, in phthisis. Further, the intestinal patches of infants are said frequently to present a similar state of change without (the presumption is) symptoms of typhoid fever having preceded death. That under some of these circumstances the patches swerve from the normal state is admitted by the majority of observers; but, as we can affirm from experience, confusion can only arise in a certain number of them, if the examination be conducted with extreme inattention. Thus, in scarlatina, redness and thickening of the patches is occasionally observed, but such state has not been known to lapse into ulceration: M. Forget suggests that in such cases the symptoms during life may have worn an ataxic or typhoid character. In the majority of the victims of cholera observed by M. Chomel, the follicles of the intestine were enlarged; but the lesion differed from that of continued fever, in that the elevation of the patches was much less considerable, that the change was of uniform character at all periods of the disease, from twenty-four hours to thirty-six days after seizure, and that ulceration was never detected. The minute enquiries of Böhm, though not precisely corroborating the description of M. Chomel, nevertheless bear him out in his affirmation, that a difference exists between the lesions developed in the two diseases. Böhm, whose previous researches particularly qualified him for such enquiry, describes two kinds

of alteration in cholera, "one affecting the mucous, the other the submucous tissue. The mucous surface of the Peyerian glands is destroyed, the small capsules ulcerate, open, and disappear, even to their outermost border. The cavities of the capsules thereby become minute fossulæ, and the surface of the patches mesh-like or retiform. The submucous tunic is sometimes filled with and swollen by a white plastic exudation. Still this change never reaches the degree in cholera that it does in typhus abdominalis."* According to the researches of M. Louis, the glandular patches maintain the natural appearance in yellow fever. The objection drawn from the condition of the glands in early infancy, loses its force in a great measure from the circumstance that the absence of continued fever during life does not appear to have been established. The diseased patches in phthisis, instead of being infiltrated throughout their cellular substratum with whitish yellow matter, are studded with minute tubercles; and where extensive ulceration has occurred, the case may be still distinguished either by the presence of tubercles on the edge of the ulceration, or by thickening and induration of the surrounding tissues; besides, the mesenteric glands are infiltrated with tuberculous matter.

Respecting the initiatory character of the lesion, when it exists, we have shown by a quotation that M. Andral's experience agrees with that of M. Louis: the character of the earliest symptoms is the ground of such belief, coupled with the fact of its being confirmed by such dissections as have been performed almost at the outset of the malady; on the 8th, 7th, 6th, (Montault, Andral,) even 5th days, (Bretonneau, Forget,) the special morbid change has been discovered. But by M. Louis, at least, the primitive character of the lesion is only asserted as regards appreciable changes of tissue: that the blood is the real seat of the primary change was pronounced in his first edition (vol. ii. p. 311) "far from improbable, and indeed in many cases to be strongly presumed:" an opinion adhered to in the second (vol. ii. p. 178). M. Chomel professes the same belief: M. Rostan has espoused it.

We have stated the reasons which induced M. Louis to regard the inflammation of the mucous tissue of the patches as of specific, or if the term be objected to, of peculiar character. The main reason for this consists in the development of morbid matter in the submucous tissue, an occurrence which the researches of Böhm, and, we might add, those of M. Chomel, prove to be of even more frequent occurrence than the experience of M. Louis warranted him in affirming. The disciples of the gastro-enteritic theory, disciples whose ranks grow daily thinner, while the relics thereof adhere to it with all the petulant pertinacity of a defeated party, naturally look with an evil eye upon, and are perpetually carping at this doctrine. M. Forget scouts the notion of any peculiar character appertaining to the phenomena attending the development of the Peyerian lesions, and apostrophises the holders of the opposite opinion in language displaying the vivacity of the enthusiast, but neither the soundness of the logician nor the caution of the observer.

The correctness of M. Louis's inference respecting the secondary importance of lesion of the general surface of the gastro-intestinal mucous

* Muller's Archiv. Heft iv. 1839.

tunic is, as might be anticipated, also contested by the followers of Broussaisism. But when M. Bouillaud exclaims that "none but observers blinded by the spirit of prejudice . . . . can maintain that the inflammation resides exclusively and solely in the patches of Peyer," he alters the terms of the question. MM. Louis and Chomel, against whom the tirade is evidently directed, have not affirmed as a general proposition that the follicular structure is the sole seat of disease in the digestive tube; their facts would not warrant any such inference: but these facts do prove that the affection may terminate fatally in rare instances without material implication of the general mucous surface. M. Bouillaud's partisanship prevents him from going further than the admission, that "the principal disease exists in the patches and in the isolated follicles:" the truth, but not the whole truth.

The proneness to membranous ulceration displayed by the anatomical details of M. Louis, has been confirmed by others, though perhaps not quite to the same extent.

That the disease occurs but once in life; in other words, that a first attack ensures immunity from a second, is a point in its history respecting which opinions in Paris appear to be unanimous.

M. Louis has attached considerable diagnostic importance to the alleged fact, that typhoid fever is a disease of youth and of adult age; he himself never having observed it in subjects who had passed the fiftieth year. The perfect correctness of M. Louis' opinion may be doubted. In the first place the experience of others does not corroborate it; thus Petit observed the disease in a subject aged sixty, M. Montault in one of sixty-five, M. Andral in one aged seventy. In the second place M. Louis has observed at an hospital—and this is true of the other professors whose names have been repeatedly mentioned—into which sexagenarians rarely find their way; and we have not known it affirmed that in the immense establishments devoted to their reception, the development of the malady has been demonstated to be either unknown or extremely rare. There seems to be a strong reason of another kind for supposing that the occurrence of the disease must be exceedingly uncommon at an advanced age, at least among the individuals forming the ordinary population of hospitals, namely, the vast frequency among the youthful and adult of the lower orders of Paris, coupled with the ascertained fact that it is a malady that occurs once only in a lifetime. M. Louis, however, in anticipation of this objection, argues its want of validity on the ground that the immunity from a second produced by a first attack of variola, of measles or scarlatina, and the equal frequency of these diseases in infancy, are well established, "yet no age is exempt from the chance of variola, measles, or scarlatina." But to this it may be replied that, as the experience of M. Louis' colleagues proves, no age is exempt from the chance of typhoid fever.

The Parisian school generally has been resolutely anti-contagionist, founding its belief on the line of argument attributed in our last article to M. Andral. M. Chomel was perhaps the first to waver in his acceptance of the prevalent doctrine. He maintained that the fact of non-contagion in Paris had never been demonstrated, inclined to the belief that the malady was actually contagious to a limited degree, and suggested that this would become matter of certainty should subsequent researches

prove the identity of the anatomical lesions of typhoid fever and camp typhus. We have exhibited M. Louis' arguments in favour of contagion prevailing in Paris, and we certainly cannot detect any flaw therein, *provided the general style of reasoning adopted by contagionists be admissible.* M. Forget, it appears, in his character of a "pupil of the Parisian school," denied the existence of contagion until his experience at Strasburg convinced him that "whether it be by inoculation of a specific virus (contagion), or by inspiration of an atmosphere vitiated in any other manner (infection)" he cannot determine, but "certain it is that the disease is *communicated* under certain circumstances sufficiently rare and as yet undetermined."

We now come to a much litigated point in the history of the typhoid fever of France, the relationship of the intestinal lesion to the disease itself. In his first edition, M. Louis contented himself with announcing the invariableness of the existence of this lesion, with inferring thence that it constituted the anatomical character of the disease, in other words the material proof of its existence, and with ascribing the intestinal symptoms to its presence. But others soon appeared less chary of inference, and at once elevated the lesion to the rank of a *cause.* These persons were not, however, precisely agreed among themselves as to the mode of sequence of the phenomena; hence two parties whose theories have been thus briefly and clearly stated by M. Piorry. The one maintains the series to be this: enteritis; ulceration; formation of sloughs and putrid matter; absorption of the latter; infection of the blood, and consequent general and secondary local phenomena. With the other, the correlation is as follows: absorption through the lungs of putrid miasmata; infection of the blood; follicular enteritis, attended with ulceration, and sometimes with gangrene; formation of putrid matter; absorption of these; general typhoid symptoms. M. Andral appears to have been unwilling to grapple with the question; nevertheless, so far as he ventures to go, he repudiates the idea of the causation of the disease residing in the intestinal change. "Admitting," he says, "that simple follicular enteritis is the starting point of many fevers, can everything be explained by it?"—No, concludes M. Andral, for "it has appeared to him that these fevers do not become severe, unless as a result of disturbance in the functions of innervation and hæmatosis." (Clin. Méd. ed. 3, t. i. p. 4.) The opinions of M. Cruveilhier are marked by vacillation and uncertainty. "How is it possible," he enquires, "to refer the general symptoms to a lesion of such limited extent?" . . . Is it not more probable that they depend upon a cause acting upon the entire system?" Yet further on we find him stating as "his conviction, after mature reflection, that the fever in follicular enteritis is symptomatic in the same manner as that attending eruptive diseases or pneumonia." (Anat. Pathologique, 7 Livraison.) M. Chomel is far from considering the intestinal change the essence of the malady; on the contrary, he infers that "all the symptoms, with the exception perhaps of diarrhœa, abdominal pain, and gurgling in the right iliac fossa," [a symptom to which, we may mention, M. Chomel attaches too high diagnostic value,] "are the expression of the influence of the disease on the whole system,—of the disturbance of the chief functions; and they are the effect of the disease itself rather than of the lesion of the follicles."

In our former article we stated, in the words of M. Louis, the inference drawn by him upon the point in question in his second edition, identically the same as that originally drawn. In the second, however, he considers the possible fallacy of arguing to the petty importance of the lesion from the want of proportion between the extent of this and the severity of the attending symptoms. Erysipelas of the face, he reminds us, is accompanied with very intense fever, at a period when the affected portion of skin is no larger than a shilling; the fever does not increase with the spread of the inflammation, nay more, it actually disappears occasionally, while the exanthema spreads to the trunk and extremities. This argument would be excellent, were it certain that the erysipelatous inflammation is the original cause of the fever; but as the latter precedes the former it cannot be its effect. It is further urged that the febrile reaction attending pneumonia is as intense when a very small portion as when the greater part of the lung is implicated. It must not be forgotten also that, besides the patches, the mesenteric glands and spleen are invariably affected in typhoid fever.—M. Forget's book is written with the express object of proving that the intestinal change is the efficient cause of the whole train of symptoms. He "cannot," he confesses, "comprehend the profoundness of the expression anatomical character," . . . as signifying a distinction from pathological character, and assumes the constancy of the intestinal lesion. Nevertheless he is constrained to admit that in some cases that lesion is secondary, inasmuch as it appears to result from a primary infection of the blood in epidemic typhus. The contradiction of previously-stated opinions involved by this admission he seeks to evade by the hypothesis that in typhoid fever of miasmatic origin, the "primary infection of the blood is utterly insignificant in comparison with the follicular enteritis," which is precisely the point in debate: in struggling to escape from a contradiction, he stumbles into a petitio principii. It has been said that the intestinal lesion cannot be the cause of typhoid fever, because typhoid fever (or better the typhoid state) may exist without the lesion, and the latter exist without the former. M. Forget observes that this argument is equally applicable to the opinion of those who regard the lesion as the effect of the fever, and illustrates this by a parallel case: "pneumonia may exist without fever, and fever without pneumonia; therefore the fever of pneumonia does not depend upon that pneumonia." This is fair enough, but M. Forget's whole argument leaves the matter just where he found it.

It is admitted even by the most determined upholders of the intestinal origin of the malady that disproportion is frequently observed between the symptoms and the lesion. Some of the counter-arguments on this point have appeared in our former article; it may be well to bring them together here. 1. In every disease the febrile reaction varies with the idiosyncrasy of the individual. 2. When the change in the intestinal glands is trifling, and the symptoms severe, the secondary lesions are to be taken into consideration. 3. Where the converse has been observed, the intestinal lesion has in many instances been found to be in progress of cicatrization. The general justness of this argumentation is not to be denied, nevertheless it does not remove the impression that the want of correspondence referred to is much more frequent in the instance of the typhoid affection of Paris than of the acknowledged phlegmasiæ.

Setting aside for a moment the question of causation, one scarcely less important arises; does the intestinal lesion once developed constitute the most important feature, and measure the danger, of the malady;—is it the point to which the therapeutical efforts of the physician should be directed? First, it is impossible to question its extreme importance in the former respects, were it only from the fact that one of its results, perforation, is the immediate cause of death in somewhat more than 9 per cent. of fatal cases.* But exclusively of this epiphenomenon, the French pathologists even of the eclectic school, Andral for example, recognize its importance. According to M. Cruveilhier, it "appears to regulate the course and phases of the malady." Secondly, in it those who advocate exclusively antiphlogistic treatment see the whole disease; but any argument founded on the success of such treatment loses its force from the fact that the mortality of the typhoid affection of Paris appears to vary to a very great extent under the same treatment, whether this be purely expectant, actively antiphlogistic, moderately antiphlogistic, purgative or symptomatic, and that, as has been already mentioned, the expectant appears to be the most successful mode of managing the disease.

From the materials of which the reader is now in possession, and from our own clinical observation, we believe ourselves entitled to draw the inferences which follow respecting the Parisian malady. Our *profession of faith* is deficient in the assured and dogmatical tone which would satisfy the enthusiasm of M. Forget, and we shall, we doubt not, be herded by him with those perverted mortals who, as he opines, "if TRUTH were one day to rise from the bottom of her well, would hasten to plunge her again to the bottom, in order to have the pleasure of looking for her still;" but for truth's sake we would bear the brunt of even bitterer sarcasm than this: 1. The various forms of continued fever occurring in Paris are all essentially varieties of the same malady, passing into each other in such manner that their separation is not admissible even in speculative nosology. 2. This malady, the typhoid affection or fever, is attended in the vast majority of cases with a change in Peyer's glands; error of diagnosis has probably swelled the list of cases of its absence. 3. It cannot be considered to be satisfactorily established as the primary cause of the disease, because, in addition to the fact of its occasional absence, the discrepancy between it and the symptoms is of considerably greater extent and more frequent occurrence than is the case with any affection in which the anatomical change is the acknowledged cause of the functional derangements. Less importance is to be attached to the want of direct proof that the lesion appears from the very first, because even in cases of pneumonia, for example, febrile disturbance frequently exists before the development of any local symptom; herein is manifestly involved the whole doctrine of the relationship of local maladies and febrile

* This estimate is founded on the following facts:

| | Deaths. | Perforation. |
|---|---|---|
| Louis | 55 | 8 |
| Bretonneau | 80 | 8 |
| Chomel | 42 | 2 |
| Montault | 49 | 5 |
| Forget | 44 | 2 |
| | 270 | 25, or 9·22 per 100. |

action. 4. The intestinal lesion probably exerts direct secondary influence in the production of the typhoid phenomena through absorption either of putrescent matter from the patches themselves, or of the morbid secretions in the bowel. But though probable this notion is far from being free from possible objection, because: *a*, typhoid phenomena arise without the existence of intestinal ulceration; this, however, only proves that this is not the sole source of such phenomena: *b*, the formation of Peyerian ulcers in phthisis is not commonly attended with the evolution of typhoid symptoms; however it is stated, or at least surmised, that in some instances it is so attended, (by Forget, a questionable witness the reader will possibly imagine,) and the different mechanism of the ulceration in both cases may be the cause of the difference in the ordinary result. 5. The lesion of Peyer's glands is primarily specific,* secondarily inflammatory. 6. The general muco-intestinal surface may be free from anatomical change. 7. The malady is attended with a peculiar proneness to membranous ulceration. 8. A first attack preserves the individual from a second. 9. It has rarely been observed after the fiftieth year; the reason of this is not clearly made out. 10. It is developed spontaneously and by contagion; both under peculiar and imperfectly ascertained conditions: the latter we only admit with the proviso already stated.

The evidence proceeding from different parts of the continent is next to be considered. M. Louis himself has in his recently published work on yellow fever described some cases of typhoid affection marked by the same lesions of function and of structure as those characterizing the Parisian malady. The place in and the circumstances under which these cases were observed are important, for they show that the disease maintains its character at Gibraltar, and during the prevalence of an epidemic fever of still more destructive nature. The descriptions already referred to prove that the anatomical change follows identically the same course as at Paris, at Dresden (Carus), at Berlin (Böhm), at a village on the banks of the Danube (Buzorini), at Vienna (Rokitansky, Staberoh, &c.) From Böhm and Buzorini come the microscopical and chemical analysis of the exuded matter, whereby a precision is given to their statements, bearing in itself efficient testimony to their accuracy. But the German physicians recognize this lesion as the characteristic of one variety of continued fever only, that in which abdominal complication prevails, to which they accordingly give the title of typhus abdominalis; they do not regard it, when established, as the cause of the symptoms.

The epidemic fever which devastated Italy in 1816-17 was, as regards symptoms, to all appearance perfectly identical with that observed in this country. The symptoms are described with sufficient minuteness to justify this conclusion; some of them indeed, for instance the eruption, with very remarkable precision. Hear Palloni's description: "Fra il terzo e il settimo giorno comparise alla cute una *eruzione* di macchie rossastre, puntiggiate, irregolari, e leggiermente scabre e rilevate (che costituiscono la vera eruzione tifoide), fra le quali talvolta si vedono sparse delle petecchie." (*Febbre Tifoide che ha regnato in Italia negli anni* 1816-17, p. 150.) Here among other points the distinction between

* It has been observed that all the irritating agents in the world will not produce follicular enteritis; though common enteritis may be produced artificially with ease.

the eruption and petechiæ, overlooked by an Edinburgh Professor of the present day, is distinctly made. Unfortunately the cadaveric history of the epidemic is not detailed with similar accuracy; the abdominal cavity and its contents are stated to have been in some instances healthy; in others the "intestines and stomach were in some points more or less inflamed, empty, and contracted, or distended with mephitic gas, in other cases containing lumbrici." (*Storia della Malattia Febbr. Osserv. Nello Spedale Provisorio di S. Lucia, ann.* 1817, p. 119.)

The physicians of the United States have furnished some contributions on the subject of continued fever distinguished by considerable accuracy of research and narration. Among these observers Dr. Jackson holds a very prominent place. His experience is stated by himself in the following terms: "Since the work of M. Louis has been known to me, I have found that the continued fever, which is so well known to us, in this city at least (Boston), was the same as that which he has described. The symptoms are essentially the same, and the appearances discovered in the body after death are precisely the same. These appearances had been noticed here before, when the examination was so made as to disclose them. From 1833 our fever has been the same it formerly was, and *in every case*, where an examination has been made, the morbid changes have been found to be the same as described by M. Louis. In neighbouring places, a similar confirmation of the identity of the disease has been furnished from different sources. I may refer here particularly to cases which occurred in Lowell, and were reported by Dr. Bartlett, the learned professor of pathological anatomy in the Berkshire Medical Institute." (Report on Typhoid Fever, p. 8.—Boston, 1838.) The testimony of Dr. Jackson to the frequency of Peyerian lesion in America is the more valuable, as from another of his works we learn that "the discoveries made by Louis in respect to typhus were in contradiction to opinions which he had maintained and taught for many years."* Subsequent observations by Dr. Gerhard upon an epidemic continued fever at Philadelphia, observations to which we are about to refer more fully, led Dr. Jackson to believe that "there are, at least, two species of continued fever both in Europe and in this country; and further researches may very possibly show more." (Op. cit. p. 10.)

Dr. Gerhard returned to his own country prepared for the observation of its continued fever by a close scrutiny of that of Paris, and had already contributed a paper to the American journal, demonstrating the perfect identity of the two maladies in respect of symptoms and abdominal lesions (of the follicles, mesenteric glands, and spleen), when an epidemic continued fever came under his notice presenting characters to which he had been hitherto a stranger. His observations appear to have extended to 214 cases (120 men, 94 women), in "about 50" of which a post-mortem examination was instituted. The symptoms of this "typhus fever" appear to have accurately tallied with those usual in this country: the eruption wore the characters of that of Great Britain, there were neither diarrhœa,† abdominal pain, nor marked meteorism; and in one

* Memoir of James Jackson, jun. M.D., p. 78.

† At the close of the epidemic, when dysentery became prevalent in the district, diarrhœa occurred; but this was evidently a merely accidental epiphenomenon, and attended with softening, &c. of the mucous membrane of the *colon*.

case only was there lesion of Peyer's patches (and this of doubtful character) discovered, the mesenteric glands retained their healthy aspect, and the spleen was enlarged and softened in only half the cases. Dr. Gerhard believes this malady identical with the "typhus fever" of Great Britain:* we too believe it identical with our more ordinary form of continued fever. We are not acquainted with any series of post-mortem examinations instituted in the British Isles, wherein the proportion of follicular cases was so low as one in "about fifty." Dr. Gerhard's experience exhibits in this respect one extreme, while the other is supplied by the observations, to be presently more fully referred to, of Dr. Bright and Mr. Goodsir. Dr. Gerhard further believes the malady he describes, as well as the British "typhus fever," perfectly distinct from the continued fever of Paris on the grounds just pointed out, as well as the alleged fact of the extreme contagiousness of typhus, while "it is *very clearly proved* that the typhoid fever or dothinenteritis is not contagious." We have seen that M. Louis, after a careful investigation of the evidence respecting dothinenteritis, is persuaded that it *does* spread by contagion; and the unsteadiness of Dr. Gerhard's own creed appears from his saying elsewhere, "in some epidemics, we have strong reason to believe that dothinenteritis becomes contagious."

In approaching the branch of our subject which refers to the continued fever of this country, we feel considerable hesitation, because we shall have to examine with unusual severity the productions of estimable men, and because we feel diffident of our own powers to influence general opinion materially upon a question which admits of very strong argumentative support whichever side be espoused. Let not the reader charge us however with the pretension to *decide;* we offer the result of our enquiries, impartial as we believe them to have been, as a simple contribution to the settlement of the question.

We can scarcely be accused of defective patriotism, especially as the same is true of other countries, if we set out by stating that the accounts published respecting the typhoid fever of these islands were, until a very recent period at least, utterly devoid of scientific precision. It is manifest that their predominating characters were vagueness of description, boldness of assertion, and deficiency of anatomical details: their epigraph might have been, *Viel Irrthum und ein Fünkchen Wahrheit.* And this spark of truth disserved rather than promoted the cause of progress,

* "The typhus fever," observes this writer, "which is so common throughout the British dominions, especially in Ireland, is not attended with ulceration or other lesion of the glands of Peyer; I mean that this lesion when it occurs, is merely accidental or a *complication* not occurring in the ordinary course of the disease." Yet it is in the very same page affirmed, that "an *error* frequently committed by the British physicians is, that they regard this affection (dothinenteritis) as a mere *complication* of their ordinary typhus, or a modified form of it." (p. 292). So that what is error with us, becomes truth when announced by Dr. W. W. Gerhard. There is a want of precision in some, indeed we might say in all, the numerical details of this writer, at which we felt no little surprise. What accurate notion is, we would ask, derivable from such statements as these: "the mortality among the cases that were treated by us from the beginning was *not great;* but the total loss of patients admitted at an advanced period of the disease, many of whom were moribund, was very considerable, *about* one in three."

by giving collateral authority to the multitude of errors with which it was associated. From such portion of our literature, as is thus characterized, we can expect no available evidence on the topic under consideration, and are obliged to come to a very recent period in search—even here the search is commonly fruitless—of documents bearing upon them the stamp of close observation and correct description. The works published of late years may be classed under four heads; of essays in Cyclopædias, ex professo volumes, papers containing more or less accurate reports of cases, and essays recording the numerical analysis of symptoms and lesions in a certain number of cases. We cannot of course examine all of the very numerous productions referrible to these categories which have appeared; we must content ourselves with one or two specimens of each.

Dr. Christison's essay is the production of a determined essentialist and of one denying that the Peyerian lesion can be considered otherwise than as of unusual occurrence in the town where he practises. The latter is at least an inference to which the general tone of the essay leads, though, as is shown by the following passages, its author has not announced the opinion in such terms as to obviate the chance of its being deemed one espoused upon insufficient grounds. At page 139 we read that the intestinal lesion "is found in the Edinburgh Infirmary only often enough to make the pathologist acquainted with its phenomena and *keep him in mind of its existence;*" at page 121 it is said that "the appearances of local disorder are now well enough known to every scientific physician and are *frequently seen* by practical men;" while at page 153 we learn that "in the fever of Edinburgh serious bowel affections are in general *very uncommon.*" But the tendency to contradiction apparent here is trivial in comparison to that exhibited in another part of the essay: Dr. Christison devotes a section to the "*anatomical characters*" of typhoid fever, while his entire previous matter is designed to show that the malady has *no* anatomical character, that there is no constant attendant organic change. And whence derives he his anatomical descriptions; from his own experience of the disease as it exists at the Edinburgh Infirmary?—No, but from the volumes of M. Louis. In other words he describes a given disease by borrowing from the description of another, which other he maintains to be perfectly different therefrom. Dr. Christison cannot shield himself from the charge of self-contradiction by maintaining that the changes of which he borrows the description are secondary lesions of typhus; even to this defence he can hardly have recourse, for he tells us (p. 153) that "most of them [the anatomical changes described,] it is reasonable to conclude, are secondary not to fever in general, but to one of its secondary disorders—inflammation of the mucous glands of the intestines," which "inflammation," be it remembered, is declared to be a "very uncommon" attendant on the Edinburgh continued fever. For further illustration of this author's doctrine we shall here transcribe, with the comments they suggest, certain other passages of his essay:

"The views entertained of fever in the abstract have not been rendered clearer since the time of Cullen." (p. 114.)

We are not to understand from this that Dr. Christison teaches us to

believe in, inter alia hujusmodi, the spasm of the extreme vessels; but to the division of continued fever into three species he reverentially adheres."*

"M. Louis has been led to the conclusion that the typhoid form at least of fever is always owing to inflammation of the glands of the intestinal mucous membrane." (p. 120.)

Here is a treble inadvertence. The first line of M. Louis' *avertissement* states, what the whole work proves, that the term typhoid fever or affection embraces all the species of continued fever of nosologists. Secondly, far from asserting that typhoid fever is *always* owing to inflammation of the intestinal glands, he does not in any instance maintain such causation. Thirdly, in applying the unqualified term inflammation to the state described by Louis, his opinions are misrepresented; the followers of Broussais are at this moment quarrelling with him for proving the malady to be of *specific* nature.

"It may in particular be doubted whether the disorder described by Bretonneau and Louis is not a local disorder which simulates fever." (Ib.)

The meaning of a local physical change simulating a group of general functional disturbances is not very clear; but inasmuch as this "local disorder" is said to correspond to "fever" in France, it follows that there is no such thing known in that country as "essential fever." Rather a curious inference to flow from the writings of a professed "essentialist."

"Physicians at Paris have plainly formed their conclusions regarding the nature of fever in the abstract from observing the characteristics of a single form *rarely presented in the epidemic shape.*" (p. 122.)

The description of Roederer and Wagler was drawn up from an epidemic at Göttingen; Cruveilhier observed the disease epidemic at Paris in 1814-15, and at Limoges in 1816-17; Gendrin's descriptions are altogether founded on epidemics of the disease; so are those of Putegnat; Pellerin described the intestinal lesions from an epidemic which reigned at the Salpétrière in 1814, at a time when those lesions had not been generally recognized; Ruef described an epidemic of it at Bischofsheim in 1832; Mistler at Andlau in 1833; Lombard has frequently known it epidemic at Geneva; it occasionally becomes epidemic at Paris (as in 1835), &c. &c. Be it observed that, except *probably* in some cases occurring at Paris in 1814, the iliac lesions invariably existed; nor has there been a single epidemic in the provinces of late years wherein these were not discovered.

At p. 122, note, the author states his impression that "M. Louis' opinions have undergone some change since the publication of his last writings on the subject of fever," because he admitted lately to Dr. Christison that a continued fever, differing from the French typhoid, prevails in England."

We really can scarcely understand the existence of *change;* for in the "writings" referred to, M. Louis made no shadow of allusion to the typhoid fever of any country but his own.

* Well does Dr. Jackson observe (Opusc. cit. p. 10): "First, such distinctions between continued fevers, as those of synochus, synocha, and typhus were not shown to exist in nature and were in truth grounded on men's fancies; and, secondly, those names were originally significant, not indeed of different qualities in nature, but of men's notions in regard to the different natures of diseases."

Among the chief distinctions between idiopathic local inflammations and inflammation attending primary inflammatory fever, is the alleged circumstance that in the latter "the inflammation arises consecutively to the fever." (p. 129.)

This is precisely the point which Dr. Christison was called upon to prove, for up to the present day we know not that such demonstration has ever been given. If Dr. Christison had said "*appears* to arise," he would have spoken in conformity with observation. But had he done this we should have told him that the same is true of every one of the acknowledged phlegmasiæ,—that there is not one of these the development of which, as cognizable by ascertained local symptoms and physical signs, may not be preceded by a febrile movement of more or less duration. Whether an anatomical change have in reality developed itself, though to so limited an extent as to produce no appreciable symptoms, is a problem in the present state of knowledge insoluble.

"Hence of 690 cases crisis took place in 470 on critical days, in 52 on the subsidiary critical days, and in *only* 108 on the days which are considered non-critical." (p. 133.)

Let us enquire whether these figures supply satisfactory reasons for assenting with Dr. Christison to the notion of "critical days." If we found that these alleged critical days were two or three in number out of twenty, the evidence would be imposing, nay conclusive; but what is the fact? Why that these critical days are no less than ten out of twenty; and that hence the chances of favorable termination are equal on the so-called critical and non-critical days, independently of any mysterious influence exercised by septenaries and multiples and submultiples of the same. Again; we find 34 cases noted as terminating on the sixth day; 129 on the seventh; and 26 on the eighth: now let the reader consider the difficulty of fixing within twelve hours the time of origin of such a malady as fever, (which not unfrequently runs a latent course for a variable period;) next let him fancy the invincible tendency in any mind, duly impressed with the mystic importance of the seventh day, to consider patients a little worse on the sixth or a little better on the seventh than common observation might adjudge, and then let him answer us whether it is not probable that day the seventh has been given a Benjamin's portion of the recoveries, to the detriment of the sixth and eighth especially, but possibly of every one of the subsequent days.

"The third variety of petechiæ presents more or less numerous spots," &c. &c. (p. 141.) Here follows a description of the rose-coloured spots of typhus.

These spots we need hardly remind the reader are not petechiæ at all: to confound a cutaneous efflorescence with a cutaneous and subcutaneous hemorrhage exhibits rather lax notions of anatomy.

"By many French pathologists one variety, the diffuse pale *petechia* (typhoid eruption), is thought peculiar to dothinenteritis." (p. 142.)

We know not who these pathologists may be: Andral, Cruveilhier, Chomel, Barth, Piorry, Bouillaud, Rostan, &c. admit that similar spots occur in other diseases, though much more rarely. Chomel even goes so far as to affirm that the number of papulæ must be considerable to give them any value in the diagnosis of typoid fever.

Having described miliaria as consisting of "*spots*.....*filled* with a clear fluid," Dr. Christison adds, "sometimes they seem connected with sweating,

break out immediately after a diaphoretic *crisis*, and then constitute what authors term *sudamina*." (p. 143.)

This is not perfectly correct; Louis and Chomel, who have the most carefully described the epidermal vesicles under the title sudamina, are precisely those who deny their connexion with perspiration; whether they do this correctly or not is another matter.

Before taking leave of Dr. Christison's essay on fever, we cannot avoid expressing our very sincere hope that he will regard the close criticism to which we have ventured to subject some of his opinions and statements as evidence of the importance we attach to any production of his. His acknowledged talents and eminent position render it most important for the cause we advocate that his doctrine should be proved to be not unopen to objection.

Dr. Hodgkin commences his chapter on fever by observing, that "the frequent occurrence of inflammation and ulceration in the inferior portion of the ileum in cases of fever is confirmed by the testimony of all the best and most accurate pathologists, who of late years have investigated the morbid appearances connected with the febrile state . . . . . It is the glandular apparatus, and more particularly the aggregate glands which are the principal seat of inflammation." (p. 459.) In the following page Dr. Hodgkin speaks of Louis as connecting their inflammation "with fever, or at least a *particular form* of fever, to which he gives the name of affection typhoïde." The error implied by this sentence has been already pointed out.—At page 463 we find the phrase "Louis, who considers *typhoid symptoms* as pathognomonic of inflammation of the aggregate glands." It is not easy to see how Dr. Hodgkin could have conceived M. Louis to maintain anything so utterly at variance with all experience: Louis is perfectly aware, as his volumes show, that typhoid symptoms (l'état typhoïde) may supervene upon almost every affection to which the system is liable, that pneumonia, that inflammatory affections of the urinary passages, of the renal textures, of the veins after external injuries, amputation or otherwise, &c. may be thus attended.—"If we admit the facts which I believe Professor Louis to have drawn up with conscientious accuracy, I do not see how it is possible to evade the conclusions at which he has arrived. . . . . It appears to me to be altogether presumable that the inculcation of the doctrine, that in fever the glands of Peyer at the termination of the ileum are always in a state of [special] inflammation; and that, on the other hand, these glands are never affected but in cases of fever, may lead many to adopt the conclusion, that the inflammation of the aggregate glands in question really constitutes the essence of fever," (p. 464.) Are we to infer that Dr. Hodgkin would suppress what he believes to be a truth, lest mistaken persons should draw false or questionable inferences from it? But both passages are important: the first for an obvious reason; the second as showing that Dr. Hodgkin at least acquits M. Louis of affirming the intestinal change to be the cause of the malady.—"The name *gastro-enteritis*, which has been introduced as synonymous with, but more correct than, fever, and which Louis himself has adopted, at least in the title of his work, to convey the same idea." (p. 465.) There is here misappre-

hension: on the title-page of M. Louis' book (1st edition) we find the words, "Researches upon the disease *known under the names* of gastro-enteritis, putrid, adynamic, ataxic, typhoid fever," &c. &c.; the author meant simply to show by this title, that his researches referred to a malady well known to his countrymen by the recently devised title of gastro-enteritis, and nothing more. Had the phrase stood, "Researches upon gastro-enteritis," the matter would have been very different; the truth is, that M. Louis never uses the term except when engaged in particularly demonstrating its total inapplicability to the affection he describes.—"Does it not seem reasonable and consistent with analogy," enquires our author somewhat further on, "to presume that . . . on the one hand there may be symptoms of general disturbance of the system prior to, and often disproportioned to, the inflammation of the elliptical patches; and, on the other hand, that these patches may be deranged, with comparatively little general disturbance?" (p. 466.) Why, no one questions this; the very cases of M. Louis prove it, and he himself believes (which destroys the force of Dr. Hodgkin's objection still more completely) that even in the instance of the phlegmasiæ "there is general disturbance prior to the inflammation." Can anything be more strange than the habit indulged in by some writers of attempting to establish doctrines upon analogical and argumentative grounds, when their soundness on the contrary might be immediately and satisfactorily settled by an appeal to direct observation?—At p. 477 we find Dr. Hodgkin referring to the "striking accordance between the results of Louis and those of other investigators, who, with ample opportunities, have given minute attention to the subject;" among others Dr. Bright. In Dr. Hodgkin's own experience, "the derangement of the aggregate glands has certainly been detected in most of the fatal cases of fever which he has examined," (p. 481.) "He is persuaded," however, that in some cases "presenting the characteristic symptoms of fever during life, he has found the aggregate glands, so far from being severely deranged, barely discernible, whilst the cerebral or thoracic derangements have been very considerable." (Ib.) Dr. Hodgkin has seen ulceration of Peyer's glands in cholera, and "in the body of a little girl who had died, not from scarlet fever, nor of the *ordinary symptoms* of continued fever, but after a much more lingering malady [what malady?] terminating in an acute attack, in which profuse hemorrhage from the bowels was the most remarkable symptom." (p. 520.) Where is the *proof* that this was not actually a case of continued fever? This author indulges in much speculation respecting the alleged verisimilitude of the symptoms attending "Cynanche maligna," and "inflammation of the patches of Peyer;" the only importance of this speculation arises from its involving the notion that the Peyerian lesion, when it exists in typhoid cases, is the cause of the fever, a notion which a few pages back appeared so noxious to Dr. Hodgkin, that he would have suppressed a truth in order to exclude the chance of its being entertained. By the foregoing quotations, the ideas derived by Dr. Hodgkin from experience in respect of the relation of continued fever and Peyerian lesion are made apparent. He would perhaps desire us to add, that he "imagines fever to depend on the suspension, or at least very considerable interruption of that process by which, during health,

the various parts of the system are continually undergoing a change." (p. 490.) Others might imagine this suspension or interruption the effect, instead of being the cause of fever.

A systematic work by Dr. Roupell (A short Treatise on Typhus, Lond. 1839,) deserves reference in this place (we noticed the volume on its publication, vol. VIII. p. 428,) from three circumstances only: the frequency with which the author observed the papular typhoid eruption,* the fact that his anatomical details are taken from Louis and Chomel, and his reference of the typhoid symptoms to intestinal phlebitis.

Dr. Bright, incomparably the most accurate observer of the anatomical changes of the continued fever of London, writes thus: "Whatever may be the primary condition of the febrile attack, there can be no doubt that early in the disease, *not only in the season of which I have spoken* (autumn of 1826), *but almost always*, the intestinal canal is irritated, and that this irritation keeps up all the bad symptoms, becomes the *chief object* to which the practitioner should turn his attention, and is not unfrequently at last the immediate cause of death." (Reports, vol. i. p. 178, London.) To the early manifestation of intestinal disease he bears witness in the following passage: "In some cases the symptoms of irritation in the bowels have followed very quickly after the first indications of fever, almost always before patients have been admitted into the hospital, which is seldom within the first week, and frequently not within the first fortnight after the attack." (p. 179.) Ten cases are given, in every one of which the special lesion of the patches, as also of the mesenteric glands, when these are mentioned, displayed itself under the precise aspects observed at Paris; the phenomena of distension with yellow cheesy matter, sloughing, ulceration, perforation and cicatrization, were all undistinguishable in character from the corresponding changes described by Louis.

The most valuable, indeed almost the only English work belonging to the third class is the essay of Drs. Henderson and Reid, in the Edinburgh Journal. The description of symptoms by the former gentleman is founded on nearly 200 cases which came under his particular care between October 1838 and June 1839, a period at which the malady prevailed epidemically. Dr. Henderson "has had recourse, as far as the records enable him, to a numerical analysis of the most important symptoms." Among the introductory remarks, we find it observed that "the difficulty of forming a satisfactory diagnosis in many instances, in which the disease terminated within six or eight days," was distinctly felt;—an excellent comment on Dr. Christison's readiness in accepting the "critical day" cases. "Convalescence was gradual, sudden improvement and speedy recovery, in connexion with abundant secretion from the skin, bowels, or kidneys being extremely rare;" so much for so-called "crises." We have changed in the following brief abstract, the order in which the symptoms are described by Dr. Henderson, for the purpose of facilitating comparison with M. Louis' results, as already given; the comparison can-

* Dr. Roupell appears to believe that in terming typhus an exanthematous disease he has effected a novelty: "Il tifo," says Palloni, "così detto petechiale, quale oggi regna fra noi, è *una febbre esantematica*." (Op. cit. p. 149.)

not, however, be as close as desirable, more especially because Dr. Henderson has commonly failed to note the number of cases in which each symptom was investigated.

Distinctly spontaneous diarrhœa appears to have occurred in 8 only of 149 cases, at a variable period of the disease, not exceeding six or eight stools per diem. Among 104 females 31 had costive bowels at the time of admission, 73 had them in an easy state, 28 of whom had taken purgative medicine; of 45 men, the bowels were costive in 19, open from medicine in 15, and moderately so without medicine in 11. Forty-six suffered from abdominal pain, 21 in the epigastrium, 19 over the whole extent, 2 in the left iliac region, none in the right, 3 in the left hypochondrium, 1 to the right of the umbilicus. Eight patients had slight meteorism, attended with general abdominal tenderness, and in one case with diarrhœa. Twelve had nausea and vomiting, chiefly at the beginning; in 5 they were accompanied with abdominal pain and tenderness. 104 of 108 females had cephalalgia, 92 of them at the outset; 49 of 51 males had that symptom; its mean duration in 29 cases was ten days; in a few cases it prevailed during the entire course of the complaint. Twenty-five females, or less than one fifth, had delirium, the mean period of origin being the eleventh day, in 3 cases, before the ninth; 23 of 66 males, or more than one third, suffered from it also, the tenth day being the average day of its appearance. Thirteen females and 3 males only are noted as having had somnolence; it appeared later than delirium. Fifteen females and one male had deafness; spasmodic symptoms of various kinds were noticed. An exact account was kept of the state of the tongue in 100 female cases. "It very early became covered with an increased and altered secretion, . . . . in 73 cases the tongue was dry at one period of the disease, in 27 it continued moist throughout, and in several of these was at the same time almost quite clean, in cases too which were not always mild, for one with the tongue in this clean and moist state died on the 13th day."

From an extremely minute and elaborate description of the typhoid eruption we extract the following particulars: of 130 cases of both sexes in which this was sought, 108 presented it; in 22 it was not found, but 6 of these were not admitted till between twelve days and three weeks from the outset, and may have had it before admission. The greater number of the 16 non-eruptive cases were slight, 1 only severe. In several of the other subjects petechiæ were detected. Four forms of the eruption, which it is unnecessary here to refer to particularly, are described. The *colour* appears to have varied with that of the skin, from the dusky red of dark persons to the light pink of the very fair. The upper part of the chest, the submammary regions and sides of the abdomen were the most favorable situations for observing it, but it occurred also on the extremities. Its first appearance was witnessed in 12 cases, from the third to the thirteenth days. Its development after the eighth day must be extremely rare, for though many patients were admitted after the first week the formation of the efflorescence was only observed three times after that day. Its development usually took place by successive daily additions; from two to four days appears to be its ordinary duration in the complete state, but a duration of five or six was in some cases observed; four or five were occupied in the fading, the whole duration commonly equalling nine or ten days. Its abundance is of

unfavorable augury—a fact ascertained by comparing the number of deaths occurring in cases where it had been abundant, or the contrary, as well as the mean duration in cases of recovery; thus:

| | Eruption copious. | Scanty. | | Copious. | Scanty. |
|---|---|---|---|---|---|
| Cases . . . . | 65 | 25 | | 55 | 18 |
| Deaths . . . | 13 | 3 | Duration ... | 13⅔ days. | 11½ days. |

Among appearances produced by ecchymosis, purple *petechiæ* were frequently present in the second week; a single *vibex* in one case only. *Sudamina* were observed in three instances only, on the tenth, eleventh and thirteenth days. Twenty-three persons had thoracic symptoms; of 25 men, 5 had cough or slight pain in the chest, 18 bronchitis, with cough, expectoration, and the usual catarrhal ronchi; of 48 women, 15 had slight cough or pain on deep inspiration, 33 bronchitis. No example of pneumonia occurred. The *ages* of 161 subjects were as follows:

| | Males. | Females. | | Males. | Females. |
|---|---|---|---|---|---|
| 60 | — | 1 | 20 and below 25 | 1 | 13 |
| 50 and below 60 | 3 | 3 | 10 „ 20 | 15 | 41 |
| 40 „ 50 | 5 | 10 | 5 „ 10 | 5 | 10 |
| 35 „ 40 | 1 | 7 | 4 „ — | — | 1 |
| 25 „ 35 | 12 | 33 | | | |

As an approximation to the mortality, Dr. Henderson gives 1 in 11½, for females, 1 in 7⅓ for males, or 1 in 9½ for both sexes: at least one fourth of the subjects denied exposure to contagion, and ascribed their illness to cold, while about as many could not refer it either to contagion or cold, so that in half the cases no plea for assigning contagion as the cause could be obtained. Nevertheless, "whether any examples of fever actually occurred without there having been previous intercourse, Dr. Henderson had no means of ascertaining,"—a phrase announcing the determined contagionist.

A description by Dr. Reid of the morbid changes discovered in 47 subjects cut off by fever, follows Dr. Henderson's contribution. The value of these details is diminished by their not referring to individuals observed by Dr. Reid during life; but in respect of our present enquiry this is a point of very secondary importance. Dr. Reid refers to the symptoms noted in the register by the physicians or the pupils.

Of 43 subjects whose *encephalon* was examined, 25 presented an abnormal quantity of serum within the cranium; the vessels of the brain were in all the cases except one well filled with blood, and in many sections of its substance appeared studded with red points; in one only was there a slight degree of softening; it was universal in this instance and occurred in hot weather. Dr. Reid enters into precise details respecting the character of the cerebral effusion and its relation to symptoms; and it appears from them that no symptoms can be distinctly ascribed to such effusion. The *lungs* were examined in 43 cases with the following results:

| | |
|---|---|
| Scarcely engorged even at the depending parts | 9 |
| Engorged at depending parts | 4 |
| „ with frothy œdema | 6 |
| „ with tubercles | 2 |
| „ with old pneumonia | 1 |
| „ with mucous effusion in the bronchi | 9 |
| Congested at posterior and middle parts | 10 |
| Recent pneumonia | 1 |

Dr. Reid was extremely careful in admitting pneumonia among the lesions of typhus, being well acquainted with the kind of error of which that inflammation occasionally becomes the subject. "When fever is prevalent," he observes, "cases of latent pneumonia in an advanced stage and attended by typhoid symptoms are very readily mistaken for continued fever, unless the attention of the physician be particularly directed to the chest. I examined the bodies of three patients within a short time, who had been sent into the infirmary as cases of continued fever, but, from the appearances observed on dissection, there could be little doubt that they were cases of latent pneumonia which had gone on to the third stage, with very little of the usual pectoral symptoms."

The abdominal organs were examined in 41 cases. The state of Peyer's glands in these cases is exhibited below.

| | | | |
|---|---|---|---|
| Apparent and distinctly defined in 24, of which | Grayish and dotted with dark spots | 22 | Not so 2 |
| | Distinctly elevated | 4 | Not so 18 |
| | Very distinctly elevated with traces of ulceration | 2 | No traces of ulceration 20 |

Indistinctly defined and scarcely visible . . . . 6
Not distinctly to be recognized with the naked eye . . . 11

The solitary glands at the lower part of the ileum were distinctly visible in four cases only. The mesenteric glands were enlarged in two cases, in one softened; some of these enlarged glands were one inch and a sixth in length, the same in breadth, and half an inch thick. The grayish discoloration with black dots (likened to a *barbe noire récemment rasée* by some French writers) has been met with by Dr. Reid in various other diseases though not with equal frequency. In the cases in which the patches were visible the average duration of the disease was nearly fourteen days, in those in which they were invisible nearly twelve.

In 101 cases of continued fever, examined by Dr. Home, the patches of Peyer are described as being well defined or enlarged in 29; in 7 of these 29 there was ulceration; in 2 of these 7 perforation. It is extremely remarkable that Mr. Goodsir, residing at Anstruther, only thirty miles from Edinburgh, who has in the course of five years examined the bodies of 10 persons dying of fever in his neighbourhood, found the elliptical patches of Peyer and the solitary glands thickened and ulcerated in every instance, and one of the latter perforated in four cases. The greater number of fatal cases occurred in individuals of from 13 to 20 years of age. The series of changes in the patches is very closely described by Mr. Goodsir.

The only other abnormal appearances in the intestines of the 41 subjects were, in 1 a few spots of ecchymosis in the jejunum and upper part ofthe ileum; in 1 a morbid gray elevation adhering to the mucous membrane; in 1 some old cicatrices at the lower part of the ileum with circumscribed red spots; in 1 small depressions, with surrounding redness, of the cæcum and colon; in 1 similar depressions without redness in the cæcum and ascending colon; in 1, numerous small red patches of transverse and ascending colon, with slight sponginess and elevation of the parts reddened. Of 6 patients dying from other diseases, during convalescence from fever, 4 were examined: they had died on the 31st, 27th, 20th, and 33d days.

In the 3 first there were no cicatrices at the lower part of the ileum, in the other the patches were uniformly dark red through the entire tract of the ileum, distinctly defined and elevated, irregular on the surface, and at one or two points presented traces of incipient ulceration. In 9 cases only of the 24, in which the patches were distinctly visible, were there any abdominal symptoms during life, and in 3 of those 9 the symptoms could not be referred to the Peyerian lesion. The condition of the stomach in the 10 cases out of 41 in which alone that viscus presented any deviation from the normal state was as follows;

| | |
|---|---|
| Mucous membrane thickened and mammillated at pyloric and middle parts | 3 |
| Slightly mammillated near pylorus, softened at splenic end | 1 |
| Splenic end corroded and largely perforated | 1 |
| Universally thickened, indurated, and mammillated | 1 |
| Presenting numerous rounded superficial depressions, with defined margins, without thickening or redness | 1 |
| Red and friable, and covered with a layer of mucus towards the pylorus; presenting also several small depressions with bright red margins | 1 |
| Splenic end acted on considerably by gastric juice | 2 |

The *spleen* was of all the organs in the body that most frequently found in an abnormal state, being generally larger than usual, soft, and in some cases almost diffluent. Six subjects had Bright's disease of the *kidneys;* the *liver* was the seat of old disease in five, as was the *heart* in three of forty-three cases. In one case there was recent inflammation of the *larynx.* In all the cases the blood appeared to be fluid, or nearly so, in the large veins; but in several, small soft coagula were found in the right cavities of the heart.

From Dr. Anderson's numerical analysis of cases of continued fever, observed by him at the Glasgow Royal Infirmary, we extract the following table of the lesions of the gastro-intestinal mucous membrane in seventy-four subjects:

| | Soft. | Follicular. | Peyer's Glands. | Solitary Glands. | Brunner's Glands. | Ulcers. |
|---|---|---|---|---|---|---|
| Stomach | 57 | 21 | — | — | — | — |
| Duodenum | 18 | — | 1 | 2 | 4 | — |
| Jejunum | 38 | — | 62 | 4 | — | — |
| Ileum | 45 | — | 68 | 26 | — | — |
| Cæcum | 45 | — | — | 11 | — | 5 |
| Colon | 38 | — | — | 27 | — | 4 |
| Rectum | 21 | — | — | 10 | — | 1 |

Some foreign physicians visiting this country have formed and recorded opinions respecting the character of its typhoid or continued fever. M. Lombard, struck with the almost identity of the symptoms observed at Dublin and at Geneva, while follicular lesion was absent in two subjects examined by him in the former town, in none dying in the latter, was induced to infer (in a first paper wherein he cannot allow our typhus and his typhoid fever to be "specifically distinct") "that typhus fever is more a general disease affecting the whole constitution than a malady depending on any local inflammation or any local change of structure," and that various circumstances "serve to impress upon this general disease a tendency to associate itself with and produce various local ailments." (Dub.

Journ. of Med. Science, vol. x., p. 23.) In a second essay at settling the pathology of fever, "a change comes o'er the spirit of his dream,"—typhus is perfectly distinct from typhoid fever, it is an Irish disease, being disseminated through Great Britain by Irish emigrant labourers; and further, it is identical with the gaol and camp typhus of French writers. This total change of opinion in the short space of less than a little month, is particularly instructive. The last version involves the idea of dissimilarity between typhoid fever and camp typhus, a notion repudiated, as we have seen, by Louis. That the Irish peasantry can be justly charged with being the originators and propagators of the continued fever of Great Britain is utterly disproved by the statistical enquiries of Dr. Cowan* and the logical inferences of Dr. Staberoh.† "Expliquez moi, mon cher," said a French to an English diplomatist, "expliquez moi, je vous prie, votre système financier; mais dépêchez vous—*je n'ai que cinq minutes.*" M. Lombard's five minutes were excellently well spent.

Dr. Julius Staberoh (Dub. Med. Journ. vol. xiii. p. 426,) observed a considerable number of patients in Scotland and Ireland, and discovered in a certain number of instances, (the proportion varied at different times,) intestinal disease with the precise characters, from exudation into the submucous membrane of the patches to perforation of the gut, attending the typhoid fever of Paris. Dr. Staberoh insists most strongly upon the secondary and asthenic character of the inflammation of the mucous tunic of the patches, and its dependence upon the sub-mucous exudation. He regards the disease when attended with Peyerian lesion as specifically different from "typhus" when not so attended: "scarcely any of the medical men of Liverpool and Manchester," he observes, "were disposed to adopt that specific difference between typhus fever and gastro-enteritis or dothinenteritis," [just as if the two latter stood in the same relation to typhoid fever,] "which, *I am very sorry to say*, will perhaps still a long time prevent the medical men who have not the opportunity of judging both forms, when they prevail epidemically and endemically, from taking a general view of continued fever." It would be impolite and ungrateful not to acknowledge the kind commiseration evidently felt by Dr. Julius Staberoh for the benighted condition of such among our countrymen as are blind to the "specific difference," which he himself is fortunate enough to see so clearly: we wish him in return, as that which it appears to us would benefit him most on the present occasion, the least possible share of that mental amaurosis he pities us for possessing.

M. Louis, in the section of his work to which we promised to return, expresses his opinion that "observation has proved, that if examples be

* Vital Statistics of Glasgow.

† The author of a "Twelvemonth's Campaign with the British Legion," in describing the fatal effects of the epidemic typhus, which attacked that body in January, 1836, furnishes abundant evidence against the notion that the constitution of the Irish is adapted in any peculiar degree for the reception and propagation of typhus. He states that "the English and Scotch suffered extremely, while the Irish Brigade, composed of the 7th, 9th, and 10th regiments, enjoyed a perfect immunity;" and adds, "had the whole been composed of Irish, instead of losing nearly 1000 men at Vittoria, we might not have lost 100. In spite of all their hardships, the severity of the weather, the badness of rations, and total want of pay, the Irish lived, throve, and grew fat, as if in the midst of clover." (See Geary's Report on Typhus, Dub. Med. Journ., July, 1837.)

daily seen in England of an acute disease apparently similar to the typhoid affection, wherein nevertheless the patches of Peyer are healthy, the reason is, that two diseases exist in England, the symptomatology of which, though presenting many points of resemblance, is far from identical; that one of these is constantly attended with the special lesion of the glands of the ileum, whereas that lesion never exists in the other." The cases upon which this opinion is more particularly founded are thirteen in number, and were collected in the metropolis by Dr. Shattuck. Seven of these cases relate to patients, of whom the two oldest were 41 years of age, the others from 20 to 24. These patients had precisely the symptoms of the typhoid affection of Paris, diarrhœa, abdominal pain, meteorism, and a rare pale pink lenticular eruption; one of them died and presented the characteristic lesions of the glandular patches, of the mesenteric glands, and of the spleen. The other group of subjects were aged from 28 to 58, had a certain degree of stupor, febrile reaction, a pulse without marked development, occasionally delirium, and considerable weakness, but they had neither abdominal pain nor diarrhœa, unless after the exhibition of purgatives, rarely meteorism, which was trifling when it existed, and instead of the Parisian form of eruption, one characterized by its abundance, its universality, its incomplete disappearance under pressure, its reddish-brown colour, non-prominence, and persistence after death, absolute effusion of blood having occurred into the cutis and subcutaneous cellular membrane. Four of these subjects died: the glands of Peyer, as well as those of the mesentery, were perfectly healthy; the spleen enlarged and softened in one case only. Symptoms justifying localization of the malady were here evidently wanting, and no abdominal lesion existed: the mean age, also, of the two classes of subjects was different. The inference deduced from these cases by M. Louis is, he considers, confirmed by the relation of Dr. Gerhard already referred to; and from the whole he further concludes: "Not only has *typhus fever* not its seat in the digestive organs, as the typhoid affection, but it is impossible to assign it any seat; in the existing state of knowledge it has no anatomical character; and we are thus led naturally to affirm with M. Valleix, that *typhus fever* might be considered as an essential fever."

The essay of Dr. Stewart was undertaken with the view of "developing *some* of the leading features of difference," which in his estimation prove that "the two varieties of fever" (the continued fever of Paris and of this country) are "totally different diseases." He successively considers, "1, the probable origin of the two affections; 2, their proximate causes; 3, their course; 4, *some* of their symptoms; 5, *some* of their anatomical characters; 6, their treatment." Premising that the word some, for the italics of which we are answerable, means every symptom and lesion which Dr. Stewart has been able to discover corroborative of his views, we proceed to accompany him in his parallel.

I. Dr. Stewart points out the mass of evidence ordinarily adduced to show, that the accumulation of typhus patients within a small space causes the production of the disease in those brought within the sphere of such accumulation, and observes that circumstances have occurred wherein the *generation* of typhus can only be ascribed to the action of the animal effluvia thrown off from the bodies of individuals unaffected

with fever, but cooped up together within a narrow space, and otherwise diseased, &c. Dr. Stewart next denies that a similar mode of origin of the continued fever of Paris can be supposed to prevail. In respect of concentrated *febrile* effluvium spreading the disease, we beg to enquire of Dr. Stewart where he has found the facts warranting his assertion; the French possess neither "fever hospitals" nor "fever wards," and without the aid of these, how can the necessary comparison be established? In regard to concentrated *non-febrile* human effluvium *generating* typhus and not generating dothinenteritis, Dr. Stewart's inference is, to say the very least, unwarranted by the facts before the world. We might tell him that the very cases of typhus alluded to by him as thus putatively generated (and we are by no means disposed to deny the probability of the fact), are declared by Louis to be identical, anatomically and symptomatically, with the dothinenteritis of Paris: but it is both more fair and more correct to state that there is no shadow of *proof* that true follicular lesion did not exist in numbers of the victims of camp typhus referred to.

II. Dr. Stewart next examines the question of "transmission or propagation from a diseased to a healthy person." The reality of the contagious nature of typhus has, he correctly observes, been generally admitted; but he is in error in questioning the admission of similar, though less marked, contagiousness in the case of dothinenteritis. We have seen that it is now recognized by such French pathologists as have made that malady a subject of serious study; and a certain prejudice, which prevented due investigation of the question, is the only reason of its not having been earlier recognized. Dr. Jackson has seen "four persons employed in the hospital attacked with typhoid fever" (dothinenteritis), within a period of ten days. (Report, p. 34.)

III. "The fact [?] that the mean duration of typhus is about one half that of typhoid fever, is one that perplexes considerably the advocates of their identity." Dr. Stewart states the mean duration of typhus as about 21 days; the inference is that that of dothinenteritis is about 42. That this distinction is far from being universally prevalent appears from the facts very fairly stated by Dr. Stewart himself: in the cases observed by Dr. Shattuck, the mean duration of those with Peyerian lesion was 22½ days, of those without it 24½: the mean duration of 25 Parisian cases of dothinenteritis in 1839-40 (the disease however was less severe than usual,) was no more than 19·6 days. Both these series of cases are far from extensive, however, and therefore let us pass over the evidence they supply. This objection does not apply to 255 cases of recovery from dothinenteritis observed most carefully by Dr. Jackson, whereof the mean duration was precisely 22·019 days. "In the *few instances* where there was a *relapse* into the typhoid fever after a temporary convalescence," this observer "noted the second convalescence as the true one." The words "few instances" are put in italics, because Dr. Stewart finds a new or rather subsidiary motive for distinction in "the frequency of relapse" in dothinenteritis and its rarity in typhus. A passage he quotes from Louis is not capable of the interpretation he would attach to it, and from our own experience of the Parisian fever we can affirm, in opposition to Montault, that *true* relapse is exceedingly uncommon. The cases adduced by Dr. Stewart no doubt prove the fact, but not the frequency of the

fact, of a repetition of development of follicular lesion. We might also refer to Dr. Reid's result in respect of duration, which has been already transcribed. But suppose Dr. Stewart had proved to demonstration that continued fever with Peyerian lesion runs a mean course double as long as that of continued fever without such lesion, he would simply have shown the influence of a certain anatomical change in protracting the duration of the malady.

IV. The symptoms referred to by Dr. Stewart are those appertaining to the abdominal viscera and the skin. The reality of marked difference is not denied by any one who has examined a number of patients affected with the two diseases.

V. With reference to the anatomical lesions of the diseases, Dr. Stewart produces no new facts; in respect of the relation of diarrhœa, abdominal pain, and tympanitis to lesion of the patches, he contributes tabular expositions of his own experience confirmatory of results previously obtained, within certain limits, by Dr. Reid. Upon these tables he observes: "What chiefly concerns us to notice is the striking fact that as the number of enlarged follicles increases, the cases of spontaneous diarrhœa diminish, and those in whom consecutive diarrhœa and costiveness were observed become more numerous. In fact, not one of those in whom the greatest number of enlarged follicles was observed but was either constipated, or had diarrhœa brought on by medicine during life. This confirms our former deductions, agrees entirely with those of Valleix, and shows that the appearances observed in typhus depend upon local irritation and not upon specific disease." But to this we reply that at Paris:—1, There is no exact relation between the violence of the abdominal symptoms and the extent of the Peyerian lesion. 2, We have seen cases wherein the merest elevation of the patches saved the diagnosis of dothinenteritis, yet here the abdominal symptoms had not excited attention from their mildness: the disease may be latent altogether, and yet end by the worst form of intestinal lesion—perforation. 3, M. Chomel speaks doubtingly of the relation of the abdominal symptoms to the Parisian lesion. 5, Diarrhœa is stated by Mr. Goodsir to be a rare attendant on the continued fever of Anstruther; yet here, as we have seen, the special lesion of the patches exists with all its anatomical peculiarities; Dr. Anderson, too, observed diarrhœa in five only of twelve cases, of what he terms "pure intestinal disease," or dothinenteritis.

VI. "From the treatment of the two diseases we can infer but little," says Dr. Stewart, and he is right; yet straightway forgetting his own principle, he proceeds to draw inferences from certain fancied therapeutical results. We shall not imitate his oversight, but take leave of his essay by stating that its general character exhibits merit of a very high order.

Dr. Anderson is of opinion that Dr. Stewart has perfectly succeeded in "demonstrating that our typhus is different from the typhoid fever;" but he is disposed to take a different view of the matter somewhat differing from Dr. Stewart's. "I do not think," he observes, "typhus distinct from typhoid fever, as typhus is from smallpox for example; that is, each being a *fever*, properly so called, and therefore incapable of coexisting with each other; but I believe them to be like typhus and

pneumonia, perfectly heterogeneous, and therefore capable of being implanted one upon another, typhus being a general febrile disease, affecting the whole system and no part in particular; the 'typhoid fever' being not a fever at all, but a purely local malady, frequently accompanied, as other local affections are, by a febrile state, and sometimes uniting with typhus to form a compound disease, partaking of the symptoms of both." According to Dr. Anderson there are three distinct maladies —"pure intestinal disease," typhus with intestinal disease and typhus without intestinal disease.

English writers, who maintain that the continued fevers of France and England are different maladies, will doubtless triumph at the espousal of their notion by M. Louis. It may consequently be as well to anticipate, and possibly to prevent their jubilee, by showing the exact character of the support they receive from that distinguished source. M. Louis regards as matter of demonstration the identity of the continued fever or dothinenteritis of Paris (A), and of camp, gaol, &c. fever (B); and maintains the non-identity of dothinenteritis (A) and the common English continued fever (C): hence, as A and B are similar quantities, and A and C dissimilar, it follows that B and C are dissimilar. On the other hand, the English writers referred to regard it as a point not open to question that camp, gaol, &c. fever (B), and the common continued fever of England (C) are in nature identical; hence it follows, with the aid of M. Louis' position of the identity of A and B, that C and A are identical, while the object of these writers is to prove them dissimilar. The nature of the argument will appear more distinctly, when the propositions of each party are placed in juxtaposition:

| M. Louis. | English Writers. |
|---|---|
| A and B are similar. | B and C are *similar*. |
| A and C are *dissimilar*. | A and B are similar. |
| ∴ B and C are *dissimilar*. | ∴ A and C are *similar*. |

If after this our countrymen like to have M. Louis' support, we can only say we wish them joy of their dilemma.

What is the legitimate inference from the facts and arguments passed in review? The more efficiently to answer this query, let us commence by exhibiting in the most convenient form for comparison, the symptoms and lesions occurring on both sides of the channel.

| France. | Great Britain. |
|---|---|
| A. Abdominal Symptoms. | |
| 1. *Diarrhœa.* | |
| Of almost constant occurrence, frequently appearing on the first day, sometimes violent. | Of rare occurrence, (23 in 139 cases, Stewart,) variable in time of origin, scarcely ever violent. |
| 2. *Abdominal Pain.* | |
| Of almost constant occurrence, often on the first day, obtuse, variable in situation and duration. | Not frequent, often on the first day when it does occur, obtuse, variable in situation and duration. |
| 3. *Meteorism.* | |
| Exists in more than half the cases, (89 in 134, Louis) often considerable, and of long duration. | Occasionally occurs (74 of 463 cases, Stewart,) commonly slight. |

| France. | Great Britain. |
|---|---|
| **4.** *Nausea and Vomiting.* | |
| Of tolerably frequent occurrence, often arising the first day. | Occasionally appear from the first; thus, nausea in 14 out of 18 cases on the first day, (Anderson). |
| **5.** *Deglutition.* | |
| Occasionally obstructed from organic causes, or deficient innervation. | |
| **6.** *Tongue.* | |
| Exhibits itself in all variety of conditions. | |
| **7.** *Intestinal Hemorrhage.* | |
| In 7 out of 147 cases, (Chomel). | Extremely rare. |
| B. Symptoms appertaining to the Organs of the Senses. | |
| **8.** *Cephalalgia.* | |
| In 127 of 134 cases; in 113 of 127 on the first day; ordinary duration eight to ten days. | In 150 of 159 cases; on the first day in 92 out of 108; mean duration ten days, (Henderson). |
| **9.** *Somnolence.* | |
| Very frequent. | Of less frequent occurrence. |
| **10.** *Delirium.* | |
| In more than half the cases; mean period of origin twelfth day; perfectly independent of appreciable lesion of the encephalon. | In about a quarter of the cases; mean period of origin about the eleventh day, (Henderson). Perfectly independent of appreciable lesion of encephalon. |
| **11.** *Spasmodic Symptoms.* | |
| In about one fifth of the cases. | Quite as frequent. |
| **12.** *Continued pain in the Limbs.* | |
| Of frequent occurrence, and extremely unimportant. | |
| **13.** *Epistaxis.* | |
| Of tolerably frequent occurrence. | Of rare occurrence. |
| **14.** *State of Eyes.* | |
| Injection of conjunctiva in about half the cases. | Suffusion of eyes extremely frequent; injection of conjunctiva very common. |
| **15.** *Tinnitus Aurium.* | |
| In about one third of the cases, very rarely at the outset. | Frequency not ascertained. Gerhard found it "very frequent" in the "typhus fever" of Philadelphia. |
| **16.** *Deafness.* | |
| In more than half the cases; developed in middle or towards the close of the disease. | Less frequent *probably;* developed in middle or towards the close of the disease. |
| **17.** *Cutaneous Eruption.* | |
| Occurring in 270 of 294 cases; appearing probably from the sixth to the ninth day, but sometimes earlier; consisting of pale pink coloured spots, slightly prominent, usually few in number, and limited to a small extent of the surface; never observed to pass into the petechial state. | Occurring in 108 of 130 cases, (Henderson,) in 2028 of 2852, (Anderson,) appearing most generally from the fifth to the eighth day, but sometimes earlier, consisting of spots varying in depth of redness, scarcely elevated, usually numerous, and diffused over a considerable extent of surface, and capable of passing (Staberoh, Stewart) into a petechial state. |
| **18.** *Cutaneous Hemorrhages.* | |
| Extremely rare. | Petechiæ* common in some epidemics, rare in others. Vibices very rare. |

* Dr. Stewart (loc. cit.) appears to have shown that true primary petechia, such as are not transformations of eruptive spots, are much more rare than has been supposed.

19. *Sudamina.*

| | |
|---|---|
| In 208 out of 262 cases, (various observers.) | Apparently very rare in this country, Dr. Gerhard, however, only says that their appearance "was not so frequent as in the 'typhoid fever' of Paris." |

20. *Erysipelas.*

| | |
|---|---|
| In 9 of 134 cases, (Louis); in 4 of 42 fatal cases, (Chomel). | In 83 of 2352 cases, (Anderson). |

21. *Cutaneous Sloughs.*

| | |
|---|---|
| In 3 of 57 cases of recovery, (Louis). | Proportion not ascertained. |

22. *Rigors.*

| | |
|---|---|
| In 91 of 109 cases; in 45 of 64 on the first day: a return after they have disappeared announces the occurrence of some secondary lesion. | In 63 of 69 cases; in 48 of 69 on the first day, (Anderson): a return after they have disappeared announces the occurrence of some secondary lesion. |

C. Pulmonary Symptoms.

Catarrhal symptoms and physical signs frequent; pneumonia rare.

D. Circulation.

23. *Pulse.*

Variable in all its characters.

24. *State of the Blood.*

Variable.

E. Mode of Invasion.

| | |
|---|---|
| In 73 of 112 the invasion was sudden, (Chomel). | The invasion of the disease is sudden in above half the cases, (Anderson). |

F. Anatomical Characters.

| | |
|---|---|
| A special change in the patches of Peyer is an almost unfailing condition of the existance of the disease. | A special lesion of the patches of Peyer is of extremely rare occurrence, as a general proposition; but in almost all, or in actually all the cases examined at certain periods of the year, or in certain situations that lesion is discovered. When it does exist, the phases through which it passes, and the influence it exercises on the mesenteric glands, are the same as in France. |

G. Etiology.

The return of the evidence in favour of origin in animal effluvia and contagion is the same in both countries; the evidence is stronger, however, for both in respect of the British malady, but this may be and probably is a merely *apparent* difference. Both maladies are most prone to attack subjects recently immigrated into large towns.

I. Duration.

| | |
|---|---|
| Commonly greater than that of the continued fever of Great Britain. | Commonly less than that of the continued fever of France. |

K. Mortality.

| | |
|---|---|
| Extremely variable, from 5 to 36 per cent. | Extremely variable, from 3 to 50 per cent. |

L. Immunity from a Second, secured by a First Attack.

Supported by the same arguments and proved to the same degree in respect of both maladies.

M. Treatment.

The disease cannot be cut short at the outset by any mode of treatment: the influence of therapeutic agency on its mortality and duration is still matter for enquiry: the most advantageous method of treatment is unknown.

While then the "typhoid affection" exists without the Peyerian lesion in extremely rare instances in France, that lesion attends a variable proportion of cases of continued fever in this country; and the same group

of *general* functional derangements attends both maladies. Here is sufficient to prove that implication of the glands of the ileum is not a necessary element in the constitution of the disease; hence the lesion in question can scarcely be legitimately put forward as furnishing motive for the separation of the two maladies as distinct species. If the symptoms appertaining to particular organs or systems be made the subject of enquiry, the only difference of moment is found in the condition of the intestinal canal and skin: the diarrhœa, abdominal pain, and meteorism of France are scarcely met with among us; of the sudamina so common there, we commonly see little or nothing; the eruptions are different. Now it is remarkable that in this country while the intestinal lesion is usually absent, the cutaneous eruption is extremely general and extensive; whereas in France the iliac disease is almost invariably present, and the efflorescence limited in extent and slight in degree. Considering this fact in union with that of the well-known reciprocity of function of the skin and intestinal canal, it becomes not altogether improbable that the lesions of these systems are supplementary of each other. That the malady is fundamentally a *general* one appears from its phenomena. This is admitted by Louis and Chomel in respect of the continued fever of Paris; the latter does not allow the Peyerian lesion even so high a rank in secondary importance as the pustules of variola, because these are always, or at least very generally, a measure of the intensity of the disease, which that is not. Few or none doubt it among ourselves, though different interpretations of the phrase "general malady" may be espoused. We know of no single fact proving that this general malady is not one and the same in both countries, though for reasons at present unassignable it is in each attended with a different amount and character of organic implication.

That the form of continued fever attended with disease of the ileum should, during life, be commonly distinguishable by its local abdominal symptoms from those not so attended is nothing more than might be anticipated; and that there are cases in which such distinction cannot be established does much towards demonstrating the secondary importance of the disease in question. But to the idea that any of the general symptoms, or the mean age of the subjects in whom it is developed, characterize the intestinal form, we cannot accede; besides, were such idea substantiated it would obviously not prove any fundamental difference in this from other forms. We cannot accede to it, because, as regards symptoms, proof is not forthcoming; and because in respect of age, while on the one hand to draw a final inference from Dr. Shattuck's thirteen cases is certainly unwarrantable, we are not by any means persuaded that the alleged limitation of the "typhoid affection" of Paris to subjects aged under fifty or thereabouts, is satisfactorily established. Not long since it was matter of faith in Paris that the disease did not occur in infancy, (an opinion which M. Louis has been, without any particle of reason, charged with promulgating); the question having been properly examined, it is now known to be extremely frequent among children: is it not fair to anticipate the occurrence of a similar change of opinion respecting persons of advanced age, at least to acknowledge its possibility?

We believe, then, that the continued fevers of both countries are the

*same species* of disease; but they are *different varieties* of that species. And there is nothing in the climate of either country, or in the influences, external and otherwise, to which their populations are exposed, capable of preventing the evolution of that form of the malady more usual in the other: the French variety is observed, though rarely, in Great Britain; the British, with similar restriction, in France. Some questions of importance remain to be decided: for example, wherein lies the cause of the almost invariable establishment of intestinal special lesion in France, and of its ordinary absence here; whence comes it that at Edinburgh the proportion of fatal cases, with lesion of the patches, amounts during a series of years to but a small fraction of the whole, while at thirty miles from that town no case is examined without their being discovered; why does the proportional number of cases with Peyerian lesion vary in the same locality with different years?

Finally, in adopting the opinion that continued fever is a *general malady*, we do not lend ourselves to the notion of its "*essentiality*," a notion to us just as incomprehensible as that there should be an effect without a cause. We believe, as all sound argument goes to show, that the *blood* is the constituent of the body, implicated from the first; though from the imperfect state of knowledge concerning the normal condition of that fluid, as well as from the imperfection of our modes of estimating its aberrations therefrom, the power of physically demonstrating this is at present and may long continue to be withheld. Meanwhile we think the judicious pathologist can hardly doubt that it is wisdom to adopt the creed, of which, while rational views militate in its favour, no fundamental principle of logic proclaims the fallacy.

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## Art. II.

*Guy's Hospital Reports.* Nos. X-XI-XII. *April and October*, 1840, [*and April*, 1841.—*London.* 8vo.

These excellent Reports hold a steady course of usefulness, yielding at convenient intervals much valuable information on a great variety of topics. Some gleanings from the three numbers now before us are here offered to our readers.

No. X.

*Some Observations tending to demonstrate the Dependence of Vascular Organization upon Physical Causes.* By Sir Anthony Carlisle, F.R.S.—This is a paper of thirteen pages, containing several very curious observations and experiments, illustrated by diagrams. It is well worthy of perusal; but being of a nature which does not admit of abbreviation, our curious readers must themselves look to the original.

*Observations on the Existence of certain Elements of the Milk in the Urine during Utero-gestation; and on the Application of this fact to the Diagnosis of Pregnancy.* By Golding Bird, M.D. F.L.S.—Dr. Bird has in this paper communicated the result of some experiments made by him on that new constituent supposed to exist in the urine of pregnant women, termed *kiestein*. When the urine is allowed to repose in a cylindrical vessel for a few days, this substance appears, first, in the form of a cot-

tony cloud, which shortly rises to the surface, forming, there, a fat-like scum, and sinks again in minute flocculi to the bottom of the vessel. This crust of kiestein is stated to be distinguishable from analogous pellicles which occasionally form on the surface of the urine, from its never becoming mouldy or remaining on the surface beyond three or four days from the time of its complete formation. The discovery of it is considered important as aiding the diagnosis of pregnancy; and Dr. Bird has experimented both on pregnant women and others presumed not to be so with reference to this application of it. Of thirty pregnant women, the urine in all but three gave out copious fat-like pellicles after two or three days' exposure, and the exceptions Dr. Bird accounts for by the fact, that in them there existed, at the time of making the experiments, considerable febrile excitement. Of a number of unmarried females experimented on at the same time and in a like manner, in two instances only was any evidence of the presence of this peculiar matter manifested; and in both these, though they indignantly repudiated the imputation of pregnancy at the time, yet such afterwards turned out to have been the fact. Strong presumptive evidence is derived from these statements if they are free from the possibility of error. In one of the last-named experiments the urine was exposed in a lightly-covered glass cylinder: in two days a dense pellicle of fat-like matter formed on its surface; this increased in thickness during three days, and then evolved a powerful odour of putrefying cheese. From Dr. Bird's investigation of the chemical nature of this kiestein, he considers that

"The greasy aspect of the pellicle arises, not from the presence of fat, but from the numerous crystals of triple phosphate, which, from their brilliancy, produce this glistening appearance; with regard to the nature of the animal matter mixed with the crystals, it is difficult, in the present state of physiological chemistry, to give a positive opinion; it is not mere albumen or casein, although much closer allied to the latter than to any other product of organization I am acquainted with, especially when we connect with its chemical character the powerful cheese-like odour so frequently evolved during its development in the urine, in the form of a pellicle.....The crust of earthy phosphate which forms on the surface of all urine by long repose, cannot be mistaken for the pellicle under consideration, as that which destroys the latter, viz. putrefaction, causes the production of the former."

Finding evidence of the presence of certain ingredients of milk, as caseous matter and earthy phosphates, in the urine of pregnant women, Dr. Bird suggests as a probable explanation, that during uterine gestation these ingredients are eliminated from the blood in the breasts, and not finding an exit therefrom, are taken up thence and carried to the kidneys, where they are discharged with the urine. According to Dr. Bird's experience, and a few striking cases in point are detailed, such pellicles are not formed in the urine after parturition, when the milk with its caseous elements finds a ready discharge from the nipple.

As a test of the existence of pregnancy, the formation of the caseous pellicle, especially if accompanied with a cheese-like odour, will, Dr. Bird has no doubt, be a valuable corroborative indication. Connected with other symptoms he would regard it in that light; but if existing alone and unsupported by other indications of pregnancy, we have no

right, he thinks, in the present state of our knowledge, to regard it as conclusive. The following are the deductions of the author:

"1. That certain organic matters, closely resembling, if not identical with caseous matter mixed with abundance of the earthy phosphates in a crystallized state, are eliminated from the blood during pregnancy; and, if not otherwise removed, are taken up and finally thrown out of the system by the kidneys.

"2. That certain accidental circumstances, especially connected with those morbid actions in which the kidney is called upon to perform a compensating function for the skin, as indicated by the abundance of azotized matter in the form of amorphous lithate of ammonia in the urine, interfere temporarily with the development of caseous matter, as they do in checking the cutaneous and other secretions.

"3. That, taken in connexion with other symptoms, as the formation of a dark areola round the nipple, and cessation of menstruation and abdominal enlargement, the formation of a caseous pellicle in the urine affords a very valuable corroborative indication of the existence of pregnancy."

The two next papers are by T. Wilkinson King, Esq.; one *On the Common Action of the Auriculo-ventricular Valves;* the other *On the Surfaces of Contact, or Attrition, on the Valves of the Heart.* (With Plates). But as there is little in them that is not already known, and as even the author himself yields assent to their want of originality, we are relieved from the necessity of noticing them further.

The next is a long and elaborate paper giving *An Account of the Fibrous Structure of the Subserous Membrane of the Aorta;* by Norman Chevers, M.D., which, likewise, we need not do more than refer to, as we find that without the plates which accompany the paper we would fail in making it intelligible. Indeed, at all events, as we are gleaning from hospital reports, we prefer, in this place, keeping to our text.

The paper which follows is one of much practical value, and deserves a more lengthened notice than we can bestow on it. It is *On the Morbid Consequences of Undue Lactation.* By Samuel Ashwell, M.D.—But little has been written on this subject; Dr. Marshall Hall being the only author who has bestowed on it more than a few incidental remarks. Dr. Ashwell considers exhaustion to be the permanent morbid state associated with undue suckling; and the following circumstances regarding the affection may, he thinks, be proved: *First.* That lactation to be morbid need not be long; evil consequences may ensue soon after its commencement; occasionally within a few weeks; more frequently within a period protracted beyond nine months. *Secondly.* That organic lesions may, though rarely, result from undue suckling. *Thirdly.* That weaning the child is generally indispensable to the cure—the remedy without which all others will be inefficient.

It would be injustice to curtail the already succinct but clear account of the history and symptoms of undue lactation; and our prescribed limits do not permit us to give them in full: we particularly recommend them to the notice of junior practitioners.

*Functional Amaurosis*, accompanied by congestion of the conjunctiva,

are frequent results of excessive lactation; as are also specks and slight ulcerations of the cornea. In these cases the sufferer may be encouraged to expect the restoration of sight provided there be no serious organic change. Weaning, avoidance of quickly-recurring pregnancy, improved diet, repeated small blisters near the eye, are the proper remedial means. *Jactitation* is an occasional attendant on this state. In the cases reported by Dr. Ashwell, leucorrhœa and general debility were present. *Epilepsy* is sometimes a product of over-suckling, on the same grounds as inanition, losses of blood, and deficiencies in its quantity and quality are productive of this malady. *Insanity*, more or less permanent, may originate from over-lactation; the progress and symptoms of such affection are well told by Dr. Ashwell. The *pathology* of these functional results of undue suckling is by no means intricate or unsatisfactory. An impaired and attenuated condition of the blood, and a consequently depressed state of the nervous system, especially of the organic system of nerves, is the clue by which all the symptoms may be unravelled. Cases are given by Dr. Ashwell wherein organic changes in the brain, lungs, and uterus were induced by very prolonged undue suckling. Respecting headach we are presented with the following sensible observations:

"It is a frequent concomitant of the malady; nor can the practitioner be too strongly impressed with the hazard arising from its continuance. So long as it is general, not very severe, and transient; so long as it does not recur periodically, with marked premonitory symptoms, it may be viewed as comparatively free from risk. But, if it be dreaded on account of the permanent uneasiness which it has already produced, or from its intensity and acuteness; if it seize on one part of the head and remain fixed there; if its paroxysms be preceded by rigors, and if the pain never entirely subsides; more especially if there be partial paralysis, mental peculiarity, or forgetfulness approaching to imbecility, or any other anomalous symptom indicative of deranged nervous action, for instance, an unusual affection of the eye, such as double or impaired vision; or of the auditory nerve injuring the hearing, or rendering it excessively and painfully acute; or if there be impeded deglutition, then danger exists, and a softened or otherwise structurally altered condition of the brain may be feared.

"*Treatment.* The indications in the merely functional affections are not difficult to meet. Where the symptoms of exhaustion are slight, a better diet, a careful regulation of the bowels, a tonic treatment, and, above all, diminished suckling, will often avail. Nor is it necessary to urge very strongly, because the propriety of the recommendation is evident, that the child should be fed two or three times within the twenty-four hours, and that unbroken sleep during the night should be secured to the mother. But let it be remembered that this will not always avail. A continuance of the debility, or aggravated prevalence of one or more of the symptoms already enumerated, will plainly indicate the necessity of entire weaning. If the child be purged, or become gradually emaciated, it will corroborate the importance of the step. Where organic disease is threatened, especial attention must be paid to the organ in which it seems likely to occur. Cupping or leeches may be required; and counter-irritation by blisters, setons, or issues may be expedient; beyond these general directions, the practitioner will proceed according to the exigencies of the case, never omitting the weaning of the child. The convalescence of such patients is generally protracted and difficult, years sometimes elapsing prior to recovery. Nor can it be too forcibly recommended that suckling should be abandoned if a fresh pregnancy succeed very quickly. The symptoms are often rendered worse by gestation and invariably by lactation. Iron, chalybeate waters, country and sea air, travelling, and exercise are most important auxiliaries."

*Practical Hints on the Treatment of Stricture.* By Bransby Cooper, F.R.S.—This is a valuable communication, which, as well as a second part in the next number of the Reports, we shall notice elsewhere. We recommend the perusal of this memoir to all who are addicted to the heroic treatment of stricture; they cannot fail to profit by its perusal.

*Cases of Excision of the Elbow-joint.* By C. Aston Key, Esq.—Mr. Key has lately performed two operations of this kind on men, one twenty-six, the other forty-three years old. They were both cases in which, to save life, amputation must have been practised had not excision been had recourse to. The first left the hospital with some motion in the joint, and with considerable power in the limb, and shortly after returned to his occupation as a postman; the second, a shipwright, was even more fortunate, as his convalescence was both more rapid and more complete.

January 14, 1840. *Operation* in case ii. "An incision was made posteriorly in either side of the joint, about four inches long; another transversely, and the flaps reflected; the articular extremity of the humerus was then sawn off, also the olecranon and coronoid process of the ulna; the head of the radius was left; two or three vessels required ligature; about a pint of blood was lost. The divided edges were brought together by sutures and plaster; the arm was then laid on a splint. The operation lasted twenty minutes, and the patient bore it remarkably well." Nothing untoward occurred subsequently, and on the 20th February the following note was taken. "From the last date he has been recovering his strength rapidly; wound quite healed; can flex the arm to a right angle, and extend it to an obtuse one with ease and comfort; has considerable strength in the arm, as he can raise a heavy stool above his head; feels confident of being able to follow his employment, which is a very laborious one." On examination of the portions of bone excised, the cartilaginous surfaces were found completely destroyed by ulceration.

The improvement that ensues after the operation shows how little the constitution suffers from it, severe as it appears during its performance. The nature of the parts excised explains the little constitutional disturbance; no veins or arteries of magnitude being compromised in the operation, and the freedom of the incisions favouring the exit of the discharge that attends the healing of the wound. The removal of a source of irritation seems to be immediately felt in the improved appearance, refreshing sleep, and returning appetite, aided, no doubt, by a suppurating cavity with an imperfect outlet being converted into an open suppurating wound. Mr. Key considers that the most favorable cases for such an operation are those in which the disease has had its beginning in the articular surfaces; and that on this account the operation will be likely to succeed better in adults than in young subjects, as in the latter, articular disease often, perhaps in most cases, arises in the cancellated ends of the bones. The state of constitution, too, that gives rise to disease of the bone, is unfavorable for an operation, much more so than the worst cachexia induced by simple joint disease.

*History of a Gun-shot Wound in which the Patella was carried away, and the Knee-joint completely laid open, successfully treated by*

W. Ward, Esq., of Huntingdon.—On the 2d of November the contents of a gentleman's gun struck the patella on the outside of his knee, carrying away the whole of that bone, except a small, solid, triangular portion which still remained attached to the ligament; there was a nearly circular wound of the integuments, completely exposing the joint, and sufficiently large to admit the whole hand into the joint between the tibia and femur; but the cartilages of these bones appeared uninjured. Rest, poulticing, and the semi-flexed posture constituted the chief treatment. No unfavorable symptoms, either local or constitutional, occurred during the progress of the case, nor was the patient's pulse even in any degree accelerated. On the 21st of January following, the wound was healed, and the patient able to dress himself and sit up in a chair. In a short time, with the aid of a suitable splint and bandage, he went upon crutches; and in March he was able to ride many miles on horseback. This case confirms the observation of writers that large wounds of joints are not so commonly followed by severe constitutional disturbance as small or punctured wounds.

*Case of Dislocation of the Shoulder-joint, with Fracture of the Humerus.* By J. H. Hingeston, Esq.—The paper contains a very minute anatomical, pathological, speculative, and practical account of the above-named accident; rather more so, indeed, than to admit of an abridgment suited to our pages.

*On the Treatment of Incipient Phthisis.* By H. Marshall Hughes, M.D.—Dr. Hughes communicates in detail his mode of treating consumption, but the chief purport of the paper is to recommend the use of emetics in the early stages of certain cases of the disease. He gives an emetic about an hour before breakfast every morning, or every second, third, or fourth day, according to the strength of the patient and the character of the disease. He finds that the sulphate of zinc or ipecacuanha, in doses of twelve grains, or a combination of six grains of ipecacuanha, and two of sulphate of copper, the most desirable forms and proportions. He says their uniform effect, with a very solitary exception, has been very materially to relieve, and in not a few instances entirely to remove the cough.

*On Disorders which are variable, and on the practical Inferences which are deducible from the character of Changeableness.* By T. Wilkinson King, Esq.—This paper, to be appreciated, must be studied in the words of the author, who "considers the fact that many diseases are variable to be a law, second only in importance to the most general law that medicine embraces."

*On the Episternal Bones occasionally found in Man.* By T. W. King, Esq., (with Plates).—For an account of these bones, as given by M. Breschet, see Brit. and For Medical Review, Number XV., p. 248. In Mr. King's description we can discover nothing new.

*On some Supplementary Muscles of the Anus, described by* Dr. Horner, of Philadelphia. By T. W. King, Esq.—The muscles noticed

by Dr. Horner consist of fibres better than an inch long, lying longitudinally, in bundles between the internal sphincter ani and the mucous membrane.

Mr. King, who furnishes a drawing of these muscles, considers them true retractors ani.

*Case of Transposition of the Aorta, Trachea, and Œsophagus; Tuberculated Liver, and Scirrho-cancer of the Rectum.* By Henry Ewen, Esq. (With a Plate.)—In this case the arch of the aorta ran behind both the trachea and œsophagus—between them and the vertebral column; but there was no transposition of the cavities of the heart. Four branches arose from the arch. The patient was sixty-five years of age. The other lesions had no connexion or dependence on the malposition of the great vessels.

*On the Forms of the Cartilages which keep open the principal divisions of the Bronchial Tubes.* By Jonas King, Esq.—Dr. Horner has described certain crescentic cartilages in the bronchial tubes. Mr. King, following up the subject, has here given a plate of them, together with a more minute description of their forms and uses than that given by Dr. Horner.

*Case of Urinary Calculi formed on a piece of straw.* By Henry Norris, Esq. (With a Plate).—The patient had been in the habit of relieving himself of difficulty in making water by the introduction of fine straws. The portion, the subject of the incrustation, had slipped accidentally into the bladder about a month before his death.

*On the History of a supposed Hermaphrodite.* By Robert Merry, Esq. *Its Dissection* by Sir Astley Cooper, Bart. (With Plates.)—The patient (Mary Bennett) died in her eighty-sixth year, of a gradual decay of her powers. She was very masculine in appearance, but never shaved; she led a very laborious life, and disliked the society both of men and women; she never menstruated. Summing up the abnormal anatomical circumstances of the case, Sir A. Cooper says "this woman, therefore, differed from others in the magnitude and length of the clitoris, in the absence of the external orifice of the vagina, and in the imperfect development of the ovarium."

No. XI.

*Operation for the Radical Cure of a Reducible Inguinal Hernia.* By Bransby B. Cooper, F.R.S.—The operation performed in this instance was that recommended by M. Gerdy, for the radical cure of a reducible hernia; and in its results has been satisfactory. The patient was a tall muscular man, aged twenty-two. The hernia was of seven years' standing, and had been brought on by violent exertion. At the time of the operation the rings were so open that the tumour came down even by the effort of changing position in bed, giving the sensation as if the belly was not strong enough to retain its contents. He had been subjected to a fit of strangulation shortly before, from which he was with difficulty relieved by the taxis.

"On the 10th of June, the patient was brought into the operating-theatre and

laid upon a table on his back, with his chest and thighs raised for the purpose of relaxing the abdominal muscles. Mr. Cooper then commenced the operation by pushing a portion of scrotal integument before the fore-finger of his left hand through the external ring, into the inguinal canal, as high as he could pass it, and upon the finger he then introduced a director. A long needle fixed in a wooden handle, and having the eye near its point, armed with a double silk ligature, was then carried along the director to the very extremity of the invaginated skin, and was pushed through the tendon of the external oblique muscle and the skin, so as to make its appearance about an inch and a half above Poupart's ligament: one end of the silk was then retained by an assistant, and the needle drawn back again into the inguinal canal, along the other end; when it was again pushed through the abdominal parietes, in a similar manner as before, about four lines distant from the other end of the thread, including necessarily so much of the skin between the two silks, which were now tied over a piece of bougie so as to retain the invaginated portion of the skin within the inguinal canal. A piece of lint wrapped around a director, and dipped into liquor ammoniæ, was pushed into the cul-de-sac of skin thus formed, and the surface well rubbed with it, in order to remove the cuticle and promote an inflammation of the cutis, so as to obliterate this integumentary canal, and to form a plug sufficiently firm to protect the future descent of the hernia."

The application of the ammonia produced intense pain, which was but little relieved by opium, and continued more or less for two days. The surface of the skin suppurated. No symptoms of peritonitis showed themselves. 14th, the ligature was removed, as purulent discharge was most freely established, but the pressure on the part was desired to be continued. In some days farther, all uneasiness left the patient, and he felt the affected side as firm as the other, which the thickening in the course of the canal seemed to justify. In the middle of July he left the hospital and returned to his work. Early in August, however, a small portion of intestine descended while at work, in consequence of weakness of his truss, which being changed for a stronger one, he became enabled to perform all the duties of his situation. September 21, the man presented himself at the hospital to show himself to Mr. Cooper; and stated that he has been able to keep at work regularly ever since he has had the stronger truss on, and that his hernia has not again descended.

*Case of Cerebral Disturbance dependent upon Disease of the Pericardium.* By Dr. Yonge, of Plymouth.—This case is reported to corroborate some views of Dr. Bright's, published in vol. xxii. of the Medico-Chirurgical Transactions. The patient, a young man, suffered in the first instance, from rheumatic fever, which run into chorea. The convulsions at one period put on a character resembling tetanus and opisthotonos, and the distress in swallowing food or medicine was not unlike that in hydrophobia. The patient died in about six weeks from the first invasion of the illness, exhausted by want of rest, laborious spasm, and probably want of sustenance. No abnormal lesion was discoverable anywhere except in the heart, which presented marks of pericarditis, with thickening and opacity of the lining membrane of the left cavities, especially about the neighbourhood of the mitral valve. The particular view which the case is supposed to corroborate is "that the more frequent cause of chorea, in conjunction with rheumatism, is the inflammation of the pericardium; and that the irritation is communicated thence, probably, to the spine, just as the irritation from other parts."

*Observations on Diabetes; with cases illustrative of the efficacy of ammonia in the treatment of the disease.* By GEORGE H. BARLOW, M.A. & L.M.—Reasoning from physiological and pathological considerations, Dr. Barlow joins in the belief that the sugar found in diabetic urine is formed in the primæ viæ at an early stage of the process of sanguification, and is not necessarily connected with a perverted action of the kidneys; and he considers the increase in the quantity of urine to be due to the diuretic action of the sugar on the kidneys. The essential character of the digestive process, in health, consists, according to Müller, in its not only effecting the solution of the food, but in its likewise annulling the peculiar properties which the nutritive matters may owe to the source whence they are derived; that is to say, in dissolving the food and converting it into albumen.

"In this disease, on the contrary," says Dr. Barlow, "the saccharine particles of the food are not changed in the stomach; whilst the starch, which most articles of vegetable diet contain in considerable quantities, not having its peculiar properties annulled, and its proneness to the saccharine fermentation being favoured by the warmth and moisture of the stomach, is converted into sugar, which, being readily soluble, is absorbed into the circulation.

"It appears, then, that, owing to a deficiency in the assimilating powers of the stomach, a lower organic product, sugar, is taken up into the system, in place of a higher organic product, albumen. This lower organic product is inadequate to perform the duties of the higher; and is, according to laws already referred to, respecting the action of the kidneys, removed from the system by these organs.

"If this view of the matter," Dr. Barlow continues, "be correct, the next enquiry is, what are the inferences to be drawn from it respecting the treatment of diabetes. The first is obviously one, the correctness of which has long been acknowledged and confirmed by experience, namely, the avoidance of all saccharine and amylaceous articles of food, the latter of which, from their tendency to saccharine fermentation, are, I believe, productive of as much mischief as the former. I have, however, little to add to what is generally received on this point, farther than to insist on the advantages of the cruciferous vegetables as articles of diet; the use of which is not only in accordance with the view taken above, but has received the sanction of many experienced physicians. Not only do greens, broccoli, turnip-tops, sea-kale, water-cresses, &c. tend to obviate the loathing which is often felt by these patients, when restricted to an animal diet, but they exert a decidedly beneficial influence over some of the symptoms.

"The next indication appears to be to introduce into the stomach a highly azotized substance, and, at the same time, by a diffusible stimulant to exalt, if possible, the assimilating powers of that organ; both of which ends appear likely to be attained by ammonia.

"Such exercise as the strength of the patient will enable him to take, and the use of a warm bath occasionally, where it can be obtained, are also valuable adjuvants. The accumulations which not unfrequently take place in the large intestines, and which are, I believe, but a consequence of the participation of the bowel in the general muscular debility, must necessarily impair these functions of the chylopoietic viscera, which it must be our main object to restore. The union of a purgative with a tonic, which rhubarb affords, seems to point it out as the most eligible remedy in these cases: this I have generally exhibited in combination with sulphate of potass; and aided its operation, if necessary, by castor oil."

The sesquicarbonate of ammonia is the preparation recommended by Dr. Barlow. He has found that under its use the function of skin is

generally restored; and although he has sometimes thought that it was aided by opium in bringing about this end, yet in some cases the same effect has been produced without the use of the latter. He prescribes it in doses of from five to eight grains and upwards, with a few minims of tincture of opium, in some bitter infusion, every six hours; animal diet, with greens, being given at the same time. Dr. Barlow relates four cases, which, he thinks, show that the practice founded on the foregoing principles is by no means devoid of success; although he is far from affirming that the remedy will prove a certain cure for all the cases of this disease, which is by some considered incurable.

*Observations on Abdominal Tumours and Intumescence. Illustrated by Cases of Diseased Liver.* By Richard Bright, m.d. f.r.s.—Dr. Bright proposes in the present communication to follow out the subject of abdominal tumours, drawing the illustrations from the *liver*, as he has already done in previous memoirs, from the ovary, the kidney, and the spleen, and from the development of hydatids, (see Br. and For. Med. Review, vol. VII. p. 462.) The memoir is a very important one, and demands so full an analysis, that we shall defer the notice of it to our next Number, when we hope to be able to devote more space to it than at present.

*Report on Primary Syphilitic Cases.* By C. Aston Key, Esq.—This report is divided into two parts, of which one is given in No. X. and the other in No. XI. The whole memoir is of so valuable a character, and so full of important practical matter, that we shall here give a somewhat extended analysis of it, notwithstanding that we partially noticed it on a former occasion (vol. X. p. 381), and although we have devoted an article in the present Number to the subject of syphilis.

Out of 719 cases of men with primary venereal sores, admitted into Guy's Hospital since 1825, the following varieties presented themselves:

| | | | |
|---|---|---|---|
| 1. | Aphthous chancre | 85 | |
| 2. | Do. with bubo | 58 | |
| 3. | Do. with open bubo | 41 | |
| 4. | Do. with phymosis | 123 | |
| 5. | Do. with phymosis and open bubo | 4 | |
| 6. | Raised chancre of the outer prepuce, | 44 | |
| 7. | Do. with bubo | 13 | |
| 8. | Raised chancre on scrotum | 4 | |
| 9. | Indurated chancre | 23 | |
| 10. | Do. with bubo | 9 | |
| 11. | Do. with phymosis | 4 | |
| 12. | Phymosis with chancres at extremity of prepuce | 46 | |
| 13. | Do. with bubo | 8 | |
| 14. | Irritable chancre of inner prepuce and glans | 41 | |
| 15. | Do. with bubo | 9 | |
| 16. | Phagedenic sore of outer prepuce | 11 | |
| 17. | Do. of glans and inner prepuce | 23 | |
| 18. | Do. with bubo | 7 | |
| 19. | Do. with phymosis | 7 | |
| 20. | Sloughing chancre | 9 | |
| 21. | Sloughing chancre with bubo | 3 | |
| 22. | Do. with phymosis | 17 | |
| 23. | Phagedenic bubo | 8 | |
| 24. | Paraphymosis | 13 | |
| 25. | Do. with sloughing of prepuce and glans | 2 | |
| 26. | Do. with ulceration of prepuce | 15 | |
| 27. | Warts on glans and inner prepuce | 16 | |
| — | Warts, with phymosis | 11 | |
| — | Do. with phymosis and sloughing prepuce | 2 | |
| — | Warts, with paraphymosis | 1 | |
| — | Warty ulceration of anus | 4 | |
| — | Condylomatous sores about the anus and scrotum | 12 | |
| 28. | Phymosis from gonorrhea | 24 | |
| 29. | Suppurating bubo from do. | 22 | |

The nature of a chancre, Mr. Key apprehends, will depend on the depth to which the action of the poison penetrates. In the aphthous chancre, this action appears to be confined to the surface of the cutis,

blistering as it were the epithelium, without indurating the surrounding tissue. Such a sore may heal quickly; but, when it is more tardy and its stages more distinct and protracted, the poison affects the parts to a greater depth, and what was at first a simple vesicle, becomes a sore with a thickened base; or it will sometimes heal, leaving behind a very slight induration, and again break out into a virulent sore, exhibiting the indurated form of venereal ulceration. Respecting the question of diversity of poisons, Mr. Key considers that it must remain in abeyance until experiments, conducted on a large scale by persons qualified for the purpose, shall have brought together an irresistible mass of evidence; and, at all events, he regards the settlement of this point one of minor importance, and considers that in the present state of our knowledge, the only well-grounded mode of proceeding is to describe and distinguish sores according to their characters, with a view of determining by experience the best mode of treatment for each. Mr. Key's own feeling in regard to this matter is that the line attempted to be drawn between the different kinds of sores is too defined, inasmuch as nature points out no such line of demarcation. Each sore is found occasionally to run imperceptibly into one of another class, so that it is difficult to decide to which it belongs.

A large number in the above catalogue of primary sores is classed under the head "aphthous." The causes of the varieties Mr. Key attempts in some measure to point out. The hardness attending a sore on the genitals, and which has been regarded by some as characteristic, cannot in itself be depended upon for determining the syphilitic or non-syphilitic nature of the case; the site of an aphthous chancre greatly modifies its condition; on the glans or inner prepuce, the sore, together with the extent of poisoned tissue, is often so superficial as to be within reach of a mild caustic, and to be therefore curable by one or two applications of the nitrate of silver; at the corona glandis, on the other hand, there is glandular structure and cellular tissue; and then we find this kind of chancre almost always firm at the base, deep in its action, and ragged instead of being smooth on its surface. On this sore the nitrate of silver has less influence, because it does not penetrate deep enough to reach the extent of the poisoned structure. Friction, scabbing, injudicious applications of nitrate of silver, long persistence of the ulcer, &c. all retard the cure and increase the tendency to hardness. Sores on the fold fringing the prepuce and occasioning phymosis are usually remarkable for their hard base, and yet, not possessing the other characters of a poisoned ulcer, yield to the simplest mercurial treatment. It would appear, then, that induration, though usually attending a chancre when seated in some tissues, cannot, when absent or present, negative or decide the action of the virus. Mr. Key remarks farther, as follows:

"Finding that chancres possessed every variety and shade of character, I came to the conclusion, either that it must be occasioned by the difference of situation which it occupied on the penis, or by a varied action or intensity of the poison, or by a peculiarity of constitution. The former position, as affording a satisfactory explanation, is wholly untenable; for though situation must be allowed to modify in some respects the appearance of sores, yet as every variety of sore occasionally appears on the same part, it is obvious that some other cause must be in operation. ..... At first it appears inconsistent with the definite progress of disease arising from one poison, that in one person a chancre should be an excavated sore, and in another an induration of tissue with scarcely

a breach of surface; we see, however, in other diseases a similar variety of action from the same cause, cancer of the lower lip for example."

Mr. Key regards it as impossible, under these circumstances, to admit more than a "modification" of poison, which, he thinks, may be considered "equivalent to a difference of poison." This view of the matter differs but little from that of Mr. Carmichael. This slight difference, however, seems to Mr. Key essential to a true understanding of the matter.

In the majority of cases of common vesicular chancre, Mr. Key considers the nitrate of silver to be the best application. He says that "when used properly and under circumstances which ought not, in the eye of an ordinarily judicious practitioner, to forbid its employment, it altogether destroys the seat of the syphilitic virus, and thus prevents infection, both local and constitutional. Such an effect can only be looked for when the vesicle is seen at its commencement. If the action is not wholly arrested, an eschar is formed which serves to protect the surface and limit its extension."

Mercurial applications seem, in the early periods of such sores, to be particularly noxious, as if the action was rather increased than stayed by them; the common astringent salts, as the preparations of lead, zinc, and copper, varied as the sate of the sore will bear, check the disposition to spread, and bring on an appearance of granulation. But Mr. Key speaks of these matters to inculcate discrimination in the use of mercury, not to forbid its use. On the contrary he may rather be regarded as a mercurialist.

But though, in the aphthous sore the tendency to spread under the mercurial applications is remarkable, in the more decided chancre the disposition to be "set astray" is less and the mercurial applications will agree better. "In these sores, local mercurial action does not render the secretion copious, nor does it render the surface yellow, loose, and spongy, or the edge disposed to break up as it does on the aphthous sore; on the contrary the edge becomes less raised and firm, and not disposed to extend by ulceration; the secretion is altered but not increased, the surface becomes more solid and fibrinous and inclined to granulate. If they present not the most decided characters, but verge towards the aphthous ulcer, a combination of the two plans may be had recourse to; an astringent may be used for the purpose of protecting the sore from the injurious effects of mercurial action; thus it may be dressed with the black wash, and washed with a pretty strong solution of some of the salts alluded to. The tone and vigour of the tissue are preserved by the astringent, while the mercurial corrects the morbid action induced by the virus."

The following observations of Mr. Key are so judicious, that our readers will be obliged to us for giving them insertion in full:

"In the treatment of primary sores I commence with mercurial medicines as soon as the preceding indications show the sore needs them, and carry them to the extent that the patient's constitution is able to bear. The principles by which the practitioner should be guided, in deciding on this difficult point, are those of general pathology. The remedial agency of mercury in arresting the progress of syphilitic virus is known to all, and acknowledged by all who study the course of this disease and the action of this medicine; but the numerous conditions that interfere with its action as a remedy and tend to convert it into a poison are less known, because they are more difficult to appreciate. I know

of no rules that can be laid down for the guidance of the practitioner, except such as are so general that they can hardly serve as rules; they are rather principles than rules; and where the straight line of action afforded by a rule fails, as in this, and indeed in every disease it occasionally does, principle comes to our aid as a never-failing guide."

Mr. Key describes three kinds of sores attended with induration as a distinguishing feature. The first is that known as the Hunterian chancre; even this may exist long without being followed by constitutional syphilis. Mere hardness is not alone decisive of the presence of the poison. Mr. Hunter himself did not, as has been imputed to him, measure venereal sores by the degree or extent of the induration. The second kind of indurated chancre occurs more often in private than in hospital practice. It commences with a thickening of the cutis or subjacent cellular tissue without breach of surface. If neglected, as it commonly is from the supposition of its harmless nature, these appearances increase until the skin becomes glossy, and at length excoriated. The excoriated part is highly florid and prominent, and a minute quantity of secretion can be seen oozing from it: there is not any appearance of ulceration. Such a sore, as far as Mr. Key's experience has gone, is always followed by absorption and a train of secondary symptoms. In the treatment, two circumstances force themselves upon the attention of the surgeon—the inability of the sore to bear mercurial applications, and the necessity of giving mercury in a cautious manner. Mercury should be used at once; but the form of the medicine should be such as will be least likely to increase the constitutional irritability of the patient, and effective enough to induce a curative action. The character of the secondary symptoms following this sore is usually severe even when uninfluenced by mercury. The third variety of induration resembles in many respects the foregoing. The virus neither excoriates nor ulcerates the skin; there is entire freedom from inflammation and consequent excoriation and ulceration, the action being of the most chronic kind.

"There is here no impediment to the prompt administration of mercury till the gums are affected, and keeping up the action till the mass softens and subsides. The constitution is usually in a state to bear it, and unless it is given to the full extent to produce its specific effect, secondary symptoms will arise on the skin and throat. The length of time that usually is allowed to elapse between the period of infection and the commencement of remedies, gives the opportunity of absorption; which in all the instances that I have seen of this form of the disease, has invariably occurred in a marked degree. The freedom from irritation of the part also invites the application of a mercurial; and the mercurial ointment is the best that can be used."

Although this notice of Mr. Key's paper is not designed to be more than an abstract, it may not be out of place, in a question of experience like the present, to state that we know of two well-marked cases of the latter kind of sores, which have been got to heal without mercury, and which, nevertheless, have not been followed by secondary symptoms, although many years have now elapsed since their occurrence; and this is not stated in an anti-mercurial spirit, for in both instances it was the obstinacy of the patient and not the judgment of the surgeon which saved the former from a salivation. It is the occurrence of such cases as these that embarrasses the mind and prevents it from adopting, unhesitatingly, any code of rules on the subject of syphilitic diseases.

In the management of venereal affections we should endeavour to distinguish between the essential characters of the unmixed effects of the poison and those features which they acquire from accidental circumstances. Phagedena is among the latter; and this is owing to an excess of irritability. Chancres are sometimes rendered irritable from local causes, as from improper applications, the action of the urine, the position of the sore on the frenum, &c.: this state, aggravated by improper diet, irregular hours, or other such circumstances, produces an irritability of the nervous and vascular system, which, too, is often increased by the use of mercury. Here after withdrawing mercury and restoring the tranquillity of the sore by emollients and purgatives, or sometimes by nitrate of silver, Mr. Key administers the cold infusion of sarsaparilla in lime water with the most beneficial effect. The irritable sore at the extremity of the glans usually extends as far as the influence of the urine reaches, generally to about the size of half a sovereign. It spreads slowly, making every now and then an abortive attempt to granulate, which ends in covering the surface, not with fibrine, but with a soft yellowish coating.

"Mercury, here, is rarely admissible; it is better to begin, as soon as this disposition is observed in the sore, with the application of a solution of nitrate of silver, from three to six grains to the ounce, as can be best borne; and to continue it as long as the coating of the ulcer is indisposed to become firm in texture. As soon as the whitish slough becomes firmer in consistence, it shows that the disposition to ulceration on the surface is on the decline; and in this state it will bear a weak solution of mercury, occasionally applied at first as a lotion and afterwards as a continued dressing. A grain of the bichloride dissolved in an ounce and a half of water, with the addition of three or four minims of the hydro-chloric acid, forms a good lotion; and alternated with the astringent already mentioned or with one of the metallic sulphates, will be found to correct the sore as soon as any application that can be used. Smearing the sore with oil, at each time of micturition, protects it from the contact of the urine, and is not to be neglected."

Phagedenic ulceration is a state of weakness accompanied with an excess of action in both the vascular and nervous systems of the part. The part first becomes inflamed and exceedingly painful; the inflamed tissue breaks down and ulcerates away; the former healthy limits of the sore in their turn undergo the same process. The constitutional state of the patient is evinced in his irritability of manner and loss of rest; in his pallid aspect and slightly vascular conjunctiva, while the heart acts quickly but feebly. An inflamed ulcer must be distinguished from an irritable one, as the treatment applicable to the one tends to the aggravation of the worst features of the other. The vivid colour, the fibrinous deposit, the ichorous discharge, and the thickened edge, all evince the presence of inflammation; while the absence of these signs, and in their place a degree of sensitiveness disproportioned to the extent of inflammation, or a disposition to spread by ulceration, is evidence of an irritable state of sore. But though the extreme of each class is thus distinct enough, the line that divides them is not so clear or defined; and many sores exhibit more or less of both characters.

"The degree of inflammation present and the degree of irritability existing, combined in various proportions, are the conditions that modify the progress of ulcers; and the discernment of the practitioner should be directed to ascertain

in what proportions they do exist. Inflammation in one degree, combined with irritability, leads to destructive ulceration; a higher degree of inflammation with a less degree of irritability leads to the yellow slough; while the highest degree of inflammatory action with a farther diminished irritability, produces the dark slough or common gangrene."

Mercurial action is here wholly inadmissible. It tends to increase irritability, to lessen the powers of the patient, and therefore to quicken phagedenic action. Opium must be given to obtain rest. Moderate doses only are required, except in some few cases that hard drinking and debauchery have rendered uncontrollable by smaller doses of opium. When this object is obtained the sore improves. If the case be marked by both vascular and nervous excitement, the cold infusion of sarsaparilla is the remedy that deserves our confidence. When, in place of a vascular conjunctiva and flushed face and white tongue, the aspect bears marks of depression and debility, quinine or ammonia, or similar stimulants, are called for. The remedy, however, on which most reliance is to be placed is iodine and its combinations; its property seems to be stimulant or tonic; it increases vital energy and action, rendering the pulse strong and full; it improves the appetite and powers of digestion; hence its benefit seems especially adapted to that kind of ulceration which depends on want of power combined with excess of irritability. Both in primary and secondary forms of this disease it is often found to arrest the progress of syphilitic action in persons who cannot bear mercury. This latter medicine and iodine have each their respective benefits, and it may be said that iodine fulfils that in which mercury is deficient. In the normal forms of syphilis the latter is rarely found to disappoint the practitioner; and in the anormal forms of primary sore it rarely fails to do harm; while iodine exerts, comparatively, little influence on the normal chancre, chiefly confining its good services to the sores that are set astray by some peculiarity of constitution. The action of mercury is sometimes wanted for sores that, having shown an anormal disposition, have lost ground under the action of iodine, and yet will not granulate or cicatrize. The syphilitic action still predominates in them and yields only to mercury, which, then, should be given in the most guarded manner.

The second part of Mr. Key's report is principally taken up with observations on the nature and treatment of phymosis in connexion with chancre. The author enters also at length into a defence of Mr. Hunter from charges of certain erroneous views regarding syphilis attributed to him, and has come to the conclusion that the principles both of Mr. Hunter and of Mr. Carmichael are those by which the profession is guided in its views regarding the nature of the disease and the employment of remedies for it.

No. XII.

The first article in this number is on *Epilepsy*, from the pen of Dr. B. G. Babington; its objects are, in the words of the author, "to give some illustrations of the disease itself, and to offer some reasons for thinking that it depends on a functional not on a structural change; and some grounds for the belief that in many instances it admits of cure."

In pursuance of these objects the author proceeds briefly to notice the appearances exhibited by the disease during its attack in man and in those domestic animals which are liable to it, and to narrate some cases illus-

trating its principal varieties. The last of these cases is cited by the author as a striking example of the intermittent character which epilepsy occasionally exhibits. It is so remarkable a case that we shall extract it in an abbreviated form.

A married woman, æt. 50, after severe mental distress on account of the loss of her children, became subject to fits of crying in her 44th year; headaches, to which she had always been liable, soon after became frequent and violent; to these succeeded pain at the side of the neck, extending in an hour or two to the top of the head, and confining her to bed. In her 46th year she had a fit that lasted four hours and a half, and kept her in bed a fortnight; and from this time she became affected every alternate day, at first with fits of crying of six hours' duration, afterwards by a state approaching to catalepsy, and finally by a state of imbecility, during which she was incapable of making the slightest exertion. A second epileptic fit occurred at the end of two years, a third eleven months after the second; and from this time they recurred every two or three months, the attack always happening on an ill day. On the intervening days "she is as well as ever she was in her life;" and "such is the regularity," adds Dr. Babington, "with which these alternations occur, that she may be considered as having her life equally divided between sanity and imbecility."

In endeavouring to investigate the nature of epilepsy we may proceed, as the author observes, either to seek for its analogy with other physiological or morbid states of the body; to infer its nature from the constitution, age, &c. of those attacked; to trace its remote effects on the living or dead body; or, lastly, to note what kind of remedies are most useful in treating it. On each of these points the author makes some observations, but we must confine our notice to the first and last. After comparing the phenomena of epilepsy to those of nightmare, of mesmeric convulsions, and of concussion from a blow on the head, to the latter of which he considers them most analogous—

"I would wish," continues Dr. Babington, "to draw attention to an analogy which exists between the functions of the brain as affecting reason and consciousness, attributes of the sensorium, and the functions of the nerves as affecting sensation and motion. It seems to me that the same cause, whatever it be, which operates on one portion of the sensorium, so as to produce consciousness and reason, when operating on another portion of the sensorium, namely, that which is connected with the nerves of sensation and motion, produces corresponding results in the phenomena which they exhibit. Thus, I venture to think, that profound coma of the sensorium has its analogy in total paralysis of sensation and motion of the trunk and extremities; and that, in all probability, a similar cause to that which suspends reason and consciousness in the one case, suspends sense and motion in the other. Thus again, where reason and consciousness and the other attributes of the mind are not suspended, but disordered and uncontrolled; where, in other words, there exists a state of mental imbecility, insanity or idiotism—I find an analogy between this state and that of chorea, affecting the nerves of sensation and voluntary motion of the trunk and extremities. Lastly, I find an analogy between the sudden affection of the sensorium which we are now considering, namely, epilepsy, and that particular morbid affection of the nerves of sensation and motion which is productive of muscular spasm or cramp."

On the subject of treatment Dr. Babington observes:

"Where the irritation, which I suppose to be the proximate cause of epilepsy,

is complicated with and dependent on organic change of structure, we cannot of course expect that treatment of whatever kind will either effect a cure or throw any light on its cause. Again: where it is combined with a state of plethora, never occurring but when the person is in a horizontal position or after a full meal, and attacking those whose habit is gross and circulation forcible, means the opposite of those which would be adopted to remove the proximate cause may be needed. Bloodletting may be necessary, as well as active evacuants, and an antiphlogistic line of treatment; but with these exceptions, it is my belief that the same class of remedies which is best calculated to give tone to the system under tic douloureux, and other spasmodic states of the nerves of motion and sensation dependent on nervous irritation, is also most applicable to this disease; and that the various preparations of bark, of iron, of arsenic, of silver, or of zinc, are those from the administration of which we shall derive the greatest advantage."

Amongst them Dr. Babington, on the whole, gives the preference to the sulphate of zinc, which, if not quite so efficacious as the nitrate of silver, is free from the objections to which the latter is subject. "Like all other remedies," he observes, "it will often fail; but whether useful or not, it has the merit of being a safe remedy, and that when continued for a longer period and in very much larger doses than is usually supposed." In some cases the author has given as much as thirty-six grains three times a day, and though these large doses are by no means always necessary, they sometimes are so. He has found this amount taken equally as well in solution as in pills, care being taken gradually to increase the dose as in the case of tartar emetic.

The second paper, by Mr. A. S. Taylor, is a *Contribution towards Determining the minimum dose of Arsenic necessary to destroy Human Life.* This is a question to which it is desirable on many accounts to obtain a correct answer, but at present no sufficient data exist on which to ground one, and accordingly authorities differ much in their estimate of the dose required to cause death.

"According to Hahnemann," observes Mr. Taylor, "two grains of arsenic might suffice to destroy life in the course of a few days, but he adduces no case in support of this opinion. Dr. Christison, in his Treatise on Poisons states, that the smallest dose of the *solid* poison which he has read of amounted to thirty grains of the powder. This dose proved fatal in six days. The smallest actually fatal dose he has found recorded is four grains and a half, but in this case the subject was four years old: death took place in six hours. In this instance the poison was taken in solution."

Dr. Lachen, of Angers,* inferred, from a case which came under his notice, that even less than two grains may prove fatal; but we agree with Mr. Taylor, that in this instance too many sources of error were present to allow of our placing much confidence in the conclusions drawn.

Mr. Taylor proceeds to relate a case of poisoning by arsenic accidentally contained in port wine, where severe vomiting, lasting three or four hours, was caused in a child æt. 16 months, a lady æt. 52, and a gentleman æt. 40, by doses of one third of a grain, one grain and a half, and two grains and a half respectively. Several days elapsed before the gentleman quite recovered, but eventually all three did well: a result, in part at least, to be attributed to the speedy rejection of the poison along with the food they had just taken.

* British and Foreign Med. Rev. Oct. 1839.

On examination it was found that not less than 28·8 grains of arsenic were dissolved in the twenty-four ounces of wine; besides which, sixty grains remained in powder at the bottom of the bottle. How the wine had become contaminated could not be ascertained, but it had been received in that state from the merchant; and in all probability, as Mr. Taylor suggests, the bottle had been formerly used to contain some arsenical preparation for destroying vermin, and had not been properly cleaned; in support of this opinion he notices several similar cases, one of which proved fatal.

The third paper by Mr. King is a continuation of one* which we noticed in a former number of this Review, *on the Safety-valve Function of the Heart*. In the next article Mr. Lever has furnished *a Statistical Report of the Guy's Hospital Lying-in Charity*, from its establishment in 1833 to 1840, during which time 4664 women have received the benefit of the charity. Mr. Lever has embodied in his report, tables showing the number of each sex born alive and those still-born; the varieties of labour which ended in still-birth; and the proportionate frequency of the various kinds of labour: for these we must refer to the original paper. He has added some remarks, in which he compares the results exhibited by these tables with those furnished by other institutions; some of these we shall notice.

Face presentations appear to have been comparatively much more frequent at Guy's than in the Dublin Lying-in Hospital; in the former they were as 1 in 179 of all cases, in the latter as 1 in 504. Merriman gives 1 in 450, Mad. Boivin 1 in 275, as the proportion. Premature birth was induced in 6 cases; in only 1 was the child born alive. Secale was but sparingly used, only once in 292 deliveries, affording a creditable contrast to the practice of those gentlemen who boast their experience of its efficacy in *hundreds* of cases. The preparation employed was the æthereal tincture, made as recommended by Dr. Rees, in the Med. Gaz., for April 4, 1840. The instrumental deliveries were 46 in number, or as 1 in 101 cases; in Dublin the proportion was 1 in 114: of these, 9 were delivered by forceps, 12 by the vectis, and in 25 cases the head was lessened. Of the 9 delivered by forceps, 5 children were still-born, and only 1 of the 12 in which the vectis was used. Thirty-three were twin cases, or 1 in 141 births, a low proportion as compared with England generally, in which twin births are as 1 to 92, or Scotland, where they are as 1 in 95, or Ireland, where the proportion is 1 in 62. Fourteen cases of placenta presentation occurred, or 1 in 333 births, a proportion four times as great as that found in the Dublin Institution, where they were as 1 in 1492; two of the cases ended fatally to the mother, and eight of the children were still-born. Forty deaths of women occurred during the seven years, or 1 in 117 cases; the great majority of these were from puerperal fever. This result must, we think, be looked on as a favorable one, especially, as the author observes, "when we take into consideration the miserable condition of many of the patients admitted to the benefits of the institution, the privations they undergo, and particularly the intemperance and imprudence in which many of them indulge." This is a valuable communication, and highly creditable to the author.

* Guy's Hospital Reports, No. iv.—Br. and For. Med. Rev. Oct. 1837.

The next is a valuable paper from the pen of Dr. Bird, *on Electricity as a remedial agent in the Treatment of Diseases.*—This is one of many remedies which has suffered from injudicious advocacy; for whilst some have attributed to it extraordinary and mysterious powers, others, failing to discover these, have decried it as unworthy of the slightest confidence; and with the majority of practitioners it has either fallen into entire disuse, or is employed only in some few cases when everything else has failed. In consequence of this the management of this remedy has been left in the hands of unprofessional persons, who, anxious to extol their specific, and incapable of distinguishing between cases likely to be benefited by it and those in which it can do no good, electrify all who come, puff the cures, and are discreetly silent about all failures. Now as we are satisfied that electricity does occasionally prove eminently serviceable, we welcome any judicious attempt to place it on its proper footing as a remedial agent. This we consider Dr. Bird's to be, who has pointed out those diseases in which it has been found most serviceable, and has related a number of cases of each treated in the hospital, sufficient, we think, to prevent the suspicion that the physician has deceived himself in estimating the effects of his remedy.

Electricity is employed at Guy's Hospital, either in the form of sparks drawn from the patient in shocks from the Leyden jar, or in galvanic shocks administered by means of an electro-magnetic apparatus.

The diseases in which it has proved most serviceable in the hands of Dr. Bird, who superintends its use in all cases treated at the hospital, are chorea, paralysis from functional disorders of the nervous system, and amenorrhœa. "A considerable number of cases of chorea," observes Dr. Bird, "have been submitted to electrical treatment since the publication of Dr. Addison's paper on the use of electricity in certain spasmodic affections, in a former number of these Reports. Of these cases notes of thirty-six have been preserved in the report-book, the histories of the others not being complete from the irregular attendance of the patients."

Of the above thirty-six cases, twenty-nine are reported to have been cured, and five relieved; one experienced no relief, and one left under alarm at the remedy. In the majority of these cases the only medicines contemporaneously administered were occasionally mild purgatives, and these as well as other medicines had often been employed before without avail. The form of electricity employed was that of sparks taken in the course of the spinal column every alternate day, for about five minutes each time, or until the appearance of a papular eruption, often excited by the remedy in this form. Several cases are detailed at length, but for these we must refer our readers to the work; the sixth affords a remarkable instance of spasmodic action of the muscles of the neck, causing involuntary movement of the head towards one side or the other with such force as to threaten strangulation, and occurring so frequently as to oblige the patient to keep his head steady whilst walking by holding his nose; in this instance electricity was of decided service.

*Chorea.* Dr. Bird "has never seen any good effect to result in cases of chorea from the transmission of electric shocks along the affected limbs: on the contrary, in every instance the involuntary movements have been increased, often to an alarming extent; and if employed when the patient was convalescent, it has invariably aggravated every symptom, and often rendered the patient as bad as when first admitted under treatment."

"*Paralysis.* Paralytic affections constitute a prominent feature in the cases which have been referred to the electrical room of the hospital; of these, forty-four cases have been fully reported by the clinical clerks, and may be found on record in the hospital books. Of these cases it may be generally remarked, that those in which the paralysis, whether of sensation or motion, or both, depended upon exposure to cold or rheumatism, upon some functional affection often of a local character, or on the impression produced by effusion in some part of the cerebro-spinal centre which had become absorbed under the influence of previous treatment, the result of the application of electricity was most successful, whilst in those cases in which the paralysis depended upon some persistent structural lesion, whether produced by accident or otherwise, I never saw the slightest benefit result."

In dropped hand from the effects of lead, electricity was employed in the form of sparks drawn from the upper part of the spine. In the majority of cases medicines were also administered, as the general health is usually deranged in this affection. Eleven cases are reported, of which five were cured, and four relieved under the use of electricity; in two cases no relief ensued.

Of rheumatic paralysis arising from exposure to damp, a current of cold air, &c. ten cases are reported, of which five were cured, three relieved, and two derived no benefit from electricity.

Of thirteen cases of paralysis from affection of the nervous centres, but where no proof existed of the presence of organic lesion, six were cured, and two were relieved under the use of electricity, four received no benefit.

Amongst the cases narrated is the following one which shows well the almost instantaneous effect occasionally resulting from the use of electricity; it would have proved quite a godsend to a lay electro-magnetist:

"Stephen Burn, aged eleven, admitted Jan. 15, 1840, into the hospital, under Dr. Addison, labouring under total loss of motion of the right leg and side, which appeared seven weeks before, without any very apparent cause, whilst in bed. He has been gradually getting worse, and has been cupped, taken mercury, had his head shaved, &c. without any marked benefit. He was carried from the ward into the electrical room being quite unable to walk. Sparks were freely drawn from the spine and affected limbs. The effect was remarkable, for the boy almost immediately recovered power over the previously paralyzed side, and he walked back into the ward with only the aid of a stick. After attending a few days longer he was presented completely well.

"*Amenorrhœa.* Scarcely any cases have been submitted to electrical treatment in which its sanatory influence has been so strongly marked as in those in which the menstrual function was deficient. ..... The rule for ensuring success in the great mass of cases of amenorrhœa is sufficiently simple: improve the general health by exercise and tonics; remove the accumulations often present in the bowels by appropriate purgatives; and then a few electrical shocks, often a single one, will be sufficient to produce menstruation, and at once to restore the previously deficient function. ..... The mode of applying the electric shocks in these cases is the following: Let the patient be placed on a chair or stool, press the brass knob of a director against the sacrum, and if the stays be loosened, so that only the linen intervenes between the latter and the knob, no further exposure is necessary. A second director, furnished with a chain connected with the outside of an electric jar, is passed by the female attendant under the patient's dress, and the knob is pressed against the pubes. The jar is then charged, and its ball touched by a third director connected with the one held against the sacrum by means of a chain. The shock thus passes through the patient's pelvis: and should be repeated ten or a dozen times. The jar employed should hold about a quart, and be about half charged."

In confirmation of the above observations, a table is given showing the result in twenty-four cases of amenorrhœa treated by electricity; four cases of well-marked chlorosis with an irritable state of the uterus, and in these no benefit was derived; the other twenty were free from this complication, and all were cured.

In a paper *on the Real and Supposed Pathological Conditions of the Urine*, Dr. Rees recurs to a subject treated by him formerly in a paper published in the Medical Gazette, namely, the appearance of a precipitate simulating albumen, when nitric acid is added to the urine of patients who are taking either copaiba or cubebs. As a ready mode of distinguishing between the true albuminous precipitate and these resemblances, Dr. Rees recommends the use of ferro-cyanuret of potassium as a precipitant, the urine having been first acidulated with acetic acid. Albumen if present is immediately thrown down, but no precipitate ensues where the appearance has been due to the other causes mentioned; or if the acid should have clouded the urine, this is not increased by the addition of the ferro-cyanuret.

Where heat is used as the test for albumen in urine, an error may arise from the appearance of a precipitate due to the presence of earthy phosphates. The frequency of such an occurrence is greater than might have been anticipated, amounting to not less than 7 per cent. in 482 cases taken promiscuously from the hospital wards, showing how important an error might be committed, in endeavouring to ascertain the comparative frequency of albuminous urine, if heat only were employed as the test.

It has been stated that the urine becomes albuminous during salivation, Dr. Rees shows that at least this does not always take place, since in fifteen cases in which the urine was tested during salivation, no albumen was found to exist.

In a short paper *on Stricture of the Urethra, Catheterism, and False Passage*, Mr. Cock has thrown together some cases intended as a warning to surgeons who are addicted to the heroic method of treating old strictures by what our neighbours call *catheterisme forcé*. Several of the cases afford striking illustrations of the bad results frequently attendant on this mode of treatment, and the observations which accompany them are very judicious.

*Cursory Observations on some Cerebral Affections of Children.* By H. M. Hughes, M.D.—The principal object of this paper is to state shortly some of the difficulties attendant on the treatment of the cerebral diseases of children; especially as regards the diagnosis between infantile fever or, as Dr. Hughes prefers to call, "irritative fever of children," and hydrocephalus; and between the latter complaint and the hydrencephaloid affection described by Dr. Marshall Hall.

Of the close alliance between infantile fever and hydrocephalus, and of the difficulties which not unfrequently prevent our coming to a decided opinion on the nature of the case, in the early stage at least, every practical man must, we should have thought, have been aware, had not a late writer, quoted by Dr. Hughes, asserted "that the two diseases can scarcely be confounded." Dr. Hughes thinks that in many of those

cases in which hydrocephalus appears to supervene on irritative fever, the progress of the case has been really such as it appears to have been, and that complication does not always exist from the commencement of the malady, an opinion in which we agree. Nor, we may add, is hydrocephalus the only disease which may be thus excited by infantile fever. In the same way, tubercular disease in the lungs and bronchial glands of children may be developed, if it do not actually originate during the progress of infantile fever; the tubercles if previously existing, of which there is often no evidence, being at all events in a latent state, and thus the disease which begins as infantile fever may end as pulmonary consumption. The following are the symptoms by which Dr. Hughes thinks we may generally distinguish between hydrocephalus and simple irritative fever:

"In the first stage of acute hydrocephalus, there generally exist some intolerance of light and sound, contracted pupils, and wakefulness by night and by day; while in remittent fever the patient though restless at night often sleeps soundly and comfortably during the day, the pupils are rather dilated, and light and sound are not complained of. The pain of the head in the latter affection is rather a general uneasiness, giving the child an expression of heaviness and languor, and, like the febrile symptoms themselves, is distinctly remittent; in the former it is almost always referred to the forehead, and though increased in severe paroxysms, is constant. The child suffering from acute hydrocephalus lays its head on the pillow, with closed eyes, and appears unwilling to be moved, questioned, or noticed, unconsciously moves its hands up to or over its head, and often screams and starts from severe accessions of pain, while its arms or legs are affected with slight spasmodic twitchings. That affected with remittent fever, on the other hand, is usually easily and not unwillingly roused, and though fractious and petulant, has not violent fits of screaming, moves its head without inconvenience, and while awake is almost always occupied in picking its lips or nose. The bowels are sometimes constipated in both complaints; but they are more easily moved, and when moved are more easily kept in a relaxed condition, and the motions are more slimy, fetid, and dark coloured, in the simply febrile than in the inflammatory complaint. The pulse also, which in the fever is almost sharp and frequent, is in the more grave affection often sluggish, tardy, and irregular."

In the above enumeration, Dr. Hughes has omitted to notice vomiting. This symptom, though not unfrequently present in simple infantile fever, is less constant and less urgent in that disease than in the first stage of hydrocephalus. In acute hydrocephalus vomiting is one of the most frequently present of the early symptoms, and though it may last only for one day or even less, it is generally very urgent whilst it lasts, everything being rejected which the child swallows. When this symptom is present, with a belly flaccid and free from tenderness on pressure, it is, we think, one of the most characteristic that can be mentioned of incipient hydrocephalus.

In an article *On the Existence of Arsenic as a natural constituent of the Human Bones*, Dr. Rees has detailed a series of experiments upon large portions of bone earth, prepared as directed by Orfila, and tested by means of Marsh's apparatus and by sulphuretted hydrogen, which latter was ascertained to be the more delicate test of the two. By neither of these means could the slightest trace of arsenic be detected, and Dr. Rees comes to the conclusion that Orfila is mistaken in stating

that this metal naturally exists in human bones, and that by some unfortunate accident it must have been present, either in his apparatus or in the materials used during his analysis.

In the next paper, Mr. Taylor, after briefly noticing those metals which chemists have succeeded in detecting in the fluids or solids of animals, to which they had been administered, relates some experiments by which he satisfied himself of the presence of lead in very minute quantity in the milk of a cow which had eaten a pot of paint containing the mineral.

Mr. Henry Oldham gives an *Account of a Fœtus in Utero invested by False Membrane*, with the view of "showing the extent to which the cutaneous surface of the embryo may be inflamed, and that the inflammatory product may be the effective cause of certain deformities of the fœtus."

"On opening the amnion a quantity of dark-coloured fluid escaped ..... Patches of a reddish-brown coloured deposit were visible on the amnion ..... The fœtus was covered throughout with a coating of a similar red-brown colour, dotted and gritty on its surface, which masked the face, so as completely to obscure the features. The limbs, thus surrounded, appeared rigid and deformed. On detaching a portion of this covering from the face, it was found to be a firm, well-formed membrane, which could be peeled off from the subjacent cutaneous surface: and when removed, the features, as the eye, nose, and mouth, were detected properly developed, the nose only being slightly flattened. This membrane was equally thick and firm throughout the body, and appeared to exert a constrictive and constraining force on several parts, producing deformities."

To the above succeed several short papers of more or less interest; amongst others *a remarkable Case of Enlargement of the Breast*, related by Dr. Ashwell, which we must, though unwillingly, pass by without further observation, and proceed briefly to notice the two remaining articles contained in this volume.

The first of these, by Dr. G. H. Barlow, is entitled *Observations on the Laws which regulate the deposition of Tubercles, with practical inferences applicable to the prophylactic Treatment of Phthisis.* In our enquiries into the laws which regulate the establishment of tubercular disease two principal questions suggest themselves: 1st. As to the causes that produce the tubercular diathesis. 2d. As tc the local conditions which favour the development of tubercles in any particular organ. The latter of these Dr. Barlow proposes to consider, and in reply to it propounds as a law: "*That any organ is most liable to become the seat of tuberculous deposit when its vascular and functional activity bears the greatest ratio to the other organs of the body.*" In illustration of this law, he cites, as a well-known fact, the greater frequency of tubercles in the brains of young children, in whom that organ is relatively largest; in the abdominal viscera of older children; and in the lungs of youths at the time when these organs are attaining their full development. Again; in children, the absorbent system is in its greatest activity, whilst the genital organs are inert; in the former tubercles are very frequent, in the latter as rare.

"It is, however, in the lungs," proceeds the author, "the most frequent seat of tubercular disease, that we are to look for the full elucidation of the laws which regulate its development, and I am especially anxious to direct attention to this part of the subject, as from it arguments have been drawn which have been thought to oppose the law which I have enunciated."

We shall endeavour to give an abstract of the arguments adduced by the author.

Previously to birth, when the liver is the depuratory organ, and the function of the lungs is at zero, tubercles rarely exist in the latter organs. In childhood, though often present in the lungs, they are generally less advanced than in any other more active organs, as the brain, or mesenteric glands. In early youth, Dr. Barlow conceives that a change takes place in the proportionate activity of the liver and lungs, the latter being developed at the expense of the former. When the lungs are sound and the chest large, this goes on safely; but if otherwise, the liver retains its former proportion, and the belly is tumid and respiration impeded; or the lung is over-excited and disease occurs. Hence the necessity of attention to the prevention of phthisis at this time. In hot and moist climates, like India, the liver is active and relieves the lungs from part of their duties; whereas in temperate climates, the liver being less active, unless stimulated by animal food, the lungs are over-worked and become prone to tubercular disease. Again, as regards the part of the lungs in which tubercles are most frequent, Dr. Barlow attributes their constant presence at the summit of the lungs to the greater activity of function in this part, in which he is directly opposed to the views of Dr. Carswell. The check given to the progress of tubercles in the chest during the increased activity of the uterus in pregnancy; and the absence of tubercles from portions of lung which have been compressed in cases of pneumo-thorax, the result of phthisis, afford, in the author's opinion, further proofs of the truth of this law, as does the frequent presence of bronchitis as a precursor of tubercles in the lungs, and militate, in his opinion, against the opinion of Dr. Stokes, that atrophy of the lungs attends the early stages of tubercles.

The arguments adduced by Dr. Barlow in support of his views, and which we have very briefly and imperfectly noticed, though not conclusive, are ingenious and well worthy attentive consideration, and as such we commend them to the reader.

The last paper contains *A brief History of the last Illness of Sir A. Cooper, and of the examination after death of his body.* It is carefully and neatly drawn up, and will scarcely admit of condensation. Considerable emphysema of the lungs with enlargement of the heart were the most important morbid appearances discovered after death; Sir Astley Cooper had also directed special attention to the remains of two cured herniæ, an umbilical and a congenital inguinal, the appearances of which are well described.

## Art. III.

1. *Maladies des Enfans. Affections de Poitrine.* Première Partie, Pneumonie. Par MM. Rilliet et Barthez.—*Paris*, 1838. 8vo, pp. 238.
*A Treatise on the Diseases of Children. Affections of the Chest.* First Part, Pneumonia. By Messrs. Rilliet and Barthez.

2. *Recherches sur la Bronchite capillaire, purulente et pseudo-membraneuse, (Catarrhe suffocant, Croup bronchique), chez les Enfants.* Par S. A. Fauvel, m.d. &c.—*Paris*, 1840. 4to, pp. 100.
*Researches on the purulent and pseudo-membranous form of Capillary Bronchitis in Children, (otherwise called suffocative Catarrh, or bronchial Croup.)* By S. A. Fauvel, d.m.p.—*Paris*, 1840.

3. *Ueber die acute Bronchitis der Kinder, und ihr Verhältniss zu den verwandten Krankheitsformen.* Von Dr. W. Cruse, &c.—*Königsberg*, 1839.
*On the acute Bronchitis of Children, and its Relation to similar Diseases.* By Dr. W. Cruse, &c.—*Königsberg*, 1839. 8vo, pp. 187.

4. *Ein Beitrag zur Lehre von der Bronchitis der Kinder.* Von Dr. R. Küttner, pract. Arzte in Dresden. (*Casper's Wochenschrift, Juni*, 1841.)—*Berlin.*
*An Essay on the Bronchitis of Children.* By Dr. Küttner, of Dresden. —*Berlin*, 1841.

In his treatise, "De aere, aquis et locis," the father of our art has mentioned the precautions which a practitioner of medicine should observe on settling in a foreign city. He directs him to examine the situation of the place, its climate, and the direction from which the wind ordinarily blows. The food, the dress, the habits, occupations and amusements of the inhabitants are all to be carefully noted; for, without a knowledge of these things no one can judge correctly of their diseases, nor adopt the measures most proper for their cure.

The lapse of ages has not detracted from the value of Hippocrates's advice; but our countrymen appear sometimes to have forgotten its importance, when appealing to the authority of French writers on the diseases of children. The field in which MM. Billard, Valleix, Denis, Berton, and others laboured, is one to which England, fortunately as we think, presents no parallel. No one would regard the strange and fearful forms of disease which he might observe in a city wasted by pestilence or famine, as affording a correct idea of the habitual health of the inhabitants; though to do so would be little less illogical than to apply to the diseases of children in general the results afforded by the hospitals of Paris.

There are two hospitals in Paris appropriated to children, the *Hospice des Enfans Trouvés, and the Hôpital des Enfans Malades.* The foundling hospital receives children from their birth until two years of age, and, like similar institutions in the rest of France, places them at nurse, and provides for their after support. The mortality in these institutions is frightfully great. Sixty per cent. of the foundlings throughout the whole of France die before attaining the age of one year; though the discrepancies are extreme between the reported mortality in different provinces; being, according to M. de Chateauneuf, only 15·25 per cent.

in Alsace, while in le Maine and the Bourbonnais it is 75·50, and 80·04 per cent.* The average stay of children in the Hospice at Paris does not exceed 9½ days, during which short time the mortality is 1 in 4·3, while the proportion of deaths to recoveries among the sick is nearly as four to one.†

With this high rate of mortality in the foundling hospital at Paris, let us contrast that in the lying-in hospital at Leipsig, where the children remain fifteen days, and die in the proportion of 1 to 41·13.‡ The number of cases yielding this result was indeed very limited, being only 1284, but the report of Dr. Collins gives us a still more favorable result, though deduced from 16,654 cases; 284 only of which, or 1 in 58½, died during a residence of eight, nine, or ten days in the hospital.§ So powerful an influence do warmth, pure air, and the suckling of infants by their own mothers exert in diminishing their mortality. In 1782 nearly a sixth of all children born in the Dublin lying-in hospital died within the first fortnight, and the present low rate of mortality is above all to be attributed to the excellent ventilation and scrupulous cleanliness which pervade that noble institution.

We should not, however, insist thus upon the mortality of the hospital at Paris, if the children there died of the same diseases as when placed under other circumstances; but that is not the case. When large numbers of young children are congregated together, diseases prevail among them endemically which are hardly met with elsewhere. In Paris the *endurcissement du tissu cellulaire* and *muguet* prevail in the same way as trismus did formerly in Dublin.‖ The various organs, and especially the lungs, acquire a proneness to inflammation truly surprising. In 1834, M. Savatier published a paper in La Clinique,¶ founded on the dissection of thirty-four children who formed the whole mortality under M. Baron, between September 14 and October 5, 1827. He found the lungs more or less inflamed in every instance, on the strength of which fact he asserts that pneumonia invariably complicates all diseases of new-born infants. M. Valleix embodies, in his essay on Infantile Pneumonia, the results of 114 dissections made by M. Vernois.** The cases were examined indiscriminately during the months of February, April, and March; and in 113 hepatization of the lungs was found to exist. M. Vernois' observations indeed were made during the prevalence of influenza, but the investigations of M. Savatier, which were not influenced

* History of public charity in France, by D. Johnstone, M.D., Edinb. 1829. 8vo. p. 367. It appears from the Registrar-General's Report, that the mortality of children under one year old, throughout England and Wales, does not exceed 14·6 per cent.

† Quêtelet, Sur l'Homme, &c. tome i. p. 258.

‡ This statement is contained in a letter from Prof. Jörg to Dr. M. Edwards, and is quoted by him in a paper, "De l'influence de la Température sur la Mortalité des Enfans nouveau-nés," in the Annales d'Hygiène Publique, t. ii. p. 304. For an account of the management of this institution, see E. G. Güntz, De viâ ac ratione quâ in Instituto Trieriano artis obstetriciæ usus et docetur et exercetur. Lipsiæ, 1837. 4to.

§ A Practical Treatise on Midwifery, by R. Collins, M.D. London, 1836. 8vo, pp. 500.

‖ It is a singular fact that trismus neonatorum is almost unknown in the hospitals of Paris. Some time since a friend mentioned to us that Professor Dubois stated in a letter to him, that in the whole course of his practice he had met with but one or two cases of this disease.

¶ Extracted in Froriep's Notizen, Band xix, No. xxi.

** Clinique des Maladies des Enfans noveau nés, Chap. ii. Paris, 1838. 8vo.

by any such cause, yielded, as we have seen, a precisely similar result. But surely these facts would not warrant any one in concluding that pneumonia attacks new-born infants in general in any such proportion.

It may perhaps be thought that similar causes cannot exert a pernicious influence upon the inmates of the Hôpital des Enfans Malades, who are not admitted below two years of age. All, however, who have been connected with that institution, concur in representing the mortality as very great. "It is necessary," says one of these gentlemen,* "to have frequented children's hospitals for any one to be able to form a correct notion of the injurious influence of an atmosphere loaded with the miasmata which are constantly disengaged from the bodies and clothes of the patients steeped in urine. It is doubtless to this circumstance that we must attribute the high rate of mortality in those wards which are occupied by the youngest children. If children but slightly indisposed are placed in one of these wards, they soon become weak and thin, and usually are seized by pneumonic catarrh or enterocolitis, diseases which under these circumstances are always very serious. The strongest constitution cannot shield them from the debility which results from the insalubrity of the air they constantly breathe, and this debility instead of preserving them from inflammations, seems to predispose certain organs to it."

But the following table† of the mortality in the Hôpital des Enfans Malades during the year 1822, will speak for itself:

| | Acute Diseases. | | | Chronic Diseases. | | | Convalescent | Total. |
|---|---|---|---|---|---|---|---|---|
| | Medical. | Variolous | Surgical. | Scrofula. | Tinea. | Itch. | | |
| Remaining on Jan. 1, 1822. | 75 | 13 | 64 | 79 | 83 | 30 | 9 | 407 |
| Entered during the year. | 1783 | 227 | 213 | 118 | 197 | 479 | 26 | 3142 |
| Died | 519 | 106 | 49 | 35 | 0 | 0 | 0 | 709 |
| Mortality. | 1 in 2 $\frac{41}{100}$ | 1 in 2 $\frac{15}{100}$ | 1 in 7 $\frac{10}{100}$ | 1 in 4 $\frac{33}{100}$ | 0 | 0 | 0 | 1 in 4 $\frac{43}{100}$ |

The investigations of M. Becquerel‡ prove that a very large proportion of the deaths in this institution result from pneumonia. He says that between April and October, 1838, out of 421 patients labouring under acute diseases, dartrous or ophthalmic affections, 133 died, and on a post-mortem examination being made, the lungs of 49 were found to have been attacked by pneumonia. From this statement it results, that in more than one third of the fatal cases, death was produced, either directly or indirectly, by pneumonia, and this, too, at a season of the year when cold, which is regarded as one of the most powerful exciting causes of that disease, could have had but little influence in its production.§ From his observations M. Becquerel concludes that

* L. Senn, Recherches sur la Meningite Aigue des Enfans; Paris, 1825. 8vo, p. 125.
† Johnstone, Op. cit. p. 393.
‡ Archives Générales de Médecine, 1839, p. 437.
§ For many interesting details respecting the statistics of pneumonia, see Recherches Statistiques sur la Pneumonie, &c., par M. J. Pelletan, in the Bulletin de l'Académie Royale de Médecine, tome iv. p. 447.

pneumonia supervenes rarely in perfectly healthy children; that it occurs most frequently in those who are exhausted by previous disease, or placed in unfavorable hygienic conditions; and that it comes on in the course of acute diseases of specific adynamic character. He further regards uncomplicated pneumonia as rare, in which opinion he is confirmed by the testimony of all French writers, some of whom, as MM. Gerhard and Rufz, have even denied its existence as an idiopathic affection, while MM. Rilliet and Barthez, (p. 77,) though they do not deny its occasional occurrence, state that of forty cases of pneumonia in children under five years of age, only three were uncomplicated.

Thus much we thought it necessary to remark upon the peculiarities of the institutions in which the French have studied the diseases of children. But, having guarded our readers against hasty generalizations, we will now pass to the examination of the works whose titles stand at the head of this article. We have arranged them in what appeared to us to be the order of merit. MM. Rilliet and Barthez and M. Fauvel were all *internes* of the Hôpital des Enfans Malades, and the last-named gentleman is secretary to the Société Médicale d'Observation. All three are evidently careful and competent observers, and MM. Rilliet and Barthez possess the talent of saying much in a short compass, and clearly. M. Cruse, a physician at Königsberg, and one of the lecturers attached to the University, is a man of much learning and ingenuity, but not sufficiently careful to distinguish fact from theory, and much given to indulge in long and excessively tedious digressions. Sixty cases form the basis of the work of MM. Rilliet and Barthez; M. Fauvel builds a theory upon eight, the accuracy with which they were noticed making up, in his opinion, for the smallness of their number; while M. Cruse, who labours to establish the distinctions between pneumonia and bronchitis in children, does not mention the number of cases which came under his observation. We shall endeavour to give a condensed analysis of MM. Rilliet and Barthez' treatise, noticing as we proceed whatever may be particularly important in the remarks of M. Fauvel or of M. Cruse.

In selecting sixty observations for analysis, MM. Rilliet and Barthez have wisely left out of consideration all cases which came under their notice before they had prepared themselves for their task by a long and attentive study of the condition of the chest in healthy children. The results of their labours are arranged in tables at the end of the book, by which means the text is left unencumbered with statistical details, while many valuable facts may be ascertained at a single glance.

The first chapter treats, with much good sense, of the labours of their predecessors, but displays all that ignorance of what has been done in other countries, which is so characteristic of the French. M. Cruse, on the contrary, gives a very good sketch of the literature of the subject.

The second chapter discusses the morbid appearances left by inflammation of the lungs. We are here presented with the results of forty-three autopsies, to which if we add seven by M. Fauvel, we have a number whence we may hope to deduce something like accurate conclusions. The remarks with which MM. Rilliet and Barthez commence the chapter appear to us to furnish the clue to many of the disputes which have arisen concerning this disease. They observe (p. 15) that "in their anatomical investigations persons have not borne sufficiently in mind the connexion

between the different forms of infantile pneumonia. They have studied each variety by itself, but have neglected to arrange them all under one head, and then to examine their mutual relations to each other." The plan which they have adopted has been first to describe each variety of pneumonia, next to examine those changes of the bronchi usually associated with it, and then to enumerate briefly the lesions of other organs which have been found to coexist with pneumonia.

The appearances left by vesicular pneumonia are especially deserving of note, since they may easily be confounded with the effects of phthisis, and were not referred to their true cause before the publication, in 1825, of a dissertation by M. Lanoix.* We do not recollect to have seen an account of this condition in any English work on diseases of children, and will therefore transcribe the very accurate description of it given by MM. Rilliet and Barthez.

"A lung thus affected is soft and flaccid, and collapses more or less according to the extent of the lesion. Its section presents a great number of granulations of about the size of a millet seed, and of a yellowish-gray colour. At a first glance these bodies might be taken for crude miliary tubercles, such as are often met with in the lungs of children; but a little attention shows that not to be the case, for these two species of granulations differ in their physical properties as much as in their nature. Tubercles form solid masses, pneumonic granulations contain a liquid. If a lung affected with tubercle is divided, some of the tubercles are cut by the scalpel and present their section on a level with the surface of the divided lung, while others slip from under the instrument and preserve their rounded shape. If these, too, are divided, they exhibit the appearance characteristic of tubercles. The granulations, on the other hand, collapse immediately on being divided, giving exit to a drop of puriform fluid, while those which are not cut by the knife still preserve their spherical form. If one of these granulations is opened with the point of a lancet, the puriform fluid escapes; but there remains in its centre a depressed point, which, however, is not always very easy to find. We once succeeded in tracing a very delicate canal some lines in length, with smooth parietes, and doubtless a small bronchial ramuscule, to its termination at this depressed point in the granulation. Their colour and general arrangement are, then, the only two characters which these morbid deposits have in common with each other." (pp. 17-18.)

MM. Rilliet and Barthez propose the term *vesicular bronchitis* as expressing the nature of this affection better than its usual name of *vesicular pneumonia*. They are of opinion that the disease is seated exclusively in the extreme bronchi; a certain number of pulmonary vesicles becoming inflamed, filled with pus, and dilated without the surrounding cellular tissue becoming implicated. To this explanation of the phenomenon, M. Fauvel (p. 68) objects, on the ground that inflammation of the pulmonary cells cannot be supposed to exist, without involving the surrounding cellular tissue. He imagines that one or more cells may be emptied of air by a forced expiration, and if this should be succeeded by a vigorous inspiration, the contents of the larger bronchi might be driven before the current of air which entered, and might thus reach the pulmonary cells. We confess that we are by no means satisfied with this hypothesis of M. Fauvel, and are the more inclined to subscribe to the opinion of MM. Rilliet and Barthez, from having found in a child who died of pneu-

* Essai sur la Pneumonie des Enfans comparée à celle des Vieillards. Théses de l'Ecole, 1825. No. cxix. p. 11.

monia some of these granulations in the upper part of the upper lobe of the right lung, although the bronchi of that lobe contained no unnatural secretion, and presented no sign of inflammation. If several neighbouring vesicles should be affected, the tissue connecting them may likewise inflame, and patches of *lobular pneumonia* would thus be formed.

Lobular pneumonia is an affection which we have reason to suppose to be more frequent in the Hôpital des Enfans Malades at Paris than among the children of the poor in London. The observations of Dr. Cuming, of Dublin,* would also lead us to suppose that it is unusual in the Irish metropolis. MM. Rilliet and Barthez recognize three degrees of it, corresponding to the three stages of ordinary pneumonia, and they propose further to distinguish the *pneumonie mammelonnée*, in which the inflamed lobules are distinctly circumscribed from the *pneumonie partielle*, in which there is no such definite line of demarcation between the healthy and the diseased structure. The *pneumonie mammelonnée* may pass on to suppuration and the formation of abscesses, the *pneumonie partielle* has a tendency to extend and involve an entire lobe. If the former has passed into the suppurative stage, a section of the lungs will present a number of cavities resembling in their form and disposition the appearance presented by the second degree of lobular pneumonia. These cavities sometimes communicate with the bronchi, though often that is not the case, and this latter circumstance, together with their irregular surface, may serve to distinguish them from the smooth and polished cavities formed by dilated bronchi. When lobular pneumonia has become general and has passed into the third stage, it is no longer distinguishable from ordinary lobar pneumonia, though at an earlier period their differences are obvious. In the latter the inflammation advances regularly from the part whence it set out, while in the former, patches of tissue in different stages of inflammation are scattered through the lungs. Pure lobar pneumonia is, according to their experience, not of very frequent occurrence; and when it proves fatal, the appearances left by it are similar to those produced by the disease in adults.

From their anatomical investigations, MM. Rilliet and Barthez conclude that all these lesions are only varieties of one disease, and that there is nothing special in any one of them; but that each is separated from the other only by a step, and one may become transformed into the other. The opinions of MM. Cruse and Fauvel differ widely from those just mentioned. We shall soon examine the facts collected by the latter gentleman.

*Carnification* of the lung is a condition far from uncommon, though it has not attracted the attention of any author with the exception of M. Rufz. The lung thus affected bears a considerable resemblance to the fœtal lung, and suggests the idea that the pulmonary cells have been obliterated by disease, and have lost their capacity for dilatation without continuing permanently engorged. This, they suggest, may be a form of chronic pneumonia; but they are not acquainted with any other condition of the lung to which the name of chronic pneumonia could with propriety be applied.

* On the Peripneumonia of Children; in vol. v. of Transactions of the King's and Queen's College of Physicians.

Tubercles were found only in fifteen out of forty-three cases, and by M. Fauvel in one of seven cases. These facts, which correspond with the results obtained by MM. Gerhard and Rufz, and which we can confirm from our own experience, present one of the most favorable features of pneumonia, since they show how rarely it is associated with one of the most frequent and most fatal complications of infantile disease.

The morbid conditions of the bronchi are especially note-worthy. In many instances their smaller ramifications present a considerably increased caliber, the dilatation sometimes affecting the tubes themselves, at other times merely their blind extremities. In two cases only were the parietes of the bronchial tubes thickened; in one of these instances they had acquired three times their natural thickness, a condition evidently chronic. This dilatation of the air-tubes was observed in all the seven cases recorded by M. Fauvel, who coincides with MM. Rilliet and Barthez in regarding it as the result of the accumulation of the secretions in the bronchi.

M. Fauvel agrees with his countrymen in representing the detection of inflammation of the bronchi as by no means an easy matter. They advise that shreds of the mucous membrane be torn off with the forceps and then examined, as the best means of avoiding error.

The nature of the bronchial secretions has been examined with great minuteness by M. Fauvel, (p. 38,) and it is upon the results thus obtained that he has founded his opinion of the identity in nature of croup and capillary bronchitis. On dividing the bronchi he usually found them full of a yellowish-white matter, containing little or no air, thick, and adherent to the mucous membrane. In many instances the air-tubes were found blocked up by this matter from their secondary divisions to their ultimate ramuscules. Where it was in contact with the wall of the bronchi it had the appearance of a false membrane, while in the interior it resembled pus. In four instances this pseudo-membranous appearance was well marked, existing on both sides of the chest and extending from the secondary bronchi to those of very small caliber. Now and then the false membrane could be traced into the most delicate ramifications, but usually it could not be followed beyond the smaller divisions of the bronchi. In a fifth subject the membrane was not well formed, being apparent only in patches; in a sixth the expectoration warranted the supposition that such matters had existed, though the patient did not die till three months afterwards and of another disease, when no concretions were found in the bronchi. In two other cases no false membranes were found, but only purulent matter. These cases are interesting, inasmuch as they exhibit a singular extent of disease of the bronchi, accompanied with very slight affection of the parenchyma of the lungs. They are not, nor indeed does M. Fauvel pretend that they are, altogether novel, analogous conditions having been described under the name of bronchial polypi by the medical writers of the last century. The appearance is alluded to, among others, by Michaelis,* and is treated of more fully by Dr. Badham,† who recognized the analogy between cynanche trachealis and acute bronchitis, and considered the latter to be merely an extension

* De Angina Polyposa, 8vo; sect. ii. Gottingæ, 1778.

† On the Inflammatory Affections of the Mucous Membrane of the Bronchiæ, 8vo, pp. 65, and in the Appendix; London, 1808.

of the disease which in croup is confined to the trachea. M. Fauvel quotes M. Jurine as having distinguished, under the name of *catarrhe suffocant aigu*, a species of croup which attacks especially the mucous membrane of the bronchi, and differs from true croup only in its seat.* It is to be regretted that M. Fauvel has confined his investigations to so small a number of cases as eight, instead of endeavouring, as he might have done, to ascertain the circumstances under which false membranes form in the air-passages, since we are much in the dark with reference to that interesting question.

Both MM. Rilliet and Barthez and M. Fauvel mention the existence of vesicular emphysema, but the last gentleman only seems to have met with interlobular emphysema, at which we are rather surprised, for our own experience would have led us to suppose that it is not such a very uncommon occurrence in young children.

The pleuræ were in many instances found inflamed; indeed they were perfectly healthy only in ten out of forty-three cases; and in a third of these the alterations they presented were quite recent.

We will not detain our readers with a minute account of the morbid conditions of the other organs, none of which were important except those met with in the intestines. The intestines could not be regarded as perfectly healthy in more than nine instances, but showed marks of disease usually of a chronic character, and consequently antecedent to the attack of pneumonia.

An account of the chief modifications of the respiratory sounds produced by inflammation of the lungs occupies the third chapter of the treatises of MM. Rilliet and Barthez, and of M. Fauvel. It must be obvious to every one that many circumstances concur to render the auscultation of the chest in young children a matter of great difficulty. Many persons indeed appear to have satisfied themselves that it is impracticable, and, like M. Berton, enumerate the obstacles which they have not perseverance to surmount. Even the careful investigations of M. Valleix have left so many points connected with the physical exploration of the chest in children undetermined, that we feel we are doing our readers good service in detailing somewhat at length the remarks of MM. Rilliet and Barthez.

In most cases of pneumonia, ronchus and sibilus may be heard at the commencement of the disease; but the irregularity of their seat, the shortness of their duration, and the very various sounds by which they are succeeded in different cases, render their diagnostic value very slight. In children above two years of age, however, these sounds become of greater importance, since in them pneumonia is more frequently consequent on bronchitis than at an earlier age.

The mucous ronchus is of more value than ronchus or sibilus, since it occurs oftener, persists for a longer time, and frequently coincides with bronchial respiration, while it is in other cases its immediate forerunner. These circumstances, too, impart to it an importance far beyond what we attach to it in the adult.

* M. Gluge, in his account of the influenza epidemic in Paris during 1837, mentions that in many cases false membranes, a quarter of a line in diameter, were found in the ramifications of the bronchi. Quoted by M. Cruse at p. 132.

Of all the râles, however, the sub-crepitant deserves the most attention, since it frequently precedes bronchial respiration, and is perceived in the very parts where bronchial respiration is subsequently heard. It is usually heard both in inspiration and expiration, though sometimes, when associated with bronchial respiration, it can be distinguished only during the former. In one instance it was detected only during expiration. Its duration was but short when it preceded bronchial respiration; when it followed it, however, it lasted for a much longer time, owing, as the writers observe, to the rapidity with which hepatization of the lungs supervenes in children and the slowness of its disappearance. We suspect that this extremely slow solution of hepatization is in a great measure attributable to the unfortunate hygienic condition of children in the Hôpital des Enfans Malades.

The existence of the *râle crépitant*, such as is heard in the pneumonia of the adult, has been denied by MM. Gerhard and Rufz, but it has been ascertained by MM. Rilliet and Barthez and also by M. Fauvel. It is heard only during inspiration, lasts but for a very short time, one or two days at the most, never reappears where it has once been heard, and is the immediate precursor of bronchial respiration. Being less frequent than the sub-crepitant râle, however, it may be regarded as of less value. A point of much importance in connexion with every variety of râle is the rapidity with which it disappears on placing the patient in the erect posture.

No modification of the respiratory sound is of such importance as bronchial respiration. It existed in two thirds of the cases, and when its existence was not ascertained, either the disease was very slight or it had become impossible to auscult the child during the last days of its life. In some instances the bronchial character was observed only during expiration, the inspiration remaining perfectly natural. This peculiarity was most remarkable in the youngest patients and at the commencement of bronchial respiration, or when it had begun to disappear. This phenomenon is regarded by MM. Rilliet and Barthez as indicating the existence of lobular pneumonia, the hepatized lobules playing the same part as crude tubercles do in a phthisical lung. In the youngest subjects, *râles* of various kinds preceded the bronchial respiration; in older patients an obscure respiration was often its precursor, while sometimes it appeared without having been preceded by any sign whatever. We invite our readers' attention to the following important caution:

"In most instances bronchial respiration is easily detected; but, nevertheless, the existence of certain *râles*, the impossibility of making children cough at pleasure, and their impatience during auscultation sometimes prevent it from being detected. But, not to dwell upon causes which interfere with our discovering this sound, we may enquire whether it can ever be confounded with other stethoscopic signs? Such an error is undoubtedly possible. In many instances we have convinced ourselves that persons but little skilled in auscultation, those especially who had but seldom explored the chest of children when in health, mistook exaggerated for bronchial respiration. But the difference between the two is in reality very considerable. How puerile soever respiration may be it always conveys the sensation of air entering vesicles; it is moreover audible only during inspiration, while bronchial respiration is heard principally during expiration." (p. 59.)

Percussion is a means of diagnosis much less valuable than auscul-

tation. Not only does it not yield any result in lobular pneumonia, but, so great is the natural resonance of the chest in children, that a diminution of its sonoreity is often not appreciable, or at any rate can be detected only by comparison of the two sides of the chest with each other. In many instances, too, both lungs are affected, and we consequently lose this means of comparison. M. Hourmann attaches but little value to percussion, but thinks that the vibration of the voice, as perceptible to the hand, is greater on the diseased than on the healthy side.

In chapter iv. MM. Rilliet and Barthez compare the results of anatomical research with those afforded by auscultation; but we have not space to follow them through the interesting train of argument in which they endeavour to prove that the difference between the various forms of inflammation of the lungs of children is a difference of degree not of kind.

Chapter v. of MM. Rilliet and Barthez, and chapter ii. of M. Fauvel, are occupied with an examination of the causes of pneumonia. In most cases the patients were previously out of health; but MM. Gerhard and Rufz stated the fact too absolutely when they denied the existence of idiopathic pneumonia in children from two to five years of age. Idiopathic pneumonia continues, however, according to the experience of our authors, to be of rare occurrence up to the age of puberty. Age seems to exert a great influence in predisposing to pneumonia, and the disease is by far more frequent between two and five years of age than at any subsequent period. Forty of the sixty children whose cases form the basis of the work of MM. Rilliet and Barthez were under five years of age, and in 108 post-mortem examinations, M. Hache found pneumonia 71 times in children from two to five years of age, 37 times only in those from six to fifteen. But the age of the patient does not exert an influence merely in the production of the disease, it likewise modifies the form which it assumes. Some writers indeed have asserted that lobular pneumonia is peculiar to children between two and six years old; but to this observation, though true in the main, there are numerous exceptions.

Tables are introduced with the view of exhibiting the relative effect of season in predisposing to pneumonia, and also to show the diseases with which this affection is most usually combined in children; but the data are much too scanty to justify any positive conclusions. The following results, however, are worthy of commemoration: 1. Out of 94 cases of pneumonia, only 13 are recorded as simple idiopathic, the remaining 81 being complicated with other diseases. 2. Between the ages of two and five years measles is the disease most frequently complicated with pneumonia, and next in order chronic enteritis and hooping-cough. Between six and fifteen years of age measles stands first, then hooping-cough. It is added: "Though gangrene of the mouth occurs but thrice in this table, we may nevertheless venture to assert that of all the diseases of infancy this is the most frequently complicated with pneumonia, for we have met with it in every instance which has come under our notice, and M. Baudelocque assures us that this complication is invariably found to exist." (Rilliet and Barthez, p. 81.)

With regard to this invariable association of gangrene of the mouth with pneumonia, we will merely observe that not long since we examined

the body of a child who had died from cancrum oris. The lungs were studded with tubercles, but presented no sign whatever of inflammation.

Dorsal decubitus and a prolonged residence in the hospital are two other causes of pneumonia to which our authors allude. After our remarks, however, at the commencement of this notice upon the hospitals of Paris, we will not again revert to the subject, but will notice one or two points of importance connected with the symptoms of pneumonia.

The condition of the pulse and respiration deserve especial notice, a sudden and rapid increase in their frequency often indicating the accession of pneumonia. In young children the pulse sometimes reached the rapidity of 150 or 180 beats in a minute, and usually ranged between 120 and 150. In children from six to fifteen years old it rarely attained so great a frequency as 140 or 150 pulsations in the minute, though it was usually 120 at the onset of the disease. In children between two and five years old the respiration varied from 30 to 80, in those from six to fifteen from 24 to 68 in the minute. We should have been inclined to mention a still higher figure as that of the occasional frequency of the inspirations in children from two to five years of age. In connexion with the frequency of the respiration, M. Fauvel mentions a fact, to which since we met with his treatise we have directed our attention, and which we believe to be correct. It is that the respiration does not continue to increase in frequency as the disease advances, but that it soon attains its greatest quickness, and afterwards shows slight diminution in its frequency, which is far from indicating any improvement. In illustration he subjoins a table from which it results, 1st, that the frequency of the respiration was always in inverse proportion to the age of the subject; 2d, that, except in one instance, its greatest frequency was observed at an earlier stage of the disease than its least frequency.

When pneumonia was uncomplicated, the frequency of the pulse and respiration was in proportion to the extent and severity of the inflammation, which circumstance likewise usually influenced the intensity of the febrile reaction. This statement, though quite in accordance with what we might expect to be the case, is yet at variance with the assertion of MM. Burnet and De la Berge, that lobular pneumonia is attended with a peculiar acceleration of pulse and respiration. MM. Rilliet and Barthez, however, remark that in all the cases detailed by those gentlemen, the condition of the pulse and respiration was sufficiently explained by the coexistence with the inflammation of the lungs of other diseases, as measles, typhus fever, &c.

We must refer our readers to chapters vii. and viii. for the able "*Tableau*" of the disease, and for the remarks on the diagnosis of pneumonia from pleurisy, bronchitis, and phthisis.

The prognosis (chapter ix.) is unfavorable in proportion to the youth of the patients; and that form of pneumonia which supervenes in the course of chronic affections is almost necessarily fatal. Secondary pneumonia is indeed at all ages a very dangerous disease, for of 81 cases which occurred under the care of the same physician in 1837, 77 terminated fatally. When pneumonia, however, comes on in healthy children or after a slight attack of catarrh, the patients usually do well, especially if they are above five years of age. To no one sign do MM. Rilliet and

Barthez attach so much importance as presaging an unfavorable termination as to the rapidity and especially the smallness of the pulse. With this character of the pulse there usually coincide cessation of the cough, coldness of the extremities, and a livid colour of the face, which serve to announce the speedy arrival of death.

The last chapter treats of the curative measures employed. These consisted of depletion, the administration of antimonials, and the application of rubefacients or blisters; but the writers observe that in almost all cases which terminated favorably, the first symptoms of improvement appeared about the same time, namely, from the seventh to the ninth day, whatever might have been the treatment, or even when no treatment was adopted. They attach but little value to the employment of depletion alone, but they speak more favorably of the administration of antimony, and observe that we need not fear to give tartar emetic in large doses, since tolerance is in general readily established, and there is but little reason to dread gastro-intestinal affections. They observe that tartar emetic appears to exert a more direct influence upon the pulse and the respiration than upon the hepatization of the lung, a statement to which we should feel disposed to add that when perfect tolerance of the medicine has been established, a perseverance in its use but seldom leads to any beneficial result; a combination of depletion with antimonials is the practice which they usually follow. The application of blisters they proscribe, and in this, as far as it regards children under three or four years of age, we most heartily coincide, for we cannot call to mind any instance in which the good they did was not more than counterbalanced by the troublesome and often dangerous sores they produced.

Eleven carefully reported observations and two tables, containing the results of the post-mortem examinations, conclude the treatise of MM. Rilliet and Barthez. We recommend it most earnestly to all our readers, and trust that it will be, as its authors promise, the first of many similar monographs on the diseases of children.

Dr. Küttner is physician to the Children's Dispensary at Dresden. His observations refer to forty cases of bronchitis or bronchio-pneumonia which came under his notice in the course of five years. In most points they confirm the statements of MM. Rilliet and Barthez, but do not present anything particularly novel. He notices the comparative immunity of children from the disease during the hottest months of the year, and the connexion of its prevalence with measles, influenza, and catarrhal affections in general. Two thirds of the cases occurred in children between the ninth month and the second year, but the disease occasionally appeared with all the peculiar features which it presents in infancy up to the fifth year. The detail of the symptoms presents nothing to remark, beyond the observation that in many of the cases, especially in those which terminated fatally, the children had been liable from birth to habitual shortness of breath, and to frequent attacks of catarrh.

More than half of the patients were in the enjoyment of robust health up to the time of their attack, but this more advantageous condition of the children at Dresden than of those in the hospital at Paris, was not attended with a proportionally low rate of mortality. Twenty-six of the forty children died, fourteen only recovered. All who were under the age of six months sank under the disease; the second year likewise was

peculiarly fatal. The treatment consisted in leeching freely, and in the employment of calomel and tartar emetic; but many of the children were not brought to Dr. Küttner till after they had been neglected or maltreated for several days.

A post-mortem examination was made in seventeen cases. The bronchial mucous membrane was almost always red and injected, especially in the right lung, to which the inflammation was sometimes confined, and more rarely to the left, and the bronchial tubes contained more or less puriform mucus. The substance of the lung was inflamed in patches, principally at its posterior surface, and never pervading the whole of the organ. The pleura was inflamed and adherent to the ribs in seven cases, and in one instance sero-purulent effusion had taken place into the pericardium, and the surface of the heart was coated with a false membrane.

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## ART. IV.

1. *Précis Théorique et Pratique sur les Maladies Vénériennes.* Par P. BAUMÈS, Chirurgien en Chef de l'Hospice de l'Antiquaille de Lyon.—*Paris et Lyon*, 1840. 2 vols. pp. 413, 583.

*A Theoretical and Practical Treatise on Venereal Diseases.* By P. BAUMES, Senior Surgeon to the Infirmary "de l'Antiquaille" of Lyons.—*Paris and Lyons*, 1840.

2. *A complete Practical Treatise on Venereal Diseases, and their immediate and remote consequences; including observations on certain affections of the uterus, attended with discharges.* By WILLIAM ACTON, late Externe at the Female Venereal Hospital, Paris.—With an Atlas of Plates.—*London*, 1841. 8vo, pp. 410.

MR. ACTON'S work is devoted almost exclusively to an exposition of the theory and practice of M. Ricord in reference to venereal diseases; we have therefore thought it right to include under the same notice the treatise of M. Baumès of Lyons, who differs, and we think very justly, from M. Ricord on many points of very great importance. Mr. Acton's book is divided into two parts: the first treating of syphiloid diseases, by which is meant gonorrhœa, its varieties, and consequences; the second including syphilis, properly so called. We do not find in the second division of Mr. Acton's book anything to detain us; and having in a late number, (Br. & For. Med. Rev., No. XX,) entered very fully into the consideration of the opinions of M. Ricord on syphilis, we shall confine this article to some points of a practical character connected with the first, and confessedly the most difficult branch of the subject.

Mr. Acton's division of venereal diseases into the syphiloid and syphilitic is precisely analogous to that of M. Ricord into virulent and non-virulent affections, the second class being truly specific, and capable of propagation by inoculation under the same form, whilst the specific character of the first class is denied by M. Ricord, we think however, in many instances at least, on insufficient grounds.

Mr. Acton includes all the discharges from the female organs under

the generic term, first introduced by Swediaur, of Blenorrhagia, and Blenorrhœa. We object to the use of this term, because English readers, more particularly the junior branches, are not familiar with it, and because we are at a loss to know what precise form of discharge the term is used to designate. Any confusion likely to arise from the use of the word blenorrhagia has been still further increased by Mr. Acton, who, although including all discharges from the female organs under this term, yet speaks of the same disease, when communicated to the male under the title of gonorrhœa: "The next form of blenorrhagia I am about to describe, as it exists in the male, is gonorrhœa." (p. 72.)

Blenorrhagia, by which expression, we must again repeat, Mr. Acton designates *all* discharges from the female organs, is thus defined:

"A simple inflammation of the mucous membrane, a consequence more or less direct of sexual intercourse, not necessarily, though often contagious; this last character depending upon a morbid secretion of the stimulating matter, which acting on another mucous membrane will occasion a blenorrhagia, but will (on inoculation) produce no disease of the cellular tissue into which it is introduced. In fine, blenorrhagia differs in no respect from other inflammations of mucous membranes otherwise than in its usual situation, and in the manner in which it is contracted." (p. 24.)

This most untenable theory, contradicted by every day's experience, and even by the results of inoculation itself, is the reiteration of one lately promulgated by M. Ricord in his notes to Richelot's translation of Hunter. M. Ricord has assumed, and his pupil Mr. Acton has taken up his theory we think without sufficient examination, that all the discharges from the female of a purulent or muco-purulent character, upon whatever dependent and however named, are capable of producing in the male that species of affection known to English surgeons by the name of gonorrhœa. Under this view simple leucorrhœa, or the secretions attendent on simple inflammatory states of the vagina or neck of the uterus, may occasion gonorrhœa in the male from sexual intercourse. Against these wild theories M. Baumès in the volumes before us has raised his voice; and, in a long chapter headed "on the contagious principle of gonorrhœa and its effects," has laboured to prove what English surgeons have never doubted, namely, that pure gonorrhœa is a specific disease. We think M. Baumès has made out a good case, and very satisfactorily established that a pure gonorrhœa is as much a specific disease as a primary venereal sore, giving the characteristic pustule of chancre when tested by inoculation.

We do not mean to deny that discharges from the male urethra, the result of sexual intercourse, may be produced by many other causes than the contagion of gonorrhœa, and even by many other species of discharge on the part of the female.

"Doubtless sexual intercourse with a female during the menstrual period, (more particularly if the discharge at these periods be of an unusually irritating character) or with one affected with uterine or vaginal discharges of a more or less acrid nature, which reddens and excoriates the vulva or thighs of the female, may produce urethral discharges in the male, but how different are these in duration, intensity, and consequences from a pure gonorrhœa." (Baumès, vol. i. p. 197.)

Benjamin Bell had long ago distinguished these two species of

gonorrhœa in the male, under the terms of gonorrhœa simplex and gonorrhœa virulenta.

Again, these simple gonorrhœas are not capable of propagation in the same manner that a pure gonorrhœa generally is; in fact they are not contagious. M. Baumès has collected a number of very valuable cases on the effects of these two forms of discharge, which will well repay an attentive perusal. Most writers have endeavoured to discover some distinguishing mark between gonorrhœa and other discharges from the vagina, which we may call leucorrhœal. Hunter, Clarke, Churchill, and others have failed in doing so, and it does not appear that the speculum has as yet thrown any additional light on the subject. That differences do exist in the nature of these discharges on the part of the female we are perfectly convinced; differences which the speculum cannot distinguish, but which are evident in their effects upon the male; they are dissimilar in their duration, intensity, consequences; and, still farther, the two are not to be cured by the same remedies.

We will put a case which constantly occurs in practice, and of which, in an experience pretty much extended, we have seen many: M. Baumès also gives several of the same character. A person has connexion with his wife during the time that she is suffering from heat and swelling of the labia, accompanied with a discharge of a mucous or even muco-purulent character, the connexion is repeated time after time without any ill consequences to the husband; after a debauch the connexion is again repeated, and the husband perceives a discharge from the urethra, accompanied by slight scalding in making water, he may conclude himself diseased, and under such an impression vast anxiety may be occasioned. Let him, however, take a smart aperient, and live low for a few days, at the same time using perhaps a mild astringent injection, and his disease shortly disappears. This is simple gonorrhœa, and may be produced by a great variety of causes. On the other hand, the female being perfectly well, the husband goes astray, and has connexion with what M. Baumès would call "an intercurrent mistress," in fact a woman of the town. In a few days he is seized with a discharge from the urethra, accompanied by a greater or less degree of active inflammation; he has intense pain in passing his urine, the discharge goes on little influenced by remedies, he gets in the third week a swelled testicle, the glands in the groin enlarge, and perhaps suppurate, and ultimately the disease terminates in gleet, and stricture. This is a pure and specific gonorrhœa, and a totally different disease from the one at first described, being in our opinion, and in that of English surgeons generally, specific in its character, to be cured by specific remedies, and always propagated under the same forms when the matter of the discharge is applied to a mucous surface.

"Between the muco-pus of a pure gonorrhœa, and the pus or muco-pus of other discharges there is a difference precisely similar to that which exists between the pus of a chancre producing a characteristic pustule by inoculation, and the pus of other sores the result of sexual intercourse, which do not give this result although no chemical or physical circumstances are capable of showing in what this difference consists." (Baumès, vol. i. p. 208.)

These differences are only to be seized in the effects which the different discharges produce.

*Of the Causes of Blenorrhagia.* Under this head Mr. Acton has some important facts.

Infants are more disposed to the affection than adults; this appearing to depend on the delicate state of their mucous membranes.

Females are much more disposed to discharges of a blenorrhagic character than males. "In Paris," says M. Ricord, "women may be said to have habitually a discharge, call it what you will, leucorrhœa, gonorrhœa, fluor albus, &c. &c., it affects all ages and stations. Were I called upon," says the same author, "to estimate the proportion of discharges in the male and female, I should say that it is a hundred times greater in the little girl than in the boy; a thousand times greater in the adult female than in the male." (Acton, p. 25.) These facts speak for themselves; they at once show that if these discharges on the part of the female are so universal in the French metropolis, they depend upon simple or constitutional causes, and are very different in their effects from the poison of a pure gonorrhœa.

The lymphatic temperament, residence in a moist climate, a damp situation, spring and autumn, light clothing, and stimulating food are adduced as powerful predisposing causes of blenorrhagia. The presence of foreign bodies in the vagina, the frequent use of enemata, intestinal worms, renal and vesical affections, menstruation, and inattention to cleanliness, are all capable of producing blenorrhagic discharges. They commonly accompany pregnancy, and succeed to labour; and may be produced by certain morbid states of the constitution, as scrofula, gout, various skin diseases, and the secondary forms of syphilis.

"I may observe," says Mr. Acton, "that thus considered blenorrhagia presents nothing that is specific; it may arise under the most varied circumstances and causes; its existence in the male and female, therefore, of itself is no proof of libertinism; it may occur in the most modest as well as the youngest child; hence, in medical jurisprudence the necessity of being guarded in our opinion; and the surgeon, in family disputes on the subject of contagion, should be especially cautious, and always lean to the weak side." (p. 30.)

Here we perfectly agree with Mr. Acton, differing from him, as we have already stated, only in maintaining that there are specific discharges as well as these enumerated, which latter we may say depend on simple and not on specific causes. It is frequently the case that in consequence of family feuds, a female presents herself and asks the surgeon for a certificate to the effect that she is not the subject of any venereal affection. In the uncertainty as to the true character of a discharge, even after examination with the speculum, M. Ricord recommends, in the absence of ulceration, the following form of certificate:

"I certify, &c., that presents no symptom of a syphilitic disease, but has a catarrh of the vagina, uterus, &c., and may probably (or not, as may be,) communicate the disease to another." (Acton, p. 33.)

*The Pathology of Blenorrhagia in the Female.* In this section Mr. Acton gives the result of his own observations with those of M. Ricord, M. Emery of St. Louis, M. Danyau, surgeon to the Female Venereal Hospital, and M. Vidal de Cassis. According to M. Ricord, "whatever may have been the cause of the discharge, still the urethra, vulva, vagina, or uterus may be alone or simultaneously affected. Nevertheless, M.

Ricord is persuaded that in the female the urethra is more generally affected alone, or at the same time with the other organs of generation, when the blenorrhagia is the result of impure connexion." The same remark had been previously made by Gibert. In some forms of blenorrhagia in the female, the mucous membrane of the vagina, &c., is found in its whole extent of a red colour, at other times the redness occurs in isolated patches, "with swelling, heat, and pain, unattended by any secretion; thus presenting an erysipelatous state, which may last a short time and then disappear." Other cases of this kind give rise to a morbid secretion, the colour and consistence of which are variable. The differences in the characters of the secretions appear to have no reference to the causes which have produced them. The first of these varieties appears to be the pathology of that form of disease which has been particularly described by M. Baumès under the name of "dry gonorrhœa."

"In examining the vulva, vagina, or the neck of the uterus, we have observed the mucous membrane covered with papulæ, or follicles more or less developed, constituting a "papular vaginitis, or utero-vaginitis," *psorélytrie*, as M. Ricord terms it, sometimes assuming the form of small spots, in size not larger than a pin's head, isolated, or more or less confluent. In other cases these papulæ look like granulations deprived of their epithelium; lastly, they may assume a fungous appearance, in the form of vegetations. On the same portions of the mucous membrane we have distinctly seen patches more or less numerous, and varying in extent, which have a striking analogy with the suppurating surface of the skin on which a blister has been applied. M. Ricord has likewise witnessed a case in which an eruption of herpes phlyctenoides was present on the neck of the uterus and the posterior part of the vagina; lastly, we may find ulcerations of every description seated on the whole or any part of the surface of the genito-urinary mucous membrane.

"The discharges from the urethra, vulva, vagina, and uterus, which we have examined have been very various; but the difference has not appeared to us connected with any one lesion or cause more than another. The acute stage, whatever may be the particular lesion, causes at its commencement a secretion almost wholly serous, or only consisting of healthy mucus, more abundant than usual, but becoming opaque, then purulent, or of a darkish yellow colour, sometimes greenish, and at times mixed with blood. The chronic stage often gives rise to a milky secretion of a thickish consistence, similar to that of cheese, or simply to a mucous flux.

"The chronic discharge may put on a rusty appearance, and become tinged with a larger or smaller quantity of blood. These secretions, whether in the acute or chronic stage may have no smell, or on the contrary, have a very unpleasant odour, particularly where the mucous papulæ exist. The smell *sui generis* is often so decided, that it is characteristic in great number of cases, as under other circumstances it resembles the smell of cancer, or of feculent matter. The only differences which result from the particular seat of the blenorrhagia are, that the secretions which come from the uterus are always more mucous, thready, and collected into flocculi; whereas, those which escape from the urethra, vulva, or vagina, present a less tenacious character than the others." (Acton, pp. 172-3.)

These varied appearances are well represented in the first three plates given by Mr. Acton. Of course the true pathology of the membranes is not to be recognized without the use of the speculum, and whatever facility custom or something worse may give the surgeon in the French metropolis, there exists in this country, even in the hospitals, and

amongst prostitutes of the lowest grades, a prejudice against the use of this instrument which is not likely suddenly to be overcome. This feeling also appears, from Mr. Acton's own account, to have existed also among the Parisian "filles de joie." "At the hospital where M. Ricord took the service in 1830, a revolution broke out, and a *strike* against the speculum occurred." Bread and water diet was necessary to quell this "revolt of the harem."

So accustomed, however, says Mr. Acton, is the Parisian female now to the use of this instrument, that M. Ricord frequently receives messages from ladies "in high life" (p. 180), requesting him to visit them, and begging he will not forget *to put his speculum in his pocket.* We do not quite understand these penchants of females in high life for speculum examinations when there is little the matter, or rather where the surgeon has to look whether anything is the matter or not. We should be sorry to see the English surgeon on such a footing with the English matron, neither do we believe we are likely to see it.

The discharges from the male urethra are dependent on several morbid conditions existing in the course of the canal. In the first place there may be a mere hypersecretion of mucus, the result of errors or excesses in diet, or dependent upon other causes which we have already passed in review. Again; discharges from the male urethra of a mucous or muco-purulent character occur after connexion with women menstruating, affected with fluor albus, or various forms of irritation of some part of the genito-urinary system accompanied by discharge. These generally soon subside, are not specific; and, following Bell, we term them simple gonorrhœas. The true "gonorrhœa virulenta" of English writers next succeeds. It may be necessary here perhaps to mention that the term virulent gonorrhœa is understood by M. Ricord and his school in a different sense to that in which it is understood by English surgeons and writers generally. Gonorrhœa virulenta, according to M. Ricord, is a discharge from the urethra depending on the existence of a true chancre in the canal, and is therefore not a syphiloid but a syphilitic disease. Mr. Langston Parker has already, in his Modern Treatment of Syphilitic Diseases, given a description of these chancres, which are termed by the French surgeons "chancres larvés." Ulcerations of other characters may exist in the urethra which are not truly syphilitic, but precisely similar in their nature to those which take place on the surface of other mucous membranes, which have been violently or for a long time inflamed. M. Baumès has given some cases of this kind (pp. 238-9); we have, in our own practice, seen similar forms of disease. According to Lallemand semen commonly exists in discharges from the urethra of a chronic character. The microscope can only decide this question by showing to the surgeon the presence of spermatozoa in the secretion thrown out.

"The existence of blood mixed with muco-pus will generally lead the surgeon to expect an ulceration of the canal which he cannot examine; but here there are many sources of error, as blood may be poured out in consequence of excessive inflammation. Usually, however, I have been able to distinguish, or at least to suspect, the existence of a chancre from the appearance of the discharge, when it has a grayish or reddish tint, and is of thin consistence." (Acton, p. 42.)

Some very interesting questions, about which patients are extremely

inquisitive, are examined by Mr. Acton and M. Baumès; these are principally the period at which a chronic discharge from the urethra ceases to be contagious, and what are the chances of secondary symptoms succeeding to such affections. Every practical surgeon knows the difficulty which occasionally occurs of entirely removing the chronic discharges which succeed to gonorrhœa; these, however trivial in quantity they may be, being still possessed of their contagious character, are capable of producing an acute gonorrhœa in a healthy female. Where persons wish to marry, it is of the first importance to determine with certainty, if this be possible,—whether these gleets, if we may so term them, be contagious or not. M. Baumès puts the question in this way: "An individual being given, with a discharge from the urethra, a consequence of gonorrhœa, to determine if this discharge is or is not contagious, to decide whether this individual may or may not communicate the disease to a healthy female." (vol. i. p. 286.) The first point to be determined is the character of the discharge; should it appear to the surgeon to be connected with a chancre of the urethra it will be well to test it by inoculation, when, if the characteristic pustule be produced, there cannot exist a doubt of the contagious character of the affection under its worst form. If the discharge be transparent, or merely mucous, both Mr. Acton, M. Ricord, and M. Baumès consider that contagion is not to be dreaded, and the patient, if so disposed, may marry without fear. It is difficult, however, to determine by mere inspection whether the discharge is mucous or muco-purulent; here the microscope will guide us, and "whatever may have been the duration of the complaint," or to whatever amount the discharge may have been reduced, should it contain pus-globules, "it is capable of causing a similar complaint in any mucous membrane with which it comes in contact."

"Should, therefore, any pus be present in the secretion, the surgeon ought never to sanction sexual intercourse; if the secretion be not purulent, let him wait some days before he gives his permission, to see if it does not become so. Impress strongly on your patients the necessity of abstaining from sexual intercourse immediately any pus appears mixed with the secretion. Patients will sometimes ask the following questions: 'I am obliged to have connexion with my husband; now, Doctor, I am suffering under a discharge, what complaint will he contract from me?' Inoculation* will alone answer the question, and the surgeon will at once be able to tell the probable consequences. The same question relates to marriage; and patients (says M. Ricord) present themselves to me to know whether they may marry, for often their fortune may depend upon a marriage. I persuade them against it if they have a simple gonorrhœa [i. e. in our acceptation of the term an ordinary gonorrhœa,] but if it be a virulent complaint [i. e. in the pathology of M. Ricord a chancre in the urethra], I wash my hands completely of the affair; if they still persist, I tell them that they may give a gonorrhœa to their wives, which unless cured previous to confinement, may cause a loss of eyesight to the child. Called afterwards to cure the lady, I attempt to explain the affection which she has contracted by speaking of the fatigue of the honeymoon, as well as the déjeûner à la fourchette, and in the interim cure both parties, of course forbidding connexion." (Acton, pp. 49-51.)

The next question we shall examine from the authors before us is the

* We refer our readers to the article on "Syphilis" in Number XX. of the Review, for a full account of inoculation in reference to venereal sores.

probability of secondary affections succeeding to purely gonorrhœal diseases. Mr. Acton does not speak of any form of secondary affection succeeding to gonorrhœa, adopting, we suppose, the opinions of M. Ricord, that such consequences do not occur. M. Baumès entertains a different opinion. M. Ricord says (Richelot's Translation of Hunter, p. 178), "there does not exist a single authentic fact in the history of science which proves that an individual, in whom the mucous surfaces have been examined during the course of a gonorrhœa, without the complication of chancre, has ever subsequently been the subject of constitutional syphilis." Many modern writers, amongst whom we may mention the late Dr. Wallace, fully believe in the possibility of secondary symptoms succeeding to gonorrhœa; we have before had occasion to state that we believe the discharges from the urethra to be of various kinds, and do not dispute the fact of constitutional syphilis succeeding to some forms of primary disease in the male, characterized by urethral discharge; on the whole, however, we are disposed to adopt M. Ricord's view of the question, believing that a true gonorrhœa is rarely if ever followed by constitutional symptoms. M. Baumès brings forward some cases in which constitutional symptoms, characterized by copper-coloured patches and papulæ, succeeded to balanitis or discharge from the external surface of the glands, and from the prepuce, without ulceration or breach of surface. This form of disease is considered by most modern writers, Ricord, Parker, Acton, &c., as a variety of gonorrhœa, differing from the urethral variety merely in its seat. In the cases mentioned by M. Baumès this external gonorrhœa was followed by the falling off of the hair, and eruptions precisely similar to those which follow primary venereal sores, and were curable only by mercury. In the first case the patient had never before had any venereal affection till he contracted a balanitis, characterized by redness, heat, and itching of the external surface of the glans penis, and neighbouring portion of the prepuce, to which succeeded a purulent discharge "without any kind of excoriation or wound." This was succeeded in four months by copper-coloured patches on the forehead and chest, and a female with whom this patient cohabited became affected with heat and swelling in the genitals, pain in making water, and two months after an eruption on the inside of the thighs, the nose, and the forehead. The female was declared diseased, put on the use of mercury (Sirop de Larrey) with sarsaparilla, and recovered. The first patient, still suffering from the affection which he did not consider syphilitic, took to himself a second mistress, the first having married after her recovery. This second female soon became affected with the same symptoms in the genitals as the first, and two or three months afterwards "constitutional affection" made its appearance which ultimately assumed a pustular form. The prospect of an advantageous marriage presented itself, and our patient now separated from his second mistress and married. In a short time the wife was affected, as her temporary substitutes had been before her, and subsequently with eruptions of a like character. The patient and his wife now put themselves under the care of M. Baumès, who states that the only disease on the genitals with which the husband was affected was redness of the glans penis, with purulent discharge; no ulceration, breach of surface, or trace of cicatrix. They

were put upon a mercurial treatment and both perfectly recovered. M. Baumès adduces another case precisely similar to this.

These primary forms of disease, which have been named by foreign writers "balanitis, or external gonorrhœa," have been of late supposed to be precisely identical in their character with that disease when seated in the urethra, differing from it, as we have before said, merely in its seat. We are inclined, however, to believe that in many cases this analogy is not correct, though it may hold good in some, since what appears a simple catarrhal affection in the first instance frequently degenerates into ulcerations more or less extensive; hence Mr. Acton, in quoting the opinions of M. Puch, one of the surgeons to the venereal hospital, states that M. Puch considers that simple balanitis may produce a chancre, and thus induce secondary symptoms. This, however, is contrary to M. Ricord's view and Mr. Acton's. We are convinced that what is termed "balanitis" may arise from several causes, and know from our own experience that what *appears* as mere catarrhal affection may be followed by bubo and secondary symptoms. Dr. Wallace supported the same view. We dwell upon this subject at some length, because of its great practical importance, and more particularly for the purpose of guarding the young surgeon against following implicitly the opinions of M. Ricord, now so fashionable on this matter, since by so doing he might compromise his reputation in explicitly stating that no secondary symptoms would follow what appeared to him a mere external gonorrhœa.

M. Baumès and Mr. Acton divide the treatment of blenorrhagia into three stages,—the preventive, the abortive, and the curative. Under the first head Mr. Acton, in imitation of M. Ricord, speaks in the first place of the precautions to be taken by diseased persons to prevent the communication of the disease to healthy individuals; and secondly, of those to be observed by a sound individual who exposes himself to contagion. For these we refer to the original. The abortive treatment is directed to cut short the disease before it can be completely established. We shall speak of it only in its application to gonorrhœa in the male.

"At the commencement of a discharge from the urethra, and previous to any redness round the orifice, or pain felt in making water, the surgeon will frequently be able at once to cut short the affection and cure his patient; under other circumstances, this plan will not avail. It consists, in addition to the general means spoken of under the head of the abortive treatment of blenorrhagia, (namely, the free internal use of copaiba, cubebs, or turpentine,) in employing during the succeeding forty-eight hours twelve injections of the nitrate of silver at regular intervals: the strength of the solution of the salt should be two grains to eight ounces of distilled water. Let the injections be then left off, and cubebs or copaiba freely given. If the cases be recent, and the disease not too far advanced, this treatment will succeed fifty times in a hundred cases in checking the disease, and there is no fear of occasioning stricture or swelled testicle at this period of the complaint. Under this treatment the running will at once cease, but to complete the cure it will be necessary to continue the cubebs, diminishing gradually the dose. No further recourse should be had to injections, as a continuance in their use would only tend to keep up irritation; at the end of fifteen days the surgeon may allow his patient to resume his usual habits." (Acton, pp. 79-80.)

The injections should be used cold, by means of a glass syringe made for the purpose. According to M. Baumès, it is useless to attempt the

abortive treatment at a period later than forty-eight hours from the first appearance of the disease, yet he particularly insists upon the propriety of attempting it in all fit cases, since in many constitutions, as the lymphatic or scrofulous, gonorrhœa, when once established, is so prone to run on to the chronic state in spite of all remedies, and thus to expose the patient to all its attendant evils. M. Baumès thinks that in plethoric subjects this treatment would be more likely to succeed were it preceded by free venesection; he brings forward some cases where it succeeded well, the disease disappearing on the third day; these, however, are contrasted with others, where the injections were followed by violent inflammation of the urethra, the disease was prolonged, became rebellious, and very difficult to cure. It is a game, even in M. Ricord's view of the question, of double or quits.

Neither Mr. Acton nor M. Baumès have added anything to our knowledge of the treatment of established gonorrhœa either in the male or female. The means generally noticed by authors are passed in review. In fact, Mr. Acton's list of remedies is rather meager. There are many preparations of copaiba and cubebs very efficacious, to which he has not even alluded. M. Baumès' work has an appendix of formulæ, which is wanting in the treatise of Mr. Acton.

One of the most frequent and troublesome concomitants of a gonorrhœa is what is known by English surgeons under the name of swelled testicle, described by M. Ricord as "blenorrhagic epididymitis." This affection rarely occurs in the first week of a gonorrhœa, and not frequently in the second. From some statistical tables given by M. Gaussaile in the Archives de Médecine, we learn that during the first week of a gonorrhœa, three cases occurred; during the second, four cases; during the third, five cases; fourth, sixteen cases; fifth and sixth, thirty-nine cases; two months, two cases; three months, one case.

Amongst the predisposing causes of this affection Mr. Acton enumerates fatigue, violent exercise, repeated sexual intercourse, and any circumstance producing excitement of the organs. Various occupations predispose to it, as those of weavers, turners, grooms, and all trades where the testes are exposed to frequent friction. A flaccid state of the scrotum is also to be ranked amongst the predisposing causes: a strong cremaster and firm scrotum is rarely met with in individuals suffering from hernia humoralis.

"The direct cause consists in inflammation of the urethra. Observation shows that this may take place by direct continuation of the inflammation along the vesiculæ seminales and vas deferens to the epididymis; or it would appear, by virtue of a common law common to many mucous surfaces, extremities of canals may become sympathetically affected, the intervening surface not perceptibly participating in the inflammation. Of the fact there can be no doubt, that the epididymis is often affected, the cord remaining free from disease." (Acton, p. 93.)

Neither Mr. Acton nor M. Ricord attribute the occurrence of swelled testicle to the use of injections, or the quick suppression of the discharge, but to the extension of inflammation in the manner already named. The tables of M. Gaussail show, in conformity with the observations of M. Ricord, that the swelled testicle is much more frequent in the fourth and fifth weeks of the disease; hence the quicker the primary affection is

cured, the less chance is there of the testes becoming affected. A curious case, bearing upon the differential diagnosis of this affection, is mentioned by Mr. Acton:

"A young man, twenty-four years of age, was in the habit of amusing himself, when a boy, by pushing his testicles into the abdomen. Two months previous to his admission into the Hôpital du Midi, he contracted a gonorrhœa which discharged profusely; he continued, notwithstanding, his employment, (that of a wheelwright), a business which requires great bodily exertion: in about a fortnight after he felt a painful sensation in the left groin, or a colic, as he expressed himself, in the loins; and this becoming worse he entered the hospital a month after the commencement of his complaint, suffering under great pain in the inguinal region, which was greatly inflamed, whilst pressure on this part produced that peculiar feeling, but in a greater degree, which is excited when the testicle itself is compressed. On examining this patient, no testis was found on the left side of the scrotum; but, on passing the finger into the left inguinal canal, a rounded body was distinctly felt, resembling the testis in shape, and the patient stated that he experienced a similar kind of pain to that felt when the testicle on the opposite side was squeezed." (p. 95.)

The case was recognized as one of hernia humoralis, notwithstanding the unnatural position of the testes. It is not improbable that such a case might be mistaken for a strangulated inguinal hernia, more particularly when such symptoms as vomiting, constipation, and tenderness of the abdomen are present. The history of the concomitant affection, and the absence of the testes in the scrotum, are the chief points which would decide the surgeon. Should such a case coexist with a strangulated hernia, it would form a curious and puzzling complication, and one which would render the operation extremely embarrassing.

There are few cases on record illustrating the morbid anatomy of this affection, since it is one which is rarely fatal. Mr. Acton has, however, collected a few, where patients have died during the course of such an affection from other diseases. "In a simple case which M. Ricord presented to the Academy of Medicine, the epididymis was affected alone. In another case where severer symptoms had been observed, the tunica vaginalis presented traces of pus and false membrane. In the most severe forms lymph was effused amongst the seminiferous vessels." In two cases observed by M. Gaussail, and recorded in the Archives de Médecine, the following appearances were noted:

"CASE I. The epididymis was double the usual size, and hard. The testis of the same size appeared to present twice its ordinary volume; this however was found to depend in great part upon an accumulation of a thick turbid serum, somewhat bloody, which flowed out when the tunica vaginalis was opened. The tunica albuginea seemed thicker than usual; its surface presented a large number of minute vessels spreading out in various directions on its surface. The substance of the testis presented no appreciable change, its consistence was somewhat firmer than usual, and the colour deeper.

"CASE II. The vesiculæ seminales larger than usual, and firmer to the finger; on the left side they were much injected, and of a dark red colour, containing a large quantity of a yellowish white substance, which was somewhat granular. Both the vasa differentia presented similar traces of inflammation, and were filled with the same matter. The epididymis of either side was voluminous; the surface resembled the colour of lees of wine, but this discoloration did not extend to the testis. The testicles had their ordinary volume, some vessels were observed ramifying on there surface. A small quantity of reddish serum was found in the tunica vaginalis." (Acton, p. 91.)

The prognosis of this disease, although generally favorable, is not always so. It may terminate in suppuration of the testicle, an event however but very rarely happening, in hydrocele or chronic disease of the substance of the testis itself. We believe the latter consequence to succeed much more frequently to purely syphilitic diseases, than to gonorrhœal affections. We do not agree with Mr. Acton that hydrocele rarely succeeds to hernia humoralis, or does not require more than a palliative treatment. We have seen numerous instances where hydrocele has complicated the affection, with apparent enlargement of the testicle itself, if we may judge from the greatly increased weight of the organ. This species of hydrocele certainly does not often require the treatment by operation. We have found mercury pushed to salivation of great service in these consecutive affections of the testicle and its envelopes.

It would be useless to revert here to the treatment of hernia humoralis by compression originally proposed by Fricke of Hamburg, modified by Ricord, and advocated by several modern writers, amongst whom is Mr. Acton. We can only again repeat, from extensive experience of its utility, its vast superiority over all other methods of treatment of the primary forms of orchitis, succeeding to gonorrhœa. A full account of the method of performing it will be found in our Twentieth Number.

Before closing this article we would say a few words on inoculation in reference to primary venereal sores, and just enquire what is the position on which inoculation stands in reference to the therapeutics of primary syphilis. Inoculation pushed to its greatest extent by M. Ricord, has proved beyond a doubt the fact which English surgeons had always acted upon, though we must admit not with so good grounds as they do now, that syphilis is a specific disease, and not the result of the ordinary forms of irritation: M. Ricord has proved that certain sores when tested by inoculation produce a pustule running through certain stages, and terminating in a specific ulcer, capable of being propagated ad infinitum by the same means. M. Ricord however, has only shown that certain sores produce a characteristic pustule by inoculation, and therefore should be those only which are truly syphilitic, yet we find other sores, the result of sexual intercourse, producing secondary symptoms of the worst kind; and also yielding magically in some instances to mercury where all other remedies had failed, the old Hunterian test of true syphilis.

In the present state of science all we can say is, that certain sores the result of sexual intercourse, and not distinguishable by their external characters from other sores equally produced by sexual intercourse, yield a characteristic pustule by inoculation; but the sores which do not yield the characteristic pustule are equally liable to be followed by secondary symptoms, and are equally benefited under many circumstances by mercury; in fact with regard to the latter point, the external character of the sore has by almost universal consent been judged the criterion for the necessity of mercury.

We would now say a few words respectively on the merits and execution of the two works before us. Mr. Acton's work is the completest exposition of the opinions and practice of M. Ricord, which has yet been given in this country: Mr. Acton has neglected the whole of the other French writers, and followed M. Ricord alone, and so closely that he

has advocated many of what we consider to be the errors of the French surgeon, and which we believe the author, when he becomes familiar with English practice, will renounce. We regret this extreme exclusiveness of doctrine, more particularly as far as the first division of the work is concerned, since it lessens its value otherwise considerably. The first part of the treatise, that devoted to the catarrhal forms of disease, is the one which to the English surgeon will present the greatest degree of novelty, and this from the circumstance already mentioned of the greater facility with which the speculum can be employed in France. The volume of letter-press is accompanied by a folio atlas of plates, representing each of the forms of the syphiloid and syphilitic diseases both in the male and female, and in their primary, secondary, and tertiary stages. These plates are extremely well executed, and add much to the value of the work.

The work of M. Baumès is divided into two parts, the first theoretical, the second practical. In the first division M. Baumès treats of the syphilitic virus, and the contagious principle of gonorrhœa: here, as we have already shown, he is at issue with M. Ricord. The first part also considers the primitive and constitutional effects of this virus and contagious principle, and the general principles of treatment. The second volume contains the particular history and treatment of each form of disease, with an appendix of formulæ. This part although complete, and well and clearly arranged, contains little that the practical surgeon is not already familiar with. The first part is the most valuable, and is directed principally against some of the theories of M. Ricord, which the author controverts by powerful reasoning, and an appeal to well-established facts, drawn not only from his own observation, but from that of some of the principal surgeons in Europe.

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## ART. V.

*Ontleedkundig Onderzoek, Beschrijving en Rangschikking der Dubbelde Missgeboorten*, door W. VROLIK, Med. Doct. en Prof.—*Amsterdam*, 1840. 4to, pp. 232, met 9 Platen.

*Anatomical Examination, Description, and Arrangement of Double Monsters.* By W. VROLIK, M.D. and Professor.—*Amsterdam*, 1840. With 9 Plates.

WE shall best express for ourselves and ensure from others the praise which this admirable monograph deserves, by giving an abstract of its contents, and by removing it from the obscurity in which its language hides it. The only preface we need make is, that whatever form of monstrosity we refer to is illustrated by one or more well-authenticated and clearly described examples of it, which have either occurred in the author's own researches or been taken from the works of others; so that an abstract can, in reality, be little more than a syllabus of the facts which are distributed in the richest profusion throughout the work.

Excluding from his present enquiries the greater number of the examples of the *fœtus in fœtu*, because in them the one body rather resides within the other as a foreign substance than forms a part of one common organic system, Dr. Vrolik regards as double monsters all those in which

either a single part, or a whole system of organs is multiplied, more or less of the constituents of two bodies being united in one. He arranges all the members of this class in several principal divisions, the distinctions of which are based at once upon the prominent external and the most important internal characters; and within each class he again orderly arranges a number of subordinate forms determined chiefly by the extent and the complexity of the monstrosity in each.

I. The first division is that of *Heteradelphia;* this name being adopted from Geoffroy St. Hilaire to indicate those double bodies of which the components are very unequal, and of which one part may be regarded as the stem or trunk to which another organized part, or even a whole body, less developed than itself, is affixed like a parasite.

Both the nature and the situation of this parasite are various. Most often indeed it is fixed to the epigastric region; but it is also not unfrequently seated on the head, the pelvis, and many other parts of the body. On whatever part it is fixed, it is held there by no mere external adhesion, but by an intimate connexion of parts; by blood-vessels and a loop of intestine, or some such important organic bond of union, as to constitute one being with the twin stem to which it is inseparably connected.

Examples of this kind of monstrosity have been published in considerable numbers. Arranged according to the nature and the seat of attachment of the parasitic portion, Dr. Vrolik refers to and describes cases in which a second head was fixed to the epigastrium, to the back, to the skull, the vertex, and the lower jaw; others, and these more numerous, in which a pelvis and lower extremities seemed to grow from the walls of the abdomen, or from the other pelvis; others in which superfluous upper or lower (anterior or posterior) extremities were connected with the front wall of the abdomen, with the back, the side of the chest, the head, or the other limbs; others, again, in which the appended parasite was a headless monster with four limbs; and lastly, in a fourth class, the cases in which it consisted of a complete individual with a head and four limbs attached to the epigastric or the pubic region of its more developed fellow.

The common character by which this whole division is distinguished from other double monsters is the comparatively smaller size, and, in general, the defective development of the part which is termed the parasite. Imagine this difference removed by the fuller development of the parasite, by its obtaining all its own organic apparatus, and by its growing *pari passu* with the other, and an exact idea of complete duplicity* will be formed. It will be observed also, (and this is a point especially to be borne in mind both in this and in all the other divisions,) that in the several members of this class there is a regularly graduate series from those in which the superfluous part is only an ill-developed limb, to those in which the parasite differs from the stem in nothing but its inferior size, and its dependence for nutrition. The cases of the last kind however are rare; Dr. Vrolik has been able to find but three; much more commonly the parasite, even when it possesses its full numerical

* The term may appear strange; but our language has no other adapted to the purpose, and its use in this sense is warranted by the similar employment of *simplicity* in descriptions of both moral and physical characters.

complement of parts, bears many signs of defective development; it is hare-lipped, or a cyclops, or has atresia ani, or some other malformation by arrest of development. And this again is a fact of much importance to the author's theory of duplicity, which depends in great measure on the proof that in these heteradelphs there are always the rudiments of two bodies, though one or both may be defective.

The beings thus formed have rarely lived many years after birth, and the histories of the few that have survived are for the most part well known in the records of medicine. Perhaps the most remarkable is that of the Chinese A-Ke, of whom and his parasite, the little models are to be found in most of our anatomical museums, and who is described by Chapman in the Philadelphia Journal for November, 1821. The parasite's life is in general only vegetative. In one of the three cases, indeed, in which it possessed all the constituent parts of a body, it moved its limbs and appeared to have its own sensations, though it neither ate nor discharged fæces or urine; but in the others less perfectly formed, even these signs of individual life were absent, and in only one, that of the Chinese A-Ke, had the man who bore the parasite any voluntary power over its limbs. The nutrition of the parasite appears to depend entirely on the stem-body to which it is fixed, and through which it both receives its nutritive materials, and discharges its excretions. The one increases and decreases in size with the other; and, of course, the parasite dies with the stem. The influence which, in its turn, it exercises on its supporter is not always important. In the heteradelphi that die early, death commonly ensues from the ill formation of the main body; if they survive, the parasite seems to do harm, only as an ordinary tumour would, by its weight and by abstracting a certain amount of nourishment, so that those who, thus burdened, have grown up to childhood or manhood have usually been thin and delicate like men subject to some unnatural waste. At the same time, the author observes, it will always be far better to tolerate this evil than to risk an operation of removal, when the results of all the examinations yet made prove that the parasite is deeply and by important organs connected with the stem.

Of the general correctness of the author's opinion on this subject there can be no doubt; but exceptions must be admitted. In a very interesting paper on Congenital Tumours of the Pelvis, lately read by Mr. Stanley at the Medical and Chirurgical Society, a case was related in which Mr. Blizard removed with complete success from the sacrum of a child a large mass in which was included an isolated portion of intestine. There was no doubt therefore of this being a *parasite*, although it presented no external resemblance to the form of a second child. The cases described in Mr. Stanley's paper afford examples of duplicity with which the author seems unacquainted. In all of them perfect children were born with large masses composed of tough fibrous cysts containing fluid, and of firm masses like the ordinary fibrous tumours of the uterus, attached to the lower part of the trunk. In two of them there were isolated portions of intestine filled with a substance like meconium.

II. It has been already said that some of the rarer kinds of the heteradelphia approximate closely to the completely double monsters. In all the cases that stand nearest to the transition, the parasite has been found adherent to the epigastric region, and the kind of duplicity which

is most closely related to them is therefore that in which the two bodies adhere by their anterior surfaces,—the kind which Dr. Vrolik has termed *Verdubbeling van voren*, or, as we may be allowed freely to render it, *Anterior Duplicity*.

The most complete examples of duplicity yet known are found in this class, whose distinctive characters are, that two bodies in a state of nearly equal development are placed exactly opposite to one another, with their sterna connected together, and with their abdominal cavities either partially or completely confounded. Here, however, as in all the other classes, examples are found of gradations towards a state of singleness. Such are those in which the upper parts of the body being completely double, the lower are united so that there are but three, or two, lower (or posterior*) limbs. And, in like manner, although in many cases the two bodies are alike in size and other characters, yet there are many more in which one has so far surpassed the other both in size and measure of development, as completely to fill up the series between this class and the decided heteradelphs. While in the former class we find the closest approximations to duplicity, in this, and even among the most perfect of the double monsters, there are peculiarities which constantly recall to mind the parasitic attachment of the others.

With this nearly perfect external duplicity there sometimes corresponds an equal duplicity of internal organs. The bond of union, as far as the skeleton is concerned, is commonly a tough fibrous connexion between the lower extremities of the sterna and the ensiform cartilages which are set directly opposite to one another. The rest of the sterna and the ribs are usually distinct, and the thoracic cavities are thereby separated. In this case there are commonly two separate and perfect hearts, but in the cases in which the sterna are less completely separated, or (as happened in one case) entirely absent, only a single heart, or one partially doubled, with, for instance, two ventricles and four auricles, or otherwise malformed, is found. But it is particularly remarkable that in these, as in all other kinds of double monsters, there is no constant relation whatever between the respective states of the external and the internal organs, for the condition of the two digestive canals, even in those who are externally almost exactly alike, is subject to still greater varieties than the condition of the heart. The abdominal organs are always in some degree connected; the two livers are usually continuous; a spleen, pancreas, and stomach are commonly found in each body, and each stomach has its own duodenum, which after some length being continued into jejunum, unites with the other to form a single tract of small intestine, which again divides into two canals which lead respectively to the large intestine of each body. The lungs, and urinary and genital systems are always double.

The most remarkable example of this class was the well-known Siamese Twins; when exhibited they were not exactly opposite to each other, but stood side by side, or rather, obliquely one by the other; but this position, there can be little doubt, was acquired by the attempts which they

* The examples cited in this and all other parts of the work are taken indifferently from human bodies and those of lower animals; for brevity's sake, however, we shall use the terms *upper* and *lower*, when applied to the limbs, as synonymous with *anterior* and *posterior*.

had instinctively made to separate from each other in walking, or in lying and sitting down, and by the extension they had thus effected in their bond of union, which was considerably more slender than in any other yet described. From the analogy of several similar cases, Dr. Vrolik thinks it most probable that these youths were connected by the ends of their sterna and by some of their abdominal organs; and in discussing the question often agitated in this country while they were exhibited, whether any separation of them should be attempted, he introduces the figure and description of a double fœtus, which very closely resembled them in all the external circumstances of its duplicity. In this the two diaphragms met in the bond of union, and were connected by a kind of third central tendon at the points of the united ensiform cartilages. The peritonea were separate; but from each of them there was sent down near the middle of the bond of union a prolongation forming a kind of coronary ligament to a considerable portion of liver which passed across the bond and connected the main masses of the livers of each fœtus. All the other organs were completely separate.

As far as the Siamese youths were concerned, the question of operation was set at rest by the observation of Mr. Mayo, communicated at the Conversazione of the College of Physicians, March 8, 1831,* that when either of the youths coughed the bond of union swelled up in its whole length, proving that they had but one peritoneal cavity of which a transverse prolongation passed bridge like through the connecting medium. And in every case the probability of having to cut through a piece of liver or a peritoneal canal, must render an operation unwarrantable, unless indeed after the death of one of the bodies during the health of the other. The case reported by König† has scarcely authority enough to support a contrary opinion.

III. The varieties of form in anteriorly-doubled monsters are closely limited by the partial union of the sterna and the general separation of the thoracic cavities, and (through their medium) of the whole upper part of the body. In the next class, the examples of which are distinguished by the name of *lateral duplicity*, there is no such limit; and between the highest degree of duplicity found in it and the lowest, or that in which the duplicity is most nearly reduced to singleness, there is a far more numerous series of intermediate forms than in any other of the divisions.

"In lateral duplicity the two bodies are not set opposite to one another, but are turned sideways from one another. They have a common thoracic cavity, for the formation of which (at least in the highest degree of duplicity) the right ribs of one body and the left of the other proceed towards the anterior and posterior aspects, and are there connected with an anterior and posterior sternum. The best idea of the construction of this osseous fabric may be formed by supposing the two complete chests of two bodies to be set one against the other, and that then the anterior extremities of the right ribs of the right body, and those of the left ribs of the left body unite with one sternum and pull it forwards, while, in the same manner, the left ribs of the right body and the right of the left unite on the posterior aspect with the other sternum and carry it backwards. The consequence is that the two vertebral columns are turned away from one

* See the Medical Gazette of that month.
† Ephem. Nat. Curios. Dec. 2, ann. viii.

another, and that all the parts above and below the thorax are double. (p. 75.) ..... By the formation of this common thorax the lateral is completely distinguished from the anterior duplicity in which the thoraces are (*commonly*) connected only by the points of the sterna, and, as to their cavities, are separate." (p. 173.)

And with these differences of external construction, others not less important of internal arrangement coincide, which fully justify the separation of the two forms, however similar the external appearances of many of the examples of either may be.

The varieties of lateral duplicity, which are so numerous that their brief description occupies nearly half the book, may be divided into two principal sets. The first begins with the complete duplicity of the whole body and ends with its perfect simplicity; in the second the duplicity of the body remains, but the head gradually becomes single. The forms included herein may be briefly summed up as follows:

1. Complete duplicity, with all the external parts, and sometimes the abdominal and pelvic organs double, and with one common double-sized thoracic cavity formed in the manner just described, and containing four lungs and (in all four cases) only one heart. The examples of this form are very numerous, and are to be met with in all our large museums.

2. In the examples of this second group, which exhibits the first step towards simplicity, one of the sterna may be traced in a succession of specimens becoming gradually narrower, and permitting a closer approximation of the two corresponding upper extremities, till, in some examples, they are completely united, and there are found only three limbs above with three or four below. The two scapulæ on one aspect of the bodies, for example, are merged into one; or there are two scapulæ on each aspect, but one pair of them have only one humerus between them, and this splits below to articulate with two lower arms; or there is but one lower arm, and this bears supernumerary fingers. In some way, the varieties of which are very numerous, there is a tendency towards union of two of the upper extremities.

3. In the third group we have a repetition of the same series of changes in the lower limbs as in the second was traced in the upper; here, as there, presenting numerous varieties, in the last and lowest of which only three lower limbs, and the third of these ill formed, are found.

4. The third limb has now gradually disappeared, and with a complete duplicity above the pelvis, there are but two limbs below it, and these well formed. In this class are placed with many others the Ritta-Christina monster described by M. Serres, which lived to the eighth year, and the still more remarkable example mentioned by Buchanan of a two-headed man twenty-eight years old, who lived in the reign of James the Third of Scotland.

5. The union proceeding, and this simplicity of the lower part of the body being retained, examples come next in which the upper parts also are united; the two superfluous upper limbs being united into one, presenting a single upper arm with a double fore-arm and hand, or a single upper arm, fore-arm, and hand with ten figures, or only a malformed limb, or a mere projection occupying the place of the superfluous limbs.

6. Even this last indication of duplicity of the upper parts ceases, a scapula only remains, or this also is absent; and next, one of the sterna

having disappeared and the vertebral columns having been connected on the corresponding side by their respective ribs united into single arches, these now become gradually shorter, and the columns approach each other more and more nearly till they are connected by only a cartilaginous substance in the place of ribs, or are at some part confounded.

7. In this next group both the upper and lower parts of the body are single; the vertebral column is single all below the cervical region, or exhibits only a trace of duplicity (to which something similar is often presented by the sternum), but at the cervical region becomes double, and on each portion bears a head. Of this also there are many examples published, and from them there is a gradual transition through the eighth group in which the portion of the cervical part of the column that is double becomes less and less, to the ninth in which the two heads are seated on an apparently single neck in which all the cervical vertebræ are simple, or only bear traces of duplicity, except the first two or the first alone.

10. Hitherto the duplicity of the head was perfect; in this group the two heads also begin to coalesce, and in a considerable number of cases referred to, gradations are traced in which the adjacent ears are very closely approximated, and the heads are united behind; then, in which one ear only is placed between the adjacent surfaces of the two heads, and this gradually and at last totally disappearing. Next in order are the cases in which the adjacent ears being lost the two adjacent and middle eyes first become very close and then are confounded in one cavity; then those in which there is such a union of the heads that the two upper jaws are articulated with one lower jaw; and lastly, those in which the head is doubled only in individual parts, or in which there is one perfect head with some imperfect part or parts of another attached to it.

11. The eleventh group of this division includes the cases of lateral duplicity in which the body is single in the middle but doubled above and below (or, in brutes, anteriorly and posteriorly). In these, which are of rarer kinds, a single neck bears two more or less completely separated heads. The vertebral column is for a considerable length single, but at its lower part again divides and bears two sets of lower extremities.

12. In this the body is single above and doubled below. In the 13th there is tendency towards or even complete singleness of the head, but all parts of the trunk and all the limbs are doubled, an arrangement by which, as already stated, these form a series entirely distinct from the rest. In some of these cases the two heads are found coalesced below; in others, to which the name of janiceps has been given, one face is directed backwards and the other forwards, the remainders of the two heads being merged into one; in others, one face is well, the other very deficiently developed; in others there are only the indistinct traces of a second head presented in the existence of one or two ears on the posterior aspect of the more perfect one; in others this trace of duplicity is still less evident; and lastly, in the remainder of the group it is entirely lost, and one head only, and that well formed, is found upon the double body.

IV. The fourth great division which Dr. Vrolik has made includes the

cases in which there are two complete bodies with the lower portions of their respective trunks united, so that there is a head with upper extremities both above and below (the bodies being placed in the same straight line), and on either side of the part at which they meet two lower limbs. One may best conceive this arrangement by supposing two children stuck together by their buttocks, and so fixed with wide-spreading lower limbs. A common body is thus formed with a head at each end, with two upper limbs both above and below, and with two lower limbs, one belonging to each fœtus on the right, and two on the left of the united portions. A few cases only of this remarkable monstrosity are recorded; and in these the duplicity was not always complete, but exhibited in some the same tendency towards simplicity as was noticed in the others. Thus in some there are but three lower extremities; in others there are but two, or two with a third ill-developed on the other side; and again in other groups there are those which have a perfect head at one end of the trunk but an imperfect one or none at all at the other.

These monsters have been known to live a considerable time; their capacity for life being probably owing to the separation of the hearts and the absence of malformation in the more important organs of the body. The umbilical cord is single and never has a double set of vessels; an apparent proof (confirmed by the similar examples in other classes), that the one body is not formed of the materials of two, a conclusion which is supported by the coincident singleness of the anus and urinary bladder, and the union of the intestinal canals.

V. The fifth chief form is the *posterior duplicity* in which two bodies are united by their backs or a part of them. The union may be at the pelvis (which is most common, and had occurred in the well-known Hungarian sisters, who lived to their twenty-second year), or at the back of the vertebral column.

VI. The sixth is the *superior duplicity*, in which the two children are connected by their skulls, the bones of which are united so as to form a single cavity. In these also the place of union varies greatly. The frontal bone of one coalesces with the malar or the occipital bone of the other, or the foreheads are attached to one another, or the side of one head to the front of the other. But all these are very rare, and of each kind only one or two examples can be found on record.

VII-VIII. With the last division is concluded the description of the true double monsters. A seventh division, however, is added to take in the *triple-bodied* monsters, of which one instance only is known to have occurred in the human subject;* and an eighth to include the numerous cases of duplicity of individual parts of the body, the rest being single. Instances of this are found in parts about the head, chest, abdomen, and limbs; such as, for examples, two mouths, supernumerary teeth and horns, two œsophagi or duodena, double hearts, or supernumerary cavities in one otherwise well formed, a double penis and urethra, a double clitoris, supernumerary breasts, kidneys, vertebræ, ribs, fingers, toes, or whole limbs.

Such is a brief account of the facts of Dr. Vrolik's work; we now proceed to point out some of the generalizations of which they evidently

* Atti dell' Academia de Cattania, t. viii. 1834.

admit, and the opinions respecting the origin and production of double monsters which they seem to support.

The double monsters form collectively one class of organic beings, which, however different in their several degrees of malformation, may be arranged in one continued series. As the lowest degree of duplicity may be mentioned that of a single part of the body, for example, a double or supernumerary finger; as the highest, a complete double monster with two heads, four upper and four lower limbs, and two trunks, such as the Siamese twins formed. And between these two extremes there are different forms of duplicity which gradually run one into the other.

There is no positive or constant relation between the external and the internal organs as to their degrees or modes of duplicity. In the completest duplicity of the exterior, for example, the heart is often single or even shows signs of having been arrested in its development; and on the other hand in the more nearly simple forms the heart is usually either partially or completely double. The histories of the cases of anterior and lateral duplicity furnish abundant proofs of this. Nor is there any closer relation between the condition of any other internal organ and that of the exterior than there is between the heart and it; for in nearly complete external duplicity any of the internal organs may be single. When there are two trunks, indeed, the urinary and genital organs are commonly double; but as for the stomach, the liver, and the lungs, the correspondence between them and external duplicity follows no other rule than that where there are two necks there are two tracheæ and two œsophagi, and through their medium the lungs and stomach are also doubled. When, in like manner, the stomach is double, each has its spleen and pancreas; but the state of the liver is very variable; sometimes there are two, sometimes but one with a single or a double gall-bladder; and these differences often occur in the same form of external duplicity.

Parts placed on the surface of the body are more liable to multiplication than the internal organs, and duplicity of a single part is therefore much less rare than the formation of a complete double body. The upper half of the body is more frequently doubled than the lower, probably in consequence of its earlier development, and the admitted preponderance of the upper parts of the body. The union of the two bodies takes place only between similar parts. The more each of the bodies is developed, the less is the bond of union between them, as the examples of the Siamese twins and the Hungarian sisters sufficiently prove. And with this law is connected another, namely, that the probability of growing up is greater in the same proportion as the bond of union is smaller, and the coincident confusion of internal organs less, as the two same double monsters, and others besides them prove. So also, the further the several organs are from the situation at which the bodies are united, the more perfect they are,—one body is almost always less developed than the other; in the heteradelphs this is always the case, and in others the difference between the two bodies, though less evident, is scarcely less constant than in them.

There are not commonly any signs of a double monster having been at first two individuals. For except in the cases of posterior and superior duplicity, and some singular examples of attachment of the umbilical

cord of one fœtus to the head or body of the other, there is never more than one placenta and one cord, and the latter usually contains only a single set of vessels, which divide when they reach the abdomen. And even in the posterior and superior varieties of duplicity it is not yet certain that there are two placentæ; in some cases the placenta was positively single, and in the remainder it has very rarely been examined.

And after all these facts, "what explanation," says the author, "is to be given of the origin of double monsters? The answer may be brief,—none," (p. 219.) It is not difficult to find in his volume of facts, enough to cast doubt on any hypothesis; yet the subject acquires so much more interest by being treated as capable of explanation than it has in its mere detail of facts, that we must undertake the consideration of the probable origin of double monsters, even at the risk of "wading in the region of guesses," from which the author, though he adopts the same views as we do, almost holds himself aloof.

The choice seems to lie between the three hypotheses, to which, variously expressed, nearly all ever suggested may be reduced, namely, 1st, that of originally double ova; 2d, that of an excess or wrong distribution of formative power in a single ovum; and 3d, that of the adhesion and fusion of two single ova. On the comparative merits of the first and third, as parts of the general doctrine of monsters, one of the most interesting physiological discussions extant is recorded in the Mémoires de l'Acad. des Sciences de Paris, between 1724 and 1743. The chief disputants were Lemery and Winslow; the contest lasted nineteen years; it engaged the attention of all anatomists, and called forth writings by Haller and a crowd of authors of less note, and was only terminated by the death of Lemery. Every argument that could be founded on the knowledge of those days was brought forward, and the subject was, for the time, utterly exhausted; but the facts accumulated in later years have furnished such volumes of additional evidence, that the same question between original and acquired monstrosity, as far as it relates to double monsters, may even now claim to be discussed.

The first hypothesis, which supposes an original formal fault in the ovum, is inconsistent with the fact, that in its earliest periods the ovum has no relation as to its form with the being subsequently produced, whether naturally or unnaturally. It contains in miniature no one of the organs of the fœtus: it is a mere cell, including two or more kinds of other smaller cells which in part form, but in greater part elaborate, the perfect organs. But none of these cells have any formal relation to the organs, *they* are contained only *potentially* in the ovum. When therefore we know that in an ovum there is neither head nor heart, trunk nor limb materially present in miniature form, it is not imaginable that there should be such a partial substantial doubleness of an ovum as should produce two heads or two hearts, or any other organ twice instead of once. When no organs are formally present single, there, *à fortiori*, there can be none double. A material duplicity of an ovum could only consist in an additional number of its homogeneous cells; but the result of such a condition would probably be, not a numerical increase, but only an unnatural size of the organs.

The second hypothesis agrees with the first, in that it admits the pro-

bability of an original fault in an impregnated ovum being the common precedent of duplicity, but differs from it chiefly in supposing this fault to be one, not of form but of power; and in supposing further that the fault may be either original and inherent, or the result of some extraneous influence.

There is one fact which must be mentioned at once, because it establishes fully that monsters of all kinds are at least sometimes the products of ova originally, or by impregnation rendered anormal, and thus decides the claim of this second hypothesis to a fair consideration to be greater than it has been commonly regarded. This fact is, that monstrosities are often hereditary, which it is obvious they could not be if their production always depended on some occurrence subsequent to impregnation. The occurrence of the same or similar defects in several members of one family, for example, must be the consequence of a fault in the parents conveyed to each several embryo. If such defects are similar (as they often are) to those existing in the mother, they must be ascribed to faults inherent in the ova; if they resemble deformities in the father, they must be regarded as due to faults in the semen; or if both parents be well formed, still the coincidence of malformation in several of their offspring cannot be regarded as accident, but must be ascribed to some fault of relation between the materials for reproduction contributed by each.

Having thus established that original fault in an impregnated ovum may be the foundation of monstrous productions, we must next point to the important fact determined by Dr. Vrolik, that double monsters form one series, among whose several members the degrees and modes of deviation from simplicity gradually increase, and pass without one abrupt step from the addition of a single ill-developed limb to the nearly complete formation of two perfect beings.* Now, if this be true, no hypothesis can be acceptable if it do not plausibly explain the origin of the whole series of double monsters, or if, though it may suffice to explain the facts in one part of the series, those in another part are opposed to it. And here is a fair objection against the hypothesis of two originally perfect germs. Grant that by it we might explain the formation of the Siamese twins, for example, by supposing that the opposite parts of the vitelline membranes, and then of the ventral arches of two normal ova became adherent and were fused, or that a similar contact of ova with their dorsal or ventral arches still open for a greater or less extent might explain (if we can imagine its occurrence otherwise than by accident) the formation of several of the more perfect instances of duplicity; still, if the same hypothesis is altogether opposed by the simpler forms of duplicity, it is surely not tenable. It may explain the union of two nearly complete digestive canals, but it cannot account for the existence on a child's sacrum of a shapeless mass, containing an isolated portion of intestine, as in Mr. Stanley's cases. And still less can it explain the

* This fact is one of many proofs, that error lies at the very foundation of M. Isidore St. Hilaire's system of teratology, which is based on the importance of the several deviations to the life or comfort of the individual. On this rule he separates widely the lower from the higher degrees of duplicity. But relative importance depends on the degree, not on the kind, of these monstrosities, and therefore cannot form the basis of a sound arrangement.

existence of a superfluous limb; for the limbs are mere off-shoots, and are produced at so late a period, that if we could imagine two embryos to come in contact by their shoulders or pelvis, and a fusion of those parts to take place, we should still have to explain how one of them leaving only an arm or a leg behind him, could, for the rest of his substance, head, trunk, and all, wholly disappear.

This difficulty lies in the way of the explanation by fusion of all the simpler forms of double monsters. For although recent observations have removed the main objections that Lemery had to contend with, by proving the possibility and even the facility with which two embryonic organs when brought together may be fused into one which will exhibit parts of both, yet we know of nothing that renders it at all probable that any extent of fusion should altogether swallow up a considerable portion of any embryo already formed. On the contrary, in such fusion as is seen in single monsters, the Cyclopes for example, the parts fused are united, but in no degree destroyed, each seems to preserve nearly the condition which it had at the moment of contact.

But there is no such difficulty if the hypothesis of excess of power be adopted. All the organs exist in the impregnated ovum solely *in potentiâ*, and we can imagine no other explanation for the production of them in superfluity than an excess of that power.* Indeed this is commonly admitted for the examples of supernumerary members, and for the whole class of monsters by excess (as they are called), by those who for other cases of duplicity are fusionists.† We contend further, that different degrees or quantities of excess of power lie at the origin of all the cases, the degree of excess determining in each the degree of duplicity. If a certain excess of power be admitted capable of producing any one case of duplicity, other amounts of excess may be believed capable of producing all the other cases which differ from it only in degree; and Dr. Vrolik's work demonstrates that all double monsters may be referred to differences in degree of deviation from the normal simplicity.

But it may be said that something more than excess of power is needed to produce any given double monster; for although it might determine the production of something superfluous, it could not determine whether it should be an arm or a leg, or the whole upper or lower half of a body; nor could it alone render these monsters so regular as they are, but arms might grow from a pelvis, and legs from a head, and so on. We should answer that diversities of power in an ovum do not of necessity carry with them tendencies to deviate from the directions in which that power usually acts, although the heteradelphs show that they sometimes do. In normal development we are obliged to assume that the definite power in the ovum has to follow definite directions; but alterations in the amount of the former may surely exist without any change in the latter, nor can

* It would seem pedantic to use *potentia* instead of *power;* but in modern physiology the former usually implies not power only, but power with a tendency to the production of a definite effect; and in this view we shall use the term *power*, and the related adjective *potential*.

† M. Serres' early idea of the extra-development depending on the accidental (?) branching of an artery which then becomes the main artery, and in plain terms the producer of the new part is certainly incorrect. The blood-vessels are formed subsequently to the parts which they afterwards supply with the material necessary for their maintenance.

we tell why the normal power of an ovum always produces parts in their right places any more than why a power greater in degree does. Only on the whole, when the power in an ovum is absolutely greater than is natural, it is, *à priori*, much more probable that supernumerary parts will be produced in or near their right places, than that they will not.

In this last respect our hypothesis has the advantage of its rival. It was a main objection against the doctrine of Lemery that if two germs came in contact by accident (as he supposed) they could not exhibit any regularity in their mode of attachment, but faces would be forced into chests, abdomens into spines, and so on. The moderns who adopt the same hypothesis suppose that the ova come in contact, not by accident, but by an *attraction de soi pour soi*, of which the influence is that the two ova being by accident set face to face, or back to back, or in any way *similia similibus* will be drawn to each other, and will unite by similar parts. But, with all respect for the authority of M. Geoffroy St. Hilaire and his disciples, who regard this as "la règle suprême de tous les arrangemens et de toutes les modifications organiques chez les êtres composés,"* we confess that we can find no good evidence that such an *attraction* exists. We can see in it nothing more than a very happy expression of a fact which it in nowise explains. The extraordinary notion of MM. Delpech and Coste that such an attraction may be the result of electric currents is certainly no evidence of its existence, theirs being entirely imaginary. And the reasoning in its favour seems no better than the facts, for we can find nothing but this kind of circle; monsters adhere by similar parts; therefore there is an attraction de soi pour soi, there is then such an attraction, and therefore double monsters so adhere. We believe therefore that this attraction is hypothetical, and if it be so, surely the hypothesis which involves it and an accident as essential elements is less probable as well as less sufficient than that which we maintain. It is scarcely better than Lemery's of mere accident, for it requires not only the accident of a particular position of the ova, but that of their being of the same sex, for it is an almost invariable rule that both of the moieties of a double monster are either male or female, a fact which plainly agrees better with the notion of a single ovum possessing potentially certain sexual characters, than with that of two ova accidentally brought together.

We have assumed hitherto an absolute excess of power, but many double monsters exhibit excess in one part and defect in another, so that we must suppose in our hypothesis that in these cases the power, more or less excessive in quantity, is also wrongly distributed. Nor is this inconceivable, for since in the normal developmental power we must imagine at least two elements, *quantity* and *distribution*, and must acknowledge that for the attainment of a perfect result, the quantity must be distributed in definite proportions to each part; so it is not improbable that in certain circumstances of fault in the ovum, a normal or excessive power may be distributed disproportionately in the several parts.† But in the hypothesis of fusion the same facts are not clearly explicable. If

* Isidore St. Hilaire, Histoire des Anomalies, t. iii. p. 530.

† Sometimes both parts of a double monster, though well formed, are very small; this, however, is the result of defective *growth* which has no necessary relation to the development.

two embryos are fused by their ventral arches there is no evident reason why their dorsal arches should not be normally developed, for these are the materials for two perfect children, of which less than the quantity necessary for both are employed in the formation of their common inferior cavity. And in like manner in all similar cases, there is no reason why two healthy ova by contact should lose power. The only probable explanation, if the hypothesis of fusion be adopted, is to suppose that faulty ova are more likely to come in contact than healthy ones, but the maintainers of that theory would probably not cede so much to those who hold that ova are often originally faulty.

We have already mentioned the best proof that these original faults do sometimes exist, and it will be found that the objections commonly urged have but little or no force against their being the precedents of most or all cases of duplicity. For although it is fair to require that the same hypothesis should serve for the whole series of double monsters, because they differ only in degree, yet it is not reasonable to expect that both this series and those of monsters by defect of development and by malposition of parts should have a common mode of origin. The members of one series may most frequently be formed from ova, faulty in power, and those of others from ova originally healthy; and the arguments against the general notion of original faults may be valid in their relation to the one series, but utterly invalid when applied against the other. For example: the fact that accidents in the course of pregnancy, such as external injuries, mental impressions, and the like, sometimes give rise to the production of monsters, is of no weight against our hypothesis of the formation of double monsters, for such accidents are not (except by coincidence) followed by formation of double monsters. In the whole series of experiments by M. Geoffroy St. Hilaire, it is expressly stated that no instance occurred in which the disturbance of an egg during incubation resulted in the production of a double monster. Yet the influence of these accidents is the chief evidence against the hypothesis of original fault in germs, and to the cases in which their influence is exerted they are applicable enough. But for these double monsters which are not referrible to the influence of accidents, we can find no valid argument against the general supposition that they are derived from faulty ova; and, we may add, that even if an accident were occasionally proved to have influence in producing them it would not disprove our hypothesis. It is not essential to it that the germ should have a tendency to duplication, either only before or only directly after impregnation; the possibility is not denied that at any period of development an accident may give rise to a greater energy of the productive power; but it is rather in our favour that such accidents are more commonly, if not always, followed by defect. They prove the power to be a thing alterable and subject to changes dependent on external influences, and that which by one circumstance may be depressed, it is imaginable may, by another, be increased.

To sum up, therefore, our reasons for rejecting the hypothesis of fusion of ova, in favour of that of excess or irregular distribution of developmental power, for preferring (to use Dr. Vrolik's expression) to regard them as examples rather of singleness tending to duplicity, than of doubleness tending to simplicity, they are briefly these: that it is probable

the whole class of monsters by excess (including those commonly so-called, and those usually regarded as double monsters) owe their origins to different degrees of one common fault; and consequently that the explanation of their origin ought to be the same for all; that no kind of fusion can account for the production of supernumerary individual organs, the rest of the body being single; but that it is not impossible that excess of power in one ovum, which all admit can alone explain the lower degrees of duplicity may, in proportionally higher degreee, produce the more completely double monsters, or even two such separate individuals as are sometimes found within a single amnion.

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Art. VI.

1. *Nouvelles Considérations sur les Rétentions d'Urine, suivies d'un Traité sur les Calculs urinaires, sur la manière d'en connaitre la nature dans l'intérieure de la Vessie, et la possibilité d'en operer la destruction sans l'opération de la Taille.* Par le Dr. Civiale.—*Paris*, 1823. 8vo, pp. 172.
2. *Exposé des diverses Procédés employés jusqu'à ce jour pour guérir de la Pierre sans avoir recours à l'opération de la Taille.* Par J. Leroy d'Etiolles, m.d.—*Paris*, 1825. 8vo, pp. 232.
3. *De la Lithotritie ou Broiement de la Pierre dans la Vessie.* Par le Dr. Civiale.—*Paris*, 1827. 8vo, pp. 254 and lx.
4. *Tableau historique de la Lithotritie.* Par J. Leroy d'Etiolles.—*Paris*, 1831. 8vo, pp. 102.
5. *Du Traitement Médicale des Calculs urinaires et particulièrement de leur Dissolution par les Eaux de Vichy.* Par Ch. Petit, m.d.—*Paris*, 1834. 8vo. pp. 58.
6. *Parallèle des divers Moyens de traiter les Calculeux.* Par le Dr. Civiale.—*Paris*, 1836. 8vo, pp. 492.
7. *Essai sur la Dissolution de la Gravelle et des Calculs de la Vessie.* Par A. Chevallier.—*Paris*, 1837. 8vo, pp. 50.
8. *Nouvelles Observations de Guérisons de Calculs urinaires au moyen des Eaux thermales de Vichy.* Par Ch. Petit, m.d.—*Paris*, 1837. 8vo, pp. 102.
9. *Traité de l'Affection Calculeuse.* Par le Dr. Civiale.—*Paris*, 1838, 8vo, pp. 714.
10. *Exposé d'un Rapport fait à l'Académie de Médecine, relative à des Expériences sur l'efficacité des Eaux de Vichy contre la Pierre.* Par Ch. Petit, m.d.—*Paris*, 1839. 8vo, pp. 51.
11. *Histoire de la Lithotritie.* Par J. Leroy d'Etiolles.—*Paris*, 1839. 8vo, pp. 120.
12. *Du Traitement médical et préservatif de la Pierre et de la Gravelle.* Par le Docteur Civiale.—*Paris*, 1840. 8vo, pp. 452.
13. *A Treatise on Stricture of the Urethra, with an Appendix on Dilatation by Fluid Pressure, &c.* By James Arnott, m.d. Second Edition.—*London*, 1840. 8vo, pp. 234.

The number of capital surgical operations performed at the present day is generally held to be smaller than it was once; and it is quite cer-

tain that the general character of operative surgery has undergone a great change within even a limited period of time. The surgery of the middle ages was as barbarous and sanguinary as possible; the instruments employed were literally instruments of torture, and the chafing-dish and searing-iron were held as necessary to the surgeon then, as lint and sticking-plaster have become of late. The surgery of former times was cruel, then, and there is every reason to believe that operations were frequent. In the present day, on the contrary, surgery is as gentle as in the nature of things it can be made, and the glory now is to avoid an operation, not to have it to do. To us, however, there appears to have been one exception as regards the general disposition to operate, and this is in reference to Stone in the Bladder. Surgeons have never shown themselves solicitous about any means proposed for the relief of this dreadful malady that did not involve an operation; and we can almost pardon them for their anxiety to preserve lithotomy. Among the great operations, there is certainly none to compare in point of brilliancy and eclat with that by which a fellow-mortal is freed in a few moments from the tortures of stone: truly it is a temporal redemption that is achieved for the sufferer; and for our own part we cannot suppose that the leader of armies and of navies in the moment of victory experiences higher emotion than the skilful surgeon when he exhibits to his patient the cause of his long suffering, and tells him that now it is at an end! And is it not a most touching sight to see one man commit himself, bound hand and foot, voluntarily made incapable of resistance, into the hands of another for life and deliverance, or for death? and is it not an office of singular sacredness and responsibility which that other undertakes when he consents to receive this man into his care upon such conditions! Alas, that the courage displayed, that the responsibility incurred, should so often fail of the end proposed! for of all of every age who undergo the operation for stone, one in seven or eight will be lost; and of all who submit to it in the interval when man's life is most truly precious, when he is the mainspring in the varied business of the world, one at least in three or four will perish. No wonder then that, despite the benefits which the operation for stone has conferred, men not devoted to its practice have long eagerly sought either to escape the necessity for appealing to it altogether, or to find for it some substitute. To give an account of the attempts that have in modern times been made in this direction will be the business of the following pages.

The means by which a stone in the bladder has been sought to be removed are these:

I. By effecting its solution; and this has been attempted in two ways: *a*, by the agency of medicines administered by the mouth; *b*, by chemical agents thrown into the bladder.

II. By removing it through the urethra entire; and this plan has been tried in two ways: *a*, with previous dilatation of the urethra; *b*, without dilatation or previous preparation.

III. By grinding or breaking down the stone in the bladder, and so removing it piecemeal through the urethra, (Lithotrity.)

IV. By removing it rapidly and at once through an incision practised about the neck or fundus of the bladder; the parts which oppose resist-

ance to the exit of the stone being dilated when it is small, torn when it is large, (Lithotomy.)

v. By removing it without tearing, or violence of any kind, through an incision into the urethra in the perineum, and slow dilatation of the membranous and prostatic portions of that canal, and of the neck of the bladder, (Lithectasy, Cystectasy.)

I. Removal of the Stone by Solution.

1. *By means of medicines administered by the mouth.* Until the discovery of the chemical composition of urinary calculi was made, none but blind and empirical attempts in this direction could be undertaken. In a science like medicine, however, empiricism has sometimes led to fortunate results; and in reference to the treatment of stone by internal medicines, which from their supposed virtues were called *lithontriptics*, it may be said that if less were known, nearly as much had been accomplished, before science shed any light upon the way, as has been achieved since she poured a noon-day splendour upon it. The use of the alkaline earth, lime, is as old as the time of Pliny, and the medicines of Stephens and her immediate successors, if less elegant in their forms, were essentially of the same nature and of the same potency as those that are in use at the present hour. Very different degrees of credit, however, have been attached to the powers of lithontriptics at different times. Like other specific remedies they were proposed as of universal efficacy, and having failed to do impossibilities they were then proclaimed as useless. But whilst many of the articles which were once regarded as lithontriptics have deservedly shared the fate of the generality of specifics for individual diseases, others have undeservedly been involved in the same neglect and oblivion.

In the nature of things there are certain articles which influence the renal function in a way that bears directly upon the disposition of its product, the urine, to deposit one or other of its concrescible elements, and which must necessarily, therefore, be of avail if directed aright in cases of calculus. The confidence once universally reposed in alkaline medicines in cases of stone could not, therefore, have been all misplaced. But an unlucky star seemed still to have dominated the proposal of every remedy for stone save in the way of operation. The advocacy of a Fourcroy may have forced the consideration of such means upon the attention of the medical profession for a time; but opposed by the most influential of that class of practitioners who are almost alone consulted in cases of calculus, they have soon sunk back into the shade. The contest, however, in regard to the value of alkaline lithontriptic medicines is by no means yet decided; it has been renewed of late years, and has hitherto been waged with nothing like defeat to their advocates. On the contrary, the most candid enquiry would rather lead to the conclusion that the extent to which alkaline medicines may prove beneficial in cases of calculus has been under-estimated rather than over-estimated.

The alkalis when first propounded as of sovereign efficacy in calculous complaints, were exhibited either in the state of subcarbonate or pure. This implies administration in a large quantity of fluid; and then the conditions were fulfilled which were necessary to the best effects of these medicines. The concurring testimony of all the best authorities of the times gives us assurance of the signal benefit that was often derived from

the use of Stephens's medicines as well as those of her immediate successors. Stephens's remedy consisted especially of a mixture of calcined egg-shells and Castile soap, which was always washed down with copious draughts of some simple vegetable infusion or decoction. The lithontriptics of all the medical practitioners of the same period were of the same essential nature. The ingenious Dr. Whytt relied upon about an ounce of Castile soap and two or three pints of lime-water in the course of the twenty-four hours. Dr. Chittick had a tin vessel of the capacity of two quarts filled with weak veal-broth sent to his house every morning for medicamentation, and this quantity of diluent with some solution of potash added to it, was the dose which each of his patients took during the day. Now, provided the stomach did not rebel against a course of this kind, we are perfectly certain, as chemists and as physiologists, that it was calculated to act beneficially in a large proportion of cases of stone; in all cases, to wit, in which calculi of lithic acid, of the lithate of ammonia, and of the triple or mixed phosphates are contained in the bladder. There is, perhaps, no fact in therapeutics better established on the basis of experience than the influence of weak alkaline solutions upon the generality of urinary concretions. Qualities can readily be communicated to the urine by the use of alkaline medicines, that give it positive solvent or disintegrating powers, which though not of any great amount, are nevertheless quite decided, and have only to be maintained for a sufficient length of time to prove of signal efficacy.

Much good, then, was done during the period that alkaline medicines were administered in this way; some were freed from their calculi entirely, and many more escaped from a life of absolute torture to one of comparative ease; for the alkalis have this most admirable quality in addition to the one they possess as direct solvents of stone, that they allay the irritability of the living tissues with which the foreign body is in contact to such an extent that frequently its presence ceases to be perceived, and the person with a stone in his bladder comes at last to be in little worse plight than another who has nothing of the kind.

The progress of chemistry and pharmacy by and by led to the preparation and prescription of the carbonated instead of the caustic alkalis as heretofore; and, used in the old way with plentiful dilution, these are as good medicines as the others; but the alkaline bicarbonates especially are so mild that they soon came to be administered in no more water than was necessary to get down the dose; and then they certainly lost a considerable portion of their efficacy. At a subsequent period, a respectable chemist, experimenting under the auspices of a surgeon possessing a wide-spread reputation, announced that calculi of the lithic acid, the most common of all, were not acted on by mere saturated solutions of the bicarbonated alkalis, a proposition which in proclaiming a truth also involved an error; and so the confidence of the profession and of the public came to be entirely shaken in the powers of these medicines to benefit the sufferers from stone, who were, therefore, taught to cast aside every hope of relief, save in recurrence to the operation of lithotomy. It was in fact during the period that the medical treatment of calculus was pursued with the greatest zeal that the surgeon began to show himself the powerful rival of the physician in that walk of practice which has derangements of the uro-poietic system for its object. This took place

especially from the improvements made in the operation for stone about the beginning and middle of the last century by the circumforaneous operators generally known under the names of Frêre Jacques and Frêre Come. The religious character of the former of these apostles of lithotomy, and the exaggerated estimates that were formed of the successes of both, contributed powerfully to direct public attention to the treatment of calculus by mechanical means, and to instal the surgeon as sole referee in cases of calculus, to the exclusion consequently of the physician; and the history of medical science since this change took place assures us that, whilst unremitting attention has been bestowed upon the improvement of the mechanical means of treating stone in the bladder, medical means have until very lately been more and more neglected. The physician, indeed, is now scarcely consulted, save at second hand, in regard to stone in the bladder. In ninety-nine cases in a hundred the surgeon has already been spoken to by the sufferer, and his mind is soon made up as to the procedure in such a case—the stone must be cut out, or it must be crushed; and circumstances not being very untoward, the cutting or crushing operation is forthwith undertaken. There is even something in the shape of an apology to be offered for the conduct so commonly pursued in such circumstances. The medical means we possess of attacking stone in the bladder are very slow in their effects; and all know how difficult it is to manage the generality of patients in cases where perseverance is indispensable, and where the result has to be looked forward to at a distant date. In the present time, too, the generality of practitioners are but indifferently acquainted with urinary pathology; the subject is one that is little studied, or they have little confidence in lithontriptic medicines. The surgeon, indeed, may be little or no better provided than the physician in the important article of pathological knowledge, but he has sounds, and catheters, and the lithontriptors and the knife at hand, and in these he has unbounded faith. The patient with a stone in his bladder, then, who consults the physician, is put into the best plight possible, and then comes in the surgeon, and in two or three minutes' time exhibits to him the cause of all his sufferings firmly held between the chaps of a pair of iron tongs. A triumph of this kind is not lightly to be foregone; and so long as mechanical means of removing stone in the bladder are held on the whole to be a tolerably safe means to this end, it never will be foregone. Neither can we expect that any others, necessarily of slow operation and of which the triumph must be altogether without eclat, will be anxiously sought after or sedulously employed if found by the operating surgeon. Nevertheless we shall see in the sequel that all the mechanical modes hitherto employed in removing a stone from the bladder are fraught with so much danger both present and prospective to the patient, that the interests of humanity and of true science alike command us to go on and strive to improve upon aught that has ever been done in the way of removing stone by other means than operation; and further, should the necessity for operating arrive, to enquire whether there be not better methods of proceeding than those in common use at present.

The chemists, especially of France, have mostly shown themselves warm advocates for the treatment of gravel and stone by medical means. Fourcroy and Vauquelin, in particular, raised their voices in their day in

favour of the reasonableness of this practice; and it is to another able chemist of the same country, M. Darcet, that we are indebted for its revival in very recent times. In a short essay on the mineral waters of Vichy, published in one of the volumes of the Annales de Chimie for 1826, M. Darcet called the attention of professional men to the property which these waters have of rendering the urine of the drinker alkaline, and to the advantage that might be taken of this circumstance in the treatment of stone in the bladder. He found that from three to four glasses of this mineral water, which contains about fifteen grains of the bicarbonate of soda per glass taken in the course of twenty-four hours, sufficed to keep the urine permanently alkaline. M. Darcet remarked, further, that the urine of those who drank the Vichy water was singularly transparent, though the portion which was secreted during the night was often high coloured, and, contrary to wont, that it even continued limpid after putrefaction had made great progress. Dr. Charles Petit appears to have been the first, after M. Darcet, to investigate the effects of the Vichy water in the direction which that excellent chemist particularly indicated. In his little work "On the Medical Treatment of Urinary Calculi," (Paris, 1834,) Dr. Petit already adduced what it is impossible to regard as other than strong evidence in favour of the solvent powers of these waters, which might aptly be spoken of as a solution of bicarbonate of soda presented to us by the hand of nature; and in the "Additional Observations" (1837), and the "Appendix" to the Tract of M. Patissier "On the use of the Vichy Water in Gout" (1840) of the same writer, the subject is continued and other and more signal instances of success are adduced. It is agreed on all hands that fragments of urinary calculi of lithic acid and lithate of ammonia, and of the mixed and triple phosphates, are speedily reduced in size by solution and disintegration when exposed to the action of the Vichy water out of the body; and several persons in whose bladders the presence of calculi had been ascertained by searching, either got rid of them entirely or had them notably reduced in size, whilst all the symptoms of stone were subdued whilst using this water internally. Much about the same time (1836), M. Robiquet, in a paper read before the Royal Academy of Medicine of Paris, adduced several instances of the successful exhibition of the bicarbonate of potash aided by plentiful dilution in cases of stone in the bladder, patients finally passing the kernels of stones which formerly were too large to enter the neck of the bladder, and to get rid of which the operation had been proposed. The range of cases in which the alkaline carbonates were found to be useful was greatly extended by the experiments of these various enquirers, particularly of Dr. Petit and M. Chevallier. It had long been generally allowed that weak solutions of the vegetable and mineral alkalis in the state of carbonates exerted more or less of a solvent effect upon calculi of the lithic acid; but it was hardly suspected that these substances had fully as much power over concretions of the phosphates, not, however, in the way of solvents, but of disintegrators, the alkali seizing upon the animal matter, which is a principal bond of union in the great majority of urinary calculi, and the particles of earthy salt then separating and subsiding in the shape of an amorphous powder. M. Darcet in his experiments found that even so solid a substance as a *compact bone*, exposed for a length of time to a solution of bicarbonate of

soda in distilled water, was finally completely disintegrated; a solution of gelatin, a gelatinate of soda, composed the supernatant liquor; the earthy matter, phosphate mixed with a little carbonate of lime, formed the powdery precipitate which lay at the bottom of the vessel. Dr. Petit even found that calculi of the mixed and triple phosphates, exposed to the action of the Vichy water, suffered in many instances a more rapid loss of weight than those of lithic acid or the lithates.

These interesting and remarkable results could hardly fail to attract the attention of the medical world at large, and particularly of that portion of it in the hands of which the practice in calculous complaints principally lay at this time. And as lithotrity had now reached its climax, having been received by the public at large with a degree of enthusiasm which in the domain of medical science is only accorded to success in surgical operations, the lithotritors or stone-grinders, like a community threatened by neighbours with an invasion that might involve the loss of goods and hearth and home, soon raised their voices against Dr. Petit in especial, and against all others, chemists, pharmacists, physicians, who had lately aided and abetted in this heresy of daring to hope for succour to patients affected with stone in the bladder otherwise than by the means of an operation—their own operation in particular. M. Civiale has shown himself very forward in this hostility to the medical treatment of calculus; but he has been joined in it most cordially by M. Leroy, his ancient enemy, his constant rival. After many articles in the journals of the day, which were met in counter-blasts with admirable temper and excellent taste by M. Chevallier and Dr. Petit, M. Civiale concentrated his strength first in the two works placed nearly last in our list, one published in the course of the year 1838 and the other in 1840. The first of these two works, and almost as much might be said of the last, is beyond question one of the most learned and elaborate productions which has fallen from the medical press of the present age. We have somewhere seen a summary of the number of references to authors and authorities, ancient and modern, and in almost every language spoken on the surface of the civilized earth, which this remarkable book contains; they amount, we think, to something like a couple of thousand. Some of M. Civiale's countrymen have not scrupled to say that there is but one man in France at the present moment who was competent to put forth such a work, a man not less distinguished by his vast erudition than by the facility with which he writes and his unwearied industry; and this is not M. Civiale but M. Jourdan. It is not our purpose to analyze these laborious and somewhat overlong productions, the first extending to upwards of 700 and the second to nearly 500 ample pages of close print in a very small type. Suffice it to say that the reader will find the entire subject of the formation, chemical and physical characters, causes and effects of calculus exhausted in the first, and in the second all idea of finding a remedy for stone, save in a mechanical operation, particularly lithotrity, utterly scouted; and those who have recently ventured to entertain hopes of affording relief in any other direction treated with very slender ceremony.

Messrs. Civiale and Leroy, then, however opposed upon other points, are very brothers in their hostility to the notion of ridding the bladder of a calculus by chemical means. M. Leroy has introduced his "Histoire

de la Lithotritie" (1839), with certain reflections on the solution of stone in the bladder, rich in reasonings wherefore the alkaline carbonate, as we find it in the Vichy water especially, should not succeed in dissolving or disintegrating an urinary calculus, but very poor in facts to support these reasonings, which, indeed, being built upon false principles, are unsupportable. What we regret more than anything else here is to find the particulars of one of the two cases which M. Leroy quotes in aid of his views not given either completely or correctly. The case in question, then, is that of M. G., who went to Vichy in the month of June, where he remained for thirty-nine days drinking the water and bathing every day. During the whole of this time M. G.'s general health was excellent, and he was believed to be cured of his gravel. But this was not so, for soon after his return to Paris he had several small concretions extracted by M. Leroy. These concretions M. Leroy stated to have been composed of a mixture of carbonate and urate of lime, and this composition he adduced as proof positive that they had been formed under the influence of the alkaline waters of Vichy. But M. Guibourt, who made the analysis on being applied to by Dr. Petit, the unwearied and in all his statements unimpeached apostle of the Vichy springs, replied that what M. Leroy had stated, *n'était nullement exact*, was nowise according to fact, that what he had found in the said concretions was a mixture of *carbonate and phosphate of lime*, which is the usual composition of prostatic calculi. The patient was in fact known to be labouring under a severe affection of the prostate. M. Leroy also stated in reference to M. G., that calculi had been reproduced four times in the course of three years, and always by so much the more speedily as the patient drank the Vichy water in larger quantity. But the patient in question assured Dr. Petit in the most positive manner that *he had never drunk any Vichy water either at Vichy or at home* before the month of June last, the date at which he came to drink at the fountain head, (Exposé d'un Rapport fait à l'Académie Royale de Médecine au nom d'une Commission, &c., pp. 32 and 34, Paris, 1839.) The zeal of our dear brothers across the channel seems to carry them somewhat far. It is indeed extremely difficult for us to arrive at the truth on every point connected with this subject.

The subject of lithotrity, as we shall by and by see, has from first to last been surrounded by an all but impenetrable crust of falsification, which it is as distressing as it is difficult to break through. And here it is plain that the lithotritists show themselves no more scrupulous in reporting cases having reference to the solution and removal of calculus by alkaline medicines than they have been truthful in rendering an account of the successes and mischances of lithotrity. Probably one of the documents the most to be relied on in regard to the value of the alkaline bicarbonate, as it exists in the Vichy water, is the "Exposé" just quoted.

The general conclusions in the report alluded to are these: "1st, Urinary concretions are attacked by the urine when this has been rendered alkaline by the use of the Vichy water taken internally and in the way of bath; 2d, It has not been proved that urinary concretions of such a size as to constitute proper calculi have been entirely removed by these waters;* 3d, Such a removal is nowise impossible; there is even con-

* This judgment was given before the case of Denis B. Jacob had occurred, which will be found mentioned immediately.

siderable likelihood of its being accomplished; 4th, The question can only be decided by experiment; 5th, The experiment does not seem to present any danger. The committee therefore request the minister of the public works, &c. to accede to the demand of M. Petit," to the effect that he might have a certain number of patients affected with stone in the bladder confided to his care at Vichy with a view to decide the question as to the power or impotency of the natural alkaline waters in this disease. It is impossible not to see that the report of the committee is extremely guarded; under any other circumstances, in connexion with almost any other pathological state, the facts adduced would have been held adequate to authorize a far bolder and more favorable tone. One patient, M. de Longperrier, having suffered for two years with symptoms of stone in the bladder, and having had the operation proposed to him as his only remedy, goes to Vichy, and after drinking the water for nineteen days he passes a calculus, the size of a large lentil, "remarkable," say the committee, "by the disappearance of the superficial layers, which do not cover or inclose each other completely," and from this time forward he recovers. Another patient, M. Lorigandie, is found by searching to have "a stone of middling size" in his bladder. After the Vichy waters have been drunk for twenty days, pieces of calculus are repeatedly expelled, and the patient is completely restored and so remains. Unfortunately this patient was not sounded after his recovery. A third patient, aged fifty-two, having been sounded was ascertained to have several calculi, estimated about the size of hazel nuts, in his bladder. After having made use of an alkaline solution, during a month the patient passed eleven small concretions, weighing together no more than four grains. From this time all the symptoms ceased, and the bladder being searched anew no stone was discovered. M. Valerix is known to have a small stone in his bladder. Under the use of the Vichy water he passes first one and then another fragment of a calculus, and recovers. The bladder searched is found empty. M. Fournier has a voluminous stone with a rough surface in his bladder. Sounded by M. Leroy on the 9th August, 1838, the stone is declared to be of the size of a large walnut. Under the use of the Vichy waters all the symptoms of stone are gradually mitigated; large quantities of gravelly masses are passed, and the patient is at length so free from complaint that it is believed his stone must be all eliminated; but on sounding it is found with difficulty, and is allowed by M. Leroy to have decreased in size; to Dr. Petit the difference between the size of M. Fournier's calculus on the 3d of July and its size at the end of September seemed comparable to the difference between a hen's egg and a pea. The patient was and long continued perfectly well. In reference to this case the committee say: "If this case still leaves a doubt as to the entire disappearance of the calculus, no one surely will be found to doubt of the notable diminution of size which this large stone must have undergone." We are happy, through the politeness of Dr. Charles Petit, to whom we ventured to address ourselves for farther information in regard to this interesting case, to lay the sequel before our readers. M. Fournier, then, continued the alkaline medicine at home for some little time; but suffering nothing he soon gave up his medicine, and would not believe that he had any remains of a stone in his bladder. He enjoyed uninterrupted health for more than a year. In the month of

October last he was seized with fever of a bad type, to which he fell a victim. On opening the body after death the long-shaped nucleus of a stone, the size of a shelled almond, was discovered. It is impossible not to see this case as little less satisfactory than it would have been had the last fragments of the large concretion which once occupied the bladder been dissolved or discharged. Several other cases of equal interest might be cited, but we prefer giving one which is perfectly conclusive, and for which we are indebted to the politeness of Dr. Petit.

Denis B. Jacob, having a stone of considerable size in his bladder, was placed under the care of Dr. Petit, at Vichy, in the season of 1839. The patient proved refractory, and followed the treatment prescribed irregularly. Remanded in the season of 1840; his stone was previously seized in various directions by the committee of hospitals, Messrs. Civiale, Blandin, and Berard, and ascertained to be of the diameters of 13, 14, and 15 lines. The treatment by the Vichy waters, begun on the 23d of June, 1840, was pursued with great regularity to the middle of September, the patient taking from 12 to 25 glasses of the water and a bath daily, and in addition having a stream of the water sent through his bladder by means of a double-current catheter, once, twice, and even thrice a day for some considerable time. In the beginning of August the patient began passing fragments of his stone, and at the same time obtained relief from his sufferings. On the 18th of September the patient was sent back to Paris, and having been sounded on two different occasions by the several members of the committee, it was formally declared that there was no longer any stone in the bladder.

Two grand objections are constantly raised to the use of alkaline or any other kind of internal medicine in cases of calculus; first, that much valuable time is lost in their administration; and second, that the alkalies especially, far from dissolving or disintegrating urinary calculi, tend rather to cause precipitation from the urine, or at most to change one diathesis into another. The first objection is taken obviously upon the presumed inefficacy of internal remedies generally in cases of stone. This is a presumption, however, which very certainly is nowise warranted by one item in our knowledge of the pathology of calculus. Stone in the bladder, indeed, is not one of those diseases the natural tendency of which is to get better; on the contrary, it is one that tends ever to get worse, and finally to destroy life, without the successful interference of art in one way or another. But there really seems to be no fact better established in therapeutics than this: that the symptoms of stone, those symptoms which directly bring the life of the sufferer into jeopardy, are in the majority of instances either entirely subdued or greatly alleviated by a course of the bicarbonated alkaline or Vichy water. The Committee of the Royal Academy of Medicine give a definitive judgment upon this point. "It cannot but be admitted as a general proposition," says the reporter, "that during the administration of the Vichy waters the health of calculous patients is ameliorated, and that the urinary passages undergo no changes from their action which could make the operation of lithotrity or lithotomy ulteriorly more hazardous." On the contrary, we venture to add, if they have a soothing and *healing* influence, to use a common phrase, which we maintain they have been satisfactorily proved to possess, is it not obvious that they must, as a matter

of course, render either of these operations much safer than it would have been, undertaken upon a patient with his bladder in a state of active inflammation, and his system fevered and disjointed by pain? Doubtless, many cases will occur which are too far gone to be benefited by the Vichy or other kind of earthy water. But does lithotrity or lithotomy supply a remedy for every case of stone that presents itself in practice?

The second objection, that the alkalis, far from dissolving urinary concretions, tend rather to cause precipitation from the urine, and to change one calculous diathesis into another, is one that is now of ancient date, that originated in a groundless assumption, and that has been answered over and over again. Urinary calculi of lithic acid and the lithates, and of the mixed and triple phosphates are certainly dissolved and disintegrated by weak solutions of the fixed alkaline carbonates, which moreover have no power whatsoever to cause precipitation from the urine, or to alter the diathesis. The urine of the female whose case Dr. Bostock recorded,* when taking *two ounces and a half of subcarbonate of soda daily*, was pale and perfectly clear and limpid; and the urine of the drinkers of the Vichy water has been universally remarked for its transparency.

The medical treatment of stone in the bladder has assumed an entirely new aspect of late years, and the subject is not yet by any means exhausted. We have said nothing of the virtues of the biborate of soda, which nevertheless passes readily into the blood and finds an exit by the kidney, and acts very rapidly upon concretions of the lithic acid and the lithates. Neither have we spoken of the benzoic acid, one of the few substances of its class which is readily absorbed into the system, and like the alkalis, finds its way out again by the kidney. It is but yesterday since this acid, from its physiological affinities and its known capacity to combine with the uric acid, and to convert a most insoluble into an extremely soluble substance, was recommended in gout by Mr. Ure, as calculated to prevent the deposition and even to effect the removal of the tophaceous masses of urate of soda, which are so commonly seen about the joints in inveterate cases of that disease; and if the urobenzoic acid which is found in the urine when benzoic acid is exhibited by the mouth have the power of combining with the earthy phosphates, and of forming with them compound salts of easy solubility, as we have been assured it has by the ingenious surgeon just named, we are weaponed afresh and more effectually than ever againt urinary concretions.

2. *By chemical reagents thrown into the bladder.* This is a method of treating stone from which every addition recently made to our knowledge would lead us to anticipate great and decisive results. Nevertheless, it is one which has not attracted so much attention lately as the indirect mode of treatment through medicines administered by the mouth. More than a century ago our countryman, Dr. Hales, appears to have been in labour with the idea of dissolving the stone by way of injection. In the course of his experiments he found that a certain menstruum of which the active ingredient was subcarbonate or carbonate of soda attacked and dissolved urinary calculi with considerable vigour. This menstruum he also ascertained could be infused into the bladder of a living animal without injury, and he invented a double-current catheter

* Medico-Chirurgical Transactions, vol. v.

for its easy and effectual application; but he never tried its effect upon a stone in the bladder of man. Dr. Hales's views were, however, carried into effect about twenty years later in another and far more clumsy way by Dr. Rutherford, of Edinburgh, with the assistance of Mr. Butter, then a clinical clerk in the Royal Infirmary, and with a result powerfully calculated to arrest attention. The subject of Dr. Rutherford's experiment was a man forty years of age, who had suffered from all the ordinary symptoms of stone in the bladder for four years. On searching the bladder "a stone was distinctly felt, and it seemed to be a large one." But under the use of castile soap by the mouth, and injections of lime-water into the bladder, the symptoms of stone were first relieved; the stone, on searching, was found with difficulty, and reduced in size, and finally every indication of its presence having vanished, the sound failed to discover aught in the bladder, and the man went home well.* It is truly remarkable that so plain and so conclusive a narrative as that which Dr. Rutherford has left us in Mr. Butter's little book should not have borne fruit. Had we records of the example there set having been vainly followed in numerous instances we should be content to reconcile ourselves to the neglect into which the treatment of stone by the way of injection had fallen. The medical treatment properly so called has indeed been revived, and with what we cannot but hold good promise of many triumphs; but the treatment by injection has not yet found its Petit. Nevertheless, the two methods ought to be held as inseparably conjoined. Beneficial as each in the nature of things is calculated to prove by itself, the two in conjunction ought to accomplish, and will accomplish far more than either separately. In Dr. Rutherford's case they were combined; the soap by the mouth rendered the urine alkaline, and the lime-water injection, besides its own disintegrating powers upon the stone, by depriving the neutral carbonate of soda of its acid, exposed the calculus to the action of a weak solution of caustic soda. The treatment by injection, however, has not been entirely neglected; the object which Gruithuisen had in view when he contrived instruments to perforate stones in the bladder, was not their destruction in the way of grinding, but that he might open up a passage to their centre for suitable solvents.† In pursuit of the same end Gruithuisen revived or reinvented the double-current catheter of Hales, for passing a continuous stream of fluid through the bladder; but this ingenious man, like Hales, seems never to have tried the measure he advocated. It was, however, had recourse to by Messrs. Magendie and Amussat, in the case of an English gentleman under their care, but with only partial success. Sir Benjamin Brodie was more fortunate, and the solvent which he used, a weak solution of nitric acid in distilled water, was original. By means of injections of this composition two concretions of the mixed phosphates were finally so much reduced in size as to escape by the urethra. Dr. Ritter, of Cassel, also succeeded in removing a stone from the bladder of a gentleman aged forty, by injections of weak solutions of the caustic alkalis, aided by the same medicines taken by the mouth. Lithotomy was to have been performed in this case. On the thirtieth day the patient was completely relieved. The whole of the debris of the calculus collected

* Butter, a Method of Cure for Stone, chiefly by Injections. 12mo, Edinb., 1754.
† Willis, on Urinary Diseases, p. 335.

together weighed about one ounce and a quarter. The patient was seen by Dr. Ritter several years afterwards in perfect health, never having had any return of calculous complaint.* In this interesting case the means pursued were of the most active kind, and were perfectly adapted to the end proposed. The particulars of another remarkable case are narrated by M. Irvine, Professor of Anatomy, &c., Geneva,† and deserve to be referred to in this place. Under the influence of an injection of simple water into the bladder every day for several months, a large calculus, which must have been of the phosphatic kind, was so completely dissolved and disintegrated that at last it was reduced to a mere shell, and then broke down into a hundred pieces within the bladder. This case unfortunately terminated fatally; the patient, a female, seems to have suffered so much in her general health before Irvine saw her that no moment propitious to any kind of manual interference could ever be seized. It is worth while to state that the object of the practitioner in this instance was not to dissolve the stone, but to dilate the neck of the bladder and urethra to such an extent as to admit of its extraction.

On the subject of remedying calculus by the means of injections then, it seems clear that so much has already been done as to hold out every inducement to perseverance. In the nature of things, indeed, perseverance guided by the better knowledge we now possess of the agents that attack urinary calculi, and of the manner in which these operate, must of necessity be crowned with success in a certain proportion of cases.

The mode of treating stone in the bladder by injection, is in accordance with the ascertained laws of chemical affinity, which are just as determinate and as much to be relied on as those of gravitation. This treatment is therefore reasonable. Farther, it has been applied with complete success in more than one instance, and on the ground of experience we can say that it will surely succeed if fairly tried again.

### II. Removal of the entire Calculi through the Urethra.

1. *With previous dilatation of the canal.* The fact that persons after suffering from symptoms of stone in the bladder often obtained relief by the accidental escape of the concretion amidst a flood of urine, and that others who suffered in the same way constantly felt the stream of urine interrupted by the stone falling against the vesical orifice of the urethra, must have led, at an early period in the history of medicine, to the use of measures calculated to aid the escape of calculi. Had the canal of the urethra been but a little wider, the stone that became engaged in its vesical extremity would have passed through. The most simple and natural method of favouring the passage of calculi was to enlarge the urethra; and accordingly we find that one of what we must presume to have been a very early means of ridding the bladder of calculi other than by the knife, had dilatation of the urethra as its essential element. Prosper Alpinus informs us in his Medecina Ægyptiorum that he saw more than one Egyptian practitioner extract calculi the size of an olive, and, as he says, even of a small walnut, through the urethra pre-

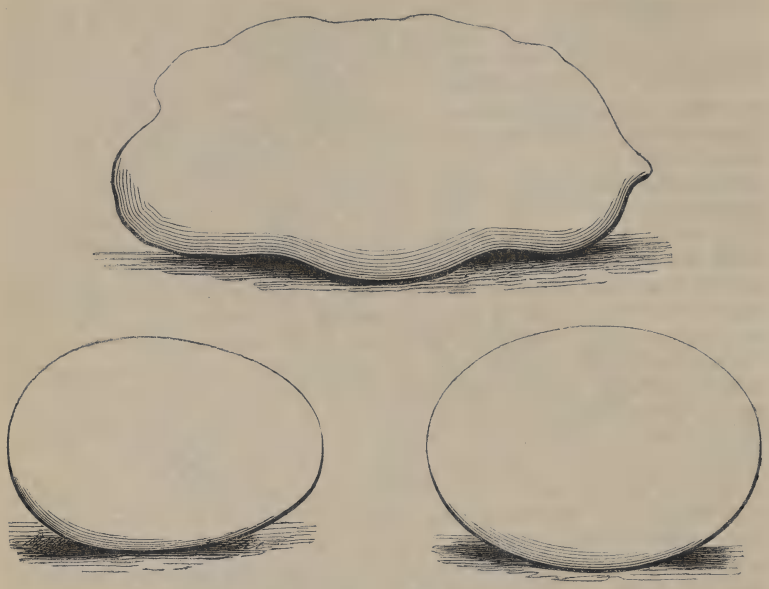
* Chemische und Medicinisch-praktische Bemerkungen über menschliche Harnsteine, von Hofrath Ritter, in Hufeland's Journal. Band xxv. s. 119.

† Bulletin des Sciences Médicales de la Société Médicale d'Emulation, t. v. p. 400.

viously dilated by the insufflation of air and the introduction of certain extensible cartilaginous (doubtless dry membranous) tubes, of different sizes, susceptible of being expanded by insufflation. When the urethra was held to be sufficiently dilated, a wooden canula was passed into it, and the patient being placed in a proper position, the operator with a finger in ano pushed the stone into the neck of the bladder and into contact with the canula. The mouth was now applied to the outer end of the tubes, and strong suction being made, the instrument was slowly withdrawn, the calculus following it as a plug. This ingenious procedure of the Egyptians is as applicable as ever, and in good hands, and with the substitution of an exhausting syringe for the mouth, would certainly succeed in some cases in which patients are now made to run the risk of their lives by being subjected to operations of a far more hazardous kind. The preliminary part of the process has indeed been revived in modern times, but with modifications that are not improvements, for solid bougies have been substituted for the extensible membranes of the older practitioners, and the second and highly important part of the procedure has been thrown by entirely. Nevertheless, modern surgeons have several times succeeded by the introduction of graduated bougies in getting away calculi which would otherwise have remained in the bladder till they had grown beyond all chance of being extracted through the natural passages. Ledran,* for instance, advised a patient who had passed many small calculi, but who still retained one in his bladder which would not come away, to introduce bougies, and when he made water to lean forward that the stone might be directed towards the outlet. On the fifth day of the treatment the patient got rid of his calculus, which was of the size of a large pea. Baron Boyer† also succeeded in getting away four considerable calculi by following the same precedure. Sir Benjamin Brodie‡ in like manner, by the use of the full-sized bougie and diluents, was so fortunate as to relieve a patient of three considerable calculi, for the removal of which an experienced surgeon had recommended the operation of lithotomy.

But if less confidence have been given to dilatation of the urethra in the male than we think it deserves, and if this means have been called into requisition less frequently than it might have been, the case as regards the *female* is different. Such confidence have surgeons now in the dilatability of the female urethra, that the use of the knife appears to have been for some considerable time past completely laid aside in the treatment of calculus among women. When such masses as those, the outlines of which are here subjoined, will pass through the female urethra, there never can be any pretext for incising it. The largest of the three calculi here figured, actually passed spontaneously, literally forcing a passage for itself. But this knowledge, and the advantage that has been taken of it, are not of yesterday. The dilatability of the urethra was well known to the old French writers on lithotomy. Folet, in his "Traité de Lithotomie," published at Paris in 1682, says in express terms, "il n'est pas croyable combien l'urèthre se dilate tant aux hommes qu'aux femmes." Ledran never made any incision in removing calculi

* Consult. de Chirurgie, p. 471. † Traité des Maladies Chirurg., t. ix, p. 308.
‡ On Diseases of the Urinary Organs.



from the female bladder. Bromfield* dilated the female urethra for the extraction of calculi with the bowel of a small animal inflated, &c. But these facts, and the practice connected with them, had fallen into oblivion, when they were again brought under the notice of the profession by Mr. Thomas† about thirty years ago, he having succeeded in removing an ivory implement three inches in length, which had been introduced into the bladder, after the use of no more than a couple of sponge tents of very moderate dimensions. These he found had so far dilated the urethra in about twelve hours, that he was enabled easily to pass his finger into the bladder and dislodge the intruder. The example once set was in this instance speedily followed, and the practice had soon many signal triumphs to boast. The only objection that can be urged to the practice as at present pursued is, that the instruments commonly employed to procure the dilatation are not the best that have been contrived, and that the process is all but invariably carried on too rapidly.

2. *Without previous dilatation of the urethra.* It had been occasionally observed, that catheters introduced into the bladders of persons who had suffered from retention of urine in consequence of the presence of gravel or calculus, were withdrawn with a small concretion sticking in their eyes; and this circumstance deserves attention, as having led to very important consequences. The accident alluded to must have occurred ever since canuli for the urethra or catheters were invented; but the first who attempted to take advantage of it was M. Bourguenod. In a modest paper in the 6th volume of the Annales de Montpellier, this practitioner gives the particulars of three cases, in which as many as five

* Chirurg. Observ. Lond. 1773. † Med. Chirurg. Trans. vol. i. Lond. 1812.

small calculi were successively removed from the bladder by its means. He used what the French call *sondes à demeure*—elastic catheters left permanently in the bladder. The eyes of the catheter were split to a certain extent, and the cessation of the flow of urine through the instrument was the signal that the concretion had entered the trap set for it. This generally took place in the course of three or four days. The concretion removed in one of the instances was as long and as thick as the end of the little finger; and in another the operation of lithotomy was rendered unnecessary by the successful issue of the simple expedient employed. The Baron Boyer* also informs us that upon a certain occasion on removing an elastic catheter he found a calculus of the size of a haricot bean sticking in one of the eyes of the instrument. The same occurrence has happened in the practice of many other surgeons. Mr. George Bell† has given the particulars of a case in which the accident furnished the patient himself with the hint of a method by which he succeeded in removing upwards of 150 small calculi, by means of an ample silver catheter used when the bladder was fully distended; and who derived so much relief from getting rid of all smaller concretions, that although one larger calculus still remained behind, he could never be brought to take the chances of lithotomy. Finally, the late—alas the late! Sir Astley Cooper, having left a catheter in the bladder of a patient affected with calculi of small size, on being informed that concretions had several times been found sticking in the eyes of the instrument, desired that he might be allowed to remove it himself next day; and this being done, a small stone was actually found impacted in one of its eyes. The advantage that might be taken of this circumstance immediately presented itself to the mind of the great surgeon: he resolved by means of a pair of delicate forceps to seize the calculi where they lay, and to extract them through the urethra. A suitable instrument was soon constructed with the assistance of the ingenious surgeon's-instrument-maker Weiss, and this was plied so successfully at intervals during several weeks, that the quarry became exhausted, and the patient went home well. The same method was shortly after resorted to in other instances, and with the like success.

This method of extracting stones from the bladder is however no new discovery; passing by the more ancient writers, it is particularly mentioned by Sanctorius,‡ and an instrument is described and figured which he contrived for this especial purpose. An instrument on the same principle, having two blades (Sanctorius's had three) was invented about a century ago by Dr. Hales, but this was rather for the extraction of stones arrested in the urethra. The same implement was reproduced by Mr. Hunter; and he or some contemporary (though we have failed to light on the passage) must have proposed going into the cavity of the bladder, and thence extracting calculi of smaller sizes, for the proposition may be found commented on in terms of great severity by M. Deschamps in his classical work, the "Traité pratique et dogmatique de la Taille," (4 vols. 8vo, Paris, 1796), which is in everybody's hands, and where a figure of Mr. Hunter's forceps is given. We have farther been favoured with an original drawing of a three-bladed forceps acting by its elasticity and the pressure of a canula,

* Op. cit. t. ix. p. 318. † Edinb. Journ. of Medical Science, vol. i.
‡ Comment. in Avicenn. fol. Venet. 1626.

which was repeatedly used by the late Sir William Blizard, at the London Hospital half a century ago, for the very purpose in question.

With regard to the value of the idea of extracting calculi from the bladder in the manner and by the means indicated, this has been very differently estimated by different writers. One excellent surgeon has proclaimed it as among the greatest achievements of modern surgery (Brodie); another has said that it could hardly fail to prove highly injurious (Syme); and a third has declared it imprudent and murderous (Deschamps). There must be cases, we believe, in which, in the absence of other and better means, this procedure would be found useful if cautiously instituted. But that the extraction of calculi by means of forceps through the unprepared urethra is an operation not without danger is quite certain; it is no such unmingled good as it was proclaimed and believed to be after Cooper's operation. Proof of this may be had by turning to a late volume of the London Medical Gazette, where a case is reported in which death ensued, though the forceps were in very competent hands, and no more than two attempts to use them were made.

### III. Removal of the Stone piecemeal, by drilling, grinding, or crushing it within the cavity of the Bladder.—Lithotrity.

The modern history of lithotrity, by which is now understood destruction of the stone within the bladder by mechanical means of any kind, begins with the accounts we have of the case of Major-General Martin, the fullest and best of which are from his own pen. This distinguished officer was a native of Lyons, and arrived in India in the capacity of a common soldier; but from this humble position he soon emerged, and finally attained the very highest rank that could be won in the Honorable Company's military service, owing his advancement to his abilities and worth alone.* The first published account which we have seen of Gen. Martin's case is in the 1st volume of the Medical and Physical Journal (Lond. 1799). There is, however, a communication, earlier in point of date, though later in regard to the time of its publication, extant in the 10th volume of the Annales de Montpellier (Montpel. 1809). It is entitled "Fragment d'un lettre de M. Claude Martin, &c. à M. Pictet de Genève, communiqué par M. P. Fine," and bears date, Lucknow, Dec. 10th, 178(9?)5. The letter is in French, and is extremely interesting. The writer informs his friend, M. Pictet, that he had been so fortunate as to cure himself of a stone in the bladder, which he imagines must have been of large size, by means of a contrivance of his own, a file, curved to correspond with the canal of the urethra, and toothed at the end, so as to cut in the withdrawing. By standing up and

* On the tomb which he constructed for himself during his lifetime, on the underground floor of a "grotesquely magnificent house which he built at Lucknow," are these words from his own pen:

"Major-General Claude Martin,
Born at Lyons, January, 1738,
Arrived in India as a common soldier,
And died at Lucknow [15th of September, 1800],
Pray for his soul!"

(*Emma Roberts, Scenes and Characteristics of Hindostan*, vol. ii.)

leaning forward the stone is brought to the neck of the bladder, and it is then that it must be attacked with the file. General Martin continued his operations through a succession of months, filing on an average three times a day, but often as many as ten and twelve times. "I filed to such purpose," he concludes, "that at length I brought away the last fragment of my stone, and am now perfectly well." General Martin's case has been noticed repeatedly since, more particularly in the 1st volume of the Journal of the Royal Institution in 1816, and by Dr. Marcet the year after, in his work on Calculous Diseases. But it remained without results long after its publication.

The next stage in the history of lithrotrity is unquestionably the publication of Dr. P. E. Gruithuisen's paper in the Medico-Chirurgical Journal of Salzburg in the year 1813. The title of this paper is as follows: *Are we then to abandon the long-cherished hope of removing stone in the bladder by mechanical or chemical means?* (Ob man die alte Hoffnung aufgeben sollte den Stein aus der Blase auf mechanische oder chemische Weise einst noch wegschaffen zu können?) and is truly a remarkable production. The author declares himself a strenuous advocate for the idea of dissolving the stone. He instituted experiments to show the extent to which calculi were attacked by being placed in a current of water, pure or holding different acids and saline substances in solution; and among other means he describes and figures a canula, through which was passed a noose of wire to secure the stone, in the same way as a loose cork is secured in a bottle, and a small trepan with which the concretion was to be perforated, its nature ascertained, and its very centre opened up to the action of the appropriate solvent. This brilliant idea was, however, never carried into practice; Gruithuisen's paper attracted no notice in the world of Germany, and remained unknown to the rest of Europe. It therefore influenced in no way the measures that were subsequently taken towards effecting the mechanical destruction of the stone in the bladder. The stream of improvement, in fact, did not set from Germany, but from England, where alone of all the countries of Europe was the subject of urinary pathology cultivated with anything like zeal or success about and previously to this time. In both France and Germany indeed the uropoietic system and its derangements seemed to have been long at a discount. With the discovery of the urate of ammonia calculus by Fourcroy and Vauquelin in 1799, the successful researches of French chemists in the domain of urinary concretions may be said to have begun and ended. What may be called the general and special medical "urinary pathology," too, found tongues and pens in England alone, to proclaim its importance, and to push it forward, about the end of the last and during the first quarter of the present century. France has no such names as those of Rollo, Cruikshank, Wells, Bostock, Marcet, Prout, Blackall, and Bright, in connexion with this important matter. Neither in reference to the operation for stone did she produce aught after the compilation of Deschamps (1796) that either added to knowledge or influenced practice; whereas in England, we observe the whole profession, as it were, for a long series of years labouring heart and hand at the improvement of lithotomy. The same may be said in regard to the diseases of the urethra: from the appearance of Chopart's work in 1792, nothing was contributed by our neigh-

bours of any consequence till the year 1822, when M. Ducamp's treatise appeared, and this seems to have been borrowed in every essential particular from the English, more especially from Dr. James Arnott's book upon Strictures of the Urethra, which was published in the course of 1819. We shall by and by show what an important influence we imagine M. Ducamp's acquaintance with this book, and with English medical literature, had upon the invention of lithotrity. France, in short, had little or no share in all that led the way to what is now generally esteemed her highest surgical glory—the fashion of treating stone in the bladder by grinding and crushing. Returning to which subject we remark that

The third direct stage in the history of this invention in these times was the appearance of a paper entitled, Description of an Instrument for Destroying Urinary Calculi within the Bladder, with a plate; by John Elderton, surgeon, in the April number of the Edinb. Med. and Surg. Journal for 1819. This, in fact, is the first broad annunciation we possess of the principle of lithotrity. Mr. Elderton's instrument is ingenious, and under favorable circumstances would almost certainly have been used with success; the first softish stone to which it had chanced to be applied would have given way and crumbled into fragments between its blades. Unfortunately Mr. Elderton published his invention before he had proved its applicability; it was passed by unheeded as a vain speculation, and seems to have had no immediate influence on the treatment of stone in the bladder. Nevertheless, there is the honour due to Mr. Elderton of having the first invented what we hold to be an available instrument for the express purpose of destroying a stone in the bladder.

The fourth and most important step of all in the history of lithotrity, as we conceive, was the publication of "A Treatise of Stricture of the Urethra, by James Arnott, Lond. 1819," and of "Cases illustrative of the Treatment of Obstructions in the Urethra, Lond. 1821." In these works, however, allusion is only made incidentally to a mode of ascertaining the nature of a calculus still contained in the bladder. "If the smallest particle of the stone could be procured," says Dr. J. Arnott, (Cases, &c. p. 85), its chemical composition might with certainty be determined. With this view the following means may be adopted: When the stone comes to the orifice of the bladder, let an open pointed catheter (having of course a ball-ended wire filling it during the introduction) be passed till it touch the stone; by this a small circular saw like a trephine may then be introduced, to grate off from the calculus, by a few turns, a sufficient quantity of dust for examination." Dr. J. Arnott was at this time intent, in concert with his distinguished brother Dr. Neil Arnott, upon finding a means of destroying calculus in the bladder by solution. After indicating the method, by the injection of certain menstrua into the bladder, which had already been essayed, and speaking of the double current catheter which they had rediscovered for themselves, as affording important advantages over the old plan of proceeding, Dr. J. Arnott goes on to describe a bag of gilded cloth which had been contrived by his brother Dr. Neil Arnott, capable of being introduced into the bladder closed, but of being expanded there, and with which he thought the stone might be caught. This done, and the mouth

of his bag closed again, he proposed to throw in strong solutions of acids or of alkalies, according to the ascertained nature of the stone, so as rapidly to destroy it. "The experiment of removing calculus," says Dr. J. Arnott, "was made by my brother upon a stone in a narrow-mouthed glass vessel with such an apparatus, and perfectly succeeded." Dr. J. Arnott goes on to speak of "the removal of the stone by mechanical attrition," and instances Col. Martin as having "thirty years ago removed a stone from his bladder by files introduced through the urethra." He also intimates that Dr. Darwin suggested or described an apparatus which might be used to break the stone into distinct portions. In spite of what he has already said of a means of ascertaining the nature of the stone, and in the very next sentence to that just quoted hinting that "better instruments might be constructed than any which have as yet been proposed for the purpose (of breaking up the stone)," our author nevertheless expresses himself as adverse to the idea, and even goes so far as to say that he thinks it scarcely probable the thing will ever be attempted.

But we have not yet done with Dr. James Arnott. In commenting upon the fact that stones of considerable size had often been expelled through the female urethra especially, either by the efforts of nature alone, or assisted by dilatation of the passage (op. cit. p. 111), he speaks of the Arabian mode of extracting calculi from the male bladder by dilating the whole length of the urethra, and by and by gives the particulars of a case in which a stone was removed from the bladder of a patient through an opening in the perineum after dilatation of the membranous part of the urethra and neck of the bladder by means of a new dilator. The subject of this case was a gentleman beyond the middle age, who had some nine months previously undergone the usual operation for stone, in the course of which the rectum had been wounded, so that a fistulous opening remained between the bladder and the bowel. In this state, and still suffering much from pain and irritability of bladder, the patient placed himself under the joint care of Drs. Neil and James Arnott, and Sir Astley Cooper. With a view to cure the fistula, Sir Astley Cooper made an opening into the urethra from the perineum, by which he passed a female catheter into the bladder, and immediately struck a stone. As it was likely to be small, Sir Astley did not object to the proposal now made by the Drs. Arnott, to try the effect of the new dilator in opening the passage for its removal. This instrument was accordingly used; and in the course of thirty hours the passage in the perineum, the membranous portion of the urethra, and the neck of the bladder, were opened up till they were about two inches and a quarter in circumference, or three quarters of an inch in diameter. The lithotomy forceps was then introduced by Sir Astley Cooper into the bladder, and the stone extracted. It was as large as a middling sized walnut. The operation was eminently successful. In four days the patient was able to retain his urine, and left his bed-chamber. On the ninth day the wound in the perineum was whole, and he began to take exercise abroad. This case unquestionably exerted a very considerable influence upon the views of practitioners at the time in this country. At the meeting of the Medico-Chirurgical Society, which was held on the 22d of June, 1819, it was particularly mentioned by Sir Astley Cooper, who spoke in high

terms of the plan of operation pursued; and its publication immediately preceded Mr. Earle's paper on the means of breaking down large calculi, and Sir Astley Cooper's communication on the extraction of calculi from the male bladder without cutting. The knowledge of this remarkable case was, further, much more extensively spread by being given at length in the immediately ensuing number of Dr. James Johnson's Journal, in connexion with a very flattering review of Dr. Arnott's book on Stricture, which is highly lauded, and spoken of as the best in the English language on the subject. At the end of this very number of Dr. Johnson's publication, page 331, it is not unimportant to observe the name of M. Ducamp, Docteur en Médecine, rue St. Martin à Paris, among the list of recent subscribers. All that was passing in the medical world of England must consequently have been perfectly well known to Dr. Ducamp, who, indeed, had already signalized himself as an English scholar by translating, among other works, Bree's book on Asthma into French; we have, besides, seen his name among the list of contributors in the department of English literature to a Parisian medical periodical publication of the time—the *Revue Médicale*, if we remember rightly.

In the course of our researches for information upon the various particulars embraced in this summary, we were struck by the essential similarity of the ideas and means of cure proposed by Arnott in his Treatise on Stricture (London, 1819), and Ducamp in his Traité des Rétentions d'Urine (Paris, 1822). It was obvious at once that Ducamp had made very free use of Arnott, though without the slightest acknowledgment, for he only quotes the English writer once (p. 171), and that is to accuse him of filching from Desault! This single citation of Arnott suffices of course to prove that the "Treatise on Stricture" was known to Ducamp, whose references besides to Home, Whateley, and Bell show him to have been familiar with the English works of highest authority on strictures of the urethra. It is not likely that he would have neglected the last publication on the subject, especially as he must have seen it spoken of in his copy of Dr. Johnson's Journal, as "the best that had appeared upon it in the English language." That Ducamp was himself guilty throughout his entire treatise of the sin with which he wrongfully charged Arnott in a single point there can be no doubt.

In the course of our farther researches for information on lithotrity in its embryo state, we were a good deal struck by observing Ducamp's name frequently quoted in the work which M. Leroy published in the year 1825, entitled "Exposé des diverses procédés employés jusque à ce jour pour guérir de la pierre sans avoir recours à l'opération de la Taille." The first implement which this gentleman imagined for perforating the stone was, he tells us, (Op. cit. p. 126,) "a button covered with asperities or a small trepan, supported upon a slender and flexible stem similar to that which supports the port-caustic of *Ducamp*." He mentions Ducamp again at page 144 as having given him the idea of the bow to work his drill instead of the crank-handle, which was his own invention. He speaks of Ducamp a third time at page 151, and recommends an instrument similar to one which Ducamp had imagined for measuring the extent of strictures, as calculated to bring back the stone when tending to escape from the lithoprione or stone-saw. Leroy in the same work further describes and figures a pouch for catching the stone in the

bladder, and at page 168 he says, "if this pouch be intended to receive solvent injections," &c. &c. Now it struck us as extraordinary on perusing all this that the man who had vindicated his claims to the character of ingenuity in one department of the diseases of the urinary organs, and who could give so many useful hints to his "*friends*," should in another and still more interesting division of the same diseases have done nothing for himself. Our astonishment was increased when we saw that M. Leroy's ideas, so naïvely laid before us as his own, were essentially the same as those of Arnott; we had the trepan for instance for piercing the stone, the object in doing which was among other things to ascertain its nature, and we had even the pouch for catching the stone previously to dissolving it by chemical reagents. But our amazement at this harmony of views and means subsided in a very considerable degree when we chanced to turn to a work entitled "Recueil d'observations médicales confirmant la doctrine de Ducamp sur la cauterization de l'uréthre," &c., published by M. P. L. A. Nicod at Paris in 1825. Ducamp, who died prematurely and much regretted in the spring of 1823, seems to have pointed to Nicod as a fit successor in his practice, and M. Nicod in the preface to his work (Recueil, &c. p. xvi. et seq.) gives the following information, which we cannot help viewing as strongly confirmatory of our persuasion that the entire subject of lithotrity in France took its origin from Ducamp, who in his turn derived his ideas from the literature of England, particularly from the two publications of Dr. James Arnott. "To an inventive genius," says M. Nicod of his friend, "great dexterity, and rare powers of observation, Ducamp added an activity of mind that would have led him to other discoveries of equal importance, had he not been prematurely snatched away from science. His researches on the urinary passages had given him *the idea of an instrument intended to destroy calculi in the bladder without recurring to the operation of lithotomy*. . . . . This instrument served subsequently as the model of that of M. Leroy, in taking advantage of which M. Civiale showed such address. Ducamp would probably have perfected his stone-breaker had he not hampered himself with the idea of a pouch in which he proposed to retain the fragments with a view to dissolve or to extract them subsequently." There is the stamp of truth upon this narration, the rather as these ideas of Ducamp are the same in effect as those of Arnott; he made a pouch in which he meant to catch the stone, and *having broken it into pieces* (and here he went beyond Arnott) to dissolve or wash it out. M. Leroy has hitherto maintained a character among his countrymen for candour and fairness. He has shown much of these excellent qualities to his rivals, doubtless; but them we opine he has always had upon the hip, to them he could afford to be generous; why has he carefully avoided the mention of his deceased friend Ducamp, in each and all of his publications as having invented an instrument for breaking stones in the bladder? He must have known that Ducamp had contrived something of the kind; Ducamp gave him the flexible stem upon which his piercer was supported, and made fit to act through a bent tube; Ducamp gave him the bow instead of the crank. Did not Ducamp do more? It were, perhaps, too much to put M. Leroy in foro conscientiæ on this occasion, but we cannot help expressing the wish that he would tell us what share Ducamp had in setting him to work at the destruction

of the stone in the bladder, and what part the same ingenious man had in contriving the instruments originally constructed for this end. The circumstances attending the announcement of lithotrity in the year 1822 point with something like a necessity, indeed, to a common centre whence the ideas emanated; for no fewer than three competitors for the honour of having discovered lithotrity actually stepped upon the stage in France nearly at one and the same moment.

These competitors were Messrs. Amussat, Leroy, and Civiale. The first in point of time is M. Amussat. This gentleman had already been engaged in studying the structure of the urethra, and had called attention to the neglected fact that straight sounds are readily enough passed into the male bladder. The title of his communication in reference to lithotrity is remarkable. It is as follows: "Note sur la possibilité de sonder l'uréthre de l'homme avec une sonde tout à fait droite, &c., ce qui a donné l'idée d'extraire des petits calculs urinaires encore contenus dans la vessie et de briser les gros avec la pince de Hunter modifié," in Nouveau Journal de Médecine, Avril, 1822. This announcement is very striking. If there were an enquirer in the direction he took independent of his contemporaries, it was M. Amussat. He informs us that on the dead body and in presence of his pupils he had repeatedly practised the extraction of calculi from the bladder, and broken others of the size of walnuts within it, having none larger at his disposal. The instrument was composed of a couple of strong blades well toothed towards their extremities, opening by their elasticity through a canula. The stone being felt was firmly grasped between these blades by their being drawn within the canula, and the pressure thus exerted upon it broke it in pieces. But there was nothing of novelty in the ideas here. Mr. Hunter's forceps was already before the world as an instrument fitted to penetrate into the bladder and thence to extract calculi; and this had already been done in England not upon the dead body but in the living subject by Sir Astley Cooper, when he freed the bladder of Mr. Bullen of its eighty-four calculi without cutting. And with regard to the second idea, that, to wit, of breaking calculi which were too large to pass, the only new point was the strength of M. Amussat's instrument; for Mr. Weiss the cutler, informed of the difficulty or impossibility of removing calculi from the bladder that were above a certain and very moderate size with the forceps which he had already constructed, made another in the course of the same year (1821) upon a similar principle but of tempered steel, and of such strength that its power of resilience sufficed to crush calculi of considerable dimensions, especially when they happened to be soft.

The next in sequence is M. Leroy, the title of whose communication is short and to the point: "Note sur un nouveau procédé pour detruire la Pierre dans la Vessie, par M. James Leroy," in the Revue Médicale for June, 1822. In this note an instrument for seizing the stone is described. It consisted of a double or outer and inner canula of silver, eight inches long, between which a number, four or more, of watch-springs connected together by a button, which served to close the canula and admit of its introduction, were passed. These straight springs could be expanded by being pushed forward one after another, when they formed a kind of cage within which the stone being caught was readily fixed, and then

perforated by means of a small circular saw or trepan passed down to it through the inner canula. The power of this instrument to reduce and remove piecemeal within the crown of the trepan those calculi that were too large to pass by the urethra is spoken of. It is farther stated that "the instrument may supply us with a means of turning the discoveries of modern chemistry to profit. . . . Among the reagents capable of dissolving a stone, there are several," it is observed, "that may be introduced into the bladder without detriment or danger; but in ignorance of the kind of calculus with which we have to deal we might perchance be adding to its size instead of dissolving it. The lithoprione by making us acquainted with the intimate nature of the stone would enable us to choose with certainty the reagent capable of destroying it." It seems impossible to overlook the similarity of ideas here with those of Arnott already quoted. Even the language rendered back into English is almost the same. In his "Exposé," too, M. Leroy tells us that he had suffered his mind to go astray after the idea of constructing a pouch in which the stone might be inclosed, and then dealt with by means of appropriate chemical reagents. M. Nicod tells us that Ducamp got entangled with a net or pocket with which he proposed to surround the stone and keep the fragments of it together after having broken it down. M. James Leroy it seems did so likewise: "To one of the modifications of the lithoprione," he says (Exposé, p. 168), "you may adapt a net to retain the fragments," &c. Now, though we may expect that ingenious men in pursuit of the same object will exhibit some similarity in their means of attaining it, still they will differ from one another in some particulars, their contrivances will not be identical. When they are, we must presume that they come from a common source.

The instruments of M. Amussat and M. Leroy were as it happened accidentally laid together before the Royal Academy of Medicine in the month of July, 1822. They were of the construction already described; that of Amussat being Hunter's forceps with the blades separated, and acting by crushing or breaking the stone in pieces; that of M. Leroy being a cage of four watch springs for fixing the stone, which was then to be reduced into fragments by being repeatedly pierced with a trephine or borer, moved with a bow in the manner of an ordinary drill. The Academy immediately appointed a committee to report on the capabilities of the two instruments now laid before it, and experiments on the dead body were forthwith ordered and instituted in the presence of the committee by the two inventors. One of the hardest kinds of calculi of the oxalate of lime was chosen for the proof. Unfortunately M. Amussat's instrument ("*ingénieusement conçu, mais grossièrement exécuté*," says Leroy, Hist. de la Lithotritie, p. 21), broke without being able to bite the stone. M. Leroy had rather better luck; the stone was seized with his lithoprione and pierced, turned and pierced again and again, so that poor M. Amussat was thrown completely into the shade, and he appears to have retired from the field discomfited with his ill-made, but really more available, instrument.

M. Leroy, though he carried off the palm on this occasion, does not seem to have been altogether satisfied with his lithoprione. He went to work again, and in the middle of April, 1823, presented to the Royal Academy of Surgery a second instrument, greatly simplified, and infi-

nitely more facile of manipulation than the former one. With this instrument the stone was readily seized; it had but to be touched, in fact, and the blades expanded over it in order to be embraced. Instead of proposing to reduce the stone into pieces by repeated perforations, very tedious at all times and sometimes impossible, M. Leroy had now also contrived a slender rats-tail file, mounted upon a spring which gave it a tendency to diverge from the straight line. This file introduced into the perforation first made and worked by the bow, soon scooped out the whole interior of the stone and reduced it to a mere shell, which by and by could be crushed into fragments by the pressure of the blades of the forceps—a very happy idea. M. Leroy seems, however, to have found no opportunity of trying this instrument on the living body, and as this essential element was wanting, it seems to have attracted much less attention than it deserved. This second instrument of M. Leroy, however, united every element of excellence: it was as perfect as an instrument acting on the principle of perforation could be.

It was not until the year 1823 that we hear anything of M. Civiale in connexion with the subject of lithotrity. In this year he published a small work entitled: "*Nouvelles considérations sur la rétention d'urine; suivies d'un Traité sur les calculs urinaires, sur la manière d'en connaitre la nature dans l'intérieur de la Vessie, et la possibilité d'en opérer la destruction sans l'opération de la Taille.*" The part of the work on strictures of the urethra is a mere epitome of Ducamp, and therefore of Arnott, or of Arnott and therefore of Ducamp. Had Ducamp and Civiale been friends (and we do not know that they were not), we should almost imagine that the former had said to the latter, *regardez un peu mon ami*, here is an ingenious enough little book, of one portion of which I have made such good use that my fortune is secure; there is another portion which I have not touched, but which well worked out may possibly do as much for you. M. Civiale, in the introduction to the Treatise on Calculus, tells us that having witnessed a most cruel operation for stone in the year 1817, which ended fatally, he began to think of finding some means of avoiding the necessity for having recourse to it altogether. He therefore informed himself of what had been done on this subject, ("Je pris connaissance des travaux qui avoient été fait à ce sujet,") and, satisfied that the notion which he had conceived was quite new, and that by its means it would at all events be possible to ascertain the composition of calculi in the bladder, he consigned the results of his meditations and of his experiments in a short memoir addressed to the Society of the Old Faculty of Medicine of Paris in the year 1818. This memoir in the work before us is alluded to under the title, "*Quelques Détails sur un Lithontriptique*," by which M. Civiale says he meant an instrument for grinding stone in the bladder; but on referring to the notification of its reception at the society we observe it designated *an instrument for the operation of lithotomy* (Bullet. de la Société de la Faculté de Médecine, Aout 1818). Grave doubts have from the very first discussion of the subject of lithotrity been raised as to the truth of these early pretensions of M. Civiale. The authenticity of the document which existed in the hands of Baron Percy in 1824, and which was shown as that originally addressed to the faculty of Medicine, has been formally called in question, and as no satisfactory answer has ever been rendered by M. Civiale to

this arraignment, and as more than one report of the Royal Academy of Sciences delivered when there had been ample time to sift the subject to the bottom, has put him altogether aside as the *inventor* of lithotrity, we must conclude that his pretensions are without foundation. We have only to do with M. Civiale, therefore, from the year 1823. Let us examine his method of removing stone from the bladder without cutting. It is proper to begin, he says, by dilating the urethra, and he refers to the Arabian method of treatment, and immediately afterwards to Cooper's case of extraction of calculi through the urethra, "Un chirurgien très distingué (Astley Cooper) a tout récemment fait choix, m'a-t-on assuré, de cette méthode pour extraire de la vessie les calculs qui ne sont past rop volumineux." To effect the dilatation bougies are objectionable; what he employs is a piece of the intestine of a cat, which is introduced upon a bougie or sound, and this being withdrawn the gut, which is left behind, is then injected with water or air, till the dilatation deemed necessary is obtained. After this the *lithontriptor* is to be introduced. This is an instrument constructed of two hollow cylinders or canulæ, of such dimensions relatively that the one can readily be received within the other, and of a stylet which in its turn is received within the inner canula; connected with the inner canula are four branches, the elasticity of which causes them to separate, unless they be kept together by being drawn somewhat within the outer canula. The stylet is a very important part of this instrument, and has two principal objects, viz. to aid the elasticity of the branches and effect their separation, and to attack the stone when once it is seized. Now, on looking at the figure of M. Civiale, we see this essential difference between the drawing and the description, that in the former the blades of the forceps are *jointed*, whilst in the latter they are spoken of as possessed of *elasticity* to cause them to separate; they are jointed however; each consists of two pieces connected together by a simple hinge; and it was this structure that made the inferior cone of the perforator necessary; the blades of the forceps possessed of elasticity would have required nothing of the kind to make them expand; but as drawn in the figure, the blades have no elasticity, and could only have been opened by the backward pressure of a wedge or cone. In considering this figure and the description given of it we see positive evidence first of a want of mechanical skill, and then of harmony between them; the drawing and the printed account do not agree; the description looks like an after-thought. Such a machine was certainly unavailable; it never could have been used to seize and perforate a stone in the bladder; there is absolutely nothing for the operator to hold by, and the drill is to be worked by being turned between the fingers! But the *savoir faire* is of more avail in the world than the *faire savoir*, and if M. Civiale had little mechanical ingenuity, he seems to have had an ample endowment of worldly wisdom. Such as his miserable instrument was, it was presented along with a memoir entitled "Nouveau moyen de detruire la Pierre dans la Vessie sans l'opération de la Taille," to the Academy of Sciences in March 1824; and this illustrious body having appointed M. Chaussier and Baron Percy to report upon the memoir and instrument, Baron Percy drew up a document that at once arrested the attention not only of France but of Europe, and placed the obscure Civiale on a pinnacle from which he has continued to overlook not only all the men of his standing

but almost every other, however eminent, in his own walk in the profession. To us, indeed, M. Civiale appears to be one of those favoured children of fortune for whom, to use a vulgar phrase, the bowls always run aright; one of those who without speed is nevertheless foremost in the race, who wins the victory or ere the battle be joined. M. Amussat charges a stone in a dead man's bladder, and his instrument fails him at the very onset, it goes to pieces, and he is disarmed and retires from the struggle. M. Leroy succeeds him, and makes a hole or two in the stone; but it is a tedious business; all present get tired, and the instrument is at length withdrawn, the stone being left behind but little the worse for the encounter. He perseveres, however; he sees the defects of his apparatus; he has the ingenuity necessary to remedy them, and is effectually implemented at length; but he finds no opportunity to try his fortune on the living body for something like a year, and when the opportunity does at last arrive, the circumstances are unfavorable and he fails. How different the course of the man on whose birth the genius of good luck has waited! He publishes a machine inferior in point of conception to every other already contrived, imperfect to such a degree that it never could have been used, and he has hardly proclaimed himself ready to take the field, when he finds not one but two, nay three occasions as "happy prologues to the swelling act" of his final greatness. His own instrument would not have served him indeed, but what signified this? M. Leroy had been sent before by Providence to provide him tools, and to work he went. Of course he succeeded—succeeded signally! In the first case which he encountered the patient was delivered of his stone in two sittings (Jan. 13th, 1824); in the second case (Feb. 4th) the stone was reduced to powder in four sittings; in the third case (March 4th) it was destroyed with the same ease and expedition. It was on the strength of these triumphs that Civiale came before the Royal Academy of Sciences, and it was with his mind's eye still dazzled with their splendour that Percy drew up the report to which allusion has been made, in which infinitely more than was his due is given to Civiale, and far less than belonged to him by indefeasible right is granted to M. Leroy. Civiale had indeed the good luck to find the opportunity of performing the operation on the living subject first, but he contributed nothing to the means by which he triumphed. His boasting in regard to lithotrity must be viewed as of a piece with that of the poor furnace-man, who lights the fire and turns on the steam, and then arrogates to himself the beautiful mechanism of the steam-engine and all the wonders it performs. The world is apt to say, the great merit of everything lies in its application. True, if this thing have been abandoned by its projector, and be rusting for want of use; but it is otherwise when the inventor is still occupied in perfecting his work, and is not only waiting but eagerly looking out for the opportunity to bring it into play himself. And this was precisely the case with M. Leroy and lithotrity; he has shown himself a good workman in numerous instances since, and lithotrity was altogether independent of the interference of M. Civiale; it would have been no less advanced had he never existed. M. Leroy's countrymen did him something like justice by and by, and, we think, all he has had is well deserved; for, in spite of our strong persuasion that the idea of removing the stone from the bladder, and even the first hints of the means of ac-

complishing this end, were not so original as he has by this time brought himself to believe, we feel it impossible not to admire M. Leroy for his mechanical ingenuity, and for the temper and forbearance he has shown towards M. Civiale, who certainly has exhibited none in regard to him.

Lithotrity, as we have followed it hitherto, had for its object the wearing down and comminution of the stone by perforations performed repeatedly and in different directions. But this is not the only mode of getting rid of calculi that has been imagined or that has been employed. Between the practice of extracting small stones from the bladder (1821), and the idea of breaking up such as were a little too large to be brought away, the distance is not great; and accordingly we have already seen that the maker of Cooper's forceps, Weiss, almost at the same time gave to the blades of this instrument such strength as he believed would enable them by their spring power to crush calculi into pieces. By and by (Feb. 1823), at the suggestion of that excellent surgeon, Mr. Thomas Davis, apparently, Weiss increased the strength of this forceps still farther; so far, indeed, that the action of a screw became necessary to open the blades. We have also seen that M. Amussat's instrument acted on the crushing principle, and that Elderton's, although not proposed, was applicable in the same direction. The idea and even the practice of breaking down the stone is, therefore, almost as old as that of wearing it down by perforations. Rodriguez, a surgeon of Malaga, delivered a patient of his stone among other means by striking or pounding it with a catheter every day or every other day (Alibert, in Journ. des Connais. Méd., tom. i.), and it is matter of tradition that the late distinguished surgeon, Mr. Thomas Blizard, when he met with a soft calculus, was in the habit of breaking it to pieces by means of a steel sound or catheter passed into the bladder.

Mr. Weiss, having made an instrument with a screw handle to *open* the blades, seems very shortly to have seen the vast advantage he would gain by using the screw to close them; and in the course of the same year he actually constructed an instrument, which may still be seen at his shop in the Strand, which differs in no essential particular from the screw lithotrite now in general use in this country, and also extensively known on the continent as Heurteloup's Percuteur. This last instrument, however, acts by the blow of a hammer, not, save adventitiously, by the pressure of a screw like Mr. Weiss's; but the two instruments are nevertheless essentially the same, the means only of procuring the power are different. M. Leroy seems also to have turned his attention to this mode of destroying the stone, and Professor Jacobson of Copenhagen at a later period contrived a very beautiful and very effective modification of the crushing lithotrite, which with a slight alteration from the hands of Dupuytren is probably to be reckoned among the most generally available instruments we possess.

We shall pursue this subject no farther. Our instruments are quite perfect, in so far as the end to be attained by their means is concerned; in the vast majority of instances no difficulty is now experienced in seizing and reducing to pieces even large calculi still contained within the cavity of the bladder. But has this object so eagerly pursued, so completely attained, answered the expectations that were even as dearly cherished of its being useful to humanity? We answer unhesitatingly

*No!* The value of lithotrity as a general means of treating stone in the bladder has been immensely overrated, and its indiscriminate application to all kinds of cases has cost many valuable lives. To such an extent has this already occurred, that it might be made a question whether M. Civiale's first successes ought not rather to be made subject of regret than of rejoicing, for successes in desperate operations are known to do vast mischief in the long run; one is saved, ten perish prematurely in consequence. It has hitherto, however, been a most difficult matter to get at the fact of the advantages or disadvantages of lithotrity. Operators generally have been somewhat chary of saying much upon the subject of the mortality. M. Civiale is in fact probably the only man who has had such ample personal experience of lithotrity as to be in a condition to speak from a large number of cases upon this point. But we regret to find that no credit whatsoever is due to the reports of M. Civiale. In his Traité de l'Affection calculeuse, p. 613, he speaks of the number of persons affected with calculus who had sought his assistance up to the year 1836. They amounted to 506. Of these 199 were either unfit subjects for lithotrity or were otherwise prevented from submitting to the operation. Supposing the whole of these to have been really unfit, this would be in the proportion of one in two and a half very nearly, to whom lithotrity held out no chance of relief, or who, were they subjected to the operation, would almost certainly lose their lives. We have, therefore, 307 subjects held favorable for the operation. Of these, says M. Civiale (Op. cit. p. 630), 296 were completely cured; 7 died; 3 were only partially relieved; and in 1 the issue is not known. But in the face of this very flattering statement let us turn to one or two of the public documents we possess and see how the conclusions in these accord with the numbers of M. Civiale. In a report presented by Messrs. Larrey and Boyer to the Royal Academy of Sciences in the month of April, 1831, upon the cases of calculus in the Hôpital Necker under Civiale, we find these respectable men challenging M. Civiale with dwelling upon his successful cases only, and making no mention of those who underwent lithotomy, lithotrity having been previously essayed in vain. "We find," say the reporters, "that twenty-four patients (not sixteen, as stated in the compte rendu,) had undergone the operation of lithotrity or lithotomy. Of these twenty-four, of whom *six* were cut [we presume after lithotrity had been tried in vain], *eleven* died more or less immediately after the operation." *Eleven* deaths in *twenty-four* cases, immediately after the operation! Verily there is little to boast of here, and we can already afford M. Civiale his seven deaths in 307 cases, and have four at our disposal to carry to the next account. Let us go on to public document the second. This is a report to the Royal Academy of Sciences presented by Messrs. Boyer, Double, and Larrey upon the operations performed at the Hôpital Necker during the years 1831 and 1832. "Fifty-three patients affected with calculus were received at the hospital. Of this number twenty-seven treated by lithotrity were discharged completely cured; sixteen having had various attempts at lithotrity made upon them, the operation was definitively found impossible or useless, or it proved fatal. Of these sixteen ten died and six remained unrelieved. Eight other patients were subjected to attempts at lithotrity and then to lithotomy; of these five died and three recovered." If we analyze this

statement we find fifty-three receptions and twenty-seven recoveries; this is as nearly as possible one recovery in two cases by means of lithotrity, but as three recovered after the failure of lithotrity by means of lithotomy, we have three to add to the list of cures, which therefore amount to thirty in all. On the other side we find ten deaths as immediate consequences of lithotrity, and five more after lithotrity and lithotomy combined, that is fifteen deaths in all. Eight cases remain unrelieved, which must terminate fatally within a brief period, all the more quickly, for the sounding, &c. they doubtless underwent at the Hôpital Necker. *Fifteen* dead, *eight* unrelieved and expecting death, make twenty-three cases of non-success, to twenty-seven of success, by lithotrity. Surely neither is there aught to be proud of here; the cases in which no relief could be afforded were within *four* of being as numerous as those in which lithotrity was found of avail! And this is an operation that boasts of all but invariable success! three hundred and odd cures to seven untoward results!

Turning from public documents, let us see what some of M. Civiale's contemporaries have made out by an analysis of his cases, somewhat more rigorous than his own. M. Velpeau,* than whom there is no more honorable man or trustworthy writer in France, has given an analysis of five series of M. Civiale's cases, and we here append his table:

| Series. | No. of Cases. | Cured. | Dead. | Unrelieved, the stone remaining. | Otherwise Success in. | Otherwise Failure in. |
|---|---|---|---|---|---|---|
| 1st. | 83 | 41 | 39 | 3 | 41 | 42 |
| 2d. | 24 | 13 | 11 | 0 | 13 | 11 |
| 3d. | 53 | 30 | 15 | 8 | 30 | 23 |
| 4th. | 30 | 18 | 8 | 4 | 18 | 12 |
| 5th. | 16 | 6 | 7 | 3 | 6 | 10 |
| | 206 | 108 | 80 | 18 | 108 | 98 |

That is to say, of 206 patients operated on, 108, (a very little more than *one in two*) recover immediately; 80, or nearly *one in two and a half* die; and 18 retain the stone, and will be lost. *One hundred and eight cases cured, ninety-eight in which death* is immediately induced or may not be averted within a brief interval of time. This is a very different tale from the one told by M. Civiale himself; the reader may adopt the conclusions of whichsoever of the two statements he pleases. There can be no question as to the one that conveys the truth.

The experience of several other practitioners, excellent surgeons, with heads to plan and hands to execute this most delicate operation of surgery, but who have not addicted themselves exclusively to lithotrity, have not met with even the very moderate success of M. Civiale from its resources. M. Velpeau, however, has been more fortunate, and as his experience and candour are greatly to be relied on we shall give a summary† of twelve cases of calculus that were under his own immediate care, and in which he essayed lithotrity. In the first case, lithotrity had to be abandoned on account of the sufferings of the patient who remained unrelieved; in the second case the patient was cured; in the third he died; in the fourth, lithotrity had to be relinquished, after which,

* Médecine Opératoire, 2de Ed., t. iv. p. 649. † Ibid. p. 653.

lithotomy was performed; in the fifth, the operation of lithotrity was also found impracticable, and lithotomy was had recourse to; in the sixth, the patient was cured; in the seventh, he was also cured; in the eighth, he died; in the ninth, he was cured; in the tenth, he died; in the eleventh, he was cured; in the twelfth, lithotrity had to be given up, and lithotomy substituted. Of the 12 cases, consequently,

5 were cured by lithotrity, (not one in two);
in 1 the operation was abandoned, and the patient remained unrelieved;
in 3 lithotrity was given up as impracticable, and the patients were cut, and recovered;
and 3 died of the operation.

5 in 12 is *one in two and a quarter*, nearly, in which success followed the operation of lithotrity.
4 in 12 is *one in three* in which lithotrity is unavailable, the operation cannot be performed.
3 in 12 is *one in four* in which a fatal result ensues.

The effect of the operation gone on with in the four cases in which it was abandoned may easily be conceived; it were not saying too much to maintain that in the hands of a man *committed* to the operation of grinding or crushing, four deaths more would have been added to the list of the mortality, when we should have had five recoveries counterbalanced by seven deaths. Other practitioners have, as we have hinted, had even less to boast of than M. Velpeau. And here be it observed that the deaths in all the preceding statements are the deaths that happen immediately upon the operation of lithotrity; there is no mention of those that occur at the distance of a few months, hardly of a few weeks, from the date of its performance. Were these taken into the account, the number of *cures* would be found wofully diminished, that of *deaths* frightfully increased. Our own knowledge and the published statements of more than one unimpeachable authority would lead us to maintain that the operation of lithotrity was even less fatal immediately than it is in brief prospective. In great numbers of instances the unfortunate patient who has undergone lithotrity *successfully*, i. e. who has got rid of his stone, and has not fallen an immediate victim to the means that have been used to deliver him, of course feels greatly relieved for a season; the stone is gone, the thorn in the living flesh is plucked out, and the patient rallies and again looks abroad upon the world with an eye of hope and gladness; but he is not quite well, more or less of irritability of bladder remains behind that soon renders the constant services of the medical attendant again necessary; this irritability increases; the patient begins to be tormented with ceaseless pain in the region of the bladder, which by and by extends up the loins and settles in the small of the back. The renal secretion has altered at an early period of these untoward symptoms; by and by it becomes like turbid whey, it has a faint sickly smell; it coagulates on the addition of nitric acid, and when exposed to heat; the patient loses strength rapidly; his stomach fails him; he becomes sick, and vomits; he begins to dose; frequently he has an epileptiform fit or two; then he falls into a state of coma, from which he never awakes, or he is seized with a more violent convulsion in which he expires. This picture is from the life; such are the symptoms which we have seen in more than one instance, occurring *within two years of successful litho-*

*trity*. Chronic disease has been excited in the bladder and urethra, and this by continuity of surface and of tissue creeps slowly upwards till the kidneys are attained, when death in the exhausted constitution in which such things take place is inevitable.

Mr. Fergusson (Edinb. Med. and Surg. Journal, Oct. 1838,) and Mr. Key (Guy's Hospital Reports, vol. ii. p. 1,) have each conferred a great boon upon true science by their observations upon lithotrity, and have effectually stripped the especial professors of this operation of the cloak of mingled pretension and mendacity with which they have robed themselves ever since they appeared upon the stage. Both Mr. Fergusson and Mr. Key have followed many patients upon whom lithotrity had been performed into their privacy, and have presented us with a sad picture of the life which these unfortunates generally lead, and of the miserable end which most of them meet at no long period after their deliverance from stone. Mr. Fergusson has recorded the particulars of seven cases in which lithotrity was performed, of which we wish our limits permitted us to present our readers with an abridgment seriatim, for their edification and enlightenment in regard to the value of this operation. All we can do is to lay before them an analysis of the results. In the 1st case the patient, though he was delivered from his stone, never recovered completely, and declared himself in worse plight than he was before he came under the hands of the lithotritist. In the 2d case the patient could not be delivered by the first series of operations; he had to retire with a fragment still in his bladder, which required a second series of sittings at the interval of more than a year, when he recovered. In the 3d, each operation was attended with such suffering that the patient, had he not been a man of great resolution, would have remained unrelieved. He recovered. In the 4th the patient nearly lost his life from the irritation induced by the attempt to operate by lithotrity. He was cut and recovered. In the 5th, lithotrity was found impracticable. The patient, cut twelve months afterwards, did well. In the 6th, the patient died four days after the operation of lithotrity. In the 7th, the patient could not endure lithotrity; he remained unrelieved, worse than before the operation. In these seven cases, therefore, we see but two *recoveries* from stone through the means of lithotrity. One man is delivered of his stone indeed, but he is left with a diseased bladder, which is, if possible, a worse evil than the stone. One is dismissed with his stone in fragments still contained in his bladder, and one dies immediately from the effects of the operation. That is to say, we have as good as three deaths in seven cases; for he who escapes from lithotrity with a diseased bladder dies, and he who is unrelieved of stone by one process, and is left beyond the pale of relief by any other perishes. Two are cut, lithotrity having failed, and recover. Two recoveries in seven cases, and each of these achieved at the cost of much suffering to the patient, and certainly with imminent peril to his life!

Besides these seven cases, Mr. Fergusson had cognizance of many others, in which lithotrity had been performed, but which were less immediately under his own eye than those the details of which he has given. Out of eighteen cases, however, in which he had known lithotrity to be performed, he informs us that *six were cured*; that *seven were not cured*; and that *five died*. Even in the number of reputed cures there is one

case in which there are strong reasons for suspecting a *return* [quere, *a continuance*] *of the disease ;* there is a second, in which, though no stone can be felt, " the patient has suffered almost as much since he was operated on as he did previously to coming under the surgeon's care. Indeed," continues Mr. Fergusson, "*in two only of these* [*eighteen*] *cases can the operation be said to have been attended with that happy success which has been generally claimed for lithotrity.*"

Mr. Key, in his excellent paper, shows even as clearly and forcibly as Mr. Fergusson, that lithotrity is an operation that is neither so universally successful, nor so extensively applicable as might be inferred from the statements of its professors. In illustration of this position he gives the particulars of twelve cases, which we should gladly lay at length before our readers, but of which our space only permits us to subjoin an analysis :

12 cases, of whom

3 were cured by lithotrity, and

3, after vain attempts by this means, were cut and recovered.

6 died, of whom *one* with abscess in the prostate soon after the operation, *four* with protracted sufferings in consequence of fragments remaining in the bladder, and *one* with disease of the bladder, brought on or aggravated by the operation, but whether any fragments were left in the bladder or not was not ascertained. We have consequently *one case in four* cured by lithotrity, and *one case in four* cured by lithotomy, after lithotrity had failed ; finally, we have *one death in two*, of all the patients who were subjected to this means, these patients having all or with a single exception been under the hands of one of the most skilful lithotritists of the day ; a man who, to his literary and scientific knowledge, the result of a most liberal general and professional education, adds a rare mechanical genius, and a dexterity that was never surpassed. If these be the results in twelve cases where the Baron Heurteloup presided, and where Charles Aston Key lent his countenance and assistance, we own ourselves utterly at a loss to imagine what the amount of disaster must be where less of knowledge, less of skill, and less of conscientious feeling, have the sway.

The conclusions to be drawn from these cases and these views are obviously melancholy enough in so far as lithotrity is concerned. And yet, when we reflect dispassionately and as physiologists and practitioners upon the nature of the entire process in this operation, we see it impossible that the results could have been very different from what they are. Let us only consider the immediate consequences of the successful administration of lithotrity, the searching for and seizure of the stone, the necessary violence that accompanies the act of its comminution, and its condition with reference to the bladder after having been reduced to pieces, and we perceive that in the nature of things it can be no trifling operation, that on the contrary it must needs be one fraught with much danger to the patient. We know that the mere act of searching the bladder with a polished sound is often accompanied by a great amount of pain, and followed by what appears a singular degree of sympathetic disturbance ; we know that the attempt to seize and extract small stones in the bladder by the most delicate forceps has ended fatally; and how shall the necessarily large and complicated implements of lithotrity be intro-

duced and brought into play within the bladder without producing a hundred times the amount of excitement and of mischief? This cannot be, and is not. And then, what shall we say in regard to the jarring and violence inseparable from the process of working a drill, or of turning a screw, or of giving the whole apparatus a smart blow with a hammer? What of a stone, which with a smooth surface was already such a source of suffering as to make the possessor weary of his life, and willing to take the chance of any odds against the solitary hope of obtaining relief, either roughened by repeated perforations, or reduced perchance into eight or ten angular and rugged fragments? All we can do is to admire the powers inherent in the delicate tissues that compose the excretory portion of the uropoietic system to withstand violence, and to repair themselves, bruised and maltreated as they necessarily must be, in such an operation as lithotrity performed by the most gentle hand.

The singular increase of irritation that takes place in consequence even of the *spontaneous* breaking up of calculi in the bladder, a phenomenon which sometimes occurs, and the danger to life that ensues thereon, is strikingly illustrated by the circumstances and the issue of a case which is related by Mr. Liston.* A medical man, who had laboured under symptoms of stone for a great many years, and who by sounding himself had ascertained the existence of a stone in his bladder ten years previously, was one day met by Mr. Liston in consultation. In three days after this Mr. Liston was summoned to this unfortunate gentleman in a moribund state, from inflammation of the whole urinary system, his urethra being at the same time blocked up by large fragments of stone. "It appeared," says Mr. Liston, "that on parting with me he had been summoned to an urgent case of labour. He ran quickly down a steep street, and at the bottom of it was seized with an urgent desire to make water, which he did in small quantity mixed with much blood. He passed some pieces of stone with sharp angles. He went on from bad to worse; he had retention, and the urethra was found much obstructed; suppression followed, and death terminated his sufferings in a few days. Many portions of the calculus were voided; much stone with the nucleus occupied the bladder and urinary passage. The kidneys were dark coloured, and one approached to a gangrenous state."

Now it is the business of lithotrity by a certain amount of mechanical violence, less or more, to accomplish such a disruption of a calculus as took place here spontaneously; and our amazement finally comes to be, how the operation should ever succeed, not that it should so often be found either impracticable, or if persevered in fatal. And this leads us immediately to consider the circumstances in which the operation is admissible, and those in which it is inadmissible. This point is soon discussed; the conclusion lies on the surface, and wants no farther fact or argument after what has been said to make it clear. Lithotrity is admissible and only admissible in cases in which the bladder is perfectly healthy, and in which the stone is small, of the size of a filbert, a shelled almond, or it may be a nutmeg at the utmost; under all other circumstances it ought to be held impracticable. In other words, lithotrity is admissible where it is estimated that the stone can at one sitting be seized and reduced to fragments of sufficient minuteness to be passed by

* Elements of Surgery, 2d ed. p. 632.

the urethra. No second, certainly no third operation ought ever to be contemplated. *If the patient who has had lithotrity performed upon him is not relieved at once, he is in imminent danger of losing his life.*

Lithotrity may now fairly be said to have been tried and found wanting as a general means of relief for stone. Restricted to the circumstances indicated above, it is a great addition to our chirurgical therapeia; applied indiscriminately, and as a substitute for lithotomy and all other means of treating stone in the bladder, it is a most fatal present made to humanity.

The next subjects we ought to examine are, first, the removal of the stone through an incision into the neck or fundus of the bladder—LITHOTOMY or CYSTOTOMY; and, second, its removal through an opening made into the urethra in perinæo and slow dilatation of the neck of the bladder—LITHECTASY, or CYSTECTASY: but we have already exceeded our prescribed limits in the length of this article; and important as the matter is, we feel that we must take another, and we trust we shall find an early opportunity, of discussing it by itself.

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## Art. VII.

*A Practical Essay on some of the principal Surgical Diseases of India.* By F. H. Brett, Esq., Bengal Medical Service, Surgeon to the Governor-General's Body-Guard.—*Calcutta,* 1840. 8vo, pp. 506. Plates.

There are obvious reasons why the work before us should be treated tenderly: it is the first important surgical contribution from our brethren in India; it has been written in India by an Indian surgeon; it has been printed in India, and printed not by professed workmen but by amateurs. Still, as we have ever regarded the system of indiscriminate praise adopted (from what motives we shall not enquire) by some medical reviewers, as alike degrading to the author and the critic, we shall not deviate from our honest and independent course in the present instance; and though we shall sincerely regret if we cannot commend Mr. Brett so highly as we would desire, he will, at least, have the satisfaction of knowing that when praise is bestowed, it is bestowed deliberately and from conviction. High praise he unquestionably merits for having written the book at all; nor can any imperfections it may present of plan or execution deprive him of this. There is so much to be done to make us acquainted with the modifications which difference of country brings about in the great features of disease, that we must foster every instalment we get, even although it do not come up to a standard we may ourselves frame.

We commence, then, by saying that we have been rather disappointed by Mr. Brett's book. It will certainly do much less for surgery than has been done by Annesley, Twining, and others for medicine in our Indian empire. We do not, however, by any means wish it to be understood that Mr. Brett's work, as far as it goes, is not the work of a sensible and well-informed and experienced surgeon; what we complain of is, that it is more "Surgery in England" than "Surgery in India." What we expected and wished for was not a compilation, however well it might be

done, but an account of the changes produced in diseases common to both countries by the climate of India; and also to know how far it modified operative surgery. In this respect we are bound to say it has in nowise satisfied our expectations.

The volume is divided into seven chapters which are devoted to the following subjects: Inflammation, and its Sequelæ; Parasitic Growths; Indian Leprosy; Diseases of the Generative and Urinary Organs; Diseases of the Eye; Autoplastic Operations; and the Dracunculus: a list which necessarily leaves many important surgical subjects unnoticed. We see no better mode of giving our readers an idea of the contents of this work than by offering any remark which may occur to us on each chapter in the order in which they stand.

The subject of Inflammation has been so lately discussed in our pages that we will not enter upon the opinions of Alison and others which form the staple of our author's observations in chap. i.; but connected with the subject are some excellent observations on the health of Europeans in India, which, on account of their practical value, we here extract:

"The inflammatory diseases of Europeans, in India especially, are chiefly induced by the highly stimulating diet they use and the little exercise they take, aided by the debilitating circumstances to which they are exposed in this climate; rendering them more liable to the exciting cause of fever, viz. malarious atmosphere. A European on his arrival in India is generally in the highest state of plethora. By such a system of stimulating diet he seldom escapes a severe attack of inflammation shortly after his arrival. ..... On convalescence the same habits are renewed, and he arrives in the upper provinces with his health restored. For the first few years of his residence in India he becomes in high condition, especially during the cold season.....But his system both vascular and nervous is under a constant state of excitement from the continued in-pouring of nutriments, stimulation, and high temperature, and he labours under plethora from *repletion* as well as plethora from *diminished secretion*. He is on the precipice of disease, and a slight impulse is sufficient to destroy the balance and precipitate him. Should he survive the repeated attacks to which he is liable his constitution becomes enfeebled, his appetite impaired, his energies diminished, the secretions and excretions scanty, and the skin dark, harsh, and dry. He is perhaps dyspeptic, and, in a word, his system becomes lowered down to the climate, and he is no longer recognizable as the energetic active European. His muscular system is much reduced. He is the 'dried-up Indian,' with habits and constitution scarcely adapted for his native land. On the other hand, should his constitution still remain of the plethoric tendency, he is gross, corpulent, and flabby. ..... If Europeans cannot adapt their mode of life to a tropical climate, but must indulge in habits totally incompatible with such a climate, they should at all events endeavour to convert their food into wholesome nutriment, and preserve the robustness of their frames by practising athletic exercises in the cool of the day, and wrestling in imitation of the Puh,lwans. It is indisputable that these individuals enjoy an immunity from disease unknown by others. There are none whose constitutions resist the exciting causes of disease so well, although their blood is abundant and their vascular system vigorous. The few Europeans who have entered thoroughly into the spirit of these exercises return to their native land with vigorous constitutions capable of really enjoying their native country. The writer has long admired and practised the calisthenic exercising of the Asiatics, and attributes a better state of health and stamina, and a capability for active pursuit, far superior to that enjoyed by him in England, to a systematic use of these exercises." (pp. 39-41-2.)

In treating of Parasitic Growths some interesting remarks are made

upon diseases which happily are unfrequently met with in Europe, viz. Hypertrophy of the scrotum and pudenda; and yet there is little in which the author has not been anticipated, by many years, by Delpech; even his "improvements in the operation" for their removal do not seem new to us. Still India affords a larger field for the observation of those affections than Europe, and it is well to have the accuracy of Delpech's views on the subject confirmed. In 1837 our author removed seven of these deformities weighing in the aggregate, when divested of their fluid, 280 pounds. The results of those operations were favorable; five of the patients recovered, two died of gangrene of the parts, which the author attributes to the operation having been performed during most sultry weather, in a Bengal climate, and in a very crowded hospital, where something like hospital gangrene prevailed at the time. It does not appear that in India the disease falls with greater severity on the sedentary than the laborious portion of the population, though such an opinion has been more than once expressed. Whether the disease be what our author supposes it to be a pure vascular sarcoma, is more than questionable. If he be correct in the opinion that "it is evidently the same disease with that which occurs in the West Indies, Africa, Ceylon, and occasionally in Europe," it is certain that his impression as to its nature is incorrect, unless he have some new view of what constitutes vascular sarcoma. Certain it is that the disease in the West Indies and in the south of Europe is an hypertrophy of existing tissues, with serous infiltration; whether dependent on lymphatic disease may be doubtful.

The section on Bronchocele is interesting; but we entered so fully on this subject in a previous Number (vol. VIII. p. 103), that we must pass it over here.

On Elephantiasis or, as our author prefers to call it, Lepra Tuberculata Virulenta, there is an interesting chapter. The symptoms of the disease are as follows:

"An individual labouring under this complaint usually presents himself in the following condition: A general torpor seems to pervade his system, so that his sensations of pleasure and pain are considerably impaired. Instead of that excessive propensity to venery which they are generally supposed to possess, they have usually little or no inclination for such indulgence, though there are exceptions to this, where it exists in a diminished degree only. The pulse is slow, not small, but heavy, 'as moving through mud.' There seems to be a sort of stagnation in the process of nutritive vitality. The bowels are generally constipated. The patient's face is bloated. His forehead, nose, lips, and ears become swollen; his nostrils expand; his eyes appear sunk and fiery, the tone of his voice is altered to a loud and nasal sound; the skin, especially of the extremities, is harsh and scaly, resembling a case of ichthyosis. He is subject to profuse perspirations, especially when exposed to the sun; these are confined to the trunk, not the slightest moisture permeating the surface of the scaly extremities. He is often distressed with thirst and a sensation of internal heat. The knees are stiff, and their motions contracted. The hairs generally fall from the brows, the beard, the pubes, and axillæ, &c. or they are seen stunted and dried up for want of moisture. His breath is fetid, and his perspiration rank and offensive. The blood drawn by venesection is very dark. A kind of dry gangrene pervades the fingers and toes, which are generally eaten away, and drop off at the first phalanges; these sores then generally cicatrize over without any rete mucosum, and the next joint becomes invaded by a renewal of the ulcerative process. It is singular to observe that notwithstanding these extensive sores, they can wear hard shoes, which are seen saturated with the sanious ichor

that exudes from these ulcerated surfaces. The disease progressively advances, eating through the ankles and wrists, and performing slow but certain and successive dismemberments, every revolving year bearing some trophy of this tardy but gradual march of death, till at last the vitals become affected. During all this, a sleepy inertness overpowers his mind, and 'seems to benumb and almost annihilate every faculty, as well of the soul as of the body, leaving only sufficient sense and activity to crawl through the routine of existence.' In the last stages of the complaint, the flesh gapes with long sores, the mouth, nose, and brain become exposed to its ravages, and death at length terminates this loathsome existence. He is usually cut off by the supervention of diarrhœa. It is astonishing, however, to witness how long the victim lingers, from twelve to twenty years being no uncommon duration. During the greater part of this period he has a good and even voracious appetite, and moves about from village to village." (pp. 163-5.)

The causes of the disease are not very evident though the inclination of opinion favours the belief that improper food is a principal element in its production. An analogy is thought to exist between this disease and that from which Europeans have suffered from the use of spurred rye; and it is strengthened by the evidence that it has prevailed to the greatest extent in those districts and seasons in which the poorer classes were obliged to eat diseased rice. "The disease is very common in the valley of the D,hoon and in Bengal, in both which provinces coarse rice constitutes the ordinary article of diet."

Our author gives evidence in favour of the non-contagious character of the disease; but much well-considered evidence will be necessary to weigh down the mass of facts which have been collected to support an opposite opinion. Certainly the faith in its contagion is waning among the natives; for in the present day we no longer hear of lepers being buried alive, drowned in the Ganges, or immolated on the Suttee; though it is said they occasionally commit suicide by leaping into the Ganges.

In the early stages the disease is susceptible of cure, though even then the failures are frequent; in its advanced stages much relief may be afforded to the sufferer, but rarely is a patient cured. Arsenic is the remedy most generally employed, but it is associated with the mudārr (Asclepias gigantea) to which Playfair, Robinson, and others have ascribed great virtue. The most accredited form is the following: Oxide of arsenic, 55 grains; mudārr powder, 4 ounces and 80 grains; black pepper, 9 ounces: the whole well beaten at intervals for four days, made into a mass with water and divided into 800 pills. Not more than two pills should be taken daily, which are equal to about a seventh of a grain of arsenic. Mr. Marshall treated 200 cases with nitric acid, a drachm in a pint or a pint and a half of water daily; above one third were cured and the remainder were greatly benefited. The Hindoo physicians place their patients during treatment on the following diet: all kinds of game, Sushtiká rice, and Urhur.

Urinary Calculi are of frequent occurrence in India, and happily the operation for their extraction seems to be attended with less risk than in the continent of Europe; whether this is to be attributed to the use of vegetable food impressing a difference of constitution upon the natives of India, or whether it be owing to the temperature of the climate, is a point of great interest, but which we want the elements to determine. In our own country we want the data to determine whether our agricultural

peasantry, consuming as they do (more especially in Scotland and Ireland), comparatively very little animal food, do better after great operations than those whose dinner is daily composed of meat. Our county hospitals should assist in the determination of this question. Again the question of temperature in its influence upon wounds is of vast importance to the surgeon. The evidence of Larrey, of the rapidity with which wounds healed during the occupation of Egypt by the French army, the experiments made by Breschet and others, the use of warm-water dressings in our own country, favour the idea that a comparatively elevated temperature is favorable to the healing of wounds. If this article should fall into the hands of Mr. Brett we would venture to suggest to him the value of a register for the purpose of ascertaining the ordinary period required for the cicatrization of amputated surfaces in India.

To return to calculous diseases. Some persons have attributed the frequency of stone in India to the errors of diet in the natives, especially the children, who are permitted to devour various kinds of unwholesome rancid sweetmeats, and particularly coarse unleavened bread, at all hours of the day. As an evidence of the frequency of stone cases, our author states that the late Mr. Burnard cut about forty patients for stone at the city of Benares; that he himself has operated on four patients in one day, all residing in the same village or its immediate neighbourhood; and that he had attended upwards of 100 cases during his abode in India. In "the natives of India," says Mr. Brett, "their systems are habitually prepared for operation, and the little advantage derivable from preparatory treatment is more than counterbalanced by fear and suspense. They come with confidence in the superiority of European skill, from hearing of our success on other sufferers, and they seek for speedy relief from agonizing pain. The patient is infinitely more pleased by immediate operation, which when set about with the least possible ceremony, is not looked upon at all seriously by him." This system might be beneficially applied elsewhere than on the continent of India. We are convinced that it might be advantageously followed amongst ourselves. The state of feverish anxiety and irritation, which is frequently developed in the patient during the time of preparation, is often extremely prejudicial to the success of operations. We by no means wish to advocate a system which has been too generally followed by some of our continental brethren, of acting in every case without submitting the patient to any preparatory treatment. In getting rid of one evil, as far as may be prudent, it surely is not necessary to adopt another. There are many cases where such preparation is unnecessary, and where in fact it exercises an injurious influence; there are others in which it cannot be dispensed with. Mr. Brett has, of course, made up his mind as to what constitutes the state of habitual preparation for operation which characterizes the natives of India; and we hope some day to be favoured with his views on the subject.

Mr. Brett in all cases causes to be administered to his patient an hour before the operation, not less than 100 drops of laudanum; and as this is the only feature, as connected with the operation, differing from those ordinarily employed, it would be interesting to know how far this has contributed to his brilliant success. His last sixty-eight cases all did well, although operated upon in different districts, viz. Calcutta, Cawnpore, Meerut, Shahjehanpore, Delhi, and Mooradabad, and although six of the

patients were sixty years of age. Malgaigne has for some years strongly recommended the use of large doses of opium, persisted in at short intervals, so as to keep down all nervous excitement, and he has adduced a considerable mass of evidence in support of the success of such practice. The natives have a method of fomenting the abdomen after the operation, which, associated with castor oil, is believed to relieve a condition simulating peritonitis, with nausea, tumefaction, and constipation, which occasionally comes on from twenty-four to thirty-six hours after the operation. "A square piece of cloth of a foot diameter is dipped in oil and placed on the abdomen. Small cloth bags, containing equal parts of potash and common salt heated on an iron plate, and placed on the fire, are made excessively hot, and alternately applied to the oiled cloth, keeping up an incessant steaming heat." In our own country similar though perhaps less energetic means for the same purpose, in the shape of hot bran, oats, or salt in bags, or turpentine fomentations, or hot poultices, are employed, and their efficacy under such circumstances is generally admitted.

Our author seems but little acquainted with Lithotrity, either practically or historically; and considering how successfully he performs cystotomy perhaps it might be as well he should continue so. He says "lithotrity can only be applied to stones of small size, not more than an inch and a half in diameter." How the *lithotriteurs* and *percuteurs* of the present day would smile at the idea that their empire was to be circumscribed within such narrow limit!

In treating of Gonorrhœa, Mr. Brett gives an explanation of the mode in which chordee is produced, which we strongly suspect presents more of novelty than reality: "A very distressing and painful symptom is liable in severe cases to occur, especially during the night, termed chordee. The inflamed membrane is stretched, and great pain is experienced along the course of the urethra. The cause of this is the inflammation having taken place in one or more of the large lacunæ, extending to the cellular tissue and substance of the corpus spongiosum urethræ or the corpus cavernosum; and according as one or other of these structures is affected, so is the penis bent down or towards one side." Now, what proof can he give us that inflamed lacunæ are the cause of chordee? Is it not quite as philosophical to assume that inflammation has extended from any other portion of the mucous membrane to the adjacent structures as from the lacunæ?

Again, he says in an equally off-hand manner, "It has been well ascertained that females not labouring under virulent gonorrhœa themselves are yet capable of imparting the disease, especially to irritable habits." Does Mr. Brett mean that gonorrhœa is not always the consequence of a virus, or does he suppose a virus is not specific in its character? Does he suppose that vaccine virus or pus from any other source placed upon the mucous membrane of the urethra would develope a gonorrhœa virulenta, as he is pleased to term it?

In speaking of one of the consequences of gonorrhœa, Stricture, he says, the most usual seat of stricture is six and a half or seven inches from the external orifice. Now we will undertake to say that not one case in a hundred is seated so far back in the urethra as seven inches or even six and a half from the orifice. We would ask the author how many times he has seen the bulb six inches from the orifice. The curvature is the

ordinary seat of the disease, and in an overwhelming majority of cases the curvature is not more than five inches and a half from the orifice.

Our author, when speaking of Extirpation of the Testicle, says, "should the spermatic cord have retracted into the abdomen it will be necessary for the surgeon to slit up the abdominal ring and pull down the artery by means of a hook." This is a caution commonly given and no doubt a proper one, but we may venture to ask whether he ever knew such an accident to occur? We ourselves happen to know three cases, where, under particular circumstances, the scrotum and testicles have been shaved off with a razor, and where there was not much hemorrhage or any retraction of the cord into the abdomen.

With great respect for Mr. Brett's opinion, we do not think it by any means proved that Cirsocele is most liable to occur on the left side, *because* of the pressure of the sigmoid flexure of the colon, commonly full of fæces, on the spermatic vein at the angle at which it enters the ascending vena cava. We are quite aware that Pouteau threw out the idea long ago, but he only suggested that it might be an occasional cause. Has Mr. Brett found after death the sigmoid flexure full of fæces? Has he distinguished it among the living? Does he know how many young men presented as recruits or conscripts exhibited this condition? Does he think the explanation accounts for the greater frequency of varicosed veins in the left leg than the right?

We should not like to submit our patients to the treatment he suggests for varicose veins of the scrotum and cord:

"In obstinate cases obliteration of the veins may be accomplished by the application of a heated wire; for which purpose the upper part of the tumour should be grasped and made prominent, when a red-hot needle may be pushed through the veins at several points. The patient should be made to stand before the surgeon, who then examines the component parts of the cord; he should then return to the horizontal posture, when the turgescence of the veins will subside, and the vas deferens be distinctly felt; this is to be held by the finger and thumb of the operator's left hand while the patient rises, and again the veins become turgid, which should now be pierced with the cautery at a black heat in one or more points." (p. 299.)

The chapter on Diseases of the Eye is composed principally of selections from Mackenzie, Lawrence, Travers, and Morgan, and contains little, probably, with which our readers are not familiar.

We could have wished that some attempt had been made to clear up the early history of *autoplastic surgery* in India. In connexion with the subject a curious fact is mentioned by our author, which, as he calls it so, we are bound to receive as an established fact:

"An interesting fact, which may be turned to much practical advantage, has been well illustrated in India, viz. that mucous membrane not only becomes integument when everted and exposed to the atmosphere, but that it becomes *black* by the deposition of pigmentum, and that integument when inverted becomes mucous membrane, as is exhibited in the enormous tumours of the scrotum where the extended prepuce becomes mucous membrane." (p. 454.)

That accidental mucous surfaces may result from the modification of cellular and other tissues is an admitted fact in science; still no one maintains that an accidental mucous surface ever acquires the exact organization of proper mucous tissues. As to the conversion of mucous

into proper cutaneous tissues we have never seen anything to prove it, and this not from want of opportunity. We have all seen cases of old prolapsed uterus and rectum; we have seen that the mucous surface has become thick, dry, and coriaceous, but still having little in common with the cutaneous tissues, and certainly we have never in any such cases been conscious of the pigmentary deposit to which our author refers; still the thing may happen in India.

The last chapter treats of the Filaria Medinensis Dracunculus, or as it is commonly known in this country, the Guinea Worm. Although the opportunity of seeing the animal in our own country does not often occur, yet it is occasionally presented in sailors who trade to Guinea and the adjacent coast, and as the subject is one of considerable interest, and our author's opportunities of observing it have been frequent, we will profit by his experience on the subject. He says that worms resembling the filaria are found in the waters of the D,hoon where also the disease is extremely prevalent; he refers to Chisholm and Morehead, who think that the localities in which dracunculus prevails are in districts the rocks of which are of the secondary trap series; but it appears that the D,hoon derives all its waters from the Himalayan Chain, and that so far the secondary trap series have nothing to do with the prevalence of the dracunculus. As evidence against the opinion some time entertained by Mylne and others, that the dracunculus was only a diseased absorbent, he refers to the testimony of many medical officers who had seen the worm exhibit every sign of life after its extrication from the body. Dr. Smyttan has found the same animal attached to the peritoneal covering of the liver and to that of the left kidney, which was writhing alive, and partly floating among the viscera. What our author considers to favour the idea that this worm may be introduced from without and not generated in the system, is that no case of dracunculus occurred in the Governor-General's Body-Guard during the first year's occupation of the valley. All occurred on the second year's cantonment at Deyrah and during the following years on the regiment's return to Calcutta, and that the troopers of the body-guard when quartered at Deyrah obtained their water from a mountain stream particularly reputed for purity.

"It is probable that the young when once imbibed into the system, and when not larger than a minute hair, threads its way through some capillary pore or secreting orifice, and finds its exit into the cellular tissue, where it derives its nourishment and grows, and as it migrates and increases often excites a real diffuse phlegmonous inflammation. A formication commences under the skin accompanied with a superficial cord-like elevation on the surface. A phlysacious vesicle or pustule forms which bursting gives exit at a circular aperture either immediately or after suppurating for a day or two to the head of the worm. These local symptoms are preceded usually by a slight derangement of the system generally. When situated about the fingers or toes, the worm is often productive of much suffering, and is with difficulty got rid of. When deeply seated it sometimes causes considerable fever, great swelling, and tedious abscesses and sinuses, giving out a serous ill-conditioned discharge for months after making its appearance." (p. 479.)

"When the animal can be distinctly felt coiled under the skin, an incision may be made by the side of it, and a portion of the worm seized and gently pulled forth. The natives are exceedingly careful not to wound it, and attribute the severe suppurative inflammation to any broken portion of the worm remaining. . . . . Certain it is that very severe abscesses form after the breaking off a portion of the worm, indeed the natives generally come to us when the mischief has oc-

curred from their taking out the worm at their own homes. A free incision having been made over the worm, it is to be seized and gently brought forth by means of a bent probe. Should the animal's head have presented through the phlysacious pustule it may be carefully pulled forth and turned round a small rolled piece of adhesive plaster. I have succeeded in this manner in rolling off three feet of the worm in the course of an hour. This may be favoured by gentle pressure on the skin over the worm, and by pouring a stream of cold water over the part." (p. 487.)

We regret that our present space does not allow us to make further extracts from Mr. Brett's volume. Although, as we have said, not exactly according to our notions of what such a work should be, it will be highly useful to Indian surgeons, and still more so to European surgeons proceeding to India. No one who reads it can doubt that its author is a most zealous and accomplished surgeon.

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## Art. VIII.

*Mittheilungen aus dem Archiv der Gesellschaft practischer Aerzte zu Riga.* Erste Sammlung.—*Leipzig, Riga, und Mitau,* 1839. 8vo, pp. 202.

*Communications from the Archives of the Society of Physicians practising in Riga.* First Collection.—*Leipsig, Riga, and Mitau,* 1839.

This small volume is one of a class which merits especial attention from reviewers, as it is an indication and proof that the members of the profession where it originates are zealous and intelligent beyond the ordinary standard. It is an easy matter for the medical associations of large cities, with a long list of members, and these only the elect of a body of resident practitioners vastly more numerous still, to collect memoirs and publish transactions; but when the same is done by the physicians and surgeons of towns having only a small population, the slightest consideration justifies the inference above stated respecting the character of the resident medical staff. The population of Riga is under 40,000, and it can boast of no important institutions, especially calculated to foster medical science. The number of medical men is consequently comparatively small; the ordinary members of the society publishing the volume before us amounts only to twenty-five. This society was founded in 1822, and we are happy to learn from the preface that it proceeds in its praiseworthy objects with undiminished vigour. The present collection contains twenty communications of some length, and forty-six miscellaneous notices of less pretensions. Many of these papers are very creditable to their authors, although several of them have become antiquated from the long period that has intervened between their presentation and publication. Others are of inferior value, and scarcely interesting to the public, although doubtless valuable at the time to the members of the society. We shall extract a few of the cases and shorter communications which appear to us the most inviting.

1. *Case of variola after vaccination, following scarlatina.* This case, related by Dr. Prevot, deserves to be recorded as an interesting anomaly, although we confess it might have been rendered more valuable had it described in detail the modifying influence of vaccination on the variolous disease.

The inhabitants of a farm-house, in a village five miles from Riga,

were attacked with variola. The farmer, a young man twenty-two years of age, whose arm presented very distinct traces of vaccination, having every confidence in its prophylactic power, attended on the sick with the utmost assiduity for a period of fourteen days, till he was disabled by an attack of "scarlatina synochalis." The disease ran a favorable course. On the eighteenth day, the farmer being sufficiently recovered to leave his bed, put on a baize great coat which he had worn before his illness, and in which he had visited those in his house who had been attacked with variola. The next day he felt ill, and in the night grew feverish; the symptoms indicative of variolous infection then gradually appeared, and were succeeded on the fifth day by the eruption. The attendant fever was synochal. Recovery took place without any untoward occurrence.

The reflective reader of the preceding case will very naturally put the question, was the protecting influence of vaccination here destroyed by the occurrence of scarlatina, and the system thereby rendered susceptible of the variolous contagion?

2. *Diagnosis of phlebitis.* In a short essay on phlebitis, by Dr. Stracksen, three cases are recorded, which correspond in presenting the following symptoms, and in so prominent a manner as, in the opinion of the writer, to warrant their being considered diagnostic marks of the disease. We give them, since every contribution to the exact pathology of this fearful malady is deserving of notice, and may be useful as a guide in practice.

1. An indescribable restlessness and anxiety, seen in scarcely any other disease to the same extent.

2. Pulsation of the veins, and proportionately violent fever, with very frequent pulse, but softer than in arteritis.

3. Short and painful breathing, very different from that which attends pneumonia.

4. The fatal termination of the disease, often occurring on the fourth or sixth day, from complete disorganization, and consequent "paralysis" (loss of power) of the veins. (p. 174.)

3. *Remarkable case of abortion, by Dr. Bohlschwing.* A robust girl conceived in February, and in consequence menstruation ceased. In June she aborted. To her dismay, soon after, the symptoms of advanced pregnancy appeared, and in the beginning of November, five months after the abortion, she was delivered of a full-grown child, which doubtless was the result of the same impregnation, as the fœtus expelled at the fourth month. (p. 180.)

*Anomalous lactation, by Dr. Hartmann.* An infant whose mother was in good health, and had borne several children, exhibited a healthy appearance for the first five weeks after its birth. The alvine evacuations then became copious, fluid, and discoloured, and the child lost flesh and strength. After the usual remedies had been vainly administered for a fortnight, the mother remarked that the child did not take the right breast willingly, and so much did this unwillingness increase that at length the mere application of the nipple to the infant's lips occasioned loud crying. On examination it was found that the milk of the right breast had a distinctly *salt* taste, whereas the milk of the opposite breast was of the ordinary sweetness. No difference of consistence or colour was discoverable. From that time the child was only allowed to suck the

left breast, and in a few days all diarrhœa and sickliness of appearance vanished. It is not a little singular that while the mammary secretion was thus unnatural, the health of the mother remained unimpaired. (p. 183.)

4. *Case of pain in the head cured by ol. terebinthinæ, by Dr. Mebes.* A single lady aged nineteen, previously healthy, was the subject of nervous fever. In the course of the disease, and particularly during recovery, there arose intense pain in the head, principally in the orbital and temporal regions, coming on suddenly, and as suddenly disappearing, and lasting ordinarily an hour, its longest duration being half a day. The pain subsequently ushered in each menstrual period. The lady married in her twentieth year, and shortly after became pregnant. The symptom which had so tormented her disappeared, but only to return as soon as the milk fever set in, when it continued with uninterrupted severity for several days. The child was now weaned. After leaving the lying-in room the patient suffered fresh and even longer attacks of pain, frequently of from eight to fourteen days' continuance. In eleven years she had borne five living children, and had miscarried twice; during *each pregnancy* the headach totally *ceased*, but no sooner was the lying-in chamber left than the pain returned, and was a constant attendant of menstruation. No relief was afforded by any plan of treatment, perhaps in consequence of the caution with respect to remedies supposed to be required by the frequent recurrence of pregnancy. Antarthritics, narcotics, antispasmodics, calomel to the extent of producing salivation, bark, and sea-bathing had no effect. At length *oil of turpentine* was tried in doses of ten drops, increased to half a teaspoonful every two hours, and continued for four months, and then gradually diminished. After pursuing this plan for some time, the patient entirely recovered. At first, the remedy was highly nauseating, but in large doses it produced an agreeable excitement. Its influence on the urinary secretion was very decided, but less so on the skin. (p. 185.)

5. *Case of consumption cured by ol. asphalti, by Dr. Mebes.* A woman presented the following group of symptoms: constant violent cough, with abundant purulent expectoration; night sweats, and frequent diarrhœa of a year's standing. She had several times expectorated blood, and had lost several grown-up children with hectic symptoms. The disease continued, the purulent expectoration increasing in spite of remedies, especially superacetate of lead. The oleum asphalti depuratum was then commenced, in three-drop doses three times a day, increased by a drop daily up to twenty drops. Manifest amendment took place from the fourth week; the cough and expectoration disappeared, and the strength returned. Goat's milk with Seltzer water completed the patient's recovery.

This case is incomplete, as the evidence from auscultation does not appear, and this is the more to be regretted, since cases of bronchitis simulating pulmonary phthisis often occur in practice, and it is in such instances that medicines of the bituminous class have often displayed their beneficial operation. (p. 186.)

6. *Remarkable formation of pediculi, by Dr. Von Wilpert.* A male infant, three months and a half old, was seized with symptoms of cerebral congestion with convulsions, on the subsidence of which an erratic form of roseola made its appearance. Scarcely had two days passed from

the termination of the exanthema, when the child, who had recovered its good looks and tranquillity, became on a sudden extremely restless, throwing its head backwards, and as constantly moving its arms so as at first to favour the idea that the cerebral disturbance had recurred. On a closer survey, however, the irritation was found to arise from a quantity of small pediculi on the occiput, neck, and shoulders. This discovery was the source of much surprise, in consequence of the remarkable cleanliness of the family; besides, the infant's body had been regularly washed and bathed throughout the disorder, and not the smallest trace of vermin had ever been detected. A slight perspiration was observed on the head for the last day or two, which was followed in two days by this extraordinary parasitic development. The number of pediculi exceeded a hundred; in size, roundness, and whiteness, they resembled the pediculi that occasionally infest the human head as well as brutes. As soon as they were removed the child became quiet, and speedily recovered. (p. 200.)

7. *Case of ventral hernia, by Dr. Von Wilpert.* A woman, aged twenty-five, complained of severe abdominal pain four days after being delivered of her third child. The pain seemed *anterior to* rather than *in* the abdomen, and relief was sought from forcible pressure. A flaccid sacciform tumour was apparent below the umbilicus, and to this spot the pain was referred. Nothing but skin and peritoneum seemed to form the parietes of the tumour, which, the narrator was told, first showed itself after the second labour. The patient herself imagined that the extreme suffering which marked the end of this her third pregnancy, depended on the child being contained in the tumour, as to this point the general abdominal pain and tenderness were observed to converge. The treatment, though active, failed, and on the eighth day death closed the scene. A post-mortem examination disclosed the morbid appearances of peritonitis. The recti muscles were found separated in the linea alba, and pressed backwards so as to allow of a protrusion of peritoneum large enough to contain the gravid uterus and part of the intestines, which after delivery had evidently lodged in the hernial sac. A similar case of ventral hernia, whose contents were the gravid uterus, is recorded by Dr. Dahlhof in Rust's Magazine, Bd. 52. H. 1. (p. 201.)

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## Art. IX.

*An Account of the Yellow Fever which appeared in the City of Galveston, Republic of Texas, in the Autumn of* 1839, *with Cases and Dissections.* By Ashbel Smith, m.d. a.m., Ex-Surgeon-General of the Texian Army.—*Galveston, Texas,* 1839. 8vo, pp. 78.

The intrinsic merit of this little unpretending volume, and the opportunity which it affords us of comparing together yellow fever as it exists in points so remote from each other as Gibraltar* and Texas, invest it with a considerable degree of interest; but a portion of the feeling with which we have contemplated it, is certainly derived from the circumstances of the distant region whence it has been transmitted to us, the

* See our Review of M. Louis' work on the same subject in our Twentieth Number.

young republic of Texas. The possession of this district by its present occupants and rulers was described by Walter Savage Landor, as "the most notable theft of modern times." Dr. Smith's book shows, however, that this theft has that redeeming quality about it with which political thefts alone are sometimes accompanied, that of being the means of diffusing civilization—through the great instruments of such diffusion, the Anglo-Saxon race—to regions which had not previously enjoyed this blessing.

The city of Galveston, the scene of the epidemic, though scarcely two years old, contains above 2,000 inhabitants. It is in lat. 29° 18′ N., and long. 96° 6′ W. of Greenwich. The town extends quite across the island of Galveston on which it is situated, and which at this point varies from a mile and a quarter to two miles. The city is thus washed on its south-eastern border by the gulf of Mexico, whilst its north-western side is washed by Galveston Bay, a broad sheet of water. The harbour is on the side of the island fronting this bay. The heaving of the tide has formed a natural mole (*levée*) along the shore of the harbour of about two feet in height and one hundred in breath, and behind this levée the land is low, being nearly on a level with the water at middle tide, and overflowed in high tides. From this overflowing of the tides and occasional rains there exists at all times either a quagmire or a sheet of shallow water three fourths of a mile long, and varying from one hundred to three hundred feet in breadth, exposed to the rays of an ardent sun, between the mole and the higher land of the island. Immediately contiguous to this morass is the Strand, the principal business street of the town, in which street the disease was first observed.

Of the general salubrity of the island, Dr. Smith speaks in warm terms, regarding it, in this respect, as unsurpassed by any place in the world. It is exempt, he informs us, from the typhus fever of cold climates, and the malignant endemics of the miasmatic regions of the south. The few diseases that occur there are for the most part of a moderately inflammatory character, and readily yield to the simplest treatment. The range of the thermometer is high during the warm season, but the bland breezes from the south which prevail very commonly throughout this period, and are usually strongest at midday, generally prevent the heat from being oppressive.

Between Dr. Smith's description of the yellow fever of Galveston, and that furnished by M. Louis, of the disease in Gibraltar, in 1828, there is the closest resemblance. There were first pains in the limbs, some sickness at stomach, chills not amounting to complete rigor, and some diminution of the sensibility of the extremities. In a variable period ranging from two or three minutes to a few hours, there succeeded pain in the forehead and eyes, pains in the loins, great restlessness, blood-shot eyes, flushed face, hot and dry skin, and full and frequent pulse; whilst the pains in the limbs and sickness of the stomach which were present at first still continued. The tongue was moist and sometimes furred and swollen, but not unfrequently of a healthy aspect. The thirst was often moderate, in some cases considerable, never intense. The epigastrium was slightly sensible on pressure in some cases, in others quite free from pain. The mental operations were generally coherent, but sluggish. There was this to be remarked in the restlessness,

that it did not consist in jactitation, but in a disposition to rise from bed and walk about.

A diminution of pain and febrile excitement very generally takes place, from eight or ten to twenty-four hours after the invasion. If the disease proceed to a favorable termination, this abatement is progressive, and convalescence takes place at a period varying from the third to the fifth or seventh day, or as was observed in one case, to the fifteenth day. Cases terminating thus favorably and speedily correspond as closely as possible with the mild cases observed by Louis in the epidemic of Gibraltar. The symptoms in fatal cases are graphically described, and they will be found to bear to those observed at Gibraltar a resemblance which places the identity of the diseases beyond all question. We regret that we cannot afford space to lay the description before our readers. The author considers pains in the head, eyes, and loins; the characteristic expression of the eyes; vascular excitement and gastric irritability in the first periods; augmented gastric irritability and black vomit near the fatal termination, and yellow suffusion after death as the pathognomonic symptoms of the disease. It sounds somewhat odd to hear a cadaveric appearance classed among these pathognomonic symptoms, but it should be remarked that there are various natural diseases besides yellow fever, in which the appearances after death are very characteristic, spasmodic cholera for example.

So far there is a correspondence between the description of the disease at Gibraltar and that at Texas sufficiently complete to prove the identity of the diseases in external character; indeed the discrepancy is no greater than might have occurred between the sketches by different pencils of the same complex object. The same exact correspondence, however, does not exist between the pathological lesions as portrayed by the respective writers; and a summary of what Dr. Smith observed in his anatomical researches will render this manifest; still, as Dr. Smith seems to have examined only *eight* cases, we are disposed to think the discrepancy between the two observers would have been lessened had the investigations been more extensive.

Other structures, Dr. Smith remarks, experience the fury of the disease, and contribute to the mortality, but the mucous coat of the stomach is the tissue on which the disease *uniformly* and mainly commits its greatest ravages. On pouring off the black vomit, which the stomach in all fatal cases contained, and detaching from the mucous coat the adherent dark-coloured *flocculi*, this tissue was found of a dull pearlish white colour, thickened and softened. In some cases the softening was so great that the villous coat could be scraped in portions almost into a pulp with the finger. The thickening was not uniform but presented in portions *rugæ*, and an uneven surface somewhat like the "unevennesses" of the rind of a lemon. These irregularities in the mucous coat doubtless correspond with the mamellonated appearance described by Louis at Gibraltar. There were a few points and some scattered stelliform particles of bright red; but these points and patches would not, except in a single case, form by their aggregation a surface of an inch square. In two of the cases examined, the whole mucous coat of the stomach presented the white, much-thickened, and softened condition above described; but in four cases, from three fourths to five sixths only of the

mucous coat presented this condition, commencing at the pylorus, and terminating within one or two inches of the cardiac orifice, whilst the remaining portion surrounding the cardia was the seat of a most intense, diffuse, red injection, preserved its usual firmness, was but little if at all thickened, and entirely destitute of flocculi adherent to the surface. This injection did not present pointed or stellated patches, but the blood appeared to be diffused throughout the mucous tissue, and the colour was more or less intense in proportion to the quantity of blood contained in the different parts, and the hue was that between venous and arterial blood. The line of demarcation between the pale or colourless and injected portions of the mucous coat was, for the most part, as well defined by the different thickness of the two portions as by their different colour; the white portion being thickened, whilst the red and engorged part still preserved its normal thickness.

The author's description of the condition of the intestines, or, more properly speaking, of their contents, corresponds very closely with that of Louis, excepting that at Texas the glands of Brunner and Peyer were more frequently and considerably affected than they appear to have been at Gibraltar, for Dr. Smith informs us that they were "sometimes greatly developed," whilst from Louis we learn that in one case only were the patches of Peyer near the *cæcum* slightly tumefied, and with this exception there was no affection of the glands of Brunner and Peyer.

The liver was found in all cases, says Dr. Smith, of its usual dimensions of ordinary firmness, and without any obvious structural derangement. *In three cases it was of a very light drab colour, externally and internally*, and destitute of blood, in one of a dark claret colour and congested with blood, and in the others of its usual appearance, and containing a moderate quantity of blood. In all cases there appeared to be a suspension of the biliary secretion; no bile could be squeezed from the substance of the liver. The author quotes a case, but, as he admits needlessly, to contradict the popular error that the black vomit is a vitiated secretion of this organ. In this case the *ductus communis* contained a little yellow bile, while the stomach was full of turbid black vomit of the deepest dye.

The author's conclusion from his pathological researches is, that two important organs have invariably suffered, the stomach and liver. The mucous coat of the former organ always has presented severe structural derangements, a condition which he regards as the proximate cause of the death of the individual, the *mortis ratio sufficiens;* whilst the latter organ, the liver, has with equal uniformity exhibited undoubted evidences of severe functional derangment, a total suspension of biliary secretion. The difference between his researches into the condition of the stomach, and conclusions from them, and those of Louis, consists in this, that the latter regards "the different lesions of the gastric mucous membrane as secondary or accessory, and that *in cases where they are found*, they were probably developed at a certain period after the commencement of the disease;"* and that whilst Dr. Smith considers these lesions indicated by thickening and softening of the tissue as universal, M. Louis says expressly, that the thickness of the membrane was natural in half the cases;

* Louis on the Yellow Fever of Gibraltar, translated by Dr. Shattuck, (p. 99.)

in others it was thickened; and that its consistence was normal in thirteen cases, in ten it was softened. Then with regard to the liver, though Dr. Smith considers its functional derangement, suspension of biliary secretion, as uniformly existing, he mentions expressly that the anemic condition and light colour of this organ were found in three cases only out of seven: but this condition and colour M. Louis found invariably existing, and finding the liver thus constantly affected, he pronounces it to be the anatomical character pathognomonic of the disease. We must suppose this difference to exist in the things observed not in the observers. Our opinion of Louis has been too often expressed to require repetition here; and Dr. Smith's book contains ample internal evidence that he too is an accurate observer and a faithful reporter.

In the section on Treatment, the author gives a statement, very creditable for its candour, of the difficulties he experienced on the first occurrence of the epidemic; his trial of the remedies, calomel especially, which he had found successful in the more ordinary diseases of the climate; their failure, his disappointment, and difficulty in attaining to a more successful practice. The inspection of three bodies, he tells us, disclosed to him the awful ravages of the disease on the stomach; mercurials had failed to procure stools, and he sought a medicine which should unload the alimentary canal without irritating the gastric membrane: this he finds in an infusion of senna and rhubarb, in which manna is dissolved. His treatment consists in bloodletting employed as soon as the excitement is fairly developed, till slight faintness is produced; hot and strong mustard-baths applied to the feet and legs; the patient is placed in bed, and most sedulously guarded against any current of cold air, which may repel the blood from the surface; and the cathartic already mentioned is administered till its operation is produced. After this no other medicine is required in the stage of excitement, but the hot mustard-bath is repeated twice or three times in the twenty-four hours, as a means of sustaining a continued glow on the surface, whilst a little tamarind-water or sage-tea is given as a beverage.

If after the abatement of excitement the extremities become cool, the hot mustard-bath should be promptly resorted to, and nausea and vomiting occurring should be allayed with black-drop or laudanum. Opiates in this advanced stage are very useful, and have appeared to save some who were rapidly sinking. The author's great reliance, however, is upon bleeding employed early, the mild cathartic, mustard-baths, mild beverages, and a room of moderate temperature and properly ventilated, the patient being at the same time secured from the direct impression of currents of air which may repel the blood from the surface. Under this seemingly judicious treatment, a great majority of Dr. Smith's patients recovered; but as he gives neither the actual number treated nor the ratio of recoveries, we are unable to pronounce whether it was very successful or not.

The question of contagion is treated briefly but forcibly. Yellow fever had been prevailing for some time at New Orleans when it appeared at Galveston, and there is much intercourse between the places. On the other hand, what Dr. Smith considers the local causes of the disease were very abundant about the limited district, the Strand, to which the infection was confined. There were animal and vegetable matters

abounding around the houses, and a marsh exposed to the heat of the sun, when the temperature ranged daily in the shade from 84° to 89°. Patients were removed from the infected district to the healthy sections of the city without communicating the disease to their attendants, or the inmates in their new abode; on the contrary, these sections retained throughout the epidemic, their unsurpassed healthfulness. Of the medical men of the place two only were attacked, and they alone dwelt in the infected district. We consider these points as very important. Dr. Smith performed many dissections, examining everything closely, and immersing his hands freely in black vomit, &c., but with perfect impunity. He *tasted* black vomit repeatedly when fresh ejected from the stomach of the living with equal impunity.

The conclusion the author arrives at seems to be perfectly justified by these premises:

"After a careful observation of the history of the epidemic, no fact has come to light which would show that the disease was contagious, that is, communicable from a person labouring under it to one in health; but that it is contracted only by exposure in the infected district." (p. 33.)

To conclude, we beg to say that this first brief but vigorous specimen of Texian literature has pleased us exceedingly; and that we shall be gratified to observe the future achievements of Dr. Smith and other sons of the young republic in the walks of medical learning, fulfilling the promise of these first fruits.

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## ART. X.

*Observations on the Surgical Pathology and Treatment of Aneurism.* By WILLIAM HENRY PORTER, A.M., Professor of Surgery in the Royal College of Surgeons in Ireland, Surgeon to Meath Hospital, &c. &c. —*Dublin*, 1841. Part I. 8vo, pp. 214.

THE high professional character of the author of this treatise, the extensive opportunities for practical investigations which he is known to possess, and the reputation he had already obtained in the surgical treatment of aneurism, made us hail the publication of his little work with much satisfaction. The subject of it is one of the most difficult and perplexing that engages the practical surgeon; we ourselves have experienced this in common with our brethren; and we cherished the hope that Mr. Porter, as one of the great authorities of the day, might be able to solve many knotty points, and set at rest many questions respecting which, from our own more limited opportunities, we felt unable to come to a satisfactory decision. That Mr. Porter has fully answered all our expectations and wants we can scarcely admit; yet is his work replete with valuable observations, and is characterized throughout by good sense, great discrimination, and sound conclusions. We have been much pleased with the clearness and precision with which the author has stated his own opinions and views, as well as those of others; and have especially admired the decided and reasonable course of practice which he has recommended, in various cases of the most interesting kind to the practical surgeon, whether among those requiring the prompt decision of the moment, or admitting of cool and deliberate investigation. The work

everywhere gives evidence that the author has not followed blindly in the footsteps of others, nor jumped to conclusions on imperfect testimony; and that in framing his code of instructions he has not had reference solely to his library-shelves, but to the combined experience of his predecessors, and that derived from his own ample resources.

Though it must be allowed that in many professional subjects the British practitioner has been fairly eclipsed by his continental contemporary, it may be asserted, with all due respect to the labours of others, that the British surgeon has in a great measure rendered the subject of aneurism peculiarly his own. The scientific principles on which John Hunter's operation is founded, its originality, and its vast importance, have long since been conceded; and the fact cannot be denied that the British surgeon has led the way in all those bold and enterprising operations on the arteries for the cure of aneurisms which were previously, and but too truly, considered fatal and incurable diseases. The femoral arteries, the iliacs, the carotid, the subclavian, the innominata (for we can scarcely look upon Mott but as a British surgeon), and the aorta itself have all been tied for the first time by British surgeons; and though their example has been boldly and promptly followed by their contemporaries in different parts of Europe and America, we have just reason to feel proud of the laurels they have won for British surgery.

Since the year 1785, when Hunter first put his operation into practice, the press in this country has afforded abundant evidence of the great attention paid by our surgeons to the subject of aneurism and its treatment; in proof of which we need merely to repeat the names of Home, Abernethy, Cooper, Ramsden, Travers, Lawrence, Crampton, Hodgson, Norman, &c., and, though last not least, of Jones. The publications of Deschamps, Maunoir, Pelletan, Roux, Dupuytren, Velpeau, and many others, bear ample testimony to the industry and splendid talent bestowed on the same subject by our continental brethren. Of all the works, however, on aneurism, whether published on the continent or in this country, none have become such standard authorities as those of Scarpa and Hodgson. Since the appearance of the last, in 1815, no separate treatise, we believe, from any high authority has appeared, if we except the little volume of Mr. Wardrop (1828), and the more elaborate production of Mr. Guthrie. The former work had no pretensions to treat of aneurisms in general, but was in a great measure confined to the elucidation of the operation of Brasdor or Desault, and the modification which its author himself originated. The excellent treatise of Mr. Guthrie, however, comprised a brief history of all that had been hitherto known or done on the subject.

The profession have already been partly in possession of Mr. Porter's lectures, as they appeared in the Dublin Medical Press; we are happy, however, that he yielded to the request made to him for their separate publication, as they will be thus more extensively useful than they could have been in the pages of a journal. The part now published is only one half of the intended work; its object is thus stated by the author in his preface:

"In the present volume my object is to treat of aneurism in its generic forms (as far as is practicable), independent of the proper and peculiar features that

may attach to it in any particular locality—to describe the different pathological conditions that precede and are supposed to conduce to the formation of the disease—to connect these with the symptoms as they arise—and to point out the mode of treatment apparently suitable to each variety. Another object in which I am deeply interested is, to endeavour to ascertain how far our curative measures may be insufficient and our operations unsuccessful, and to explain (to the extent our pathological information will permit), the causes and circumstances that thus exert an unhappy influence on our practice." (pp. viii-ix.)

In noticing the circumstances likely to conduce to the natural cessation of arterial hemorrhage, our author refers to an experiment of Jones's, which bears on a point of much practical importance hitherto little noticed by writers on the subject. In his sixth experiment Dr. Jones found, "that a teacupful of blood, taken immediately after the division of an artery, coagulated in five minutes and a few seconds. The same quantity, taken a quarter of an hour afterwards, (by which time the animal had lost an immense quantity of blood, and appeared very faint,) coagulated in three minutes and a half. Thus it appears that in proportion to the loss of blood, and the imminence of the danger, is the tendency of that fluid to coagulate increased, and the chief, if not the only measure that is eventually to preserve life furnished when most required." We much fear that any hope, grounded on this experiment, would prove fallacious, for, without at present taking into account the different circumstances under which the blood is placed when out of the body and when circulating in its natural channels, the results of some of the cases of aneurism which we ourselves have had opportunities of observing, lead us to conclude that great depletion is not productive of such advantages in this particular respect, as we might, *à priori*, be led to expect from it; nay, quite the contrary opinion is occasionally entertained, namely, that as the quantity of blood in the body is diminished, it becomes thinner, and hence less coagulable; and some even go so far as to imagine that, under Valsalva's treatment, the fibrin is so lessened in this fluid by the spare diet of the patient, whilst at the same time the heart's action becomes so irritable and increased in rapidity, that actually the opposite results from those expected are obtained. Certain it is that we, in common with most surgeons, have often seen the abstraction of blood in aneurismal cases, whether taken in large quantities occasionally or by small and oft-repeated bleedings, and whether combined with the most rigorously spare diet, or with a reasonable proportion of animal food, equally unsatisfactory in producing coagulation. That the reverse has been the result from time to time we admit, but then we also often see cases where large laminæ of fibrin have been deposited without any interference on the part of the practitioner. Mr. Wardrop, we believe, entertains the opinion, that the smaller the quantity of blood in the body the greater is its disposition to coagulate; but we cannot help thinking that one of his own cases, that of Mrs. Denmark (which however, be it observed, he does not bring forward to illustrate this subject), might be adduced as an example to the contrary.*

The whole of this question, particularly with reference to the treatment of aneurism, is well worthy of further investigation, and we could have wished that Mr. Porter had said more upon the subject.

* Cyclopædia of Practical Surgery, vol i. p. 231.

On the process by which hemorrhage is naturally suppressed and a wounded artery permanently healed, Mr. Porter succinctly states the best established doctrines in language not to be misunderstood; but we wish we could say the same of his statements on the question, whether a wound of an artery ever closes and leaves the canal of the vessel entire. At p. 141 Mr. Porter records his belief that "arteries are wounded in the operation of phlebotomy much more frequently than is imagined, and that many a patient has thus been injured without ever having been aware that he had been exposed to so imminent a peril." We wish much that we had been favoured with the author's grounds of belief, for, in our own experience, we have never met with an example of the kind; we have seen some instances where the evil effects have been of trifling extent, but the patients have always been aware that something unusual had occurred; and, indeed, were we to believe with the author (p. 19), that "a wounded artery never heals unless by obliteration," it appears to us altogether improbable that such a change could go on in a patient's arm, whether spontaneously or under the means employed by the surgeon, without his being conscious of it. In the first of these quotations (p. 141), we are left uninformed as to the condition of the vessel after injury, unless we conclude that the second (p. 19) reveals it; and if it does, the statement is irreconcileable with the impression conveyed at p. 18; for here he observes, that "a case of aneurism at the bend of the arm, and produced by the puncture of a lancet in bleeding, was treated by compression in Stevens's hospital, and recovered, *the artery remaining perfectly pervious through the entire of its course.*" Some doubts seem to have been entertained as to the nature of this case, but the quotations show that our author's views, or at least his statements, are not free from confusion. Our impression as to his opinion is, that he is doubtful if a wounded artery ever heals unless by obliteration; we ourselves are decidedly of opinion that it rarely does, but agree with him when he states that there has not been a sufficiency of evidence collected to decide the question in a satisfactory manner. The question is, however, one more of curiosity than of much practical import; for, in the arm, for example, when the injury is inflicted, the surgeon applies his pads and bandages with the sole object of suppressing hemorrhage, feeling perfectly indifferent whether, in attaining it, the canal of the vessel is obliterated or still left permeable.

The surgical treatment of hemorrhage has been, we think, too hurriedly passed over by our author. Pressure by means of compress and bandage, or by instruments; the ligature, torsion, styptics, the cautery, are all rapidly glanced at; we need scarcely say that the author gives his voice in favour of the ligature; but he is not by any means a bigot, and where circumstances are favorable, recommends a trial of other methods. We have often wondered that, in these days when novelty is so much the fashion, torsion has not been more extensively tried in this country. The practice, however, does not seem to be gaining ground among our hospital surgeons, notwithstanding the favorable reports of it from the continent; and for our own part we must confess our partiality to the ligature.

We wish that Mr. Porter had given something more on the subject of styptics; and we should have been pleased if he had raised his voice against the quackery which, from time to time even in these days,

attempts to palm upon simple or unthinking members of the profession, some specific fluid, "warranted" (as the advertisements say) to restrain the flow of blood from such vessels as the carotid or femoral. We hold it to be the duty of every teacher to warn his pupils against reliance on any alleged specific in such cases; and to give them clearly to understand that no styptic employed to suppress hemorrhage from a large artery has ever yet, from the time of Warner downwards, produced any effect without the aid of pressure. It is worthy of remark that Mr. Porter attributes the efficacy of the cautery, styptics (for of course certain styptics are specific against oozings and small wounds), torsion, and ligature all to pressure; and we cannot question the reasonableness of his views, though we confess that we were in some measure struck with the novelty of the arrangement.

The observations on "pressure by means of compress and bandage, or by mechanical instruments," are so much to the point, and so characteristic of the author, that we cannot refrain from quoting them in full:

"Whatever predilection," says he, "a surgeon may entertain for the ligature, he nevertheless will meet many cases in which he will be disposed to try compression, and some in which he can have no other resource. Thus, his business may be with a patient who has an unconquerable dread of the knife, and who will steadily refuse to submit to those incisions that are almost always requisite to expose a wounded artery, so that it may be tied with safety. Or the wounded vessel may lie deep, or it may be of such size as to encourage a reasonable expectation of the bleeding not proving very profuse; or there may be nerves, or other very important parts liable to be injured; or the accident may have happened at night, and in a situation where it might be impossible to procure light and the assistance necessary to the performance of a very delicate operation. I pass over cases where there may be reason to suspect a diseased condition of the vessel, and the still more dreadful cases of secondary hemorrhage, where the ligature had been already tried and failed. Independent of these, the recent cases are numerous, in which a surgeon is obliged to trust to compression and bandage, and the success that occasionally attends this practice will appear surprising when the difficulties and dangers that accompany it are considered.

"If we reflect on the subject physiologically, it appears necessary, in order to the perfect compression of an artery, that the vessel should be small in size and situated superficially, for if a strong muscle intervenes, its contraction will be sufficient to raise the compress and allow the vessel to bleed internally. The artery should lie upon a bone, so as to afford a counter-resistance to the pressure from without; there should be no accompanying vein, nor indeed any vein in the neighbourhood through which the current of blood could be intercepted by the compress; there should be no accompanying nerve; in a few words, the artery should be small and healthy, superficial, insulated, and resting on a bone, and a very slight anatomical reflection will show how few the arteries are that are so fortunately circumstanced. Even in the most favorable cases, there are still some apparent difficulties. The compress and bandage ought to be applied so tightly as to lay the opposite sides of the wounded vessel fairly in apposition, and in general this occasions such a degree of pain that few patients (once the actual terror of bleeding to death has passed away) will, or even can, endure it. All bandages will stretch and become loose, the patient will shift about and endeavour to alter its position and its bearing, and I have known one who actually took it off. If the bandage is too loose, it may not control the bleeding, or, what is worse, preventing it from appearing without, it may permit the vessel to bleed internally, and in a few hours the surgeon, summoned by the agonizing

distress of his patient, finds the limb swollen, its cellular tissue in every direction injected with blood, and the entire ready to fall into gangrene. If, on the contrary, the bandage is too tight, and particularly in persons of bad habit and debilitated constitution, the pressure necessary to control the bleeding may occasion mortification of all the structures beneath it. Of all the arteries in the body, the temporal has been justly considered to be that which may with most safety be entrusted to pressure, yet even here very calamitous results are by no means improbable. Soon after the application of the bandage, the patient complains of intense pain; and when the dressings are removed, the integuments are found in a state of erisypelatous inflammation, threatening to run into gangrene, the wound opening, and the blood bursting out again; when (as I have unfortunately witnessed,) the compression is reapplied, it only hastens the mortification without at all controlling the bleeding, and nothing that the patient can endure will restrain the hemorrhage, which is profuse, and often periodical, until that condition of constitution is established, known by the appellation of hemorrhagic fever; at last, perhaps, the trunk of the artery is tied at a distance from the slough, and such an operation may succeed, but, in consequence of the freedom of anastomosis among the branches, is far more likely to fail. Thus harassed and broken down, pale and exsanguine from repeated losses of blood, either the patient dies at an early period, or the foundation is laid for dropsy, consumption, or some other disease that will eventually carry him off. This is a sad picture of events, and unfortunately it is sometimes a true one, but not by any means so frequently as a mere theoretic reasoning on the subject might lead us to believe: in practice we find the treatment by compression tolerably successful, and that too in situations and under circumstances that might not always be anticipated.

"When compression is resorted to for the purpose of controlling hemorrhage, a graduated compress is to be placed exactly over the aperture in the bleeding vessel, and secured there by a bandage rolled so evenly, that no one part of the limb is subjected to greater pressure than another, and so firmly as to lay the opposite sides of the vessel fairly in contact, but at the same time occasion as little pain as possible. This is generally accomplished by a comparatively moderate degree of pressure, and I believe the important point to be attended to is the evenness of the bandage rather than the force it exerts. In the arm, for instance, which is frequently the seat of this operation, in consequence of the artery being punctured by an awkward and ignorant phlebotomist, the bandage should commence with the fingers, each of which should be rolled separately and firmly; it must then be carried evenly up the arm, and terminate a short distance above the position of the compress. The arm should then be laid upon a pillow, and the patient receive the strictest injunctions not to attempt to move it. For some hours the case will require the most watchful attention on the part of the surgeon. If there is no increase of pain, or throbbing, or other uneasiness, the compression will probably succeed, and I believe many an individual has had the brachial artery opened without ever entertaining a suspicion that he had been exposed to so much peril: if there is this increase of pain or tension, the arm must be opened and examined lest bleeding should be going on internally." (pp. 21-5.)

Aneurisms have been classified by our author, according to the usual doctrines of the day, under the heads of *true*, *false*, and *mixed; circumscribed*, *diffused*, and *traumatic;* the *aneurismal varix*, the *varicose aneurism*, and a particular form of the disease which has been described by him at greater length in the first volume of the Cyclopædia of Anatomy and Physiology. We are pleased with the general distinction Mr. Porter draws between dilatation of the whole circumference of an artery, and that at one or other side of the vessel, which latter really constitutes what a surgeon of the present day understands to be an aneurism. We are induced to refer to this point, as we occasionally find even now, when

pathology is so much cultivated, that with some of our friends the value of the distinction does not seem to be sufficiently appreciated. A dilatation or enlargement of the caliber of an artery in its whole periphery, even though it should bulge a little on one side, does not constitute aneurism. We have watched such dilatations of the aorta, innominata, and other large vessels for years, and have observed no marked changes in them. On one occasion we were actually in consultation on a case of this kind, in which it was in contemplation to tie the iliac artery. There certainly was a wonderful throbbing in the common femoral, but the vessel on the opposite side was in a similar state, and so were most of the other large arteries. The surgeon's attention had been attracted to one side only, in consequence of an injury to the hip: we watched the case for a very long time, but could perceive no change, nor could we observe that the patient was in the least inconvenienced by this condition of her arteries.

We have often contemplated an attack upon our present nomenclature as regards aneurisms, and wish we could have taken the field in company with Mr. Porter: he seems, however, to prefer the terms as used in the schools of the day; and perhaps, for the sake of his pupils, he has been unwilling to make the subject appear more complicated than it actually is. To some of the names we make no objection; but we hope that Mr. Porter agrees with us, that the terms *true*, *false*, and *mixed*, are most objectionable. In many instances, even at a *post-mortem* examination, they cannot be applied with propriety, and certainly much less so on the living body; even if correctly applied, the distinction is not of the smallest practical utility; the disease in the one case being attended with no particular symptoms; being equally formidable as in the other; and, in all, requiring exactly the same kind of treatment, the result of which is in no shape, unless under very particular circumstances, influenced by the condition of the vessel.

"There is a pathological condition of an artery producing all the symptoms of circumscribed false aneurism, and apparently cureable by the same means, which, as yet, has only been noticed and described by myself. It seems to be formed by a dilatation of the fibrous and cellular coats, and the absorption of the internal lining. It appears to be so far a true aneurism as that the artery is uniformly dilated around its entire circumference, and it is so far a false one, that the internal lining membrane has been removed." (p. 34.)

We are convinced that such examples are occasionally to be met with, and we have some in our own possession which we have been accustomed to refer to, not so much for the purpose of illustrating Mr. Porter's form of the disease, as to show the folly of limiting the component parts of an aneurismal sac to one, two, or three of the tunics. In one instance it is impossible to say whether the serous tunic of an artery lines a sac or not; in another, although we are satisfied that it cannot have stretched so far, we however feel unable to say where it has given way, or whether the middle coat ceases at the same point, extends somewhat further, or even lines the greater part of the sac; then again in some examples there can be no doubt that the sac is mainly if not entirely formed by the outer tunic, whilst in other cases it is impossible to believe that this tunic has still continued to form this part. It suffices to instance aneurisms of the lower part of the thoracic aorta which have made their way to the surface of the body, and which are so

common in our museums. But lest it should be objected that these are internal aneurisms and in many respects different from the cases in which the surgeon takes a therapeutical interest, we shall refer to a preparation in our possession of an aneurismal tumour of the superficial femoral artery, about the size of a large orange, which during life presented, as Mr. Porter would say, all the symptoms of circumscribed aneurism, but whether it was *true*, *false*, or *mixed* we must confess we made no attempt to ascertain, being unacquainted with any diagnostic difference for aneurismal tumours of such a size. After an unsuccessful operation the tumour came under our inspection, and a large part of the circumference of the sac was found to consist of consolidated muscular fibres; portions of the sartorius, vastus internus, &c., so mingled up with cellular texture and newly-deposited lymph, as to give it (the sac) the appearance of an old and thick cyst of an abscess; and what made it impossible that in this case the dilatation of any tunic or of all these could have existed was, that the aperture between the artery and the sac was something less than half an inch in diameter. The parts were much in the condition of some forms of *traumatic* aneurism, yet it had been of spontaneous occurrence.

"When there are two or more idiopathic aneurisms present," (says our author, p. 36,) "it exhibits such evidence of unhealthy condition of the whole system, as to hold out no reasonable expectation of recovery, and confines us to the employment of palliative measures alone." Such a condition should in our opinion demand a careful investigation on the part of the surgeon, but ought not to be looked upon as so utterly irremediable. It is to be hoped that Mr. Porter does not, unless there be positive proof of an internal aneurism, hold out such melancholy prospects to his patients; and we suspect that he must occasionally gainsay his own *dictum*, for we find at p. 118, "in March 1835, a man was admitted into the Meath Hospital with popliteal aneurism in each ham," on whom an operation was sanctioned; and, though the patient died sixteen days afterwards, there is no proof that death was the result of the double aneurism; but on this point it would have been desirable that Mr. Porter, for the sake of his pupils, had not been so dogmatic; for there are numerous cases on record where patients have been cured of double aneurisms by operations. Mr. Guthrie, for example, (p. 61,) refers to a man whom he knew, who survived the operation for popliteal aneurism on both limbs for twenty-five years; and Sir Wm. Newbigging (Edinb. Med. and Surg. Journal for Jan. 1816) cured an inguinal and popliteal aneurism at the same time by tying the external iliac; but we need not multiply such examples, as we trust they are sufficiently known already.

Our author's observations on the alleged causes of aneurism (p. 37 et seq.) are worthy of note for their good sense; he seems to hint at inflammation being occasionally a cause, but if it is, the occurrence, in our opinion, must be rare.

On the symptoms of the disease (p. 53 et seq.) we have nothing new, unless it be that the crooked and stiff condition of a limb may sometimes lead to suspicion. Such a state has been observed by Mr. Porter in the fingers, wrist, and elbow, in axillary aneurism; the toes and foot he has also known affected in popliteal aneurism, but we doubt if such conditions are truly characteristic of the disease. We have at present under our charge a young female with abscess in the ham, with the toes, foot, and

knee crooked and stiff, and have seen many of a similar kind. In bubo of the groin or armpit similar effects are seen, and though we do remember a patient of ours with a popliteal aneurism complaining for the first time to us of pain and stiffness in the ankle, we must say that we place no value on the observation. Mr. Porter is not inclined to consider the *bruit de soufflet* as pathognomonic, and we heartily concur with him; we have lately had under our charge two cases of supposed aneurism, in which the sounds were exactly similar, and on dissection, one proved to be dilatation merely, whilst the other was an aneurism the size of a turkey's egg.

At pp. 63 and 132 some judicious observations will be found on the difficulty of diagnosis in thoracic and in diffused external aneurisms. The stethoscope is very properly recommended, but is shown to be only of limited value in such cases.

Our limits will not permit us to follow Mr. Porter through the remaining portions of his observations on internal aneurism, but we recommend to notice a case supposed to be laryngitis, in which our author cut into the trachea, though with no permanent benefit, as the disease was aneurism of the arch of the aorta. We ourselves have seen something of the same kind in aneurism of the innominata, where the real disease was not even suspected. We cannot but admire the candid manner in which Mr. Porter admits his mistake in this case. Such ingenuousness elevates him in our estimation; were all surgeons equally candid the younger branches of the profession would have less to learn for themselves.

In the section on Treatment there is no affectation of novelty; no views or plans which our author claims as peculiarly his own; yet there is, as usual, much good sense and judicious discrimination; and there are very few points in which we are disposed to differ from the author. Inflammation of the sac he considers a very rare spontaneous cure, and we are of the same opinion; gangrene with sloughing he deems equally rare; pressure by the tumour on any part of the artery above or below he admits may produce obliteration, but, he truly adds (p. 76), "in the majority of cases, we seldom see one of them exemplified." To this list of spontaneous causes he might have added displacement of a mass of fibrin or of the entire lamina, for we are convinced that this must occasionally be a spontaneous cause of cure, that is, a cure without the particular interference of the surgeon; and we could refer to cases illustrative of our opinions which have occurred under our own observation.

Amidst a profusion of valuable practical observations in this part of the work, the following seems to us so interesting that we cannot forego the opportunity of adding to its publicity. In December, 1831, Mr. Porter attempted to pass a ligature round the innominata for the cure of a very large subclavian aneurism, but failed in consequence of the vessel being extensively diseased. The aneurismal tumour entirely disappeared; the patient's health and strength were so completely restored, that he was able to resume his occupation as a day-labourer, and he is alive and well at the present date. Mr. Porter knows not whether to designate this cure as spontaneous or otherwise.

The following passage, we think, ought not to be passed over without comment:

"At first, I imagine the process by which aneurism is cured by the applica-

tion of a ligature at its cardiac side, is not the same in the false aneurism and in the true; at least it is difficult to explain the varieties met with on any other supposition. In the false the great object to be accomplished is, the removal of the impulse of the heart from the blood contained within the sac for a sufficient time to allow of this reservoir becoming slowly and gradually filled with blood, and for that blood to become firm and coagulated. This coagulum, then, is pressed upon or against the wounded and bleeding vessel, and produces those consequences within it that terminate in its obliteration; the blood, if the case proceeds favorably, is afterwards absorbed, and the sac, in process of time, is converted into a solid piece of ligamentous substance similar to that into which the arterial trunk has degenerated. The completion of this process requires different periods of time, according to the size of the vessel, the dimensions of the sac, and the quantity of fluid blood it can contain, but is always accomplished long before the separation of the ligature, and therefore, if there could be any other means devised for removing the impulse of the heart during the required period, a ligature need never be applied for the cure of aneurism." (p. 110.)

We feel assured that Mr. Porter cannot have bestowed much attention to the composition of this passage, else he would never have deviated so far from his usually accurate style, nor allowed to appear under his sanction such a tissue of misapplication of terms, inaccuracies as to facts, and illogical deductions as are comprised in the above quotation. In the first part of it we are told that the process of cure after the application of a ligature is not the same in *false* aneurism and in *true;* now, as these terms are generally understood we know of no shade even of difference; the process as described in reference to the *false* appearing to us as being equally applicable to the *true.* We are much mistaken, however, if in this passage Mr. Porter does not refer to that kind of *false* aneurism termed *traumatic,* in which case "all the symptoms of circumscribed false aneurism," we admit, might be made out, but even allowing the case to be traumatic, if a ligature is applied on the cardiac side of the disease, and the cure proceeds favorably, we cannot see any distinction of importance in the subsequent phenomena. We know not our author's meaning when he talks of the operation giving time for "this reservoir (sac) becoming slowly and gradually filled with blood," for an empty aneurismal sac is what we suppose never exists in the living body: if by "blood" clot or fibrin is meant, then could we make allowances; but the after-statement "and for that blood to become firm and coagulated" removes all doubt of the author's meaning on this point. Again, the assertion that the conversion of the sac and artery into a "solid piece of ligamentous substance is always accomplished long before the separation of the ligature" is, as Mr. Porter must well know, not consistent with fact. Let us refer to what may be considered the progenitor of all such preparations, viz., that in the collection at the College of Surgeons, London, which illustrates Hunter's first case, taken from the person fifteen months after operation; even at this distant period the sac is still distinct, and we have seen similar conditions many years after successful operations. The ligature from a main artery may separate at ten, twenty, or thirty days, and we will not task the patience of our readers by an attempt to prove, what must be plain to all, that the subsequent alterations in an aneurismal sac, and the main artery connected with it, must be the work of a longer period than the few days Mr. Porter's remarks

would lead us to suppose. Perhaps it is meant that the ligature, ere it separates, produces those consequences which lead to obliteration and conversion into a solid ligamentous substance, but it will take some ingenuity to give the passage above quoted this meaning. The process for the completion of the cure, says Mr. Porter, "is always accomplished before the separation of the ligature, and, therefore, if there could be any other means devised for removing the impulse of the heart during the required period, a ligature need never be applied for the cure of the aneurism," a conclusion exactly the reverse of that we should draw; for, had we stated as much for the effect of the ligature, we should have said "and *therefore* if other means could be devised for removing the impulse of the heart during the required period, unless these means were proved to be decidedly superior, the ligature should be preferred to all." We object not to the intended hint that if other and better means could be devised, &c., the surgeon should prefer them, but we doubt if Mr. Porter's deduction from his premises is consistent with the rules of logic.

We make these remarks with most perfect good will towards Mr. Porter; and it is quite foreign from our intention to cavil maliciously at either word or expression; we have stated what we imagine to be the real meaning of the above passage, and should be sorry to find that we have misunderstood it.

It appears, from the work before us, that Irish surgeons have had no experience in Brasdor's or Wardrop's operation; and in consequence, Mr. Porter, with his usual good taste, says little on the subject; he however declares his conviction that such operations should be resorted to in cases deemed favorable, and when Hunter's method cannot be followed out, which is all that its warmest advocates claim for the proceeding. It is highly desirable, in the state in which this question has been left, since the publication of Mr. Wardrop's article on aneurism in the Cyclopædia of Practical Surgery, that some one enjoying opportunities such as those possessed by Mr. Porter, should take a prominent part in giving a just estimate of these proceedings. Mr. Guthrie, in the work to which we have already referred, is the only English author who has bestowed much consideration on the subject; and without adverting to the manner in which he has done it, we shall only say that our opinions do not entirely coincide with his, nor has time proved that he was right in some of his most important conjectures.

We have perused the section on *diffused* aneurism with less satisfaction than some of the others. In the first place we cannot agree with Mr. Porter in some of his distinctions between *diffused* and *traumatic* aneurism; nor can we admit the practical utility of the distinctions drawn between the two kinds of traumatic aneurism which he refers to. If, as is supposed by Mr. Porter in illustration, an artery is ruptured by external violence, though there be no external wound, or is punctured by a sequestrum in necrosis, we see no good reason for refusing to adopt the word *traumatic*. If a person has a compound fracture of the foot or leg, causing gangrene even as high as the thigh, we call it *traumatic* gangrene; all that the term implies being that the gangrene is the result of a wound. We are still less inclined to agree with the following statements:

"An external wound so leading down to, and communicating with, the in-

jured artery as to permit the escape of a portion of the blood from it,..... is the essential condition of traumatic aneurism. Thus, the affection may have been produced by a wound, yet if that wound is healed, or not being healed, if it is very oblique, or if there is any circumstance that prevents the blood from passing out of the limb or part, it falls not under the appellation of traumatic aneurism. It is necessary to be precise on this point, for it *seems* to be a misapplication of the term, the word 'traumatic' being generally used in surgery to imply that the symptom or condition referred to has been the result of external violence; here, however, it is much more limited, and is confined to one particular class or condition of injury. Thus, a student, in bleeding a patient in the arm, may puncture the artery and an aneurism be the consequence, yet if the wound heals, it is not a traumatic aneurism nevertheless, the latter condition implying the existence of a wound through which fluid blood might trickle or a coagulum protrude." (pp. 128-9.)

That such distinctions are not drawn by the professsion we are firmly convinced, and they are too arbitrary to influence us. Pads and bandages may, and indeed often, "prevent the blood from passing out of the limb or part," yet the effusion of blood from the wounded artery is not the less traumatic aneurism, and the mere circumstance of the puncture of the skin in phlebotomy having healed or not makes, in our opinion, no great distinction; the cutaneous wound cannot heal before the lapse of some hours, yet it would be absurd to apply the term traumatic aneurism before union, and immediatety after to say that the case is not traumatic aneurism; and as for the additional distinction that the true traumatic aneurism implies "the existence of a wound through which fluid blood might trickle or a coagulum protrude," we maintain that it is equally useless. In drawing these distinctions, however, Mr. Porter seems to have this practical conclusion in view, that, in what he terms the traumatic aneurism, as the blood may still be conveyed through the wound in the skin by the collateral circulation, even after Hunter's operation has been performed, it is the most correct practice to cut into the swelling, turn out the clots of blood, and secure the bleeding orifices. We cordially agree with him here, but dissent from the practice inculcated in other cases, in which, after judicously recommending a trial of pressure, he seems to advocate the application of the Hunterian operation. Even though the wound may be closed on the surface, the collateral circulation, it appears to us, may still cause an increase of the internal bleeding; and as we are not so deeply impressed with the difficulty and danger (either at the moment or subsequently) of cutting into such collections, as Mr. Porter seems to be, we hesitate not to recommend this proceeding as the practice most likely to be attended with favorable results; and indeed we must express our surprise at our author's apparent reluctance to cut into such collections, when we find him, regardless of the danger from exposure and inflammation, in another part of his work advocating the practice of cutting freely into aneurismal tumours when suppuration has ensued after operation. It is to be observed, however, that these remarks of ours are chiefly applicable in the case of wound during venæsection, the instance most frequently referred to by our author himself.

Among the various causes of failure of operations for aneurism, and "to show what is already but too well known to every one conversant with the history of surgery, that operations, even when well planned and well executed, sometimes fail," Mr. Porter has brought forward a number

of interesting cases and facts in illustration of his views. He enumerates many of the usual and acknowledged obstacles to a successful result; but we need not go over ground already sufficiently well known. We cannot help, however, referring the reader to some singular observations on what Mr. Porter terms "reflex action of arteries."

Perhaps the best section in the volume is that on Secondary Hemorrhage, and we recommend it most particularly to our readers. Although we do not even here accord with the author in every point, we have been struck with the soundness of the principles everywhere conspicuous; and we hardly know which to admire most, his valuable doctrines or the modesty with which he propounds them.

The author first takes some notice of the causes considered likely to induce the frightful result of secondary bleeding, and then states what he considers best to be done under the circumstances. He clearly proves that neither considerable disturbance and denudation, nor the application of a ligature near to a collateral branch are of necessity followed by this result, but, notwithstanding, he duly cautions the surgeon against negligence on these points; he seems to imagine that he can foretell the coming on of hemorrhage by certain symptoms (p. 179), and he asserts that on all occasions the blood flows from the distal end of the artery. We give him credit for referring to this last point, as we are convinced that until within these few years the reverse was generally supposed, yet we cannot go the whole length with him; for if this were "always" the case, it would be difficult to account for secondary bleeding after amputation. He ingeniously endeavours to prove the existence of two kinds of inflammation in an artery near the seat of deligation, *healthy* and *unhealthy*, the former being immediately above the ligature, the latter below; and hence he ascribes the frequency of secondary hemorrhage at this end of the vessel. But unfortunately the only case he brings forward in proof is one in which secondary bleeding did not occur. We are not therefore convinced of the truth of this singular view given by Mr. Porter; and from a most careful examination of the artery, in several cases which have happened in our own practice, we must say that if such remarkable differences existed they were overlooked by us. From the consideration of these cases, however, and others that we have seen, we entirely concur in Mr. Porter's general assertion that unhealthy inflammation is the most frequent, perhaps the only cause of secondary hemorrhage; taking this latter term in the sense usually understood by surgeons, and meaning by the term unhealthy inflammation, merely what Mr. Porter means, viz. that nature shows little or no disposition to deposit and organize the quantity of coagulable lymph, which seems requisite to render the obstruction of the artery complete and permanent.

He next proceeds to examine the resources possessed by art, if any, "when the blood has burst forth." He objects decidedly to the plan of placing a ligature higher up on the artery, that is, as we understand him, at some distance from the original seat of operation.

"I will not dwell for a moment on the unreasonable nature of such practice, as long as there is a collateral circulation to convey blood to the open vessel; but merely enquire if the superior segment is closed and blocked up with lymph, or if, as I have before noticed, the ligature still remains upon it, whilst the lower had separated and was bleeding, of what possible utility could the tying of the artery higher up prove? It could only remove the impulse of the heart

from the superior section, from which no danger is to be apprehended, but could have no influence on the lower, already deprived of that impulse, and supplied only by the collateral branches. If the operation, then, be useless, that would alone be a sufficient argument against subjecting an unfortunate patient to additional suffering and misery; but it is decidedly and positively injurious, and constantly aggravates his condition. I say 'constantly,' because, although a few solitary patients may have recovered after, or, more correctly, in despite of the operation, yet the numbers are so small as scarcely to constitute an exception to a general rule." (p. 191.)

We cannot here help thinking that Mr. Porter's judgment is warped by the prevailing idea that hemorrhage occurs *only* at the lower orifice; we have already given our opinion on this subject, but cannot avoid again reverting to the case of secondary bleeding after amputation. In the leg, for example, ligature of the femoral seldom (if it ever) fails to arrest the bleeding from the stump below the knee; and this is no imaginary case of collateral circulation, for we know that such vessels positively exist.

After some further remarks on the operation, on the necessity for pressure as an additional security, and on the attendant danger of gangrene, Mr. Porter proceeds to describe the treatment he recommends:

"On the appearance of the blood, the wound should be opened up, the granulations broken, and the mouth of the bleeding vessel fairly exposed; on it a small piece of prepared sponge should be placed deep in the bottom of the wound, care being taken that the pressure should be confined to this one spot; on this a small compress of lint, and another and another successively, until the graduated compress shall have attained a level higher than the adjacent surface of the limb. This must be firmly retained without being permitted to slip, and without allowing the escape of a single drop of blood; and, if this can be accomplished for three or four days, there is every reasonable probability of success. But here is the great difficulty. It is scarcely possible to apply a bandage, so as to answer the purpose, without interfering with the venous circulation and occasioning great pain; and as all bandages stretch and slip, and become relaxed, no certain reliance can be placed on them in a case where the patient's feelings encourage him to disturb them." (p. 194.)

Were the case ours, and did we resort to such bold practice as cutting down to the bleeding vessel, we should feel most inclined to place a ligature on each end of the vessel: we should, for the time, place more reliance on the thread than on pressure; there would be less immediate danger of gangrene, and we suspect as much ultimate chance of a permanent closure, as by pressure in the way recommended above. To be sure, it may be said, that as the ligature has failed at this part once already, why try it again? The case, we admit, is a most difficult one, but from what we have been able to learn on this subject, and what we have seen in our own practice, should we ever again be so unfortunate as to have similar cases to manage, we have made up our minds to try this proceeding. Pressure, amputation, or ligature higher up may afterwards be thought of, in the event of this method not succeeding. Of course, in some vessels, such as those at the root of the neck, no other plan than pressure can be followed.

But our limits will not permit us to follow Mr. Porter further at present; and we shall therefore conclude by thanking him for the information he has afforded us, and earnestly recommending his work to our readers. Mr. Porter has everywhere evinced a thorough mastership of his subject, and

has stated his observations, his summing-up of the opinions of others or of his own, in a concise, unaffected, and generally clear manner. His style is not altogether free from faults; but for this the value of the materials makes ample atonement.

Mr. Porter has promised us a Second Part, and we confidently hope that he will not, like his distinguished contemporary, Mr. Colles, in the case of his Surgical Anatomy, give us reason for disappointment and regret that a work so well begun has not been completed. It is to be hoped, also, that ere long Mr. Hodgson will favour the profession with a new edition of his treatise; and should both works appear about the same time, we have laid out for ourselves a most agreeable and instructive task, in again bringing under the notice of our readers the pathology and treatment of aneurism.

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## Art. XI.

*Leçons Orales de Clinique Chirurgicale faites à l'Hôpital de la Charité, par M. le Professeur Velpeau.* Recueillés et publiées par le Docteur P. Pavillon.—*Paris*, 1840. 8vo, pp. 565.

*Clinical Lectures on Surgery delivered at the Hospital of la Charité, by Professor Velpeau.* Collected and published by Dr. P. Pavillon. —*Paris*, 1840.

The success that attended the publication of the Lectures of M. Dupuytren has induced other students to communicate to the public the clinical remarks of their professors also. The present volume is an instance. The extensive professional erudition, the accurate powers of observation, and the vast opportunities M. Velpeau possesses, render any remarks made by him interesting to the profession; and we have been much gratified by the perusal of this volume, though occasionally inclined to smile at the little pompous preface with which many of the different articles commence, denouncing the inadequacy of previous knowledge and heralding the additional light the professor is about to throw on the subject. We must confess, however, that the author has in some of these made good his premises.

The articles in the present volume on the subject of diseases of the eyes occupy more than one half of its pages; but as we have so fully commented on this department of surgery in the late numbers of our Journal, we omit any notice of them here, and confine our attention to the other principal subjects treated of. From the different articles, often unnecessarily prolix, we shall endeavour to glean and lay before our readers the chief original or peculiar views of the professor.

*Hydrocele.* In treating this subject M. Velpeau divides hydrocele into acute and chronic. The acute form, "which is little spoken of by authors though of sufficiently frequent occurrence," is caused by the same accidents that produce orchitis, of which it is almost a constant accompaniment, more especially of that variety of orchitis arising from discharges from the urethra. M. Velpeau combats the opinion of M. Rochoux, who affirms that acute hydrocele forms almost the entire swelling and disease in "gonorrhœal orchitis." M. Velpeau affirms that usually only a fourth

or sixth of the bulk of the tumour is caused by the effusion into the tunica vaginalis, and he considers that the soft spongy feel that the enlarged testicle communicates has been mistaken for the effused fluid. He gives rules for distinguishing the two varieties; but as we cannot commend them for clearness or precision, we shall pass them over. The treatment of acute hydrocele is the same as that for common orchitis, with addition of the evacuation of the fluid in the former, which M. Velpeau occasionally employs. It is chiefly on this account that it is important to detect the complication of this acute hydrocele in orchitis, the symptoms being much relieved by puncture of the tumour. M. Velpeau at first performed this operation merely with the view of detecting the presence of fluid; but finding always where fluid, in however small a quantity, escaped, that the patient was relieved, he adopted the puncture as a regular mode of practice in such cases, and with great benefit. The puncture is made with the point of the lancet, gives immediate relief, and accelerates the cure where only the smallest quantity of fluid (or even it is said none at all) escapes.

M. Velpeau considers at great length the causes and treatment of chronic hydrocele. He has observed in many cases small cysts developed in the tunica albuginea or on the surface of the testicle or epididymis. Sir B. Brodie had already described a species of encysted hydrocele developed between the visceral layer of the tunica vaginalis and the tunica albuginea. The observation of M. Velpeau confirms that of Sir B. Brodie, the only difference between them being that the cyst observed by the latter was large and single, and those by M. Velpeau small and more numerous.

For the radical cure of hydrocele, M. Velpeau considers none of the numerous methods that have been used by surgeons so fitting as that of injection of the cavity. He expresses his surprise that the surgeons of London, Berlin, and America should still perform dangerous, long, and bloody operations, instead of the one he is in the habit of using. We think that since the publication of Pott's excellent treatise on this disease few surgeons in this country have deserved this reproach of M. Velpeau. But he himself recommends incision of the sac, in cases where the contained fluid is thick, or where the coats of the tunica vaginalis are so hard, thick, and solid that injection of the sac will fail to cure the disease; and in cases where the sac has degenerated into a stony or osteo-calcareous shell, or into lardaceous matter, he recommends excision. In the radical cure of hydrocele M. Velpeau has been in the habit of using an injection of tincture of iodine instead of wine or other stimulants, and he considers it to have the following advantages: a small quantity only is required, half an ounce to an ounce; it produces no ill consequences even if allowed to remain in the sac; it does not cause any serious injury if injected into the cellular tissues; and lastly it is almost uniformly successful. The cure is also more prompt, being effected in eight, ten, or twenty days. If there be swelling of the testicle, this also is frequently cured by the injection; and in cases where the swelling appears uninfluenced by the operation, M. Velpeau has found that at the end of five or six months it has entirely disappeared. The proportion of iodine used by the professor is two or three drachms of the tincture to an ounce of water; he has even used the pure tincture with-

out any bad consequences: he considers that the reason why M. Fricke of Hamburg has failed in his operations, is that he has not made use of a sufficiently strong injection. M. Velpeau, reasoning from the relief afforded to the cases of acute hydrocele by puncture of the tunica vaginalis, has employed this means also after injection with the iodine solution, and has found that the tumour has been thereby much more rapidly discussed. One of the most eminent hospital surgeons of London has for some time been in the habit of using the iodine solution in the radical cure of hydrocele, and he has the same opinions of its efficacy as M. Velpeau. It is necessary to prepare the solution of iodine with the hydriodate of potass. In congenital hydrocele M. Velpeau recommends the iodine injection, while at the same time pressure is made on the inguinal canal.

*Dislocations of the Shoulder.* Injuries of this kind have been considered for some time a subject of surgery perfectly well understood; but M. Velpeau maintains that many of the views already set forth are either erroneous or incomplete. Considering it impossible for a dislocation to take place downwards on the inferior costa of the scapula, he divides the luxations of the shoulder into those which take place in a direction anterior and internal, or posterior and external to the glenoid cavity.

The posterior and external dislocations comprise complete dislocations on the infra-spinous fossa of the scapula (of which too many cases are now recorded to leave any doubt of their occurrence), and incomplete dislocations in the same direction. This species of luxation has not before been described by any writer on surgery. M. Velpeau instances three cases which he has met with in practice in which he considers this dislocation had occurred. He defines incomplete dislocations to be those in which "the humerus is arrested by some point of its anatomical neck on the border of the glenoid cavity.....If by incomplete dislocations," he adds, "we understand the cartilaginous surface of the head of the humerus to have only half escaped from the glenoid cavity, there is no doubt that we cannot admit the existence of such cases." The following are the symptoms, as described by the professor, of an external and posterior incomplete dislocation: "The arm half an inch longer than that of the opposite side, fixed immoveably by the side of the chest; the elbow directed forwards and inwards; the head of the humerus causing no prominence under the coracoid process, but projecting about six to eight lines on the posterior or subspinous border of the acromion; the axilla free, and the pectoralis major as well as the internal portion of the deltoid much depressed." In the case in which these symptoms were recorded, the dislocation had existed for ten or eleven months, and it was afterwards reduced by M. Sedillot, who considered the luxation a complete one. M. Velpeau gives the following reasons for believing the dislocation incomplete:

"If the dislocation had been complete, the head of the humerus would have been found more than an inch behind the summit of the acromion, but it was only a few lines; the glenoid cavity would have been completely empty, but it was so slightly so that it was easy to confound (in a certain degree) the depression of the deltoid with that which results from a fracture of the surgical neck of the humerus; the elbow would have been brought forwards as far as the costal cartilages, and it was only slightly inclined inwards and forwards; lastly,

the fossa infra-spinalis would have been almost entirely occupied by the tumour, on the anterior border of which it, in a measure only, rested."

In a recent case of this accident the same symptoms were observed: by fixing the scapula, raising the arm, and making slight extension, the humerus resumed its proper situation without any difficulty. M. Velpeau concludes that the extreme facility of reduction is an unanswerable argument in favour of the incompleteness of the dislocation. In the third case, a dissection, of what M. Velpeau regards as an incomplete dislocation backwards, was made. The patient injured his shoulder twenty-one years previously to the examination. The author gives the following description of the appearance of the new articulation:

"All the muscles were of large size [the patient was a muscular man], without any indications of either ancient or recent lesions. ..... The head of the humerus occupied all the glenoid cavity, the neck of the scapula, and a small portion of the infra-spinous fossa: it was very much flattened, narrow and thin externally, enlarged internally, and articulated in an almost immoveable manner with the scapula by a capsule, which could only be so called at the internal part of the articulation, where it covered a considerable thickness of the osseous head, for below and externally it was inserted almost at the point of contact between the two bones.....On dividing the capsular ligament the articular surfaces presented the following appearance: The new articular surface of the scapula was much larger than natural, it was formed at the expense of the glenoid cavity and of all the neck of the scapula as far as beneath the coracoid process; hence a complete disappearance of the glenoid prominence was the result. A change in direction indicated very distinctly the situation where this cavity might be divided into two portions; one external, the old normal articular cavity, still a little concave, much lengthened and united by a smooth surface but a little less prominent to the second; the other internal, which was directed to the subscapular fossa, where it terminated by an osseous border. ..... The head of the humerus was not less altered, and instead of presenting a sphere, it was strongly channelled in its centre from above downwards by a deep fissure which divided it into two portions, one external, thin, but rather broad, irregularly convex, corresponding to the old glenoid cavity; the other internal, almost smooth, very thick, in contact with the prolongation of the new articular surface of the scapula."

We have given the description of this dissection in the words of the author, as he believes it confirms incontestably his views on the nature of these dislocations; but so far from agreeing with M. Velpeau we do not understand how this case bears at all on the point in question; it appears to us to be evidently a dissection of a dislocation (probably incomplete) in a direction internal to the glenoid cavity, and must either have been introduced here by mistake, or the palpable absurdity of supporting opinions with such so-called facts would induce us to regard with considerable suspicion the assertions of the author. We do not mean to deny that a partial dislocation in a direction external to the glenoid cavity may take place, but these cases do not appear to us sufficient evidence of its occurrence; and before admitting the existence of such an accident we should wish to hear of its confirmation by other surgeons.

*External and anterior axillary Dislocations.* These form the second class, and M. Velpeau admits these varieties of them: First, sub-pectoral dislocations, where the head of the humerus is lodged between the pectoral and subscapular muscles, the axillary dislocation of authors; secondly, subscapular dislocation, in which the head of the bone rests

in the subscapular fossa, but is separated from the hollow of the axilla by the subscapular muscle; and thirdly, subclavicular dislocation, "where the head of the humerus rises towards the root of the coracoid process or the clavicle, and is constricted, as it were, beneath by the superior border of the subscapular muscle."

There is nothing worthy of notice in the remarks of M. Velpeau on axillary or subclavicular dislocations; he considers the former most easily reduced by traction upwards in the direction of the axis of the body, and the latter by the force being directed downwards and inwards.

The subscapular dislocations of M. Velpeau, where the head of the bone is separated from the hollow of the axilla by the subscapular muscle, do not seem to differ, except in name, from those described by Sir Astley Cooper as incomplete dislocations inwards under the coracoid process. M. Velpeau finds these luxations are most easily reduced by the knee in the axilla, the humerus being used as a lever, or else by traction in the horizontal position. The following are the symptoms of these subscapular dislocations as described by the author:

"The pectoralis major is slightly pushed forwards or raised, the hollow of the axilla is nearly effaced, the acromion less prominent, and the deltoid less stretched than in the natural state; the head of the humerus, separated from the skin of the axilla by a thick layer of tissues, is felt with difficulty through the pectoralis major; the fingers can be passed between it and the internal wall of the thorax; the movements of the arm are very limited, and produce a kind of crepitation; the elbow is separated from the thorax and carried forwards; the arm a few lines lengthened, shortened, or of the same length as that of the opposite side."

Crepitation is regarded as the most valuable diagnostic sign of this species of dislocation.

The author enters at great length on the question of the relative frequency of shortening and lengthening of the arm in dislocations; numerous cases are recorded to prove that either symptom may be present, and various methods of measurement are mentioned. M. Velpeau arrives at the conclusion that the arm is more generally lengthened than shortened in luxations of the shoulder; and in cases where shortening occurs the dislocation is usually under the clavicle.

*Varix.* M. Velpeau reviews in detail the different methods that have been used in the treatment of varix, and discards them all in favour of the following operation, which is a modification of that first employed by M. Davat, and which he prefers to all others. M. Velpeau passes a pin, similar to those used in the operation for hare-lip, underneath the varicose vein; around the ends of the pin, which protrude from the integument, a piece of strong waxed silk is firmly twisted, which strangulates that portion of the vein underneath which this pin is placed and the integument above it; the pin is allowed to remain for eight or twelve days, until an eschar is produced. Several of these pins are placed in the course of the varicose vein. This method of treatment must be familiar to many of our readers, as it has been practised in the hospitals of London and elsewhere. We have had opportunities of witnessing the results of many cases treated in this way, and we must confess we consider it far superior to any of the other numerous methods by which it has been attempted to obliterate varicose veins. M. Velpeau operated on a hun-

dred cases without the occurrence of any serious accident; slight erythema and abscess were occasionally the result, and in only one case has this operation in his hands been followed by phlebitis and death. We quite agree with the author in his remark, that no operation will effectually cure varicose veins: the blood is directed into another channel, the smaller veins are distended and become equally distressing to the patient as those which previously existed. There is the same necessity for the support of a bandage or laced stocking after as before the operation. When the pins are allowed to remain under the vein for more than seven or eight days, an ulcer difficult to heal is the result. M. Velpeau aims at producing this, and considers that the vein will not be effectually obliterated without its occurrence. In a great number of cases where we have had an opportunity of observing the effect of this operation, the pins were removed at the end of five days, no ulcer was produced, and the cure was no less effectual. In this chapter a long history of the different operations for this disease will fully inform the reader of their nature and relative merit.

In cases of *varicocele* M. Velpeau recommends the same treatment by the application of the pins and ligature as in common varix. In this operation it is of course very necessary to separate the vas deferens from the veins, and this is best done by seizing the scrotum at its root, separating the vas deferens, which is easily distinguished by its greater hardness, pulling it inwards, and guarding it at the same time with the finger and thumb before passing the needles under the veins. The needles should be applied about the middle of the scrotum, and two are required with an interval of about an inch between them. We have seen the application of this mode of treatment in varicocele, and can recommend it as being equally efficacious and much less severe than many of the operations that are practised for the relief of this distressing complaint.

*The introduction of air into the veins* during surgical operations, though known to all practitioners as occasionally producing most serious consequences, has, from its infrequency, received but little attention from surgical authors, and we have to thank M. Velpeau for having collected together all the known facts on this subject in the present volume. He considers separately what experiments on living animals have taught us; what facts we can collect from observations on the living human subject during the last twenty years, and the value of therapeutical means proposed in similar cases by physiologists and surgeons. It was proved by Bichat and Nysten, that air introduced into the veins in sufficient quantities could cause death. By succeeding experimenters, Barry, Magendie, and Poiseuille, it was found that air would enter of itself in sufficient quantity to produce death in some veins which were opened by any incision; and Poiseuille, in continuing his researches on this subject, discovered that the veins in which this accident could occur were those in which a venous pulsation existed, namely, the subclavians, internal jugulars, and axillary veins. The explanation of this circumstance was given afterwards by M. Berard, who showed that the veins at the root of the neck were so united by cellular and fibrous tissue to the bones in the neighbourhood, that they were prevented collapsing when an opening was made in them, and which remained gaping in the same way as an incision in a perfectly inactive canal would do. M. Velpeau considers

that this capability for the introduction of air exists in the internal jugular vein as high up as the larynx. This is not the case in every instance, as we have seen a patient in whom the internal jugular was most freely opened between the thyroid and cricoid cartilages, in his attempts to commit suicide, and yet there were no symptoms to indicate the introduction of any air into the circulation. From the experiments of M. Poiseuille, the introduction of air into the veins of animals is accompanied by a dull sound, though sometimes, especially in horses, by a kind of gurgling. Death usually takes place in from five to fifty minutes, but not instantaneously. The post-mortem appearances exhibit distension of the right auricle and ventricle of the heart, and these cavities contain generally red blood mixed with a great quantity of air, a bloody froth (mousse sanguine). In some cases the same appearances have been found on the left side of the heart, and air has been discovered in the vessels of the brain. M. Velpeau has collected with great industry all the cases that have been hitherto recorded of this accident, about thirty in number. Of those in which death took place a post-mortem examination was only made in seven instances. In these, a quantity of air dilated the right auricle of the heart in some cases, in others the veins only, in one the bloody froth formed in the experiments on animals was present. The sound which accompanies the introduction of air into the veins of man is usually a kind of whistling; and death, when it does take place, is generally instantaneous. From this difference in the symptoms that have been observed between these accidents as occurring in the human subject and produced by experiments on animals, M. Velpeau seems to doubt whether we can with propriety conclude that in these cases death was absolutely produced by the introduction of air into the veins. In our opinion he lays too much stress on the necessary accordance between the result of the experiments on animals and the accident in the human subject; for, though in some of these cases where death took place, the veins opened were the external jugular, the veins of the shoulder, and the subscapular, (which in the experiments recorded were not found capable of admitting air,) yet the symptoms and instantaneous death of the patient could only be accounted for by the occurrence of this accident, and we think the perusal of the cases must satisfy every one that it had occurred. We should be inclined to arrive at a different conclusion from that of M. Velpeau, namely, that air may be taken into the circulation by many smaller veins than was supposed by the experimenters on animals.

The symptoms of this unfortunate accident, when air has been taken into the circulation in large quantities, are, a whistling or gurgling sound, a feeling of faintness, occasionally universal trembling, syncope, and death. When a smaller quantity of air has entered the veins, a feeling of faintness or even perfect syncope may take place, and the patient afterwards recover. Under the head of treatment, M. Velpeau discusses the question, whether preventive measures should be taken in operations at the base of the neck, where wounds of the veins in this situation might occur, these preventive measures consisting of compression of the chest or of the veins between the wound and the heart; and he decides that both these precautions are unnecessary or useless, and that the only resource that remains to the surgeon in these cases is: "1st, to do everything to avoid wounding in operations the internal jugular or subclavian

veins; 2d, in cases where it is necessary to penetrate to the neighbourhood of these vessels not to separate the pedicle of the tumour without having compressed it on the side of the heart between the fingers, or surrounded it with a strong ligature; 3, to avoid as much as possible, stretching, pulling, or separating the tissues, elevating the arm or pulling backwards the shoulder or neck when the bistoury approaches the large veins at the summit of the chest." The curative means, or those by which the air may be expelled from the veins of the chest when it has once entered, are very ineffectual. Compression of the chest is perfectly useless, and can have no effect in expelling the air in the human subject, and we have no known remedies by which this accident may be combated.

*Gonorrhœa.* In the treatment of this disease M. Velpeau has tried the effect of iodine, balsam of storax, and gunpowder, each of which remedies has been recommended by some surgeons, and he finds these therapeutical agents useless. He discusses the respective merits of copaiba and cubebs. Patients do not suffer so much from the exhibition of the latter as of the former, and the tone of the stomach is sometimes to a certain degree improved by the cubebs; but the combination of these two medicines in the proportion of one part of copaiba to two of cubebs, produce the most beneficial results. This form of medicine has been recommended by Mr. Liston in the last edition of the Elements of Surgery. M. Velpeau believes that the rheumatism, which has been said to be caused by copaiba is more probably induced by the disease for which it is administered. The rash produced by this medicine is of no consequence. Besides these specific remedies, M. Velpeau recommends in acute cases general bloodletting and leeches to the perineum; and where there is much thickening of the urethra, he has found friction with mercurial ointment of great benefit. The application of small blisters from the anus to the scrotum, as recommended by Mr. Poirson, has been occasionally useful in cases of old standing. In the local treatment of this disease M. Velpeau considers the objection to injections absurd; the abuse only and not the use of these remedies is to be condemned. Sulphate of zinc is, in the opinion of the professor, of no use in recent cases of discharge from the urethra. In gonorrhœa of old standing or in gleets, in very small doses its effects are beneficial; one grain to an ounce of water is the proper proportion. Nitrate of mercury has also been recommended, but is far inferior to nitrate of silver, combined with compression of the urethra. The injection should contain one grain of the salt in every ounce of water, and is used at the same time that the cubebs and copaiba are exhibited internally in the early stages of gonorrhœa. The injection should not be continued longer than three days (about three times a day), lest it produce a fresh irritation in the urethra. This canal should be kept constantly compressed by means of small graduated compresses, dipped in starch, applied from the bulb to the fossa navicularis, and retained by means of a bandage. M. Velpeau's plan of treatment is in all respects one which we should be inclined to recommend, and which we have for some time been in the habit of using, with the exception of the compression of the urethra, of which we have had no experience.

*Artificial anus.* The researches of Dupuytren and his subsequent excellent explanation of the causes, pathology, and treatment of artificial

anus would lead us to suppose that this subject would afford but a barren field for fresh research.

"Yet," says M. Velpeau, "in this, as in many other instances, surgery is far from having arrived at perfection. The adherence of the intestine to the neck of the sac is often less intimate than Scarpa supposed. The membranous investment is often wanting. The adhesive inflammation is not always set up on the application of the *enterotome*, and sometimes it is almost impossible not to embrace between the teeth of this instrument a portion of some important organ at the same time with the spur-like process; lastly, artificial anus and fecal fistulæ persist for an indefinite period in spite of the destruction or absence of any spur-like process between the ends of the intestine."

M. Velpeau instances cases of artificial anus in which post-mortem examinations have made good these assertions. In the first case the patient died twelve days after the operation for strangulated hernia. The intestine was found so little adherent to the neck of the sac that the slightest force was sufficient to separate them, and any artificial interference would have been followed most probably by effusion into the abdomen. In the second case the enterotome had been applied, but very little adhesive inflammation had followed its application not sufficient to prevent effusion of fecal matter into the cavity of the abdomen, which was the cause of death. In the third case the infundibuliform space between the two ends of intestine was occupied by a coil of intestine so that the application of the enterotome would inevitably have embraced and opened this portion of bowel as well as the spur-like process itself. M. Velpeau describes these as the chief difficulties the surgeon will have to contend with in the treatment of artificial anus, and they explain the non-success that is met with in many of these cases: moreover fecal fistulæ will persist after the destruction of the spur-like process, or when no such partition between the ends of the intestine ever existed. This may take place in two ways:

"1st. An ulcer commencing in the mucous membrane may cause adhesion of the intestine to the corresponding part of the abdomen; continuing its progress in depth, and having traversed all the tissues this ulcer will cause a fecal abscess which may terminate in a fistula. 2d. If after the reduction of a hernia an opening forms on the convexity of the intestine, the contents may pass out towards the skin and an artificial anus be the consequence. 3d. After the destruction of the spur-like process by the method of Dupuytren, the fecal fistula may resist all attempts to close it."

These observations are supported by numerous cases and dissections, which are too long for insertion here. The treatment of these obstinate cases of fecal fistula demands the best attention and patience of the surgeon. Pressure, styptics, and cauterization will be sufficient to effect a cure in some, but will too often fail. Autoplastic operations have been attempted, but with little success. After a fistula has existed some time the edges become perfectly callous, and the structures in the immediate neighbourhood so changed as to be little inclined to contract adhesions with the new tissue that is placed in contact with them in autoplastic operations. It is also almost impossible to close the opening so effectually as to prevent the escape of a small quantity of fecal matter, which will be quite sufficient to prevent the union of the raw surfaces. The operation M. Velpeau recommends in old cases of artificial anus is that of paring the edges of the opening and bringing them in contact by suture. This ope-

ration was effectual in a case, which presented at the hospital of La Pitié, of old-standing artificial anus of the groin, and was performed in the following manner: "The borders of the fistula were first deeply pared so as to form an elliptical opening; four points of suture united the raw edges, and in order to prevent the borders of the wound being too much stretched, semi-elliptical incisions, their concavities towards the wound, were made on either side." We have had an opportunity of witnessing the effect of the autoplastic operation in a case of fecal fistula in the left lumbar region, and though the operation was performed with the greatest care, and every precaution taken to ensure its success, it ultimately failed. We should think, in cases where the artificial opening was but small, the operation recommended by M. Velpeau well worthy of trial.

The article on artificial anus closes the volume before us, and we look forward with pleasure to the perusal of the additional part of this work promised by the author in his preface, and which (from the large portion of the present volume that is occupied by articles on diseases of the eye) will probably be devoted more especially to subjects of general surgery. The publication of these clinical lectures cannot fail to add considerably to the fame M. Velpeau has already acquired for erudition and accurate observation.

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## Art. XII.

1. *Library of Medicine:* Vol. VI.—*A System of Midwifery.* By Edward Rigby, m.d., Physician to the General Lying-in-Hospital, Lecturer on Midwifery at St. Bartholomew's Hospital, &c. &c.—*London*, 1841. 8vo, pp. 314.
2. *The Principles and Practice of Obstetric Medicine and Surgery, in reference to the Process of Parturition; with One Hundred Illustrations on Steel and Wood.* By Francis H. Ramsbotham, m.d., Consulting Physician in Obstetric cases to and Lecturer on Obstetric and Forensic Medicine at the London Hospital.—*London*, 1841. 8vo, pp. 672.
3. *The Elements of Obstetric Medicine, with the Description and Treatment of some of the principal Diseases of Children.* By David D. Davis, m.d. m.r.s.l., Professor of Obstetric Medicine in University College, and one of the Physicians to University College Hospital. Second Edition.—*London*, 1841. pp. 1002.

The increased and increasing degree of attention that is devoted to the department of Midwifery upon the continent of Europe is amply shown in the great number of systematic treatises upon that subject that have of late years appeared in France and Germany. In the French language we have elaborate and excellent general works on this branch of medical science published within the last fifteen or twenty years, by Gardien, Capuron, Maygrier, Boivin, Velpeau, Halmagrand, and Moreau, besides several smaller manuals, by Duges, Hatin, Chevreuil, and others. Almost every German medical school has of late sent forth a text-book of its own on the subject of obstetric medicine. We need only refer, amongst others, to the learned systematic works and hand-books of Busch, Carus, Kilian, Horn, Joerg, Siebold, Froriep, Wilde, &c. The modern

medical literature of England contains many valuable isolated essays and monographs on particular departments of midwifery, but comparatively few general synopses of the principles and practice of this branch of the profession. We point out, however, with pleasure, as exceptions to this dearth of systematic compendia of obstetric science, the well-known and esteemed work of Dr. Burns, the clear and practical Synopsis of Dr. Merriman, the short Manuals of Drs. Conquest, Reid, and Maunsell, the excellent Introduction to Midwifery of Dr. Campbell, (a work highly valuable from its presenting the best account now extant of the late Dr. Hamilton's doctrines and practice,) and the notes of the lectures of Blundell and Gooch, that have been published by some of their pupils. It gives very great pleasure to see added to this list, and that within the current year, the three works at the head of the present article. The simultaneous publication of systematic works on midwifery, by three of the most distinguished obstetric teachers in the metropolis, is in itself a sufficient sign of the greater importance that has become attached to the study of obstetric medicine in our own country; and we willingly embrace the opportunity thus afforded us of directing the attention of our practical readers to some of the principal changes and improvements in doctrine and treatment which the researches of recent years have effected in scientific midwifery.

In some of the preceding volumes of our Journal we have already taken occasion to discuss at some length the subjects of embryology and the physiology of generation. We set these subjects aside therefore at present, both for this reason and because we must not search in the works of such practical men as Drs. Davis, Rigby, and Ramsbotham, for what is new in this wide and complicated field of investigation. In the following remarks we shall confine ourselves rather to such notices as our space will permit of the opinions of these gentlemen in natural, morbid, and instrumental midwifery. We shall speak more particularly of the works of Drs. Rigby and Ramsbotham, as we have already in our Third Volume treated at some length of Dr. Davis's Elements of Obstetric Medicine, as formerly published in the more extended quarto form.

The different authors whose works are now before us classify differently the forms and varieties of labour, normal and pathological. It is a matter of little moment which of their arrangements we follow in considering and contrasting their particular opinions. We shall adopt, therefore, that of Dr. Ramsbotham (the old classification of Denman), both because we consider it as good as any that has hitherto been proposed, and because it is the one with which we believe our readers are already most intimate.

In looking to the subject of natural labour, the first important topic which meets us is, that of the mechanism of the process. The mechanism of parturition, or the mode in which the fœtal head is applied to and enters the pelvis of the mother, and the different changes which it undergoes in its relations during its passage from the brim through the cavity and outlet, independent of its physiological and anatomical interest, is undoubtedly of the highest importance in its bearings on practical midwifery. It forms indeed the very basis both of the theory and of the art. Its mere mechanical nature may, in the eyes of some, place it inferior in medical interest to other pathological and physiological questions in the

department of obstetric medicine, but it is certainly subordinate to none in actual utility.

We cannot, with Dr. Rigby, see how any one can detect, with certainty, the deviations that are apt to occur from the usual course of labour, unless he be thoroughly acquainted with every step of the mechanism in the natural process. Nor can we see how any one can expect to assist instrumentally with perfect certainty, and to the fullest advantage, the various evolutions and motions of the fœtal head in the different varieties of labour, without knowing what the most natural and consequently the most easy evolutions of each particular variety actually are. No remark, however, is certainly more true than that made by Dr. Denman, that natural labour was the last sort of labour properly studied and properly understood, and we have no hesitation in pronouncing our opinion, even at the present day, that though it is a subject worthy of the greatest attention it has, at least in this country, hitherto excited very little notice, and though a topic on which, above all others, it is necessary to have correct opinions, there are certainly few departments in midwifery upon which the practitioners of Great Britain entertain, generally speaking, more loose and more incorrect ideas.

It is curious in looking back to the past history of practical midwifery, to weigh the talent and ingenuity that have been bestowed upon the fabrication of instruments to assist, as it was called, in difficult labours, and at the same time to reflect that the fundamental facts on which the principles of their application and employment rest, were entirely unknown to their inventors. Thus, when the Chamberlens first invented the forceps to drag the fœtal head through the pelvis, they neither knew the manner in which the fœtal head enters the pelvis, nor any one of the relations which it occupies in its passage through the pelvic cavity. They and many of their followers endeavoured to assist nature in her movements before they knew what these movements were.

In making these remarks we beg to state that we are perfectly aware that various authors, both British and foreign, have long ago endeavoured to describe more or less minutely the mechanism of natural labour and its different modifications. Their descriptions, however, were drawn from theory rather than from experience, from fancy more than fact, from rude experiments made with the dry bones of the head and mother's pelvis, and not by careful and minute observation of the process as it is conducted in nature. Hence the accounts of different authors, as might readily be expected, show the greatest discrepancy and discordance even on the same points. Whole paragraphs, and whole pages, as Naegele in his comments upon this subject has remarked, are to be found in obstetric works, with their contents respecting the mechanism of parturition perfectly disagreeing with one another, and still more disagreeing with nature. Relations and movements are described which do not occur, and those that do occur are in many instances left undescribed. Authors have but too often shown how they would move the head through the pelvis, if they had the direction of it, rather than endeavoured to show how nature herself conducts the process.

In an article such as the present it is impossible for us to enter at length into the various questions connected with the mechanism of labour. We will offer merely a few remarks to illustrate some more

general facts with regard to it, and to show the truth of what we have previously stated in regard to the discrepancy of opinions that long prevailed upon the subject.

In the first place, it is almost unnecessary to observe that all admit that the head of the child presents at the brim of the pelvis in a very large proportion of cases of labour. This proportion is as great as 94 or 95 in the 100; or, in other words, the exceptions to this general rule are only about 1 in every 30 cases of labour according to Collins, 1 in 33 according to Osiander, 1 in 34 according to Carus, or as Meckel has derived it from various obstetric statistics, only 1 in 35.

But the next question in the consideration of the mechanism of parturition is a much more undecided one, namely, in what direction does the head of the child enter the brim of the mother's pelvis? Important as an accurate knowledge of this fundamental fact is to an acquaintance with the subsequent evolutions which the child's head undergoes, and to the practical management of labour, it was not till the middle of the last century that the slightest approach was made to an accurate opinion on this subject. Since that period several different doctrines have been promulgated in respect to the direction in which the long or antero-posterior diameter of the child's head enters the brim at the commencement of the process of parturition.

A glance at the history of these opinions will illustrate the subject better than any formal comments on it.

1. By all the older authors the head seems to have been supposed to enter the superior margin of the pelvis, in the same relation in which they observed it to emerge from the outlet, namely, with the sagittal suture or long diameter parallel to the conjugate or antero-posterior diameter of the brim.

2. In 1742, Sir Fielding Ould, of Dublin, called this first opinion in question, and maintained that, in entering the pelvis, the long diameter of the head lay parallel to the transverse diameter of the brim. We shall quote his own words, as his work is now somewhat rare, and his original observations on this point little known:

> "When a child presents itself naturally, it comes with the head foremost, and (according to all authors that I have seen) with its face towards the sacrum of the mother, so that when she lies on her back, it seems to creep into the world on its hands and feet. But here I must differ from this description in one point, which at first sight may probably seem very trivial; the breast of the child does certainly lie on the sacrum of the mother, but the face does not, for it always (when naturally presented) is turned either to the one side or the other, so as to have the chin directly on one of the shoulders." (Ould's Midwifery, p. 28.)

From this it is evident that Ould believed the child's head to be so greatly twisted round on its trunk "as to have the chin on one shoulder." In 1752, Smellie corrected this notion with regard to the contortion of the child's neck, but otherwise fully adopted Ould's views of the direction of the head. In 1771 Deleurye first promulgated the same opinions in regard to the position of the head in labour in France; and in later times Schmitt and Mampe, in Germany, have upheld the same doctrine.

3. In 1771 two celebrated obstetric essays were published upon the continent, the one by Saxtorph of Copenhagen, and the other by Solayres de Renhac of Montpellier, containing observations made independently

of each other, but both agreeing in this one great fact—that the long diameter of the head of the child in natural labour entered the pelvis in a direction neither parallel to the conjugate nor to the transverse diameter of the brim, but parallel to one of its *oblique* diameters; that is, with the sagittal suture running in a line directed at one extremity to the sacro-iliac synchondrosis behind, and to the foramen ovale anteriorly. They further both showed of the two oblique diameters, the long axis of the head, in a very large proportion, occupied the right, or that running between the right sacro-iliac synchondrosis and left foramen ovale.

Baudelocque, the pupil of Solayres, adopted the opinions of his master as one of the great foundations of his classification of labours, though he did not adhere exclusively to his master's views. The great influence of Baudelocque over the French and other schools of midwifery has spread widely the knowledge of this doctrine of the oblique position of the head at the brim. It was assuredly, however, not until Professor Naegele of Heidelberg published his incomparable essay on the Mechanism of Parturition in 1818, that the great and almost exclusive predominance of the oblique direction of the head at the commencement of labour was fully acknowledged. His opinions have daily gained more and more ground on the continent of Europe, but though his essay was translated into our own language in 1828, we regret to add that the older opinions on the subject of parturition are still taught in many of our British schools. We feel certain, however, that the more his essay is studied, and the more his observations are accurately compared with what occurs in nature, the more will Naegele's opinions be found to gain weight with the practitioners of this country.

From the preceding slight historical sketch it is evident that it has been supposed that the head of the child may, at the commencement of labour, enter the brim of the pelvis in four different positions; namely, with its long diameter, 1st, parallel to the conjugate diameter of the pelvis; or, 2d, parallel to its transverse diameter; or, 3d, parallel to its right oblique diameter; or, 4th, to its left oblique diameter. The first two have been sometimes named the direct or straight positions of the head, the last two the oblique or diagonal. Further, the long diameter of the head may be placed in each one of these pelvic diameters, in two different relations, viz., with the forehead directed to one extremity of it, or the head being completely reversed, the occiput may be directed to the same point.

We make these remarks in order to show the foundation on which different authors have based the classifications of cranial presentations that they have proposed. Various writers have described the head as capable of entering the brim of the pelvis in natural labour in all of these eight positions; others have reduced them to six, and among the authors who have done so, there are some who, as Baudelocque and his followers, have thrown out the two positions referrible to the conjugate diameter of the brim, and others who, as Lachapelle, on the contrary, have renounced the two positions referrible to the transverse diameter. All our best observers for the last sixty or seventy years have reckoned the oblique positions as the most common. Capuron, Naegele, &c. consider the four oblique as the only positions seen in cases of natural labour, and Dr. Rigby has endeavoured still further to refine upon this, by reducing the natural presentations of the head at the brim to the two that can

occur in the right oblique diameter alone, namely, 1st, with the face pointed towards the right sacro-iliac synchondrosis, and the occiput to the left acetabulum; and 2dly, with the face looking to the left acetabulum, and the occiput to the right sacro-iliac synchondrosis.

The different authors who have adopted the different classifications of cranial presentations to which we have referred have affixed to each an ordinal number to designate them, but, unfortunately, they have but too often employed the same number to designate different presentations. Hence we find, for example, the third or fourth positions of one author not at all corresponding to the third or fourth of others. It is unnecessary to comment on the discordance to which this has led. In taking up each separate author on the subject, we have first to teach ourselves his particular language on this point, before we can either understand his observations or the rules of treatment which he lays down. As a key to the obstetric reader in his study of some of the more valuable authors of the present day, we beg to offer the following table:

TABLE, showing the Numerical Order of Classification followed by different Authorities in their systematic Arrangements of Cranial Presentations.

| Numbers affixed to presentations, by | | | | | | | Description of Presentation. |
|---|---|---|---|---|---|---|---|
| Rigby. | Naegele. Capuron. Maygrier. Duges. Halmagrand. | Baudelocque. Dubois. Gardien. Davis. Dewees. | Lachapelle. | Boivin. Flamant. Moreau. | Ramsbotham. | Anterior part of Cranium pointing |
| 1 | 1 | 1 | 1 | 1 | 3 | to Right Sacro-iliac Synchondrosis. |
| | 2 | 2 | 2 | 2 | 4 | ,, Left ,, ,, |
| 2 | 3 | 4 | 3 | 4 | 6 | ,, Left Foramen Ovale. |
| | 4 | 5 | 4 | 5 | 5 | ,, Right ,, |
| | | 3 | | 3 | 7 | ,, Promontary of Sacrum. |
| | | 6 | | 6 | 8 | ,, Symphysis Pubis. |
| | | | 5 | 7 | 1 | ,, Right Os Ilium. |
| | | | 6 | 8 | 2 | ,, Left ,, |

The classification of head presentations adopted by the authors whose works we have under review will be sufficiently apparent from this table. In it we have placed Dr. Davis as following Baudelocque's arrangement, as he evidently professes to do at p. 621 of his work, though at the same time we must remark that, after announcing it, he himself does not strictly adhere to that arrangement, for (and we could quote other examples) while at p. 622 he describes the third position as that in which the occiput lies behind the symphysis pubis, in p. 808 he describes the same third position as that in which the right ear lies immediately behind the symphysis pubis and the occiput directed to the left side of the pelvis. We do not stop to comment on the confusion to which such a discrepancy of statement must lead in the mind of the obstetric student.

Though Dr. Rigby has admirably described the mechanism of parturition in his work, we confess that we feel somewhat dissatisfied with his innovations upon the arrangement and nomenclature of Naegele, whose work he himself first introduced to the British public.

We are so old as to dislike to learn a new language to designate objects with which we have long been familiar. In other respects we can earnestly recommend Dr. Rigby's chapter on the mechanism of labour

to the attention of all those who wish to obtain accurate ideas with regard to this matter.

Dr. Ramsbotham has, as will appear from the foregoing table, proposed an arrangement of cranial positions with a numerical order peculiar to himself. We have the same objection, of unnecessary innovation, to urge against him in this respect as we have just made against Dr. Rigby. Further, we certainly consider the number of presentations that Dr. Ramsbotham has described as too great and complicated. We are not inclined with some authors absolutely to deny that the direct position of the head in the conjugate and transverse diameters cannot possibly occur. We could, we think, if this were a proper place, point out some particular elongated forms of the brim of the pelvis, which would almost oblige nature, if we may so speak, to enter the child's head in one or other of these diameters. But these forms are so exceedingly rare, and these varieties of presentation so very seldom occur in practice, when the child's head and mother's pelvis are of the natural size, that they may certainly be kept out of any classification of labours that are purely normal. Obstetric writers have almost always numbered the presentations in correspondence with the comparative frequency according to which they respectively occur, or, as Naegele has done in regard to his second position, in deference to the numbers already appended to them by others. Dr. Ramsbotham has set aside both these circumstances as the basis for his nomenclature. The numbers he has appended to the positions are, we believe, different from those appended to them by any preceding author; and though he inadvertently states, at p. 135, that his first four presentations are "by far the most frequent," yet his fourth (the second position of Naegele) is certainly seldom met with, and is greatly less frequent than his sixth (the third position of Naegele), while both his first and second are alleged by Baudelocque, who introduced them into his own classification of presentations, to be so exceedingly uncommon, that he had himself never met with one instance of it out of many thousand cases; Boivin found them twice in 19,000 labours, and they are hence entirely rejected from the works of most of our recent and best authors.

After this last remark we could have wished that our space had permitted us to show, by the statistical reports of Lachapelle, Boivin, Naegele, Duges, Halmagrand, and others, the comparative proportion of instances in which the different positions of the head have been ascertained to occur in actual practice. It is an important topic, and one to which we may recall the notice of our readers at an early period. In the meantime we shall content ourselves with presenting the few following calculations of the per centages of frequency of the four oblique positions of the head.

The *first* position of Naegele (with the child's face to the right sacro-iliac synchondrosis) has occurred to that author in his own practice in 69 per cent. of all his head presentations, to Halmagrand in 74 per cent., to Madame Lachapelle in 77 per cent., and to Madame Boivin in the ratio of 80 per cent.

The *fourth* position of Naegele (with the child's face directed to the right foramen ovale) has occurred in his own practice in the very small proportion of ·03 per cent. It has been met with equally rarely by

others. Lachapelle and Halmagrand have found it in ·04 per cent., and Boivin in ·05 per cent.

So far we find a striking correspondence in the different observations of these authors, in a series of cases amounting in all to nearly 60,000. Their results, however, are more discrepant as regards the relative frequency of the second and third positions of Naegele. They all nearly agree, be it remarked, in regard to the absolute proportion of cases in which the face is directed to the left side of the pelvis, in the same way as they all nearly agree as to the absolute proportion of cases in which the face is directed to the right side. They differ, however, much as to the proportionate number of instances in which the face being directed to the left side, is pointed originally towards the left sacro-iliac synchondrosis (constituting the *second* position of Naegele), or towards the left foramen ovale (constituting the same author's *third* position of the head). Thus while Naegele states that in above 1,290 cases he only met with the *second* position in one instance, or in the proportion of about ·07 per cent., Halmagrand describes it as occurring in 5 per cent. of his cases, Boivin in 19 per cent., and Lachapelle in as great a ratio as 21 per cent. On the other hand, while Naegele found not less than 359 cases of the *third* position in 1,210 cases of cranial presentations, or as many as 29 per cent. of instances in which the face looked at the commencement of labour to the left groin, Lachapelle reports only ·077, and Boivin 0·5 per cent. as presenting in the same situation.

Various explanations have been attempted of these and other reported differences of results in the comparative frequency of the second and third positions. The observations of Naegele were, we believe, made by himself in most if not in all the cases to which he refers, most of those of Lachapelle and Boivin by females attached to the Maternité Hospital of Paris. The observations at the hospital were collected, without there having been previously strongly pointed out a great source of fallacy in confounding the second and third positions, and those of Naegele were conducted with a perfect knowledge of and a view to this fallacy. We are inclined, therefore, both from what we have ascertained by our own experience, and from what we know of the high character and candour of Naegele, to attach more credit to the results of his observations than to those who have been opposed to him on this point. And we further believe that the great source of fallacy to which we have alluded and which is so apt to deceive is this, that if the face present to the left, and the position of the head be not examined till it is emerging through the outlet, it will in a very great proportion of cases be found by that time turned round into the second position, though it originally presented in the third. We shall afterwards see that this rotation of the face backwards is the general and normal course followed by cases of the third position.

We have not space to pause and point out the anatomical reasons which, on physical principles, prevent the occurrence of the *direct* positions, and lead, on the other hand, to such an overwhelming proportion of cases in the right oblique diameter. The subject, however, is one of great interest.

The part of the child's head that lies lowest in the pelvis, or in other words, the exact nature of the presenting part in cranial presentations,

has given rise to almost as great difference of opinion as the direction of the head itself. Daventer long ago pointed out the oblique position of the fœtus and the uterus itself in relation to the vaginal and pelvic cavity; but, notwithstanding his observations, we find misstatements in works up even to the present time in respect to this point, and more especially in respect to its influence upon the regulation of the presenting part. The anterior fontanelle, sagittal suture, and still more commonly the vertex, have each been alleged to be the parts that the finger of the accoucheur first touches upon. The acknowledged obliquity of the child's body and head in relation to the vaginal canal might have been sufficient alone to have taught the fact, that it could not be a *mesial* part of the child's head that presented in natural labour, and we believe that every one now, who has actually studied the matter in nature, agrees that what Naegele first distinctly pointed out, that the posterior and superior portion of one or other of the parietal bones is first felt on examination by the accoucheur. When the head presents with the face looking towards the right side of the pelvis, it is the right parietal bone that first descends; when the face looks towards the left, it is the left parietal bone. On the above points Dr. Rigby offers the following sound generalizations, in his consideration of the mechanism of head-cases of the first position, or with the face looking to the right sacro-iliac synchondrosis:

"The head enters, passes through, and emerges from, the pelvis obliquely; and this is the case not only as to its transverse diameter, but also as to the axis of its brim; the side of the head being always lowest or deepest in the pelvis. This shows the beautiful mechanism of the process, for on account of its oblique position, there is no moment during the whole labour at which the greatest breadth (still less length) of the head is occupying any of the pelvic diameters; even at the last, when the head is passing under the pubic arch, the complete obliquity of its position, in order that it should take up the least possible room, is very remarkable, for the ring of soft parts, by which the head is now encircled, passes obliquely across it, running close behind the left and before the right parietal protuberance. The head never advances with the occiput, forwards, under the pubic arch, as is stated in works on midwifery, still less with the sagittal suture parallel to the antero-posterior diameter of the pelvis; for the direction of the right lambdoidal suture, as also of the posterior fontanelle, and the position of the cranial swelling, or caput succedaneum, as it has been called, completely prove the inaccuracy of such a theory, the sagittal suture crosses the left labium at an acute angle, the right lambdoidal suture being parallel with the left descending ramus of the ischium." (p. 126.)

He adds another important remark:

"Not less incorrect is the theory (for we can call it nothing else) of the head presenting with the vertex, and turning with its long diameter from the oblique into the antero posterior or conjugate diameter, and the face into the hollow of the sacrum, for it is disproved by all the above-mentioned facts, which careful examination during labour puts us in possession of. When the head is born, the face looks backwards and to the right, viz. to the back part of the mother's right thigh, for the shoulders are by this time passing through the pelvis in its left oblique diameter, the right shoulder being forwards and to the right, and lowest in the pelvis; it is also that which is first expelled." (p. 127.)

Dr. Ramsbotham, in the beautiful plates which accompany his work, has given us eight figures illustrative of the modes in which the head of the child may be supposed to enter the brim of the pelvis in each of his

eight positions. In these his artist has unfortunately represented throughout the fœtal cranium as entering the pelvis directly, and not in the oblique mode in which it does in nature. In plate 35 indeed, the fœtal heads are seen, by looking at the basis of the cranium, to be somewhat oblique, but the artist has shaded them so that they look obliquely backwards instead of forwards. In plate 39, as representing the second stage of labour, the face is turned far too directly into the hollow of the pelvis. We much admire, however, plate 41, representing the position of the child immediately after the head is expelled. Dr. Ramsbotham's own description of the usual course of natural labour is in most respects excellent, as are his directions for its treatment. On the management, indeed, both of natural and morbid labours, we have a precision and minuteness in them that must render his work a valuable practical compendium for the student and practitioner. Dr. Ramsbotham recommends the first two fingers of the *left* hand to be used in vaginal examinations in the first stage in those cases in which the os uteri is situated so high that it cannot be perfectly investigated by the fore-finger of the right hand. We believe this to be a very useful direction. The artist in representing this mode of examination (plate 42) has incorrectly represented the whole hand, with the exception of the thumb, as introduced into the orifice of the vaginal cavity.

In supporting the perineum, Dr. Ramsbotham justly states that it is not necessary that we should make powerful pressure, nor resist the child's exit by the employment of any exertion. Continuing, as it were, the resistance and curvature of the sacrum by our hand we are only to afford passive support; to allow the head covered by the perineum to be protruded against our hand rather than forcibly press our hand up against the head. We must recollect further that, as Dr. Rigby particularly mentions, "the passage of the head is not the only moment of danger to the perineum, for laceration is even still more liable to be produced during the expulsion of the shoulders; any slight rupture of the anterior edge is now apt to be converted into a considerable laceration, unless the support be continued until the thorax be expelled." As a specimen of the excellence of Dr. Ramsbotham's practical directions, and the vigorous style in which he lays down his rules and doctrines on this and other subjects, we extract the following short passage on an important point of treatment first very prominently brought under the attention of the profession by Mr. White of Manchester, viz. the treatment of the delivery of the body after the head is born:

"It used to be the custom to surround the neck with the thumbs and fingers of both hands, and forcibly extract the body the moment the head was in the world, for the purpose of liberating the woman from pain, and terminating the delivery as speedily as possible. Such practice is attended with double danger; great chance of injury to the child, by the tension of the neck, and no small probability of hazard to the mother, by the uterus being prematurely emptied. It is thus left in a flaccid state; the stimulus which previously disposed it to contract is suddenly taken away; that disposition ceases or is suspended; hemorrhage is induced; and necessity probably arises for the artificial removal of the placenta, and incalculable mischief is the consequence. Those persons who commend such meddling interference, and who estimate the skill of the obstetrical attendant by the rapidity with which he can extract the body after the head is born, found their eulogium on the most dangerous premises." (p. 173.)

Dr. Rigby describes with great minuteness and clearness the treatment, &c. of natural labour, and of the puerperal state. He directs the attention of his readers to auscultatory and other phenomena of labour that have hitherto been little described in English systematic treatises. We extract from his work some remarks relative to a source of prognosis of the duration of labour which we believe will be new to most of our readers:

"The celebrated Wigand of Hamburg considered that the form of the vagina would frequently furnish the means of a pretty certain prognosis as to the duration of labour; thus, if it were wide and yielding throughout its whole length, the labour would be quick, both at its beginning and termination; if on the other hand it were small, rigid, and contracted throughout, the labour might be expected to be of a very opposite character. If, on examination, the vagina is found roomy and well dilated at its upper part, but contracted and rigid near the os externum, the labour will be probably quick and easy during the first half, but slow and difficult afterwards; on the contrary, where the os externum is yielding and wide, but the upper portion of the vagina narrow, the labour may be expected to be slow at first, but to be brisk and active afterwards. We have already stated that the course of labour varies in every possible way; in some cases the same peculiar character of labour shows itself through two or three successive generations; hence it has been observed that very tedious or very violent and rapid labours sometimes seem to be hereditary; the mother, daughters, and grand-daughters being all remarkable for their lingering or rapid labours." (p. 111.)

We extract from Dr. Rigby's chapter on natural labour one more passage to show the different positions, &c. in which women are placed in different countries during the process of parturition. After speaking shortly of the history and form of the old labour-chairs of the continent, he remarks:

"In some remote parts of Ireland and also of Germany, the patient sits upon the knees of another person, and this office of substitute for a labour-chair is usually performed by her husband. Labour-chairs, as far as we are acquainted with their history, were never used in this country, nor have they been used for the last century in France, where the patients are usually delivered in the supine posture on a small bed upon the floor, which has not inaptly been termed *lit de misère.* A modification of the labour-chair is the labour-cushion first used by Unger, and afterwards by the late Professor Von Siebold of Berlin, and Professor Carus of Dresden; it is a species of mattress, with a hollow beneath the nates of the patient for receiving the discharges which take place during the labour. The patient is compelled to lie upon her back during the greater part of labour, and thus maintain the same posture for some time, which must necessarily become irksome and even painful to her. In this country and in Germany the patient is delivered upon a common bed, prepared for the purpose as above mentioned; in England she is placed upon her left side, the nates projecting to the edge of the bed, for the greater convenience of the accoucheur; in Germany, except in Vienna and Heidelberg, where the English midwifery has in a great measure been introduced by Boer and Naegele, the patient is delivered upon her back. In former times the supine posture was also used in this country, but for about a century the position on the left side has been preferred." (p. 109.)

The position upon the left side is still we have reason to know changed by some old practitioners at the present day into that upon the back whenever the labour becomes instrumental. This practice is certainly open to many objections and attended with no advantages with which we are aware. We have heard that in the Edinburgh Infirmary a regular

perforated obstetric chair was employed above half a century ago, and we recollect that some thirty years since it was shown by the late Dr. Hamilton to his midwifery class. In Mr. Michael's work on the Ergot of Rye, it is alleged that in Cornwall even at the present day it is difficult to persuade the female in labour to adopt any other than the standing posture, or a position upon her knees.

Following Dr. Ramsbotham's arrangement we come next to his chapter on the "Irregularities of head presentations," under which he describes, first, vertex presentations with the face behind either groin. This presentation constitutes the third and fourth of the natural positions of the head in the classification of Naegele. We have already shown that the face is very rarely directed to the right groin. On the other hand the presentation of the head with the face looking to the left groin is exceedingly common. If we wish to prove how very little attention has hitherto been paid in this country to the mechanism of natural labour, we might strongly appeal to the errors both in observation and treatment that are to be found in most of our English works, in regard to the presentations in question. Taking one example alone, we find that Dr. Collins in his valuable Report of 16,000 cases of labour in the Lying-in Hospital of Dublin, mentions only twelve cases of presentation of the face anteriorly having been observed. The observations of Naegele, Professor Stoltz of Strasburg, and others, show that when the mechanism of labour is accurately watched, presentations of the face to the left groin alone occur in the proportion of above 20 per cent. of all head cases. Naegele found it in 359 cases out of 1210, or in the ratio of about 1 in every $3\frac{1}{2}$; Dr. Collins detected it only in the ratio of 1 in 1342. In a late valuable paper in the Dublin Medical Journal, Dr. Murphy, formerly assistant in the Dublin hospital, states upon his own observation, that he had discovered this position of the head in the proportion of 1 in 7 out of the 74 cases investigated with a view to this point.

We have already hinted that when the face presents to either groin, it is a general rule, liable to few exceptions, that as the head descends through the cavity of the pelvis, the face comes to be turned round to the nearest sacro-iliac synchondrosis, and the occiput emerges at last under the arch of the pubis, exactly as in those cases in which the face originally presented posteriorly. This spontaneous rectification, as it has been termed, was known to Solayres, but for the knowledge of its great frequency, we are indebted to the zeal and the observation of Naegele. He found the rotation in question take place in 93 out of 96 cases which he carefully watched. In the remaining three instances alone did the face maintain its original position and protrude at the outlet still directed anteriorly. Carus has thought fit to ridicule the mode in which the distinguished Professor of Heidelberg confirmed these curious observations by keeping his fingers in contact with the child's head during the whole course of labour. Too many obstetricians, however, have certified to themselves the truth of Naegele's views, to make ridicule in such a case anything except a proof of the absurd prejudices of him who used it.

A practical fact of the utmost importance has been amply ascertained by Naegele and others, with regard to positions of the head with the face looking to either groin, namely, that such cases are almost universally terminated without any interference on our part, or without much, if any,

additional struggle on the part of nature. The change in the position of the head, by which the face turns from the anterior to the posterior part of the pelvis, requires, says Naegele, "no peculiarly favorable circumstances, but these species of labours can be completed by the natural powers under the most usual proportions, in the same time, with the same expense of strength, and without greater difficulty than when the head takes the most common position." We know ourselves from sufficient experience, the fund of truth there is in these remarks. These positions require no peculiar interference on our part; they should not even be considered as morbid; if there is any truth whatever in statistics, we venture to say, from the data we have just adduced, that they daily occur and pass over unobserved in hundreds of instances in which the labour is supposed to be, and no doubt is, perfectly natural; and yet, in many of our text-books, elaborate rules are laid down for their treatment, and some authors doubt the possibility of their completion if they were abandoned to nature alone without the employment of instruments.

Dr. John Clarke published, in the second volume of the Transactions of the Society for the Improvement of Medical Knowledge, an essay on this presentation, in which he recommended a method of manual interference "to be always pursued, when the face is found in the situation above described." By this method, he alleges, he managed to turn the face backwards in 13 out of 14 cases. In 13 out 14 cases nature turns the face backwards without any management of ours. Drs. Burns, Hamilton, and others, have zealously recommended the same unnecessary treatment, and Capuron, Bazignan, and Omoboni, speak of delivery with the face anteriorly as actually impossible without the aid of the forceps or other forms of operative interference.

No accoucheur in this country has probably done more to simplify obstetric practice and disencumber it of operative superfluities than Dr. Blundell. Yet we find this gentleman recommending, in the case under consideration in some instances, rectification of the position by the hand or with the forceps or lever, "when experience and practice are not wanting," or even occasionally by turning "when the softer parts are lax and the pelvis is capacious, and our dexterity from long practice such that we can introduce the hand into the uterus and lay hold of the child's legs and bring it away with facility." (Lee's edition of Dr Blundell's Lectures, p. 65.) Three lines above our last quotation Dr. Blundell ominously recommends us in the treatment of this very same case not to "commit the unpardonable sin of midwifery, the sin I mean of those obstetric reprobates—the meddlesome and pragmatic!"

After speaking of presentations of the face to the groin and to the pubis, and after describing eight varieties of brow presentation (surely a most unnecessary multiplication of them,) Dr. Ramsbotham comes to direct presentations of the face, a class of cases the whole history of which forms one of the best examples to which we could point, of the advantages in obstetric practice of carefully observing how much nature can effect in particular instances, before we attempt to rectify her alleged errors by rude and perilous interference. The history of their treatment in fact is but another illlustration of how very much may be gained by studying obstetric mechanism and the powers of nature.

Many authors of the 17th and first half of the 18th century, seem to

have looked upon face cases as so preternatural and difficult that it was generally necessary in the treatment of them, as in the treatment of a hand or shoulder presentation, to have recourse to the operation of turning. Lamotte, Cooper, Smellie, Burton, &c., all appear to have followed this practice, particularly when called in at an early stage of the labour. Giffard, in his posthumous work published in 1734, seems to apologise for finishing two face cases with his extractor or forceps by truly observing, that turning is attended with great difficulty where the "waters are gone off, and the uterus closely envelopes the child." Again Perfect, Baudelocque, and others, while they deem it unnecessary to have recourse to the dangerous operation of turning, still considered active interference necessary, and have, as a common rule, recommended the face to be pushed up and the vertex brought down, contenting themselves thus with a rotation of the head of the infant, and not a rotation of its whole body. Peu speaks of the crotchet—if the face has already descended into the pelvis; and Stein recommends the conjoint use of the fillet and forceps in managing these presentations. Indeed Stein maintained that without some such interference face cases would always be cases of delay and danger. He avers that their spontaneous termination is impossible, except where the fœtus is preternaturally small, or the pelvis preternaturally large. Mesnard endeavoured to prove this by geometrical measurements of the child's head and mother's pelvis, and we regret to add, that one of the most distinguished accoucheurs of the present day, M. Capuron, still upholds the same doctrine, and obstinately opposes the result of his observations with the rule and compass upon the dead and dry head and pelvis, to the result of the direct observations of all his contemporaries upon living nature; thus forgetting or despising one of the great rules of the father of the inductive philosophy—"Non arctandus est mundus ad angustias intellectus, sed expandendus est intellectus ad mundi imaginem recipiendum qualis invenitur."

The sufficiency and success, however, of unassisted nature in face cases was long ago announced to the profession. In 1685, Paul Portal, who in this and in several other matters far outstripped the age in which he lived, taught that in face presentations the child might suffer, and its face be black and swollen, but there is not, he adds, more mystery in this than in a natural or vertex presentation; "I have (he says) delivered several women whose children came with the face foremost, and always without any great difficulty, it being only observed, that in such cases no violence must be used, but nature left to its own course, which done, there is no danger either of mother or child." In 1769 Wallace Johnson in this country, and in 1770 Deleurye in France, both publicly avowed the same opinion in regard to the safety and propriety of leaving to the unaided efforts of nature face presentations; and latterly such an accumulation of statistical proof has been collected upon this question, as must, we think, prove perfectly satisfactory to every unprejudiced mind, and carry conviction to all that prefer the force of simple facts to arguments and dogmatic theories. In 1789 Zeller published 43 cases of face presentations all of which terminated without operative or artificial interference. In 1793 the celebrated Boer declared, that out of 80 face cases, which he had himself observed and noted down, none of the mothers suffered in the slightest degree; three, or at most four, of the children

were born dead, and in one case only was artificial assistance (by the forceps) deemed necessary. In 1826, Chevreuil, physician to the Maternité Hospital at Angers, stated that in 18 face cases which he had met with, all terminated naturally. The infants were of the ordinary size; 15 were born alive and 3 were dead, but apparently, he adds, in these cases death had taken place before delivery. In 1836, Dr. Collins of Dublin, in his excellent account of the Cases in the Lying-in Hospital of that city, reports 33 face cases as having occurred during his charge of the institution. All the 33 were delivered without assistance and recovered well. Four of the children were still-born, and one of the four was acepahlous (anencephalous). In one of the four the labour had been allowed to be prolonged thirty-six hours. With the two others it lasted seven or eight hours. In 5 the labour was over within an hour. Some cases of face presentations, Dr. Collins ingenuously adds, I am disposed to think were not noted, delivery having taken place so very speedily.

If it were necessary we might add to the foregoing the statistics in face cases of the Prague hospital, as given by Kilian; or those of Bourg, as reported by Pacoud; and the observations in Copenhagen made by Bang; but it is unnecessary to accumulate further proof, when that proof all tends to the same conclusion. These last three authors conjointly report above 200 face presentations, all showing the great sufficiency of nature in such cases, and the impropriety of rash interference.

In the works before us we find different doctrines of practice inculcated in regard to face cases by Drs. Rigby, Ramsbotham, and Davis. Dr. Rigby gives no direct rules for their management, but his observations all show that he believes face cases to require no particular interference on the part of the practitioner. Dr. Ramsbotham expresses the same opinion, and shows the same repugnance to the employment of any artificial management in ordinary face cases. We have already seen some practitioners advising an attempt at rectifying the position of the head by transforming the face into a cranial presentation. This is a line of practice too often attempted even at the present day. We believe its performance to be impossible after the face has been pushed down into the cavity of the pelvis, when the child and pelvis are both of the natural dimensions, for we believe it physically impossible to turn the fœtal head on its long or occipito-mental axis of five inches through the cavity of the pelvis which is normally only four inches and a half. We quote one or two excellent remarks of Dr. Ramsbotham on this topic:

"It becomes a most important question whether, under a face presentation, any means should be adopted to place the child under a more favorable position. So difficult and almost impossible was the transmission of the head under this presentation at one time thought that it was recommended that the hand should be introduced into the uterus—that the feet should be laid hold of, and that the child should be delivered by turning. This operation, performed under the most favorable circumstances, is always attended with great pain, and frequently with great danger, danger both to the mother and the child: to the mother, from the chance of injury to which her structures (particularly the uterus) are exposed—to the child, in consequence of the pressure which the funis umbilicalis must more or less experience, when the head is passing through the pelvic cavity. All these circumstances, then, being taken into consideration, the practice of changing the position of the child under a face impression by turning is now almost entirely exploded; and we rather leave the case to nature so long as we can safely trust her, than subject the woman and the in-

fant to such dangers. But suppose, on watching the case, we find no advantage gained, no alteration in the position of the head, no advance from hour to hour, what then is to be done? We must here also act upon the same unerring principles elsewhere laid down, wait till symptoms require our interference, and then use that instrument which seems most applicable to the emergency. For it is impossible, by any counter pressure, to make a beneficial change in the situation of the head under a face presentation. We cannot cause the head to turn upon the neck so as to approximate the chin to the chest by pressure." (p. 209 )

We have said that Dr. Rigby has not thought it necessary to offer any rules for the treatment of face cases. Dr. Davis, on the other hand, has laid down rules enough for the management of these cases, but these rules are in our opinion, in various respects, highly objectionable. Indeed if our continental neighbours judge by them of the state of British midwifery, we fear they will consider us as undoubtedly retrograding rather than advancing in this department of the healing art. In order to avoid all risk of misrepresentation we shall here quote his own words:

"When the face is discovered to present at the brim of the pelvis at an early period of a labour, whether before or very soon after the escape of the liquor amnii there can, in the author's opinion, be no doubt as to the preferableness of turning to all other modes of treatment. That operation dexterously performed would, at all events, give the child a good chance of perservation of its life; whilst it would also be the means of rescuing the mother on the very brink of a great impending danger." (p. 659.)

We certainly regret to find such practical doctrines published by an author of Dr. Davis's name and standing, and, above all, by a lecturer on midwifery. The interference he thus teaches is, as we have already seen from the statistical data, useless and unnecessary. We wish this were all we could say against it. We must add our solemn conviction that the practice is not only superfluous but that it is fraught with great danger to both the child and mother. In original footling cases about one in every four is lost from compression upon the cord, and other causes which it is unnecessary at present to enquire into. Dr. Davis advises us to change artificially the face into a footling case, and if so we could not certainly expect to save more than in the natural footling cases. We believe we would save fewer. How strongly then does this degree of danger to the child contrast with the degree of danger to which it was found subjected in Boer's cases, when leaving the case to nature not more than 1 in 20 was lost. In the exercise of our profession we have too often to blame ourselves for errors of omission. Here we think human life is manifestly endangered by an error of commission, if meddlesome and unnecessary interference may bear that name. But the practice affects not the safety and life merely of the child: it affects also the safety and life of the mother. Turning the fœtus in utero is an operation which, as Dr. Churchill calculates it from various data, proves fatal to the mother in about one in every fifteen cases in which it is had recourse to. Are we justified in subjecting any woman to even a fraction of this danger in rectifying or attempting to rectify a presentation, any supposed difficulties connected with which nature every day shows to us she can easily and safely overcome without any assistance on our part?

Let us not be misunderstood. We by no means inculcate that the practitioner remains perfectly apathetic because the presentation is discovered to be a face one. We would rather wish to guard him against

the opposite and more dangerous extreme of direct and dangerous interference with the head or body of the child itself. In all such cases we firmly believe, as a general rule, that the *infant* or its position should not be interfered with. We shall not stop to enquire whether the conical-shaped head of the child, when it passes through the pelvis with the face or base of the cone first, is much or in any degree larger than when the vertex or apex of the cone forms its presenting part. But even admitting, what we conceive to be true, that the head when it passes down with the face presenting is slightly larger and more unyielding, and consequently requires a somewhat larger space to pass through than in vertex or cranial cases; still we would strongly repudiate any attempt to manage such cases by intermeddling with the child, while we deem it proper and justifiable to use every simple and safe expedient within our power, which may act upon the *mother* so as to render either the uterine action more decisive, or the parturient passages more dilated, or at least more easily dilateable. For this purpose we avert more carefully all causes that could interfere deleteriously with the functions of the uterus, and at the same time, we also have recourse to bloodletting and other measures calculated to fulfil this indication earlier and more decidedly in face than in cranial presentations, when even there is any tendency to delay: the membranes should be allowed to remain as long entire as possible; the rectum and bladder kept empty; and if, as not unfrequently happens, a fold of the cervix uteri happens to be pushed down before the presenting part, it should be very gently relieved and pushed aside; and the perinæum in this, as in all malpresentations of the head, must be guarded with especial care under the unusual degree of distension to which it is subjected.

Causes of delay and difficulty and danger will happen with presentations of the face, as with presentations of the cranium, and from the very same causes which in cranial cases may render the uterine powers insufficient to complete the expulsion of the child. Under such circumstances our interference or non-interference must be regulated by the same principles as in natural or cranial presentations. We will only add one remark, and we do so in consequence of it being omitted by all the authors whose works we are considering:—If in a laborious face case we use the forceps or the vectis as an extracting instrument, we must hold it ever in our recollection to direct at the outlet the chin and not the forehead under the arch of the pubis. If the chin were turned backwards instead of forwards all our physical efforts at the extraction of the head would be utterly fruitless.

Drs. Davis and Ramsbotham discuss in single pages the nature and symptoms of inefficient uterine action as a cause of lingering labour. The subject is one most highly interesting to the practitioner from the frequency with which it occurs, and important as an object of study from our defective knowledge of its pathology. The various *local* morbid causes of delay and difficulty both on the part of the child or body to be expelled, and of the maternal passages through which that body has to pass, have each and all been made objects of successful investigation by various accoucheurs. We do not recollect one author who has endeavoured, on the other hand, to ascertain and fix the varieties of morbid action and states that, by their presence, may interfere with the organ

or organs which are concerned in mechanically expelling the child. And yet we believe, as we have already stated, that, practically speaking, the consideration of this last subject is one of very great moment—perhaps of greater moment than the former.

Dr. Rigby has devoted a very instructive chapter to this topic. It is a chapter which is full of matter calculated to excite the reflection and direct the observations of the obstetric pathologist. We are not certainly prepared to adhere to all the opinions that Dr. Rigby has broached in this part of this work, but we think it requires no stretch of sagacity to predict that it will prove highly useful in rousing the attention of British accoucheurs to the subject, and probably it will be more frequently referred to by after writers than some of the more finished parts of Dr. Rigby's publication.

The mechanical powers by which the child is expelled from the uterus are of two kinds, 1st, the involuntary action of the uterus, assisted, secondly, by the partly voluntary and partly involuntary action of the abdominal muscles and diaphragm. Dr. Rigby, in reference to the morbid conditions of the latter or accessory expulsive powers, states briefly various pathological causes which may more or less impede their action. It would have delighted us if he had entered into this fertile field of enquiry more fully. We have a conviction that the cultivation of it will ere long yield results which will explain the difficulties of many dangerous obstetric cases, and determine more precisely the practice to be pursued in them. The full investigation of the effects of existing cardiac and pulmonary diseases alone upon labour would, we believe, clear up the history and treatment of many at present anomalous cases. Dr. Rigby discusses at considerable length the causes which may retard labour, by inducing a faulty or morbid condition in the involuntary action of the uterus. We wish we had space to extract his views on this matter. For them we earnestly refer our readers to the work itself. We shall give merely some of his opening general observations, to show the spirit in which he treats the subject.

"On the approach of labour, the uterus, which hitherto had been merely performing the office of a receptacle and a means of conveying nourishment to the fœtus, now assumes a totally different character; from being in a nearly passive state, it assumes an entirely opposite condition, namely, of high irritability and powerful action. We might almost suppose, that its connexion with the nervous system was become more close and intimate; for it is now sensible to the influence of impressions which had before produced no effect upon it. Thus, we see, that affections of the mind, even but of moderate intensity, and to which it was before labour nearly if not quite insensible, are now capable either of rousing its efforts to the utmost violence, or of arresting them in the midst of full activity; and on the other hand, we see that where its action has been deranged or interrupted, it gives rise to serious affections of the nervous system, or even convulsions. With all this it now displays peculiarities of function which strikingly distinguish it from all other organs of the body; in some cases it appears to annihilate or to absorb, by its all-pervading influence, the functional energies of other organs; and in spite of its increased nervous power and susceptibility to various impressions, it seems to possess the faculty of continuing its efforts uninfluenced by general disease, unimpaired by exhaustion, and for a time almost independent of the life itself of the mother. In convulsions and paralysis, in general fever and inflammation of vital organs, its powers appear to be undiminished; on the contrary, where the patient from whatever cause is rendered

incapable of assisting its efforts by the abdominal muscles, the uterus will take upon itself the whole task of expelling the child, which will be born apparently without a single effort on the part of the mother. We also observe that organs, the various conditions and derangements of which have exerted little or no influence upon the uterus in its state of quiescence during pregnancy, now affect it powerfully, and are capable of modifying its action very considerably. The stomach, the intestinal canal, and the skin, are remarkable instances of this, and seldom fail to disturb or prevent the natural efforts of the uterus whenever these organs deviate from a healthy condition. It will be, therefore, of the highest importance to watch their functions narrowly, in order that we may form a correct estimate of their effect upon the uterus." (p. 205.)

Dr. Rigby goes on to show that derangements in the contractile powers of the uterus itself may arise from various morbid states of the organ, both functional and mechanical, as from relative inactivity or debility, from derangement in the digestive organs, from affections of the mind, from age and temperament, from general plethora, or what is probably more frequent, from local uterine congestion, from simple or specific inflammation of the uterine tissues, from irregular or spasmodic contraction in its fibres, from organic disease in its fundus, body or cervix, &c. Each of these divisions receives a separate notice, and their nature, diagnosis, and treatment, are excellently commented on, but for their full discussion we must again refer the reader to the work itself.

In the general management of such cases Dr. Rigby makes one remark which we beg to extract, namely, that "the more carefully such cases are investigated, the less frequently will practitioners require ergot and other oxytoxic medicines." We quote this observation because we believe it strictly true, and because we have seen and heard enough to convince us, that from want of attention to it, the ergot is too often given under circumstances for which it is little or not at all adapted, and where its exhibition is attended with deleterious results both to mother and child. We do not repudiate the use of that drug; we would wish merely, as far as such an observation may serve, to guard against its abuse, and we fear that few medicines are actually more abused in practice. When employed, Drs. Davis and Ramsbotham both recommend the ergot to be given in the form of infusion, and in doses containing half a drachm of the powder, every ten or fifteen minutes. Dr. Davis alleges, that the ordinary mode of exhibiting this drug in America is in an infusion of seven or eight grains of the powder every ten minutes. We have not Prescott's Dissertation at hand to refer to, but assuredly either the dose is accidentally understated, or is in itself far too little to be of any certain avail. Dr. Rigby employs the powder of ergot in cinnamon water (a scruple or two of the former to an ounce and a half of the latter), and adds a few grains of borax. The cinnamon is an old oxytoxic, and long ago the borax was held in high repute by some practitioners upon the continent, for its supposed efficacy in exciting or increasing uterine contractions. Lamotte speaks of its employment, and in our own day its use was again revived by Hufeland, L'Offler, and the late Professor Lobstein of Strasburg. Its alleged efficacy is surely a legitimate subject for direct experiments and observations.

Drs. Ramsbotham and Rigby both treat at considerable length of the various local morbid conditions of the maternal passages and of the fœtus and its appendages, which may impede parturition. As causes of this

kind referrible to the ovum, they both describe preternatural thickness and toughness of the membranes; and Dr. Rigby states properly that their premature rupture, as well as their unusual distension with a profuse quantity of liquor amnii, leads occasionally, perhaps more frequently, to the same result. Knots upon the cord are brought forward by Dr. Rigby among his causes of difficult labour from faulty condition of the state of the child. We doubt much the propriety of considering such lesions of the cord under the head in question. They may, as Leveret maintains, occasionally prove hurtful, or even fatal to the child, a result which with Saxtorph, Baudelocque, Gardien, and Rogers, we believe to be extremely rare, but they still more rarely if ever can prove a cause of difficulty in the labour; the knotted cord, as Dr. Rigby himself remarks, generally being of unusual length. This arrangement, as well as that of entering the placenta as a cause of difficult labour, we object to as apt to mislead the young student.

Most modern British accoucheurs have recorded their opinion against the idea of Dr. Denman that shortness of the umbilical cord, either from its original formation or from its being coiled around the neck or limbs of the child, is liable to act as a cause of impediment to the birth of the head at least of the infant. We refer to Dr. Ramsbotham for the usual argument stated against the old opinion, that as the child descends in labour, the fundus and body of the uterus descend with it, until the head is born, so that the same relative distance is maintained during this part of the process of labour, between the two extremities of the cord, as existed during pregnancy. We grant that when very short it may impede the birth of the body, and have seen it do so. We are surprised to see Dr. Rigby on this subject referring to the distinguished Naegele for opinions on it that are common to the general body of English accoucheurs.

As causes of lingering labour on the part of the child itself, we have described in the works under review, strong ossification of the head and its enlargement from natural formation, deformity or disease, accumulations of fluid and tumours of the chest and abdomen, preternatural size of the child, monstrosity; and, lastly, that very rare case which we have described in a former number of this Journal, namely, an anchylosed state of the joints of the fœtus. Dr. Ramsbotham makes some excellent remarks in treating of hydrocephalus as one of the causes in question. It is certainly surprising how little resistance is given by the child's head, even when very considerably distended with fluid; but we are assuredly of Dr. Ramsbotham's opinion, that it is especially dangerous to allow a hydrocephalic head to remain for any considerable time locked in the pelvic cavity. Its compressibility permits it to be moulded into all the irregularities of that cavity, and thus to occasion almost universal pressure on its lining membrane, while the fluidity of its contents adds, on physical principles, to the dangers of these effects. We know one case of this kind, in which a hydrocephalic head produced fatal laceration of the cervix uteri. In another case, where the child presented footling, the spine of the neck, and part of the soft tissues covering it, gave way under the traction employed, and the dropsical head was thus rapidly emptied and allowed to pass.

Accumulations of fluid in the chest or abdomen of the child rarely

occasion any impediment to the delivery. In one instance a fœtus, whose abdomen we afterwards found to measure when redistended with fluid nearly twenty-one inches in circumference, could not be delivered until the diaphragm was lacerated. We have met with one case of impediment to the birth of the body of the child after the head was born from great effusion into the thoracic cavities. In reference to this and other causes impeding delivery of the shoulders and thorax, Dr. Rigby very properly advises us to disengage in the first instance the shoulder that lies under the pubic arch.

Instrumental labours form the next subject in Dr. Ramsbotham's arrangement. In some prefatory remarks to this division of his work, he properly animadverts upon reckless operative interference and its miserable effects, and cautions the young practitioner that urgent necessity alone should warrant him in taking an instrument in his hand. In his practice, as Dr. Ramsbotham very truly observes, he will find it much more difficult to determine the time *when* instrumental aid may have become necessary—and, let us add, the *kind* of instrument to be used—than to administer the aid itself. It requires calm and sound judgment to determine the former points; it requires some mechanical dexterity merely to apply the latter.

Dr. Ramsbotham's *first* order of instrumental cases consists of those which are accomplished by instruments perfectly compatible both with the life of the child and the safety and continuity of the mother's structures (including the short and long forceps, the vectis and fillet); his *second* order comprehends those in which either the child's body must be mutilated (embryotomy), or a cutting operation be performed on the mother's person (Cæsarean and Segaultian operations).

In their chapters on the short forceps both Drs. Ramsbotham and Rigby have stated much that is excellent, but little or nothing so novel as to detain us. They each give us a brief history of the forceps, a subject which we have sufficiently discussed on previous opportunities. Dr. Rigby recommends a pair of curved forceps, having the joint invented by Brüninghausen; namely, a combination of the pivot with the common English lock. Dr. Ramsbotham thinks the short forceps should be straight, and says he is in the habit of using Denman's. There is, we believe, as much or more in the hand that uses the instrument than in the instrument itself, but still we could never see how a curved short forceps could be used either with success or safety in those cases (which we have already shown to be so common), where the face presents to either groin. If the instrument is applied to the head in that position, and before the usual turn is made by the face backwards, and if we adhere to Dr. Rigby's rule that the pelvic curvature of the instrument must correspond with the pelvic curvature of the sacrum, it would be impossible, or next to impossible, to give the head its proper rotation as it passed through the pelvis, without the projecting points of the forceps lacerating the passages. We know that in some such cases the head may be brought out with the face still looking anteriorly, but we know well also that in others the greatest physical force will not effect the delivery in this way, when by merely adapting the head, so that it follows the natural mechanism of such presentations, we are speedily enabled to extract the infant. Dr. Davis, perfectly aware of the difficulty we allude to, and

particularly of the impossibility of using his own short curved forceps if the head required to be rotated, has proposed to employ two new forms of the instrument with unequal right or left hand blades according as the face presented to the right or left groin. It is almost unnecessary to remark that such complex machinery will never find its way into common practice, even if it possessed any of the varied merit which its author ascribes to it.

Dr. Ramsbotham (p. 297) speaks, in applying the first blade, of first carrying as a director two fingers of the left hand "over the uppermost ear." Dr. Rigby, on the other hand (p. 137), remarks that the position of the head must be determined by the direction of the fontanelles and sutures, and not by feeling for the ear.

"The ear can seldom be reached without causing a good deal of pain, even under the most favorable circumstances. In cases where the head is so impacted as to be incapable of advancing by the natural powers, it cannot surely be justifiable to force up the finger between the head and pelvis to ascertain this point, the more so as the soft parts soon become swollen and more or less inflamed, and therefore little able to bear such rude treatment. No operation requires such an intimate acquaintance with the mechanism of parturition as that for applying the forceps; it is simple and generally perfectly easy when the precise position of the head and its relations to the pelvis are accurately known; on the other hand it is not less injurious and painful to the patient than difficult and unsatisfactory to the practitioner." (p. 138.)

The forceps are always applied in the contrary oblique diameter of the pelvis to that in which the long diameter of the child's head is placed; and Dr. Rigby makes in relation to this circumstance one remark which we think of importance in directing the application of the instrument:

"The trochanter major will guide us as to the precise position of the patient's pelvis, and is especially useful in pointing out the direction of the left oblique diameter, in which the forceps (on account of the first position of the head being in the right-oblique diameter) should be most frequently applied; in this case we pass the upper blade beneath, as it were, the trochanter, and the lower one in the opposite direction." (p. 139.)

Drs. Ramsbotham and Rigby direct attention to the importance of always introducing and extracting the instrument in the line of the pelvic axis. Dr. Davis has given a woodcut very illustrative of the rule. All add strong injunctions with regard to the necessity of care and attention in managing the instrument. The following observations of Dr. Ramsbotham on this point are extremely good:

"Cautiously and tenderly must this iron instrument be used! We must recollect that no sensation can be imparted to the operator's hand of any injury that may be done to the woman; and we must remember that one injudicious thrust, one forcible attempt at introduction, one violent effort in extraction, may bruise, may lacerate, may destroy! Bearing in mind, however, the kind of case in which it is useful and admissible; bearing in mind the principle on which it ought to be employed; recollecting that it is a lever of the first kind; that the metallic blades have no feeling, and cannot communicate to our perceptions a knowledge of any mischief we may inflict, we are not likely to fall into any grave error in its application or its use." (p. 299.)

Dr. Rigby has given few remarks upon the long forceps or their application. Dr. Ramsbotham, however, presents us with an exceedingly valuable chapter on this subject. The long forceps is in British practice

an instrument much less generally known, and, as we are inclined to believe, much less generally employed than it ought to be. It is an instrument whose powers should be well known to all. Those who are not prepared to have recourse to its assistance must inevitably, in particular cases in which the head is too high for the short forceps, fall back upon the fearful alternative of mutilating and murdering the infant. Those again who do use it can readily join in the just feeling of pride with which Dr. Ramsbotham speaks of its advantages: "I have," says he, "extracted many children alive by the agency of the long forceps who had been doomed to death by other parties, and who must have been sacrificed to preserve the mother, unless we had possessed this instrument."

The long forceps are especially useful under two sets of circumstances. First, where the child's head is arrested at the brim of the pelvis by any mechanical cause calculated to prevent it passing that obstacle with safety to its own life or to that of its mother, provided its expulsion be left to the unassisted powers of the uterus. The obstacle at the same time must not be so great in degree as to prevent the child's head actually passing if a sufficient additional extractive force be applied. Secondly, the long forceps are, we believe, specially indicated when symptoms or circumstances of complication on the part of the mother arise and show immediate delivery to be necessary, while the head is still at the brim and the membranes have been for some time ruptured.

In construction, the long forceps differ from the short principally in two points, namely, in their greater length and in the divergence of the blades from the shanks, and not from the handles of the instrument. From inattention to this last circumstance, the long forceps of Dr. Haighton cannot, as we have ascertained by experiments upon the dry pelvis, be really employed, for, in consequence of this imperfection in structure, before the head of the child could be well grasped by the blades of the instrument, the parts situated at the outlet of the pelvis are necessarily greatly and injuriously stretched by the diverging shanks. Smellie, Haighton, Hamilton, and Dr. Davis, have recommended long forceps having a double curve, whilst Blundell, Conquest, and Radford use instruments without the pelvic curve; or, in other words, of the same form as the short straight forceps. Dr. Ramsbotham's long forceps have the double curve, and our own experience inclines us to adopt this modification, for we have found that we could fix and use a long forceps possessed of the pelvic curve after a straight pair, from not having such an extensive hold of the head, had repeatedly failed. The long forceps, described and delineated by Dr. Ramsbotham, appear to us to be a good form of the instrument, and we have found it so more than once in practice.

In their mode and place of application, the long forceps differ essentially from the short. The short can be used only after the child's head has completely or nearly completely entered the cavity of the pelvis. Some instruments, indeed, are of such a length, as the forceps of Drs. Orme and Collins, that they could only be applied to the head when it was already pressing upon the perineum. Again, the short forceps are generally if not exclusively fixed over the parietal and temporal bones. On the other hand, the long forceps are applied to the child's head before

it has passed the brim, and they must be introduced and adapted, not as the short forceps, in relation to the *head* of the infant, but in relation to the *pelvis* of the mother. In fact, when the pelvis is either of the standard form, or slightly diminished below it, the long forceps are of such a construction that we cannot, with safety to the perineum, apply their blades antero-posteriorly, in relation to the brim; or, in other words, with one blade behind the symphysis pubis, and another before the promontory of the sacrum. Even in most dry pelves the coccyx prevents the long forceps being applied in this position, provided their extremities be at the same time opened to such an extent as to receive a body the size of the fœtal head. Yet there appears to be no more common error than that the long forceps should be applied in this manner. We have heard it alleged as the proper method by practitioners, who at the same time owned that in those cases in which they examined the hold they had taken of the child's head, they found this marked in such a way by the instrument as to defy their own explanations.

In this country Dr. Davis and Dr. Radford have both shown at length that, in applying the long forceps, we do, as a general rule, seize the child's head more or less antero-posteriorly, and not as with the short forceps, transversely. The simple reason is merely this: from the relative form of the long forceps and pelvis, we can only with safety apply the blades of the former along the *sides* of the brim; the head presents obliquely, or, when the conjugate diameter of the brim is contracted almost transversely; and, consequently, either blade must seize the head more or less obliquely on its anterior and posterior aspects. Saxtorph, Stein, and Weidemann speak of applying even the short forceps without any reference to the position of the head, and in a direction regulated by the pelvis alone. We certainly totally disapprove of such a principle as applied to the use of the short forceps, but it is physically impossible to act otherwise when we employ the long to seize the head at or above the brim.

Dr. Ramsbotham states that Deleurye first pointed out in 1779, that in using the long forceps the blades required to be adapted to the forehead and occiput of the child's head. When Smellie, the great reformer and master in English midwifery, advised, still earlier, that occasionally one blade of the long forceps "be introduced *behind* one ear and its fellow *before* the other," he evidently saw a dawning of the truth in regard to this question. The doctrine was opposed by Baudelocque, and Baudelocque's opinion has been too implicitly adopted by Hamilton, Burns, Dewees, &c. We refer to Dr. Ramsbotham's work for a refutation of Baudelocque's views, as well as for the views of others, who, adopting another extreme, think that one blade should be placed over the occiput and the other actually over the face of the child. The very bent position of the child's head upon its breast, and the proportionate large size of the cranium over the face, prevent this last application, and make the anterior blade impinge upon the brow, and not upon the face. "I have employed," says Dr. Ramsbotham, "this instrument on very numerous occasions, and I never, to my recollection, bruised a single feature. In general," he adds, "the point of the instrument has not ascended further than the eyebrow, or (if the head is transversely placed instead of diagonally) than the root of the nose." In a previous part he states, and from what we have ourselves seen we believe correctly, that the blades

will be generally found to be applied somewhat diagonally, one reaching to the superciliary ridge or upper part of the orbit, and the other exactly opposite to it on the other side of the occiput.

If our space permitted it would be easy to show, by the most simple mechanical rules, that various authors have promulgated an opinion entirely wrong when they have averred that we have more power with the long than with the short forceps in compressing and injuring the head of the child. The child's head, on the contrary, acts against us with the long end of the lever when the long forceps are applied. They are more dangerous to the child, not from the *degree* of pressure, but from its *direction* being in the antero-posterior axis of the head. The long forceps are assuredly a dangerous instrument to the mother if used injudiciously and incautiously. The same remark applies more or less to every instrument and almost to every drug; but there are some cautions that require to be attended to in the use of the long forceps that do not apply to the short. We shall close our remarks on the instrument by quoting some of these cautions as we find them laid down by Dr. Ramsbotham:

"First. We should not apply the long forceps in a case where great distortion exists. It has already been laid down as a general principle, more than once, that, unless the pelvis possess, in its conjugate diameter, three inches of clear available space, we cannot expect a full-grown well-ossified head to pass entire; and through such a diminished aperture we are not to hope that we shall be able to extract it by the forceps. Burns (Op. cit. p. 440), indeed, fixes the limit of the deformity which would indicate the use of the long forceps at that space. Davis (Operat. Mid. p. 230), from the observations he has given us, would lead us to believe that rather more was generally required than that which Burns specifies; and I am inclined to think that unless the pelvis measures at least three inches and a quarter, we shall generally be foiled in our attempts at delivery; or, at least, be disappointed in our hope of extracting the child living.

"Secondly. In introducing each blade, we must be particularly careful that the point slides within the os uteri, and does not run up between the vagina and the neck of the womb, lest we should bruise or lacerate that organ at its junction with the vagina; and especially lest, in attempting to lock the blades, we should pinch its structure between their extremities and the child's head. This mischance cannot happen with the short forceps, because the os uteri must be entirely dilated before the application, and when the longer pair is used, may be avoided by taking care that the point is constantly kept in contact with the fœtal cranium, guided by two fingers previously inserted.

"Thirdly. We should not employ any strenuous endeavours for effecting delivery, nor work with them for too long a period continuously. The longer the instrument, the greater leverage we possess; and it must be evident that each increase of leverage augments our power: we are not only therefore liable to use too much exertion, but we run the risk of making pressure upon structures less capable of sustaining it uninjured, than when we employ the short forceps. Unless, then, a decided advance be evident after a few minutes' well-directed efforts, we should desist from renewing our attempts; and we must judge of the progress we are making, by examining after each backward and forward movement of the instrument. We must most scrupulously avoid using forcible means.

"Fourthly. We must be guarded in our promises regarding terminating the labour by the means we are about to resort to; because it is impossible that we can measure the head accurately while its base remains above the brim, and it is equally impossible that we should be able to form an opinion of the degree of ossification it has acquired, and of its compressibility. In all these points we may be deceived, although we have made ourselves acquainted with the capacity of the pelvis to the greatest nicety; and while such chances of deception exist,

we must be most cautious not to add disappointment to suffering. I have myself, in some instances, been foiled in attempting to extract the head entire through a narrowed aperture, and been obliged, eventually, to have recourse to the perforator, I always feel more satisfied, however, in lessening the head after having made these attempts, because I have good reason to think that nature unassisted would seldom be able to expel a child through a pelvis of such small dimensions as would not admit its passage by the aid of the long forceps; provided in other respects, the case was fitted for the employment of that instrument. I would advise the operator, then, before proceeding to act, not to make a promise of delivery, but merely to state that he is about to do something which will most probably relieve the patient materially, and that, perhaps, he may at once terminate the labour." (p. 337.)

The fillet or lacque is now very generally and very properly discarded from obstetric practice. Dr. Conquest is, as far as we know, the only modern English author who professes to use it. He recommends one made of whalebone, and passed over the chin or occiput of the child, as an occasional substitute for the forceps or lever. (See his Outlines, edit. of 1837, p. 117, and explanation of plate xii.) We doubt if cases in which such an instrument could be thus freely passed, ought to be considered as coming within the range of instrumental labours; and we have no doubt whatever that we have both more efficient and more safe instruments in the forceps and lever, provided artificial assistance were actually required.

Dr. Rigby does not, as far as we observe, speak of either the fillet or vectis. Dr. Ramsbotham has stated in a much more clear and explicit manner than we recollect to have elsewhere met with, the advantages and disadvantages of the vectis or lever as compared with the forceps. We have ourselves no experience of the lever, and hence abstain from offering any remarks upon it. Indeed its employment seems now (and we have made pretty extensive enquiries into this subject) to be confined to the hands of some of the older class of obstetric practitioners. Many of these no doubt use it with great dexterity, but altogether it has always appeared to us a much less useful instrument than the forceps, and, in most respects, one much more difficult and dangerous in its employment.

No surgical operation whatever is, abstractly considered, more revolting to human nature than that of craniotomy or embryulcia: it is at the best a dreadful expedient. In too many instances it implies the direct and deliberate murder of a fellow-being by the hand of the accoucheur. It is one of the few operations, the propriety or non-propriety of which has engaged all the logical subtlety of the metaphysicians. Some of the greatest foreign authorities of the present day doubt if it ever ought to be performed as long as the child is alive. But we have no desire to discuss the question of the morality of the operation. We hold it as a sound and well-established principle in British midwifery that when the lives of both mother and child are in danger, it is lawful and justifiable to sacrifice the life of one in order to save that of the other; and further, in making the selection, the preservation of the life of the parent is ever to be preferred to that of her unborn infant. In the works before us we find nothing new in regard to the indications for craniotomy; and we pass on to a brief consideration of the different modes which the authors follow in the various steps of the operation.

In performing the first part of the operation, namely, the perforation

of the head, Drs. Ramsbotham and Davis both advise us to use Smellie's scissors made with a cutting edge to both sides of each blade; Dr. Rigby recommends the perforator of Naegele, the chief peculiarity of which is that, like the analogous instruments invented by Steidele and Von Busch on the continent, and by Drs. Bone and Holmes in our own country, the blades do not cross at the lock, and the operator can, by closing the handles together with one hand, make their cutting points sufficiently diverge, leaving thus free the fingers of the other hand in the vagina to guard and protect the maternal parts from any threatened injury. Both authors properly advise the perforation to be always made in the presenting parietal bone and not at a suture. Dr. Rigby, after perforating, throws a strong stream of water into the cavity of the skull through a long elastic tube, in order to break down and wash out as much as possible of the substance of the brain, and thus allow the cranial bones to collapse more readily and completely.

Dr. Rigby speaks as if it were desirable in all cases to wait a few hours after perforation is performed before we resort to extraction. His remarks cannot necessarily apply to those instances in which the operation is adopted in consequence of some urgent necessity for delivery on the part of the mother; and in cases in which there is only a slight disproportion between the head and pelvis, there is little or no advantage to be gained in delaying the second part of the operation, particularly if the labour-pains still continue. Where the deformity is great, there can, we conceive on the other hand, be no doubt regarding the propriety of the delay, provided there is no direct danger from pressure of the mutilated head upon any of the soft structures of the mother. In these cases, if we operate sufficiently early, the mother's system does not certainly become hourly (as Dr. Ramsbotham expresses it) more depressed. It is a well-ascertained fact, observed in many such cases by many accoucheurs, that in the interval the woman's strength generally recruits, the incipient congestive swelling of the soft parts often disappears, and the commencing putrefaction of the fœtus instead of adding to the difficulties of the extraction usually greatly facilitates it.

In extracting, after perforation, Dr. Ramsbotham uses the common straight crotchet, and well remarks that simple as the instrument is, it is one of the most difficult in midwifery to procure good, as the least variation in the degree of sharpness of its point makes a considerable difference in its value. He states further that the use of the blunt hook may sometimes supersede the employment of the crotchet when the bones are too loose and fragile to afford a purchase to the latter. The craniotomy forceps are in his opinion more dangerous, and less to be trusted to than the crotchet, but the several objections which he makes to these forceps refer to the instrument in its worst form, with a Smellie-joint and immense teeth, and scarcely apply to the simpler and more efficient varieties of it.

In those cases in which there is not great resistance to the passage of the child after its head is reduced, Dr. Rigby advises us to avoid extracting instruments, and to insert the finger only into the perforation of the cranium, and to act with it as a blunt hook. In this way, if assisted by the pains, we may often certainly be enabled to exert a sufficient degree of force to bring the head through the pelvic cavity. In cases, again, where the resistance is greater, and still there is no very unusual degree

of deformity, he recommends us to grasp and compress the head with the common curved forceps, and use them as an extractor. "On several occasions," he remarks, "where the craniotomy forceps and crotchet have failed to move the head, the midwifery forceps has been applied, and the delivery easily and quickly accomplished." In fact, occasionally when we fail with the long forceps they may be left on till the skull is perforated, and then used successfully to compress and extract the mutilated head. Dr. Rigby prefers the simple single crotchet when any such instrument requires to be ultimately resorted to.

It would be tedious to enter into the various modifications of crotchets, &c., which Dr. Davis has proposed for extraction; and his opinions on these subjects are already so generally known to the obstetric profession that they need not delay us.

We beg to add only one remark in regard to extraction. Whatever means or whatever instruments are employed to effect it, we have long been of opinion that one great principle should direct their application, namely, they ought to be fixed in such places and used in such ways as will enable us to bring down the head in its most natural and consequently most favorable *position*. We have long been convinced that the great peril not merely of injury but death to the mother from this operation, arises in a great measure from the head being often so altered in its presentation, and hence necessarily increased in its diameters by the mode and place in which the crotchet is fixed, that the cranium is at last torn as it were through the pelvic passages by pure physical force alone, while a little attention to the proper adaptation of the instrument, so that the head may be brought down in a more favorable position will generally be found to lessen immensely both the difficulty and the danger of the operation. We speak here of the more common cases of embryotomy. We are perfectly aware that when the pelvis is much contracted, we are obliged, as has been well pointed out by Dr. Hull, to alter in different ways the presentation of the head, always, however, bringing it into such positions that its diameters are in each case those requiring the least possible space.

Our remarks on the mode of operating by the forceps and crotchet have extended so far as to prevent us entering into the respective indications for the use of the one or other of these instruments. We had wished to have shown statistically that even as regards the *mother* craniotomy has in general proved an infinitely more dangerous operation than the forceps, and we have the greatest doubts of the soundness of the rule that in individual cases the presence of signs of death of the child ought to determine our having recourse to the perforator and crotchet in preference to the forceps. The rule is a dangerous one in two points of view: First, because, as we have just stated, craniotomy seems attended with actually greater risk to the mother; and secondly, because it is exceedingly difficult to make ourselves certain in special instances of the death of the infant. All negative symptoms of its death are very equivocal; even if the cord be felt pulseless still we may not be certain that it is the cord of the child whose head is presenting. Dr. Ramsbotham has properly shown this one source of error, that it may happen to be the cord of another twin infant. The stethoscope in some of these doubtful instances is of the greatest use in determining the presence of *life* in

the infant; and in thus encouraging us to persevere as long as possible in other means (as in the use of the long or short forceps) before we have recourse to the final and fatal operation of craniotomy. By its aid in this way we have been induced to use the forceps, and with the happy result of saving the child, when, previously to auscultation, its head had been doomed to perforation by others. We do not however, on the other hand, hold that because the sound of the fœtal heart cannot be heard the indication is of equal value. We do not perform craniotomy in individual cases because the child is dead, but because the mother's life is in danger, and delivery cannot be completed by other means less safe to her. That the employment of the forceps is, as a general rule, safer to the mother herself than the employment of the crotchet is a proposition which Dr. Churchill, of Dublin, and others have proved by ample statistical data. Our German and French neighbours have of late years, in consequence of this and other reasons, been employing the forceps much more and the crotchet much less frequently than we do in Great Britain. We trust yet to see the same reform work its way into English obstetric practice.

We cannot pass from this subject without adverting to a singular mistake into which Dr. Ramsbotham has fallen in speaking of the stethoscope, as "the most satisfactory means of ascertaining the state of fœtal vitality." (p. 361.) He mentions both the "tick" of the fœtal heart and the placental *souffle* as "means that have been resorted to for the purpose of ascertaining whether the child be alive under lingering labour." Now we venture to assert that no one who really knows practically obstetric auscultation ever resorted to the placental *souffle* for the purpose alleged, because it has been long and often proved that this sound may and generally does continue after the death of the child in labour, and until the uterus is contracted and the placenta separated. The presence or absence of the sounds of the fœtal heart is the only proper auscultatory criterion of the presence or absence of fœtal life.

In none of the treatises before us do we find any account of the new continental operation of cephalotripsy, or crushing of the infant's head. It has been practised now with alleged success by the younger Baudelocque, by Champion, Paul Dubois, Halmagrand, Von Busch, and others, and the high names of these men might have entitled it to some passing notice from our authors.

The Sigaultian section in its different modifications is altogether so useless and dangerous an expedient that neither Dr. Rigby nor Dr. Davis treat of it. They both, however, as well as Dr. Ramsbotham, speak at some length of the Cæsarean section. Dr. Rigby gives us some of the statistics of the operation upon the continent and in this country. In the British islands we have only three cases of perfect recovery recorded, viz. in the instances operated on by Mary Dunally, Dr. Barlow, and Mr. Knowles. Six or seven successful operations have been of late years published by different American authors. Dr. Ramsbotham adds an elegant and classical history of the Cæsarean operation in his Appendix. He there makes Shakspeare in his Macbeth declare Macduff as not "one of woman born" for the purpose of enhancing the hero's character by investing his birth with circumstances of an extraordinary nature. This supposition is more ingenious in theory than true in point of fact. The

story and almost the very words which Shakspeare uses in the dialogue in which Macduff's alleged Cæsarean birth are brought out were no doubt taken from the Chronicles of Hollinshed, or of Boece whom Hollinshed strictly follows in this subject. Indeed the tale of Boece as set forth in the language of his old translator Bellenden, is certainly far too circumstantial to be historically true, but some of the expressions, as that of "*shorn* from my mother's womb," of the ancient Scottish historian, is little less poetical than the corresponding "untimely *ript*" of the great poet himself.

We have only gone through a small portion of the truly obstetric and practical part of the important volumes before us, and yet the unexpected length to which our remarks have already extended reminds us that, for the present, we must bring our observations to a conclusion. The chapters of Dr. Rigby and Dr. Ramsbotham on Preternatural and Complex Labours contain many interesting and excellent views on some of the forms of morbid parturition included within these two classes. We can only, however, in the meantime refer our obstetric readers to the study of them in the works of the authors themselves, sincerely regretting our inability to lay before them the opinions and practice, in these and other cases, of such distinguished accoucheurs.

In some of our late Numbers, we have had an opportunity of speaking of the various morbid states of the pelvis and soft parts of the mother, that may offer obstacles to the safe and natural delivery of the child, and hence we deem it unnecessary to examine at length those parts of Drs. Ramsbotham and Rigby's works that are devoted to the consideration of this subject. The chapters in both, that relate to it, we can confidently recommend to our readers. No error is more common than that of supposing that exostosis of the pelvis is a frequent disease, and an easily discoverable cause of difficult labour. Naegele has forcibly shown, that most of these supposed exostoses consist merely of a protrusion inwards of the promontory of the sacrum. We are happy to find that a man of Dr. Ramsbotham's extensive experience so far confirms Naegele's views. It is a disease "fortunately," he says, "of very rare occurrence, indeed so infrequent that I have myself never met with an instance." We cannot agree however with the remark that follows, that these exostoses are "generally situated at the back part behind the rectum, and spring from the cavity of the sacrum." In the recorded cases, and in the preparations of them which we have ourselves seen, these exostoses were, in a large proportion of instances, attached to the inner surfaces of one or other of the pelvic joints.

It will be sufficiently apparent, from the whole tenor of our remarks, that we regard the treatises at the head of the present article, particularly those of Drs. Ramsbotham and Rigby, as very valuable additions to English obstetric literature. On certain points, as we have shown, we differ from these authors in their opinions; but, generally speaking, we are happy to find our own principles and practice corroborated by their authority. The freedom with which we may have criticised them on individual topics has arisen more from the position in which we are placed by the very excellence of the works themselves, and the high opinion which we entertain of the judgment and skill of their authors. Dr. Davis's work, though still full of faults, is in its present abridged form in many

respects much improved as a compendium of obstetric medicine. Dr. Ramsbotham's treatise we regard as an exceedingly excellent synopsis of practical midwifery. It is rich in minute and sound rules of treatment, and all that the practical accoucheur most needs to know. The very superior style in which the plates are executed reflects the greatest credit on the enterprise of the publisher. Dr. Rigby's work contains so many views comparatively new to the English accoucheur, and brings before us prominently so many of the opinions of the German schools, and is written with such a perfect knowledge, theoretical and practical, of the whole subject, that we entertain little doubt but that it will be soon recognized as an important gift both to the actual practitioner and to the mere student.

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## ART. XIII.

*Three Memoirs on the Development and Structure of the Teeth and Epithelium, read at the Ninth Annual Meeting of the British Association in August* 1839; *with the Diagrams exhibited in illustration of them.* By ALEXANDER NASMYTH, F.L.S. F.G.S. &c.—*London*, 1841. 8vo, pp. 47.

IT appears, from a long but temperately-written introduction to this small work, that the abstract of the memoirs of which it consists, after having been printed with a view to publication in the Transactions of the Association, was afterwards suppressed, on what certainly appears to us most insufficient grounds, and, notwithstanding the author's remonstrances, has not yet been published by the Association. We have neither time nor inclination to enter into the merits of the controversy between the author and the council of the Association, though we think it clear that he has been very ill treated. Our space will permit us merely to give a condensed analysis of the contents of the memoirs, which we submit to our readers without other remark than that Mr. Nasmyth deserves the highest credit for the zeal, industry, and intelligence with which he appears to have conducted his researches.

*The Pulp.* This is principally composed of cells or vesicles, varying extremely in size and shape, and disposed in layers. Its substance is traversed by vessels of which the direction is generally vertical. The vessels do not penetrate the reticular surface of the pulp, but are still so closely connected with it that they are often lacerated in attempting to separate the ivory from the pulp. The nature of the contents of the cells cannot be exactly ascertained. They frequently shrink or collapse to such an extent that the pulp nearly disappears, and this seems to take place more decidedly when the tooth has been in a healthy state, and more frequently in adult than in temporary teeth. In the body of the pulp they are arranged in ill-defined layers; at its surface they are more regularly disposed, more distinctly cellular, and are arranged in reticular leaflets; in short, at the surface they are prepared by some peculiar change in their form and arrangement for the reception of earthy matter, or in other words for their transition into ivory. The compartments of

this beautiful reticular, formative surface are oval and overlap one another. Each compartment on being insulated is found to be curiously and regularly organized. This reticular conformation of the surface of the pulp presents peculiar diversities in different animals. Mr. Nasmyth first observed them in the human subject; afterwards in all other animals which he examined, varying in size and arrangement in different cases. In the human subject the reticular leaflets have a well-defined scolloped border, on which at regular intervals processes are observed. It is into the cells of these reticular leaflets that the osseous matter is deposited, constituting the ivory of the tooth; the precise mode in which this process is effected Mr. Nasmyth has not been able to discover. On the completion of this process the leaflet becomes a layer of perfect ivory, in connexion with the subjacent reticular leaflet, which is now in its turn become part of the formative surface of the pulp; by the time that this latter has been ossified, the same process has commenced in the leaflet beneath it; and the same phenomena are renewed in each successive leaflet or layer until the whole of the tooth is formed. Mr. Nasmyth's diagrams in illustration of this interesting portion of his subject are *fac-similes* of preparations showing the surface of the pulp under a variety of circumstances. In some it is entire; in others it is more or less damaged by disruption from the already ossified layers above, and is strewed with cellular fragments of those layers, and presents also patches denoting extravasated fluid containing probably osseous matter. The whole appearances are those of a peculiar system of cells external to the peripheral ramifications of the blood-vessels, the functions of which are highly interesting, and the study of which will ultimately lead, the author thinks, to the unveiling of many vital processes connected with the growth of animal tissues, which are at present shrouded in obscurity.

The cells of the reticular surface appear, during their transformation into ivory, to be subdivided into minute cellules, for they present the appearance of being filled with granules in progressive stages of development. The reticular cells, as is shown in Mr. Nasmyth's diagrams, have a regular fibrous framework, which, he thinks, becomes, on the deposition of ossific matter within the former, the fibres of the ivory. The diameter of the fibrous framework is precisely that of the fibres of the ivory, and the points or projections observed on the surface of the pulp, when the superincumbent ivory has been separated from it, may be traced to belong to the substance of the latter. Whilst forming a portion of the pulp these fibres are in a great measure coiled up; on the distension of the interfibrous cells by ossific matter they are drawn out, but still continue to a certain extent spirally curved.

The foregoing may be regarded as the synthetic portion of the author's observations; we now proceed to his analytic researches; for not contented with tracing the formation of ivory by the ossific transition of the pulp, he has followed out his investigations by restoring it as far as possible to what may be termed its pulpal state, by submitting it to the action of acid, and thus emptying the cells of their ossific contents. One of his diagrams shows the appearance of the ivory when the earthy matter has been almost entirely removed by acid, but where the cells still retain their position, general appearance, and connexion with each other. An-

other represents a more advanced stage of decomposition where there seem to be attached to each fibre minute lateral filaments, which he presumes to be the remaining portions of the emptied cells. He compares these cells in the former diagram, where they retain the erect distended form which they acquired by the deposition within them of ossific matter, with their original state in the reticular surface of the pulp before its ossification, where they lie collapsed one above another. The fibres of the ivory, on being submitted to the action of acid, present a peculiar baccated appearance. The size and relative position of their portions or divisions differ in various series of animals. In the human subject each compartment is of an oval shape, and its long small extremity is in apposition to the one next adjoining. In some species of the monkey-tribe, the fibre appears to be composed of two rows of compartments parallel to each other. In the ourang-utan the fibre is composed of rhomboidal divisions. In another part of the work he states that the convoluted fibre of the pulp appears, on microscopic observation, to be made up of single successive granules. Nothing can show more forcibly the progress made by the author in this department of science, and the extent to which he differs from previous enquiries, than the fact that the best writers hitherto, including even Purkinje, Müller, and Retzius, either positively state that the interfibrous substance of the ivory is structureless (*strukturlos* is the word made use of by Müller in speaking of Purkinje's researches), or else, though it forms four fifths of the substance of the tooth, pass it over without notice. Mr. Nasmyth appears to have proved the fact of its organization both synthetically and analytically; and from a microscopic examination of perfect ivory, he concludes that it presents in different animals peculiarities so remarkably well defined as to furnish a most important and interesting accession to the odontographic basis for a classification of the animal kingdom. Nothing can be more distinct from the interfibrous cells described by Mr. Nasmyth than the corpuscles or cells, represented by Retzius as existing, though scarcely demonstrable, in human teeth, towards the periphery of the ivory, and in which, according to him, the dental tubes terminate.

The enamel, from Mr. Nasmyth's observations, appears to present compartments or divisions, but of a different character from those of the interfibrous substance of the ivory. Each compartment of the enamel is of a semicircular form, and the convexity of the semicircle or arch looks upwards towards the free external portion of the tooth. The enamel, the author states, is covered in all instances by a layer of *crusta petrosa*, in which he has distinctly traced the corpuscles of Purkinje analogous to those found in bone.

*The Epithelium.* This Mr. Nasmyth defines to be a layer of non-vascular but yet organized substance, covering the vascular surface of mucous membranes. Both the cuticle and epithelium are formed, it appears to him, from a fluid secretion on the surface of the chorion. They are composed of particles which pass through various stages of development, viz., first, that of the formation of nuclei and corpuscles; second, that of cells; third, the growth of the latter by vital imbibition; fourth, their compression and gradual conversion into minute lamellæ or scales. In a section of the epithelium and mucous membrane, numerous nuclei are found in opposition to the latter, which more externally are surrounded

by a cell, whilst at the surface this cell is found compressed, and assuming the appearance of a scale. The epithelial cells are connected together by an elastic gelatinous substance interspersed with minute granular bodies. From the surface of the skin and mucous membranes, the external layer of scales is continually being thrown off, in consequence of pressure from those in process of development beneath. After cuticular lamellæ have been detached, their place is regularly occupied again by newly-formed scales.

It appears to the author that the component parts of the cuticle and epithelium have within themselves a power of growth; it remains for pathologists to determine what share the derangement of this function has in the production of cutaneous diseases. Mr. Nasmyth concludes this division of his subject by a more detailed description of the epithelium of the mouth, which in the fœtal subject forms a white, dense, projecting layer on the alveolar arch, which has been erroneously termed dental cartilage. In the interior of this alveolar epithelium, where it corresponds to the molar teeth, small vesicles may frequently be observed, varying in size from a quarter to an eighth of a line in diameter. Under the microscope their parietes are found to consist of alternated scales, and their cavity to contain a fluid abounding in minute granules or cells. The internal surface of the alveolar epithelium frequently presents concavities corresponding to larger vesicles on the mucous membrane, which contain floating globules and scales similar to those of the epithelium. Its external surface presents, after slight maceration, numerous fringed processes composed of elongated scales.

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## Art. XIV.

*Die Venerische Krankheit der Pferde.* Von J. L. Harthausen, der Arznei-und Wund-Arzneikunde Doctor, &c. &c.—*Breslau*, 1839.
*The Venereal Disease of the Horse.* By Dr. J. L. Harthausen, &c. —*Breslau*, 1839. 8vo, pp. 60.

This is a notice of a disease occurring among stallions and broodmares, which it is stated "is developed under peculiar miasmatic and individual relations, and is then communicated, only however by contagion, in the act of copulation." It is characterized "by marked and continued depression of the nervous power; by mucous discharge, especially from the parts of generation; by disturbed functions of the cutaneous, lymphatic, and glandular systems; by consequent formation of ulcers upon and of knots in and under the skin; by paralysis of the hinder extremities; and finally, by fatal emaciation." This affection, although probably of prior occurrence, does not appear to have been distinctly recognized before the year 1818; but between this period and the year 1821 a disease with symptoms as above mentioned seems to have prevailed in Styria. A similar affection showed itself in Bohemia in the year 1826-7, and continued for several years, and both before and since the same disease appears to have visited West Prussia, Lithuania, Hanover, and part of Switzerland. In the spring of the year 1833 two sporadic and isolated cases came to the knowledge of the official

authorities in the village of Steindorf, on the borders of Austrian Silesia. Three years subsequent to this period, during an epidemic of catarrhal rheumatism, the strangles (glandular disease of the throat) being also prevalent, the disease occurred, pretty generally spread and very severe, in the department of Neisser, always however only after a previous covering, and in far greater proportion after covering by private stallions than by those belonging to studs. It is remarkable that, notwithstanding careful examination, not the slightest trace of disease was detected in the greater number of the stallions. The disease again appeared during the month of March of the year 1838, but was checked by the observance of appropriate regulations. (pp. 8-11.)

The morbid appearances in the genital organs usually show themselves in a few weeks, sometimes however in a few days, after the act of copulation; but a peculiar depression, resembling narcotism, may be observed within the first three days. This state of the nervous system has been confounded, it would seem, with that peculiar depression observed to follow the act of coition, which has given rise to the aphorism, *post coitum animal triste;* and the symptoms of local irritation, the frequent micturition and looseness, the repeated switching of the tail, the restless state, and alternate raising and extension of the hind-foot, have been no less confounded with the results of the natural excitement of the genitals at these times. In the more severe cases, after a certain time, there is swelling of the parts of generation, without either pain or sensibility on pressure, heat or increase of temperature, but attended rather with a sense of coldness to the touch. This swelling commences at the under part of the pouch or entrance of the vagina, from which it spreads round the orifice, and not unfrequently to the perineum and udder, &c. Upon closer examination of the parts, the mucous membrane of the vagina is found to be relaxed, slightly reddened, moderately swollen, and covered with a viscid mucus resembling the white of egg.

"The redness shows itself, without increase of the natural temperature, as an equally diffused tinge, which, here and there, at indeterminate points, higher and deeper, is traversed in an isolated course by injected capillary vessels. These are collected in groups, and form, though less frequently, pale wine-red spots from a quarter to half an inch in diameter. When they occur in the *fossa navicularis* of a deeper red and wreathed, a small black point, the orifice of a sebaceous crypt, may be observed in the centre, from which coagulated particles of caseous matter may be pressed out in unusual quantity." (p. 12.)

The discharge from the vagina, which commonly makes its appearance before the swelling, is at first neither very abundant nor discoloured, and is apparently nothing more than an increased secretion of the natural mucus; but in the further progress of the disease it becomes not only viscid, ropy, and of a reddish-yellow colour, but the secretion is gradually altered in its characters. From being bland and affording no trace of either acid or alkaline properties, it is found to contain free phosphoric acid, showing great tendency to coagulate and form incrustations, so that the cleft of the vagina and the neighbouring parts frequently become covered with yellow or red-brown amber-coloured crusts. The discharge of the mucus, which had been frequently so considerable as thoroughly to moisten the tail and hind-feet, becomes now periodical, a viscid secretion collecting within the vagina, and only exuding therefrom on the

movement of the animal. The vaginal membrane becomes flaccid and wrinkled, and of a pale yellow, grayish-white, and sometimes of a livid appearance. The cold œdematous swelling of the labia (*Tasche*) and perineum sinks remarkably, forming loose folds, and the relaxation extending so far that in the greater number of cases the cleft of the vagina gapes and the clitoris becomes exposed. In the natural course of the disease, or in consequence of the absorption of the vitiated secretion, favoured by its retention within the vagina, there appear, sparingly scattered over the external surface of the labia, small phlyctænæ, which rapidly pass into chapped ulcerations incrusted with lymph. The crusts are quite superficial, circular, and never exceed the size of a fourpenny piece, very rarely becoming confluent or running into each other, but remaining distinct like the crusts of the varioloid eruptions. Similar ulcerations have been elsewhere observed situated on the mucous membrane of the vagina; but Dr. Harthausen has never seen an example of this in his own neighbourhood, although he has noticed that the mucous membrane at this period of the disorder has assumed a marbled appearance from white and livid spots on its surface. The extension of these ulcerations to other parts follows a peculiar course, the eruption spreading from the genital organs forward to the rest of the body, and occupying the head and throat in countless numbers, but never appearing on the extremities. After the healing of the ulcers, innumerable isolated milk-white spots deprived of the hair are left behind, so that the horse appears as if speckled. These spots never present any inequality of surface, pitting, or marks of cicatrization, the naked skin being on the contrary perfectly smooth, and differing from the healthy skin in nothing but its conspicuously milk-white colour. (pp. 13-4.)

Accompanying and frequently preceding the local symptoms is a general failure or depression of the nervous power; the senses become blunted, the expression of the animal is dull and heavy, motion of any kind is effected with difficulty, and there is evident loss of power in the loins and hinder extremities. This state cannot be attributed to debility arising from the profuseness of the discharge or from emaciation, but seems to be owing, as Dr. Harthausen thinks, purely to an impression made on the nervous system, since frequently when most severe the discharge has been by no means extensive. Neither does it appear to be owing to absorption of the altered secretion, as it occasionally precedes the occurrence of the local symptoms.

After the ulceration has continued for some time, knots or tubercles of various dimensions are developed beneath the skin: some of these are as large as a dollar in circumference, but the greater number do not exceed the transverse diameter of a walnut. They are smooth, firm, and elastic, circular in form, and sharp-edged, and altogether similar to those produced by the bites or stings of insects. There is neither pain on pressure, nor any remarkable redness or heat of the parts, and neither fever nor any appreciable premonitory symptom has been observed to precede their occurrence. They first make their appearance upon the haunches, then upon the head and throat, afterwards upon the shoulders, breast, and other parts of the body, but have never yet been observed on the extremities. When they open on the surface, which very rarely occurs, a sero-lymphatic matter exudes; after a time this dis-

charge dries up and the tubercles disappear. The duration of these, as well as of the tubercles from which no discharge takes place, is uncertain, and the latter frequently disappear from the place which they occupy as suddenly as they had been previously developed there. (pp. 16-7.)

In proportion to the duration and number of the tubercles beneath the skin, and the ulcers on the surface, is the liability of the large glands to take part in this disease: swelling of these glands takes place, and in very many instances a chronic glandular affection is the consequence, which not unfrequently terminates in glanders, and, when the formation of the tubercles has prevailed throughout the course of the disease and the morbid secretion has been checked, in farcy, rapidly followed by a fatal result. Glanders and farcy are however by no means the most usual terminations of the disease. The ulcers and tubercles having continued for a few weeks, and gradually gaining firmer hold upon the system, a general state of relaxation more commonly ensues. Twitches or shiverings (zuckende Bewegungen, wie bei den Empfindungen des Frostes), never of the whole body, but occupying individual layers of muscles only, and in single muscles resembling the catchings produced by galvanism, may be observed. These partial rigors are accompanied by extreme weakness of the posterior parts, and are followed by paralysis, varying in the site which it occupies and the degree to which it attains without any regularity or apparent dependence upon lesion of the central organs. In some singular instances the loss of power affects one ear or the under lip, in others the extreme portion only of these organs, so that the tip of the ear, for instance, is bent at an acute angle as if the cartilage was broken: from this circumstance Dr. Harthausen infers that the paralysis does not exclusively proceed from the nervous centres, but may be owing also to affection of the extremities of the nervous trunks. Finally, the paralytic affection varies as much in the form which it assumes as in degree, in situation, and in the symptoms accompanying it. Cases occur in which its origin seems to be respectively in the brain, in the spinal marrow; in the nervous centres, in their peripheral extremities; while in some instances sensation and motion are equally impeded, in others the motor nerves alone are affected. (p. 19.) In a few weeks the animal sinks under these symptoms; the ulcers and tubercles in some instances declining before death by desquamation of the skin, in others gradually disappearing. The entire duration of the affection is from four to nine months, during the whole of which time the pulse continues slow and sluggish, almost without change, to the end.

The foregoing account refers to the disease as it appears in the mare; as it appears amongst stallions it differs in that the constitutional symptoms precede the local affection of the genitals,—emaciation, weakness of the loins and hinder extremities, paralysis of different parts, and symptoms of a diseased state of the lymphatic and glandular systems, preceding the occurrence of swelling of the testicles, vesicular eruption in the penis, &c. This is however stated to be the case only where the disease is of spontaneous origin, or arises without evident cause; where it has been communicated by contact the progress is, as in the mare, from without inwards. The first symptoms of the infection are swelling of the prepuce and the appearance of small miliary vesicles on the penis,

which give rise to slight excoriations and the formation of thin lymphy incrustations. The vesicles are usually first formed at the extreme point of the urethra, and, as well as the excoriations, are of the same pale rose-red colour as the lining membrane of the canal; they are also visible even from the commencement, in greater or less number, in the fold of the prepuce, at the base of the glans penis. Subsequently there is swelling of the testes, followed by irritation of the lymphatics of the groins and the development of superficial lymphatic tubercles upon the skin about the loins, and the disease then pursues its course with the formation of superficial ulcerations, &c., as before described in the mare.

The preceding details are sufficient to characterize this severe and dangerous affection should it be found to exist in this country; but further and closer research is required before the question as to the origin of the symptoms from a specific virus can be determined. It would seem to be frequently of spontaneous origin, both in the horse and in the mare, and at the same time appears to be communicable from the one to the other. There is however some cause to think that this communication may take place rather as a consequence of the local excitement depending upon the generative act, under a peculiar epidemic constitution predisposing to the development of glandular disease, than as a result of the absorption of a specific poison. The appearances after death, as may be conjectured, present nothing peculiar, and throw little light on the nature of the disease. Those most frequently observed are morbid alterations of the mucous membranes of the absorbent vessels and glands, and of the brain and spinal marrow. The changes of structure in the brain, and especially in the spinal marrow, are the most important, there being few cases recorded in which softening of the substance of some portion of these organs, congestion of the membranes, or serous effusion and infiltration did not occur.

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## Art. XV.

*Om de Sanitaire Forholde i Fængsler efter nyere Systemer.* Ved Professor Fred. Holst, m.d.—*Christiania*, 1840. 8vo, pp. 30.

*On the Influence of the Systems recently adopted in various Prisons upon the Health of their Inmates.* By Professor Fred. Holst, m.d. —*Christiania*, 1840.

At the meeting of the Scandinavian Naturalists in Copenhagen in July, 1840, Professor Holst of Christiania gave a long and interesting account of the sanitary results which have been obtained under those systems of prison discipline which have recently been adopted in Europe and America. The learned Professor reminds his assembled colleagues that he addressed them upon this subject at the meeting of the previous year in Gothenborg, and hopes that he then fully proved that the Philadelphia system, as adopted at Cherry Hill (United States) and elsewhere, was that which presented the most advantageous results, both in a moral and economical point of view. The object of again bringing this subject before the meeting was to discuss the important question of the effects of this and of other systems upon the health of the inmates of those prisons where they have been adopted. Both the Auburn and

Philadelphia systems have been decried as more injurious to the bodily and mental powers of the prisoners than any of the older plans of discipline in prisons, and this blame has in particular been attached to that of Philadelphia. Two years ago these heavy charges obtained additional weight from being corroborated by two individuals whose position gave no small increase to the value of their testimony, viz. the physicians Gosse and Coindet of Geneva.

The efficacy of the sanitary regulations of a prison will be best determined by the relative degrees of sickness and mortality within its walls. But in this regard we experience the greatest difficulty in obtaining correct reports. Many individuals are entered upon the sick-list by one physician who by another would be passed by as healthy; and again we must take into consideration the average duration of each individual's term of punishment, for unquestionably the longer an individual is confined within the walls of a gaol the greater is the probability that his health will become seriously affected.

Having adverted to these sources of error, Professor Holst presents us with a table of the relative mortality in those prisons of Europe and America which are regulated on the plans of Auburn and Philadelphia, as well as in those conducted on the older systems. From this table it appears that the mortality, *in proportion to the general mortality of the country*, has been always greatest in the prisons conducted on the older plans, and least in those which are regulated according to the system of Philadelphia. But on referring to the table we find the highest *real* mortality to occur in Cherry Hill prison (Philadelphian system), the deaths being there as 1 in 26, while in the Glasgow Bridewell, under the same system of discipline, the mortality was only as 1 to 55. Professor Holst maintains, however, that we cannot form any accurate conclusions from the high rate of mortality in the prisons of Cherry Hill and Pittsburg, as in these two places of confinement is to be found a much greater proportion of the coloured population of the country than in the other prisons of the United States.

Out of the results of these investigations Professor Holst has constructed another table, showing the excess of mortality in prisons under the three different systems referred to, over the general mortality of the country.

| Mortality in Prisons. | Mortality of the general population of the country. | Excess of Mortality in Prisons. |
|---|---|---|
| Previous Systems.<br>1 : 27 = 3·70 per cent. | 1 : 42 = 2·38 per cent. | 1·32 per cent. |
| Auburn System.<br>1 : 35 = 2·86 | 1 : 47 = 2·13 | 0·73 |
| Philadelphia System.<br>1 : 40 = 2·50 | 1 : 47 = 2·13 | 0·37 |

It is obvious from this last table, which admits of a far more practical application than the former one, that the causes of this unequal ratio of mortality must be sought for in the difference of the systems themselves.

It cannot be expected that individuals deprived of their liberty by being confined within the walls of a prison should enjoy such perfect health as the free population of the country.

"Let us then suppose," says Prof. Holst, "that all the necessary conditions for preserving the health of the prisoners have been fulfilled, and this under the two recent systems is unquestionably the case, as well as in some other gaols, where the new discpline has not as yet been adopted. ..... But in these last the prisoner enjoys many advantages and privileges which are either absolutely prohibited, or conceded on a much more limited scale, under the recent systems of Auburn and of Philadelphia. ..... For instance the prisoner in the unreformed gaols passes both the day and the night in company with his fellow-captives; he is permitted in many cases to converse with his friends and with his relatives, and to take abundant exercise in the open air, all which advantages are highly valued by those in confinement." (p. 8.)

Professor Holst then proceeds to lay before his auditors a short account of the systems of Auburn and Philadelphia. Under the Auburn system, each prisoner is shut up at night in a solitary cell, and during the day the inmates of the prison labour indeed in community, but the strictest silence is enforced. Besides this they are daily permitted to pass a certain time in the open air. Silence is in fact the groundwork of this system, and the observance of this law is considered of the greatest importance. But offences against this rule are naturally numerous, and the minds of the prisoners are exasperated by repeated and severe punishments for such transgressions. In the House of Correction at Geneva, where a modification of the Auburn system is pursued, various privileges were for some years conceded to the inmates, by which indeed the number of punishments for offences within the prison walls was considerably diminished, but relapses into crime on being released became in a corresponding degree more frequent. Since 1833 a severer system has been adopted in this prison, and the average of relapses has diminished from 33 to 7½ per cent.

The main principle of the Philadelphia system consists in the total separation of the prisoners one from another. Both day and night they remain perfectly apart; but silence is not enjoined, and their cells are considerably larger than those under the Auburn system. In the House of Correction at Cherry Hill (United States), the prisoner is *never* permitted to leave his cell; in Glasgow he takes his solitary walk in the corridors of the gaol; in the Millbank Penitentiary and at Lausanne, the prisoners meet for exercise in the open air, but silence is then strictly enforced. The prisoner is visited daily by the different officers of the gaol, who are expected to observe a considerate and kind manner towards those under their charge, and corporal punishment is unknown. By these means the inmates are brought to expect with pleasure the visits of their gaolers, and that spirit of hostility so often observed in those confined in other prisons towards their officers is here hardly ever to be met with. It appears obvious that, in prisons regulated under either of these two recent systems, the causes of the unequal rate of mortality must be sought for in the difference of the discipline in operation, and in the nature of the means by which that discipline is maintained.

The disorders most prevalent in prisons are obstructions of the intestinal canal, dropsy, phthisis pulmonalis, scrofula, diarrhœa, rheumatism, hysteria, &c. &c., and of all these, chronic thoracic disease is undoubtedly the most prevalent. According to Dr. Bâche of Cherry Hill Prison (United States), three fourths of the deaths in that establishment proceed from this cause alone, and of these 37 per cent. died of phthisis pulmo-

nalis. It is worthy of remark that the mortality from this last-named disease is much more serious under the Auburn than under the Philadelphia system. Coindet suggests that the strict silence enjoined under the former plan may have a most deleterious influence upon the organs of respiration of those subjected to it, and also in a less direct manner upon their whole digestive system. But the perpetual seclusion from the open air, as is practised at Cherry Hill and Pittsburg (United States), cannot fail also to prove highly favorable to the development of thoracic disease. It is singular that the epidemic cholera never attacked the inmates of the prisons conducted on the Philadelphia systems, while it raged with violence in many of the others.

But the most important charge hitherto brought forward against the recent systems is that of increasing greatly the number of insane persons among the inmates of the gaols where they have been adopted. The complete silence enjoined under the Auburn system would of itself no doubt exercise a most debilitating influence upon the intellect; but we should remember that ample occupation for the mind is provided by a plentiful supply of appropriate books, and that a considerable portion of the day is spent in manual labour. Besides it is now pretty generally acknowledged that this perfect silence cannot be maintained, and the real origin of the numerous mental disorders in the Auburn prisons will perhaps be found in the severe discipline employed to enforce the observance of the silent plan.

With respect to the Philadelphia system, it has been urged that man is a social being formed to live in society, and that if he be long deprived of the benefit of this natural law he must necessarily suffer greatly both in his mental and corporal faculties. But the system of Philadelphia is not based on the principle of total seclusion, for all that is required is that, to avoid moral contamination, each prisoner shall be kept strictly separate from his fellow-captives. Here, as in the Auburn system, each prisoner is daily visited by the governor, the chaplain, the physician, and by the other officers of the gaol. In the prisons of Glasgow, Cherry Hill, &c., each inmate is daily visited at least twelve times by different individuals. "We must therefore be careful," says Professor Holst, "to draw the line of distinction between *separate* confinement and *solitary* confinement."

When the Philadelphia system was first adoped, it was justly objected to it that no manual labour was enjoined to the inmates of the prisons where the plan was pursued. This was unquestionably a serious oversight on the part of the projectors of the system; but the necessity for introducing manual labour soon became so apparent that it was universally enforced in the Philadelphia prisons, and in all others conducted under that system. Again it has been urged that separate confinement under this discipline must tend greatly to increase the habit of masturbation, which is now recognized as a frequent cause of mental disorder in young people. But it is evident from the prison reports that this crime is but too frequent in all places of confinement, nor can we discover that it prevails to a greater extent in the prisons of Philadelphia than in others under different discipline; while the evil consequence of prisoners occupying the same cell is well known to all who have investigated the records of the different gaols.

Professor Holst then proceeds to investigate at considerable length the reports furnished from the various prisons of Europe and America, in reference to the number of cases of insanity they present. Here his love for his favorite Philadelphia system has perhaps induced him to overstep the bounds of solid logical deduction. It is certain that in the American prisons cases of insanity are most numerous under the Philadelphia system; but Prof. Holst ventures to suggest that as but few or no retreats for the insane are provided in Pennsylvania, the more dangerous lunatics may be placed as a measure of safety within the walls of Cherry Hill or Pittsburg. The calculations of Coindet also, with respect to the prevalence of insanity in the penitentiary at Geneva, appear to our author to rest upon insecure and unsatisfactory data. Finally, in favour of the Philadelphia system Professor Holst produces the testimony of the commission appointed in 1837-8 by the senate of Pennsylvania to enquire into the influence of the recent systems upon the mental powers of the prisoners. It is here stated that after the most careful investigation, the prison of Cherry Hill presents as few if not fewer cases of insanity than any other prison of the United States, and that those instances of mental disorder which have occurred can be occasionally traced to other causes than the plan of discipline pursued.

During the twenty-five years that Mr. Brebner has been governor of the Glasgow Bridewell not a single case of insanity has occurred, although during that period of time from 40,000 to 50,000 prisoners have been confined within its walls, and in this gaol the Philadelphia system is enforced in the most complete possible form. Here Professor Holst seems to have forgotten that for at least twelve out of the twenty-five years above named the Philadelphia system was not in existence; but at all events it is hereby proved in the plainest manner that the introduction of that system has not increased the number of cases of insanity. The essay of Professor Holst concludes with a brief notice of the various prisons in which these several systems are pursued. Twenty-seven prisons in North America are regulated after the recent plans, seventeen being upon the Auburn system, which has been in operation since 1816, and ten upon that of Philadelphia, which was established in 1829. This latter discipline has also been adopted since 1834 in the Penitentiary of Lausanne in Switzerland, and at Warsaw in 1835 on the modified plan of Count Skarbeck. In France and Belgium the Philadelphia system has been as yet introduced only into a portion of the gaol at Ghent, and into the *model prison* at Paris. In this latter place it was first tried experimentally in that portion of the gaol allotted to children sent thither at the request of their parents (à la correction paternelle), and its good effects having soon appeared, it was adopted in all the other departments of that prison. In Great Britain and Ireland the Auburn system was adopted in 1835, but in 1839 the more recent system of Philadelphia was recommended by the parliamentary commissioners.

It is intended shortly to establish an experimental prison in Christiania in Norway for 100 prisoners, and which will be conducted on a plan very similar to that proposed by the general inspectors, Messrs. Crawford and Russell, and which is about to be tried on a large scale in the neighbourhood of London.

Professor Holst gives the decided preference to the Philadelphia sys-

tem, but on a plan somewhat modified in its details as compared with the original.

"But," he continues, "while I declare my steady adherence to the Philadelphia system of prison discipline, I trust that I shall not be thought to uphold it in every particular, as it is enforced in the prison of Cherry Hill. I am not blind to the faults of this as well as of other systems. Within the last ten years the administration of prisons has made wonderful progress towards perfection, and the experience acquired and the researches entered into during that period have greatly aided to free the system from many of its faults, and to supply many of its deficiencies. This system (the Philadelphian), which had its origin in Europe, has gained immeasurably by its transit to America; and let us hope it may yet improve more fully when it returns to be readopted in the land of its birth. Two chief improvements I would venture to suggest, and in these I doubt not but that I shall be supported by the voice of the whole medical world: *first*, that the prisoners be allowed *daily* exercise in the open air; and *secondly*, that the period of their punishment be as much as is possible curtailed." (p. 30.)

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## Art. XVI.

*A Treatise on Pyrosis Idiopathica, or Water-brash, as contrasted with certain forms of Indigestion and Organic Lesions of the Abdominal Organs; together with the Remedies, Dietetic and Medicinal.* By Thomas West, M.D.—*London*, 1841. 8vo, pp. 108.

The theory of Pyrosis, advanced in the present volume, appears (p. 24) to have been suggested by Andral. According to Dr. West, pyrosis is a disease of debility of the nervous system generally, but more particularly of the nervous apparatus of the stomach and first passages; which debility, partial or general, may, as we shall by and bye see, be produced by a variety of causes. The pyrotic discharge he regards as a sort of passive serous "percolation" from "the terminal exhalants of the œsophageal, stomachic, and intestinal mucous membrane," very analogous to the colliquative sweats that occur in states of great exhaustion; or to the internal dropsies, anasarcous or serous (i.e. in serous cavities), resulting from the same causes. Indeed, the author proposes to designate pyrosis, "a dropsy of the stomach:" and quotes Andral to show that that eminent pathologist had already pointed out certain analogies between the two diseases so named.

"It may be objected that it is a solecism to name any collection of fluid a dropsy, if not occurring in a serous or close cavity. The objection is plausible only at first sight: it will not bear reflection. Make your close cavity an open one by paracentesis, and does your dropsy cease? Have you not dropsy of the womb recognized by the older nosologists? Open a hydrocele, and is it not yet a dropsy, until you have altered the action on the surface of the tunic so as to obviate further effusion? But it may be said that the effused fluid in a pyrosis is sure to be discharged by vomiting or rumination. This also is doubtful. In some cases the effusion collects until it has produced a sudden and visible distension of the stomach itself, and gradually passes off (as may be inferred) through the pylorus into the intestines," &c. (p. 71.)

He elsewhere remarks (p. 9,) . . . "where the weakness is limited to the serous structure of the stomach itself, we have pyrosis only; but in

protracted and inveterate cases, as the debility extends to the rest of the serous system, we have dropsy of cavities, or general dropsy."

These views, if correct, would give to water-brash a degree of importance which it has not been customary to assign to it. We must, however, remark that dropsy, as a sequel of simple pyrosis, and as connected with it, is exceedingly rare; although, as ensuing on chronic derangement of the chylo-poietic viscera, of which pyrosis may have been a prominent *symptom*, it is not uncommon. Now, it would have been satisfactory to have found Dr. West expressing himself clearly and consistently on this subject; but we are at some loss to reconcile the following statements: "Pyrosis then, in our view, is only an *aggravation of one of the common symptoms* of derangement of the stomach." In like manner, at page 33, he heads a paragraph thus: "Pyrosis may be merely *symptomatic* of pregnancy." While at page 73, he commences the chapter on treatment by saying: "Is pyrosis idiopathic or symptomatic? I answer confidently, the disease of which I am treating is decidedly idiopathic . . . If this be not an idiopathic malady, neither is cholera, nor dropsy, nor scurvy." These statements appear to us to be discrepant. In our opinion pyrosis has no more direct connexion with dropsy than any other affection by which general debility is indicated. Dr. West's meaning in the above passages probably is, that while pyrosis is occasionally symptomatic, it *may* be, in other cases, idiopathic.

In one way, indeed, pyrosis may have a somewhat directer relation with the other more formidable disease. We have seen cases in which the daily discharge of pyrotic fluid was by no means inconsiderable; and Andral mentions a case in which it amounted to four pints. In such cases, should the discharge by any cause be interrupted, it might, by being thrown on some other surface or channel,—by "suddenly changing its character for deposition in close cavities," to use Andral's words as quoted by Dr. West, give rise to forms of dropsy.

Dr. West's account of the source of the pyrotic fluid is at least questionable. At pp. 6, 32, and elsewhere, he invariably assumes that it undoubtedly issues from what he calls the terminal serous vessels. We think it accords more with analogy to suppose that a secretion, possessing such physical characters (acridity, acerbity, &c.,) as that of water-brash often does, is more likely to be a glandular secretion, than one from terminal exhalants. It is very probable that the fluid given out in the disease designated by Andral, "Follicular Gastric Dyspepsia," has the origin which he conjectures, and which is implied by its name. We are disposed, on the other hand, to think it probable that the pyrotic fluid comes from those glands described by Dr. Boyd, in his Inaugural Essay on the Structure of the Mucous Membrane of the Stomach, of which a good engraving is given in Müller's Physiology, as translated by Baly. We have no reason to suppose that serous vessels have any power of modifying the fluid which passes from them. And as it is impossible to believe that a fluid so acrid, acid, &c. as the pyrotic occasionally is, exists free in the blood, this makes it more probable that it is *elaborated* from the latter by some glandular apparatus, than that it merely "percolates" from terminal exhalant vessels. We are rather confirmed than shaken in this view, by Dr. West's reasonings, at pp. 24-5, where he remarks

that this dropsy of the stomach may "suddenly exchange its character for depositing in close cavities, that the reverse may happen; that hydrothorax may terminate by a sudden serous discharge from the air-passages; hydrocephalus by inordinate discharge from the skin, kidney, or bowels. .... Who doubts," he continues, "that in all these cases, the discharge is the same liquid which would have gone off by sweats, or which would otherwise have remained shut up in the close cavities?" Granted: but the serous discharges referred to are wholly unaccompanied by anything analogous to the acute cardialgia which generally characterizes pyrosis, with an acrid discharge: and which seems to prove that the nature of the cases is distinct.

To pass now to practical matters, Dr. West's account of the symptoms of pyrosis is very accurate:

"The rumination of the liquor is preceded by anxiety, a sense of uneasiness, cardialgia, pain or burning heat in the epigastrium, which mark the period of secretion; and the ejection of the fluid is always followed by temporary relief. The fluid ejected is usually copious, transparent, hot. Sometimes it is acrid and corrosive, leaving the tongue rough, as if an acid had passed over it: at other times it is insipid or slightly brackish; in rarer instances, of various shades of colour. In inveterate cases, the rumination runs into vomiting, and sometimes into true erythematic gastritis. In these cases, after the inflammation of the stomach has been subdued, the pyrosis still remains to be dealt with, and is, for the most part, more unmanageable." (p. 2.)

His account of the usual causes is also very correct:

"Among the most frequent causes are, deficiency of clothing in a damp and cold atmosphere; error in diet; coarse barley-bread; salt fat pork and bacon; with an undue admixture of coarse vegetable matter, such as greens and potatoes, &c.; the habitual ingestion of ardent spirits of bad quality, or in undue quantity; smoking, snuffing, habitual depraved positions of the body, as in cases of shoemakers, tailors, ribbon-weavers, &c., a poor and scanty diet, with irregularity of meals; hot tea drinking in excess. In other cases, I find none of these or such like causes, but rather that the malady may be referred to the *vires vitæ* having been shattered by long and incessant labour in approaching old age; or in women by rapid and frequent breeding; and in some I have been disposed to refer it to excessive dosing with empirical remedies, to frequent mercurialism, to the abuse of purgatives, to a depraved habit of voraciously eating or rather bolting of food. In other cases, I have satisfactorily referred the mischief to excessive constipation of the lower bowels." &c. (p. 10.)

In pointing out the diagnostic distinctions between follicular gastrorrhea or stomach-gleet and pyrosis, Dr. West thus sums up (p. 32): "I would infer, therefore, that pyrosis may be readily distinguished from stomach-gleet, by the burning pain, by the hot serous secretion, and by its not being complicated with catarrh, nor with humoral asthma, nor with rheumatism." With regard to the alleged absence of burning pain, in stomach-gleet, it does not correspond with our experience; it is not invariably absent. This particular in the diagnosis is therefore fallacious. We have remarked pain or gnawing, and that of a very acute character, in cases of pure follicular gastrorrhea; nor can we theoretically understand why this may not occasionally be; seeing that stomach-gleet is sometimes owing to the same causes, local irritation, or disturbed innervation, as pyrosis itself is.

The pathological post-mortem appearances are not decisive of the origin and nature of pyrosis.

Passing over the remarks at pp. 72, et seq., the object of which is to establish what we are constrained to regard as an entirely fanciful analogy between Asiatic cholera and pyrosis—the same effects being supposed by the author to be suddenly produced and in an extreme degree in the former disease as are induced slowly in the latter,—we come to the treatment of the malady, which, as stated by Dr. West, seems judicious and successful.

The general indications (p. 104) are thus given: "1st, To provide azotic food in order that the blood of the patient may be more highly animalized; 2d, to increase steadily the vigour and heat of the circulating system, especially in the capillaries of the surface; 3dly, to give new tone and fresh impulse to the absorbent system." In preceding parts of his treatise, Dr. West points out the necessity of these means. We willingly quote the following observations, bearing, as they do, on a too common error of the public, and sometimes even of practitioners; namely, that of expecting from mere medicinal means that relief which can only be obtained from proper diet or regimen.

"We may try to rectify secretions, to brace up and fortify the stomach; but unless we can supply good wholesome food, animal and vegetable, together with warm clothing, a dry habitation, and the peace of mind which is the attendant upon a cheerful countenance, or at least a good hope of a continuance of a comfortable 'daily bread,' all our measures will fall short of relief, and our pills and potions, our chemicals and our galenicals, will be a miserable mockery, a wretched satire upon that half science and half art, which Cicero denominated 'God's second cause of health.' Now, as a deficiency of wholesome food, of fuel, and of good clothing, is a common cause of pyrosis, so it is also, of anasarca. To what and whither shall we retreat for these necessaries of life, in a variable and fickle climate? Do our patients find these comforts in our provincial dispensaries? We bind their stomachs with bismuth or with bark, and we send them back to beggary." (p. 19.)

Besides its merely superior nutritive qualities, Dr. West assigns another reason for the employment of azotic food (p. 57): "That non-azotic food will not eliminate the same quantity of heat in the human body as aliment of a more generous quality is a fact admitted and implied in establishing our indications for the treatment of disease. We avoid strongly azotized materials in fevers, &c. . . . When I see the powers of the body, the pneumo-gastric energies especially, giving way as in pyrotic patients, under the use of an aliment seriously deficient in azote, and when I see such patients gradually becoming lower in their temperature," &c. Dr. West notices that the frequency of the cases in country districts augments in winter and diminishes in summer, which he attributes to the more plentiful supply of milk in the latter season. A milk and bread diet and shelter often suffice for cure with patients accustomed to a poor and scanty mode of living, and suffering from exposure. The bowels should be regulated by tonic aperients. Dr. West appends some useful prescriptions in the various forms, liquid, pilular, and pulverulent.

This monograph, though containing a good deal of extraneous matter and untenable theory, will probably be useful, not only from its own intrinsic merits (our opinion of which may be gathered from the tenor of the preceding remarks), but also from its drawing attention to a somewhat obscure and neglected subject.

## ART. XVII.

*On Gout: its Cause, Nature, and Treatment.* By JOHN PARKIN, Mem. of the Roy. Coll. of Surgeons, &c.—*London*, 1841. 8vo, pp. 140.

THE main object of this work is to "endeavour to ascertain if any theory can be proposed, which is not only sufficient to account for the origin of the disease (gout), and at the same time to explain the various anomalies that belong to all other theories, but which leads to the adoption of a mode of treatment as scientific and as successful as that adopted for the majority of diseases." By the term "cause," Mr. Parkin would be understood to refer to that primary or original cause, which produces the gouty diathesis, or morbid state of the system, and not those predisposing or exciting causes which merely favour or accelerate the development of a paroxysm, in those in whom the seeds of the disease have been already sown by the agency of another and more specific cause. The attempt to explain the "primary or original cause," the proximate cause of gout, or of any other disease, has at least the merit of courage. For our own parts we confess that, in our opinion, the labour that has from the earliest ages been bestowed upon the effort to discover the essence or intimate nature of gout, and perhaps of any other malady, would have been much more usefully exerted in the careful consideration of the rise, progress, and variation of the essential and apparent symptoms of disease, and the effect of remedies. We are not at all inclined to admit the validity of the very plausible and frequently repeated dogma, that he who knows nothing of the essence of disease can know but little, if anything, of the mode of curing it. If this were true, the practical utility of our art would be bounded by very narrow limits; for not to assume the knowledge we certainly do not at present possess, it must be admitted that the essence of disease, the *materies morbi*, still remains a mystery to us. In natural philosophy and medicine, it is often sufficient for all practical purposes that we should be acquainted with particular phenomena, although the causes which produce them may escape our closest scrutiny. "Sufficit si quid fiat intelligamus, etiamsi quomodo quidque fiat ignoremus," says Cicero. As we have no wish, however, to check the ambitious and aspiring efforts of those who are willing to cope with the difficulties of such an investigation, we proceed to enquire what light Mr. Parkin has thrown upon the "cause" of gout, confining the term within the limits that he assigns to it. He commences by giving us a critical sketch of the various speculations that have been indulged in by various writers upon the subject, all of which are dismissed as more or less obnoxious to doubt and objections—the learned authorities quoted by Mr. Parkin being chiefly those referred to in the medical dictionaries and cyclopædias. It is well known to every practitioner at all acquainted with the literature of his profession, that the doctrine of a morbid matter in the blood, as constituting the cause of gout, has had powerful supporters from the earliest ages, and that even in the present day this belief has many advocates.

"If, however," says Mr. Parkin, "we enquire into the nature of the morbid matter, we must draw a different conclusion to that of the ancient writers, who considered it to be either bile, phlegm, or blood, which latter they supposed was propelled into vessels which did not contain it at other times, a very natural in-

ference for those who were ignorant that the vital fluid circulated in the arterial as well as the venous system. That bile, again, is not the cause of gout may be inferred from the fact, that when there is an obstruction to the natural flow of this secretion, and the arterial system becomes in consequence overloaded with it, no attack is witnessed. With respect to phlegm, we have in the first place no reason to believe that this excreted matter becomes absorbed into the system, or that, if present there, it would give rise to the effects in question, while we are certain that the presence of healthy blood in the capillaries will not account for the phenomena, inasmuch as they contain this fluid at all times. But, although the presence of the above matters in the blood is insufficient to account for the production of gout, it does not follow that there are no other substances capable of producing the disease, when present in the system.

"That the presence of an extraneous or morbid matter in the blood would be capable of giving rise to effects similar to those witnessed in this disease, we might presume, *à priori*, while the supposition is confirmed by what has been observed after the introduction of various poisonous substances into the animal frame." (pp. 13-4.)

Inasmuch as there are scarcely any limits to our presumption, we certainly "might presume" this fact "*a priori*;" but why we should so presume, we are as much at a loss to discover, as we are how, or in what manner, the presumption is at all strengthened by the examples brought forward by Mr. Parkin to show what nobody denies, that various morbid substances, when present in the system, give rise to irritation and inflammation, not only on the internal surface of the body, but also in the extremities. We require proof for the assertion, "that various poisonous elements, when present in the system, produce effects similar to those observed during attacks of this disease," gout. Mr. Parkin goes no further than to show that there is a slight resemblance between the symptoms produced by the presence of deleterious agents in the system, and the symptoms of the preliminary stage of gout; but he fails entirely to show such an identity of effect, as to justify the inference of identity of cause. Having "endeavoured to show," and as we think unsatisfactorily, that gout is produced by the operation of a poison in the system, Mr. Parkin proceeds to enquire, "what the nature of the poison is and whence it is derived;" and from a train of, we think, equally loose and inconclusive speculation with that upon which he has tried to prove the presence of some poison in the system, he comes to the conclusion that gout is the product of malaria! A few brief comments upon the "nature" of gout are summed up by the following passage:

"We thus learn that gout is sometimes of an inflammatory, sometimes of a congestive, and sometimes of a nervous character; and that it assumes a variety of forms in different individuals and under different circumstances. It is, therefore, neither a purely inflammatory, nor yet a purely nervous disease, nor is it characterized by an affection of one particular part of the body, or one particular system of nerves; for it attacks every organ and tissue, and produces a disturbance in each part of the circulating system, and in every function of the body, the same in those which are under the direct control of the cerebro-spinal, as in those which are under the influence of the ganglionic nerves.

"To view gout, therefore, as Dr. Barlow and other writers have done, only 'as a constitutional disturbance of an inflammatory character, attended with local inflammation of a peculiar kind in one or more joints,' would exclude from our definition a series of effects evidently allied to the former, and produced by the same cause. The definition of Frank is not only more general, but also more correct; for he says, 'gout is a disease *sui generis*, which can

assume the form of all diseases, and present itself under the character of a fever, an inflammation, an eruption, a flux, a retention, or a nervous affection.'" (pp. 87-8.)

In this statement we agree with our author.—Although we do not deny the force of many of the objections Mr. Parkin urges to the use of colchicum in the treatment of gout, we are by no means prepared with him to expunge it from our list of remedies. We sincerely hope that Mr. Parkin's opinion of the value of "carbonic acid gas" in the treatment of gout will bear the test of experience. He says that "it may be considered a specific in gout," and that it acts "by neutralizing or rendering inert the poison productive of the disease," and that "the first and most obvious result which has attended the administration of carbonic acid gas in gout is that of shortening the paroxysm to a much greater extent than that of any other remedy which I have seen administered, or of which I have heard mention. This has been an *invariable* result, as far as my own experience goes." Now without for a moment doubting the good faith with which Mr. Parkin makes this statement, we must observe that we are always a little sceptical when we hear of the "invariable" virtues of either medicines or of men. Mr. Parkin's views of the general treatment of gout are postponed, "as the want of time, and some important engagements, prevent him from entering fully into this part of his subject." We beg to submit, that as Mr. Parkin was not compelled to publish an unfinished book, his plea of "want of time," &c. does not justify his haste. We do not find anything in the volume so essentially necessary to the welfare of the public, or the instruction of the profession, as to have precluded Mr. Parkin from waiting until he could snatch a little more leisure from "important engagements," and give us a whole work instead of half a one. We are promised a "second part to the present work," containing the directions that Mr. Parkin considers proper for the "treatment of the different stages, as well as the different forms of gout." We can wait for it patiently, and we trust that the author will not again enter upon his undertaking, until he has sufficient leisure to fulfil the contract between himself and the purchasers of his book implied in his title-page.

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## ART. XVIII.

*An Essay on the Chemical, Botanical, Physical, and Parturient Properties of the Secale Cornutum: with an Engraving.* By T. H. WARDLEWORTH, Surgeon.—*London*, 1840. 12mo, pp. 69.

THE purport of this book is to increase the confidence of the profession in the "parturifacient" effects of the secale cornutum. As a proof of the author's zeal in the cause he advocates, we quote the following passage:

"The efforts of the profession will doubtless, ere long, ascertain the full merits of the medicine in question, and if our expectations are realized, will place the secale cornutum amongst the most important discoveries in medicine, and mark the age in which its merits were established in the same page with those that record the discovery of the VACCINE VIRUS." (p. 35.)

To us, the comparison which is here so clumsily instituted, seems

to be quite absurd, but we will state the grounds upon which Mr. Wardleworth founds his very high opinion of the "secale cor,"—for thus does he, in his prescriptions, deprive his favorite agent of its fair abbreviated proportions.

"In the course of my practice the secale cornutum has been administered to 1500 patients, without selecting or in any way giving a preference to any individual case, or professing that knowledge by which a partiality might have been given, for so little has been published on the subject, that, in common with other practitioners, feelings of hesitation have sometimes passed in my mind when administering a drug of which nothing was known but the general character of its common effects. Yet under these circumstances I have administered it to 1500 patients, as before mentioned, with the most satisfactory results: and that a mode of treatment which has proved so safe, and so salutary, and which to a great extent has lessened the period of suffering, may become a great object to medical practitioners, this attempt to excite more general attention to the subject has been made." (p. 32.)

Now we submit to Mr. Wardleworth, that such a statement as this is just as little adapted to lead to the conclusion he wishes, as it is to prove either the elegance or the accuracy of his composition. We will not insinuate even a suspicion of its accuracy as to facts; still the number of patients mentioned is very considerable, and shows both the vast extent of Mr. Wardleworth's practice and the exuberant fertility of the ladies in and about "Rochdale." But it does not show either the necessity or the propriety of having thus administered the "secale cor." And we declare it to be our belief, that 1500 cases of labour have never fallen to the share of any practitioner, in which the use of the ergot of rye, or any other "parturifacient" agent was either required or justifiable. We have not the smallest doubt that, in the great majority of these 1500 cases, in which "without selecting, or in any way giving a preference to any individual case," this "partûs accelerator" was given, that the efforts of nature might, and ought to have been relied upon. Mr. Wardleworth quotes Chevreul, who, we are told, is of opinion that the use of the secale cornutum "is applicable in the early stages of all natural labours;" "an opinion with which I (Mr. Wardleworth) fully concur, provided the patient has, in the last months of pregnancy, submitted to a prescribed diet, and if advised has taken a few doses of medicine and lost a few ounces of blood: by these means, simple as they may appear, the sorrow and danger of child-birth will be greatly lessened, and its duration the subject, not of uncertainty, but of calculation," (p. 41.) We have no fear that others will take to this mischievous and meddlesome midwifery, and we strongly advise Mr. Wardleworth to allow his "feelings of hesitation" to have a little more influence upon his practice, and that in at least some part of his next 1500 cases, he will give nature fair play, and we venture to prophecy that he will find "natural labours" do not require the impertinent interference which he so unwisely and so injudiciously recommends.

The extracts we have given are sufficient samples of Mr. Wardleworth's loose and very inaccurate style of writing; but many of the blunders we have detected we attribute, in charity, to the printer: as for example, "Desmoreaux" for Desormeaux: "per vaginum:" "dilitation" of the os uteri, "agrum" for ægrum, &c. &c.

## PART SECOND.

# Bibliographical Notices.

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Art. I.—*The Surgical Anatomy of Inguinal Herniæ, the Testis and its Coverings.* By Thomas Morton, one of the Demonstrators of Anatomy in University College, London. *Illustrated with Lithographic Plates and Wood Engravings.—London*, 1841. 8vo, pp. 330.

We have here another production from the industrious pen and pencil of Mr. Morton. The object of the author is to give an accurate anatomical description of the parts he treats of, and to combine with it all the surgical information which can be brought to bear upon the subject. We willingly yield to Mr. Morton all the credit due to the successful accomplishment of the object proposed.

In a work like the present we do not look for anything novel in a surgical point of view: but we have a careful selection of orthodox opinions from the writings of our most approved surgical authorities, on which, therefore, the student may rely.

In the anatomical division of the work we have noticed some slight inaccuracies, which are probably oversights. Thus, in describing the dartos, Mr. Morton speaks of it as a modification of the superficial fascia of the abdomen, "upon which the vermicular movements and gradual contractions of the scrotum are dependent." (pp. 214-5.) The dartos is a structure *sui generis*, and completely distinct from the superficial fascia: and the corrugation of the scrotal integument is alone referrible to the contraction of the dartos, whilst the vermicular movements of the testis are doubtless dependent on the spontaneous action of the cremaster.—Again, it is not an usual arrangement of the fibres of the intercolumnar fascia, that "near the apex of the external ring, they are disposed with their *concavity* downwards towards the ring." (p. 222.) Mr. Morton here confuses the intercolumnar bands with the fascia of the external ring: the arrangement he speaks of would materially interfere with the purpose which the former structure is destined to answer.—The existence of *one*, instead of two, epigastric veins, we consider the exception to the rule. (p. 253.) These, however, are trifling defects in an excellent work.

The present volume contains many engravings laboriously coloured, and some woodcuts: we cannot help repeating the preference we give to the latter. In conclusion we must in justice state that the student who wishes for *assistance* in the acquirement of his surgical anatomy, will find a safe guide in the work before us: but let him beware of the temptation which is held out to him of resting satisfied without consulting the book of nature for himself. We are sure Mr. Morton is the last man to counsel acquiescence in such a plan of study.

Art. II.—*De la Compression contre les Tumeurs Blanches des parties dures.* Par le Docteur De Lavacherie, Professeur de Clinique Externe à l'Université de Liège, Membre Correspondant de la Société de Médecine de Gand, &c.—*Gand*, 1839. 8vo, pp. 99.

*Memoir on the Treatment of White Swellings by Compression.* By Professor Lavacherie, of Liège.—*Ghent*, 1839.

The author admits that pressure has been tried by many surgeons in "white swellings," but imagines that no surgeon hitherto has applied it so universally as himself, nor to such formidable cases, nor as the sole remedy. He applies compression to every stage and form of the disease; to acute cases as well as to chronic ones; where there is but slight enlargement, and where the tumefaction of the soft parts is very great; to simple enlargement, and to disorganization of the cartilages and ligaments; to inflammation of the bone, and to superficial or deep caries of the articular surfaces with or without fistulous communications with the joints themselves. By compression he has cured affections which had been pronounced incurable, and for which amputation had been proposed. When compression does not cure, he has found that it contributes powerfully to stop the progress of disease, and especially to relieve pain, which in many instances is intolerable. Even when applied where there is copious purulent secretion from deep-seated disease, strong pressure diminishes the secretion, and in all cases the pus makes room for itself between the skin and the plaster, however great has been the degree of compression employed.

M. Lavacherie makes pressure by means of strips of adhesive plaster, (emplâtre agglutinatif) firmly applied round the diseased joint. At first he used bandages with starch, but these did not exert sufficient pressure, and he found that the contact of adhesive plaster with the skin produced none of the irritation he at first feared. He applies the first straps in circles over that part of the joint which is most swollen; the other straps more obliquely, in order that they may be flat. Having covered the whole joint he then applies a roller over the whole limb. When the bones themselves are enlarged much stronger pressure may be used than when the soft parts are the principal seat of the disease. Stiffness is generally complained of, which ceases after the first twenty-four hours; after which time the bandage applied over the lower part of the limb to prevent stagnation of the circulation may be removed. In order to be useful, the pressure should not produce actual pain; the sufferings of the patient should gradually diminish, and in some hours cease. If the pain increases, the bandage has been badly applied, and should be removed and reapplied. In general the comfort produced is such that as soon as the apparatus ceases to exert pressure, the patient requests it may be renewed. This is generally the case after a few days, and it should then be reapplied. In many of the reported cases, the strapping was renewed every fortnight.

Thirteen cases are given, which M. Lavacherie has thus arranged:—6 perfect cures of white swellings, arrived at a stage reputed incurable; 3 equally bad cases so much improved that cures may be shortly expected; 3 other cases in which improvement is so marked that cures may be expected; one case much more severe than the others, where there is a sensible improvement. The conclusion drawn by the author

from these facts is, that white swellings are amenable to treatment even at an advanced stage, and that compression is the best remedy; for if it does not cure, it delays the progress of the disease. He is convinced that formerly he has amputated limbs which he could now save. Even when from constitutional disease palliative treatment alone is advisable, compression is the best means, as it stops the progress of slow hectic fever, by excluding the air.

It is almost needless to say that the treatment of white swellings by compression is no novelty in this country. Mr. Scott, of Bromley, has the credit of introducing it; and although he does not depend on simple pressure, yet surgeons generally have regarded the methodical and scientific way in which he applies his bandages, so as to produce constant, equable, and long-continued pressure, as the essential part of his treatment. If this is a new thing on the continent, our neighbours must be much behind us in the treatment of diseased joints. In our own practice we have tried simple compression by adhesive plaster, and seen it used by others, in aggravated cases where there were fistulous communications with deep-seated disease of the bones, but certainly not with the success of our author. Nevertheless, it is a remedy that should be tried, being both simple and often efficacious. If the rule is attended to, that any increase of pain is a proof that the pressure has been ill applied or is producing mischief; and if the patient is carefully watched, and the bandages at once removed, no ill effects can follow, and many limbs may be saved which are now amputated; for doubtless the general disbelief in the power of bone to repair itself when ulcerated often leads surgeons astray when they feel a piece of rough bone beneath their probes, and thus many are maimed for life unnecessarily.

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Art. III.—*The Principles of Physiology applied to the Preservation of Health and to the Improvement of Physical and Mental Education.* By Andrew Combe, m.d. &c. *With Thirteen Woodcuts.* Tenth Edition, revised and enlarged.—*Edinburgh*, 1841. 8vo, pp. 420.

In our Second Number for April, 1836, we noticed, at some length, the Third Edition of this work, and bestowed on it that high and unqualified praise which can only belong to productions of the very first order. It is unnecessary to go over the same ground again, but we feel it due to such of our present readers as may not have seen our former article, to call their attention to a treatise which contains more sound philosophy, more true practical wisdom, relative to the all-important subject of preserving health, than any other volume in our language; and which, while it is calculated to please and benefit the public generally, is no less adapted for the study of professional men. The extremely large sale of the former editions of this work is, at once, evidence of its merits, and a gratifying proof that the reading part of the public are becoming daily more awake to the importance of that branch of philosophy of which it treats. Since its first appearance in the spring of 1834, "nine editions, consisting together of 14,000 copies have been exhausted in this country, and upwards of 30,000 copies in the United States of America." We believe such a wide circulation as this of a medical book, can only be stated of that before us and of the other works of Dr. Combe.

ART. IV.—*Traités et Découvertes sur la Physiologie de la Moelle Epinière.* Par J. VAN DEEN, M.D. &c.—*Leide*, 1841. 8vo, pp. 224. *Essays and Discoveries on the Physiology of the Spinal Marrow.* By J. VAN DEEN, M.D., &c.—*Leyden*, 1841.

THIS volume contains a collection of papers, which have been published by the author at different times, from 1838 to the present year, embodying the results of a large number of experiments on the spinal cord and its nerves. The opinions which he deduces from these do not altogether coincide with those usually entertained in this country. For example, he states his present accordance with the proposition of Magendie, that the anterior columns are as much concerned in sensation as in motion; but his earlier views were by no means the same; and the facility with which he changes his interpretation of the same experiments, rather diminishes our confidence in the value of his inferences. The white portion of the anterior column he considers to be the part concerned in movement; and he thinks that he has proved that by the gray matter sensory impressions may be either conveyed to the brain or reflected through the motor nerves. We are left so much in the dark, however, as to the mode in which the persistence of sensibility was determined, that we cannot regard the proof of this and other similar propositions as by any means sufficient. The movements, which the author relies upon as indications of pain, are evidently nothing more than reflex actions; an interpretation which is not forbidden by their adaptive character. In his general conclusion, however, that in the gray matter of the nervous system resides its whole active power, and that the white matter has only the functions of establishing a communication between the central and peripheral organs, we think him much more justified. He fully admits, also, the class of reflex actions as distinguished by Dr. M. Hall; but he refuses his assent to the doctrine of the "distinct system" of excito-motor nerves; maintaining that there is but one kind of centripetal and one of centrifugal fibres. We think it right to give his own summary of his latest conclusions, for the information of such of our readers as may feel an interest in them.

1. That the white substance of the anterior columns ministers solely to motion.

2. That the anterior columns with their gray portion minister to sensation as well as to motion.

3. That the white substance of the posterior columns is concerned solely in sensation.

4. That the posterior columns with their gray substance are also destined solely to sensation.

5. That the white substance of the posterior columns does not require to be continuous with the brain, to communicate to it the impressions received by the posterior roots.

6. That the white substance alone of the posterior columns cannot readily transmit sensory impressions to the brain.

7. That this transmission may readily take place, if the gray substance is still in contact with the white substance of the posterior cords.

8. That the white substance of the anterior columns, without the gray substance, cannot directly communicate voluntary power to the muscles through the posterior roots; but can only excite vibrations in the muscles.

9. That the same conditions which are necessary to cause a real sensation are also necessary for a reflex action; that is to say, as real sensation depends on the posterior roots and columns, and on the gray substance, the impression necessary for reflex action depends on the same parts.

10. That the same conditions required to communicate to the brain a real sensation by the anterior columns are also necessary to transmit by the same columns the movement of reflexion in the direction of the brain; neither one action or the other can take place without the gray matter.

11. That by the gray matter, impressions are communicated from the posterior to the anterior columns.

12. That by the gray matter impressions are communicated from one centripetal fibre to another.

13. That by this medium, the same takes place among the centrifugal fibres; since only a few of the white fibres of the anterior columns seem concerned in transmitting, by the medium of the gray substance, the voluntary impressions proceeding from the brain to the anterior roots.

14. That the centripetal and centrifugal fibres are to be regarded as conductors; and the gray substance as the active centre of the nervous system.

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Art. V.—*Observations on the Structure and Diseases of the Testis.* By Sir Astley Cooper, Bart., f.r.s. Second Edition. Edited by Bransby B. Cooper, f.r.s. *With Twenty-four Coloured Plates.*—*London*, 1841. 4to, pp. 330.

The republication of this splendid volume supplies a want that has been long severely felt from the exhaustion of the first edition of it. With the exception of "some new matter upon the subject of varicocele," by the author, and the adoption of a "somewhat more perfect arrangement" of the old materials, and the addition of a new preface by the editor, the work is precisely such as it appeared originally from the hand of its lamented author; and assuredly, as the editor well observes, "it reflects no inconsiderable merit on Sir Astley Cooper's industry and acuteness, that although a period of ten years has elapsed since the first appearance of the work, a period, too, in which there has been no lack of labour in the field of anatomical and surgical science, no new facts of importance have been advanced on this subject." The extraordinary merits of this treatise have been so long and so universally acknowledged that it would be a work of supererogation to represent them in our pages: we will only say here, that the practical surgeon who is not master of its contents cannot be fully aware of the imperfection of his own knowledge on the subject of diseases of the testicle. No surgical library can be complete without it.

The preface to the volume does credit to the feelings of the editor; we trust that, ere long, we shall be indebted to him for a detailed account of the life and writings of his distinguished relative, in regard to whose character and conduct he but echoes the voice of the whole profession when he says: "In the exercise of his calling he exhibited an acute penetration, a solid judgment, a benevolent care, great suavity of address, and a most tender anxiety for the comfort, relief, and recovery of his patients to whatever rank of society they might belong." (Pref. viii.)

Art. VI.—*Over de Indische Sprouw* (*aphthæ orientales.*) Door W. Bosch, Chirurgijn-Majoor bij het leger in de Oost-Indiën, &c.—*Amsterdam*, 1837.

*On the Indian Thrush.* By W. Bosch, Surgeon-Major in the Army in the East Indies.—*Amsterdam*, 1837. 8vo, pp. 60.

This little work is addressed chiefly to young medical practitioners in the Dutch possessions in India, to acquaint them with the nature of a dangerous and peculiar disease there prevalent. The author divides his history of it into three periods, each of which however is, in different cases, of different duration. In the first stage there are loss of appetite, weight at the stomach, and nausea, and the tongue is clean or covered with a yellowish fur on the right side. The expression of the countenance is natural, the eyes bright and light yellow, the perspiration diminished, the pulse pretty natural, the intestinal evacuations irregular and bilious, and mixed with undigested substances. The abdomen is distended, the epigastrium tender, the urine red and scanty. In the second stage, which is regarded as inflammatory, all the preceding symptoms are increased; there is more fever, a quantity of gas and acid in the stomach, and evident torpor of the liver. In this stage also there is observed a great tenderness with redness of the tongue, mouth, and fauces. All these disorders are in the third stage still more increased, so that nothing can be taken into the mouth but mild mucilaginous substances, and there now come on excessive and colliquative diarrhœa of citron-yellow or gray fluid, with local, fetid, and rapidly exhausting sweats, and with scanty and very acrid urine.

The author regards the ganglionic nervous system as the essential seat of the disease; and ascribes, as its predisposing causes, partly the climate of Padang, which is situated on the west coast of Sumatra, and is frequently damp and has a very variable temperature, and partly the food, which in great part consists of raw potatoes. The proximate cause of the disease is considered to be a cachectic condition of the fluids and a relaxed state of the solids of the body.

In reference to treatment, the author states that he disapproves of emetics, but can recommend gentle purgatives to be administered on first seeing the patient. Calomel is not tolerated, but mercurial ointment may be rubbed in over the upper part of the abdomen with great advantage. At a later period diluted sulphuric acid is useful, but only in very small doses and given with great circumspection. At the close of the disorder, tonics, especially the lichen islandicus, are advantageous. Of narcotics, the extract of hyoscyamus and the aqua laurocerasi are beneficial. Among external remedies, the best are leeches applied in small numbers; and for counter-irritation, tartar emetic dissolved in water and applied so as to produce an eruption. Galvanism produced no effect. The treatment of the disease however is, like that of all diseases in which there is increased action with diminished power, difficult. The transition to the adynamic state takes place easily, and either stimulants or antiphlogistics must be administered with the greatest reserve; the first soon bring on exhaustion by their indirect action, the last by their direct influence.

ART. VII.—*A New Operation for the Cure of Amaurosis, Impaired Vision, and Shortsightedness.* By JAMES J. ADAMS, F.L.S. G.S. &c. —*London*, 1841. 8vo, pp. 50.

THE following brief extracts indicate the nature of the cases termed Amaurosis by Mr. Adams, their alleged pathology, and the mode of cure recommended and practised by him:

"The term *amaurosis*, as here about to be employed, will be used more in accordance with its true meaning, by signifying, simply, a dim or darkened sight, without implying any organic disease of the optic nerve.

"Having thus restricted the term amaurosis to the expression of a symptom, common to many diseases of the eye, namely, dim or darkened sight, a necessity arises for distinguishing, by an additional term, the different cases of impaired vision in accordance with their degree or cause; therefore, as the *cause* of the amaurosis, which will form the subject of the following pages, will be found to depend on muscular action, and not on any disease or change of structure in the nerve, I propose to distinguish it from all the other forms of blindness by the term *Muscular Amaurosis*." (p. 11.)

"The simple and uncomplicated forms of Muscular Amaurosis, which it is, particularly, the intention of this paper to enquire into, will be characterized, on the one hand, by a perfect transparency of the humours of the eye, and a healthy condition of all its structures; on the other hand, by the absence of those signs which render the presence of permanent diseases or changes in the brain or optic nerve evident and certain." (p. 12.)

"I am of opinion that the peculiar form of amaurosis, here designated by me as *Muscular Amaurosis*, depends on the bending or partial folding, and compression of the optic nerve, caused by the shortening and thickening of the recti muscles during a state of morbid contraction, which further may be attributed to an affection of the third and sixth nerve, probably at or near to their origins." (p. 42.)

"The operation which I propose for the cure of muscular amaurosis consists in the division and *the extensive separation* of one, two, or more of recti muscles: of which, be it observed, the separation must be *equal* and *even* in each instance. The mode by which I perform this operation I have made peculiarly my own, by adapting a particular set of instruments to it, and by requiring, for its perfect success, a very extensive separation of the muscle, not only from the sclerotica, but from the cellular tissue and conjunctiva which lies in front of it." (p. 25.)

The general statements are illustrated and supported by fourteen cases supposed by Mr. Adams to be examples of the affection of which he writes, and which, generally speaking, are of that kind familiarly known to surgeons not as amaurosis but as nervous asthenic affections of the retina, and originating for the most part in over-exertion of the organ. For nine of these cases no operation was performed, and no special treatment adopted. In the remaining five the operation was performed and, according to the statements of the patients as here recorded, with more or less benefit in all.

The following are some of the remarks which are naturally suggested by the contents of this pamphlet:

1. The theory of the disease maintained by Mr. Adams is not only not supported by the known anatomy and physiology of the eye, but is much at variance with these.

2. In the nine cases not operated on, there is not a shadow of evidence that the symptoms depended on the alleged cause; and in the cases operated on the only evidence rests on the subsequent improvement of

vision: and even admitting the improvement to be as here stated, it may be accounted for as plausibly on other grounds as by Mr. Adams's theory.

3. But supposing the theory to be false, if it is true that Mr. Adams by his operation has put it in our power to cure or greatly relieve a numerous class of cases often untractable by other means (although these cases are not Amaurosis) it must be admitted that he has done much more for the healing art than if he had excogitated the most consistent theory. Has he done so? We must be allowed to doubt this for the following reasons:

1st, Because some of the cases detailed by Mr. Adams do not seem to us so strongly in favour of immediate benefit from the operation, as they are supposed to be; 2dly, because the period of alleged improvement as here recorded, that is, the time since the operation, is in all the cases too short, being in the longest of the five cases barely three months, in one six or seven weeks, and in two only eight or nine days; 3dly, because much of the evidence in favour of the improvement rests on the testimony of the patients, and because we know that some of the patients have given to others *different* evidence from that given to Mr. Adams.

These we think are sufficient reasons for the qualified opinion we now entertain of the merits of Mr. Adams's operation: we are, however, quite open to conviction on further evidence, and shall be delighted to acknowledge these merits to be of a very different kind when he has favoured us with proofs that appear to us satisfactory.

We cannot, in justice to the office we fill or to Mr. Adams's own character, close this notice without reference to a subject rather of professional etiquette than of literary or scientific criticism. What does Mr. Adams mean by appending to a surgical treatise the following memorandum?

"N.B. Communications, intended for the Author, are particularly requested to be directed to Mr. JAMES J. ADAMS, No. 27, NEW BROAD St., City, there being another Surgeon of the name of Adams in the Street."

What communications does Mr. Adams expect? Is he so little sensible of his own merits as a surgeon, that he must guard against being mistaken by the passer-by? We are disposed to regard this unfortunate notice as a mere piece of bad taste thoughtlessly committed; but all Mr. Adams's readers may not be so charitably disposed, and we notice it here to warn him to guard in future against the painful mistakes such an advertisement is calculated to occasion in the minds of his brethren.

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ART. VIII.—*The Surgeon's Vade Mecum.* By ROBERT DRUITT. Second Edition. *With Fifty Wood Engravings.*—*London*, 1841. 8vo, pp. 524.

ON a former occasion (Br. and For. Med. Rev., vol. VIII., 536,) we gave a favorable judgment of the first edition of this work, which the voice of the profession, in so soon demanding a second, seems to have ratified. The work is much enlarged and improved; and we can safely recommend it to the notice of the junior members of the profession, as one that may be relied on for the accuracy and fulness of its practical details.

ART. IX.—*Researches into the Physical History of Mankind.* By J. C. PRICHARD, M.D. F.R.S. &c. &c. Third Edition. Vol. III. Part I. —*London*, 1841. 8vo, pp. 507.

ON former occasions (Br. and For. M. Rev., vol. III. 365, vol. V. 543,) we had the pleasure of noticing the first two volumes of this classical work, and of bestowing on them the humble meed of our praise. The cultivators of science throughout the world have, by their unanimous acclamation, justified and sanctioned that award. The present volume is worthy of its predecessors; and we know not that we could give it higher praise.

The portion of the work now entered on comprehends the ethnography or description, physical and moral, and history of all the different people who have inhabited Europe and Asia from the earliest period of which any record remains. The present volume is confined to the European tribes, and will be followed by a Second Part devoted to those of Asia. The whole, when completed, will be a lasting monument of the sagacity, judgment, industry, and learning of the author. We regret that the press of matter having a more immediate relation to medical science prevents our entering upon any detailed notice of this volume; but we can assure our readers that its perusal will afford to every scholar and, indeed, to every one interested (and who is not?) in the history and customs of his race, a treat of no ordinary description. And it will not please our readers and the readers of the present day only—it will please their successors for many generations. It is one of the few works that, in these later times, has been undertaken and executed in the spirit of former days, when men devoted their lives to the prosecution of one subject, and, thinking of the attainment of truth alone, were never troubled with the miserable jealousies, envyings, and fears which wither up every lofty enjoyment in the little minds who can never separate from the study of philosophy the consideration of paltry evanescent trifles having relation only to their own selfishness and vanity.

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ART. X.—*The Philosophy of Mystery.* By W. C. DENDY, Surgeon of the Royal Infirmary for Children, &c.—*London*, 1841. 8vo, pp. 434.

THE main object of this volume is to give an account, and, as far as may be, a rational explanation, of the mysterious subjects of dreams, ghosts, visions, prophetic warnings, &c. &c., and it must be admitted that the author, in his attempts to attain it, has contrived to write a very clever and most amusing book. Amid the mass of scientific and practical matter that calls for notice on all sides, we cannot devote sufficient space to do justice to Mr. Dendy's elegant production; we shall therefore pass it by with the general remark that its perusal will both gratify and instruct all readers of taste, especially those who, like the author, have at once a turn for poetry and metaphysics. Mr. Dendy's industry and erudition are equally conspicuous with his talents in this production, which comprehends a mass of fantastic legendary lore of astonishing extent and variety. We think, indeed, if the book has any fault it is in its over-copiousness of illustration, and occasional diffuseness both of method and style: it is, however, on the whole, extremely creditable to Mr. Dendy, and cannot fail to give him an honorable place among our literary writers.

Art. XI.—*An Enquiry concerning the Diseases and Functions of the Brain, the Spinal Cord, and the Nerves.* By Amariah Brigham, m.d. —*New York*, 1840. 12mo, pp. 327.

We have had more than one occasion to express our high appreciation of the beneficial tendency of Dr. Brigham's publications, and the unpretending little treatise before us does not fall short of its predecessors in its character of practical utility:

"Its object," says the author in his preface, "is to call the attention of those practitioners of medicine into whose hands it falls, to the importance of the nervous system, and to persuade them to embrace every opportunity that is presented for studying its functions and diseases. For this purpose," he continues, "I have endeavoured to give a partial summary of what is now known respecting this system. I have collected a large number of cases explanatory of its diseases and functions, cases that are scattered through many volumes, to which I have added a considerable number that have fallen under my own observation, and have thus sought to indicate the way that this system should be studied, in order to increase our knowledge of its functions and our means of remedying its diseases. In the second part, I have briefly treated of a number of diseases, the pathology of which is not yet settled. I have not sought to give full accounts of these, but to direct attention to a few important circumstances, and such as require further investigation."

The first part of the volume is occupied by a general enquiry into the functions of the nervous system, as elucidated by its anatomy, by experiment, and by observation of its natural and morbid actions. Though this does not comprehend the details of the latest researches, there are no general views that can claim a fixed rank in neurology, which are not embodied in it; and the whole may be read with great profit by those who have not already given express attention to the subject. Dr. Brigham is evidently a partisan of no system, but in the true spirit of eclectic philosophy is ready to receive truth from whatever quarter she may present herself. The inductions on which he ventures are expressed with much caution; and there is throughout a complete avoidance of that dogmatic spirit, which, though more absurd when applied to medicine than to almost any other subject, figures perhaps more frequently in the pages of our literature than in that of any other branch of science.

The second part treats of the special diseases of the brain and spinal cord; and here the young practitioner will find much sound information, derived from the experience of a judicious physician, conveyed in a simple and attractive form. It seems to us that Dr. Brigham has most successfully attained the object he proposed to himself, and we cannot doubt that the influence of his little work will be highly beneficial wherever it receives due attention.

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Art. XII.—*The Cure of Strabismus by Surgical Operation.* By W. Mackenzie, m.d., &c.—*London*, 1841. 8vo, pp. 30.

This pamphlet is an Appendix to the author's "Practical Treatise on the Diseases of the Eye," and contains, in a condensed form, a complete account of all that is at present known on the subject of which it treats. We cannot pay it a higher compliment than to say that it is written in the same admirable style as the work to which it is a well-timed and fitting supplement.

ART. XIII.—*Du Système Nerveux de la Vie Animale, et de la Vie Vegetative; de leur Connexions Anatomiques, et des Rapports Physiologiques, Psychologiques, et Zoologiques, qui existent entre eux.* Par A. BAZIN, D.M.P., Professeur de Physiologie Animale, et de Zoologie, à la Faculté des Sciences de Bordeaux, &c. &c.

*On the Nervous Systems of Animal and of Vegetative Life, their Anatomical Connexions, and their Physiological, Psychological, and Zoological Relations.* By A. BAZIN, D.M.P., Professor of Animal Physiology and of Zoology to the Faculty of Sciences at Bordeaux, &c. —*Paris*, 1841. 4to, pp. 180. *With Five Plates.*

THE title of this work, and the promises set forth in the preface, joined to the reputation which its author has attained, caused us to anticipate much interest in the perusal of it. This anticipation, however, has not been by any means fully realized. Many doctrines are put forth as novel which have long been familiar to physiologists under a form but little different, and that which there is of novelty rather consists of vague speculation than of real scientific induction. The work contains, however, much that is in itself sound and valuable; but this is frequently expressed in a form so diffuse, as to prevent its merit from being at once discovered. This is the case, more especially with the portion of the work in which the actions respectively due to instinct and to intelligence are under discussion.

The anatomist will find in this work a very full account of the structural connexions which exist between the cerebro-spinal and sympathetic systems in man; and there is a statement which, if true, is certainly important, that the pituitary gland is really a ganglion belonging to the sympathetic system. But we do not perceive that any such general deductions are drawn from these facts, as we were led to expect by the author's introductory remarks. Some other interesting observations are scattered through the work; such as, that there is a decussation between the fibres that connect the cerebrum and cerebellum, which we do not recollect to have seen noticed. And on the whole, we regard the anatomical portion as more deserving of attention than the physiological, which is in many parts very deficient; we have looked in vain, for instance, for any clear enunciation of the author's views of the influence of the ganglionic system on "la vie vegetative," although the subject is frequently alluded to.

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ART. XIV.—*A Series of Anatomical Sketches and Diagrams.* By THOMAS WORMALD, and A. M. M'WHINNIE, of St. Bartholomew's Hospital.—*London*, 1841. Part IV.

THE present number of this elegant and useful series of Anatomical Sketches contains views of the subclavian, thyroid, and axillary regions, as well as two drawings of the superficial and deep relation of parts at the bend of the elbow, and a diagram of the communication between the spinal nerves and sympathetic ganglia. They are equally unpretending and bear the same impress of accuracy as those of the numbers which preceded them. They need not our recommendation, for they cannot fail to recommend themselves to the anatomical student, who desires just so much aid of this nature as he ought to receive in the prosecution of his dissections.

ART. XV.—*A new Process for Purifying the Waters supplied to the Metropolis by the existing Water Companies.* By THOMAS CLARK, Professor of Chemistry in the University of Aberdeen.—*London*, 1841. 8vo, pp. 16.

IN this little pamphlet we have detailed the particulars of an invention, —for so we may term this new application of a chemical fact already known—which promises to be of vast importance to the inhabitants of this metropolis, and, indeed, to the inhabitants of every city supplied with water artificially from a common source or sources. Professor Clark has obtained a patent for the invention; and, as very little preparation is necessary to put it in operation, we hope that we may look forward to see the process applied, ere long, to the cleansing of the filthy fluids we are all compelled to drink in this great city. That it will be eventually adopted we entertain no doubt; although it is not unlikely that it may at first meet with neglect and even with opposition.

The process employed by Dr. Clark is one no less simple and beautiful in its simplicity, then it is strictly scientific; its admirable applicability to the end in view, and its perfect efficiency will be self-evident to any one who considers the subject. We ourselves have had the advantage of witnessing some of Professor Clark's experiments, and the result was in the highest degree satisfactory. The following extract explains very clearly, and in familiar language, the theory of the process:

"To understand the nature of the process, it will be necessary to advert, in a general way, to a few long-known chemical properties of the familiar substance chalk; for chalk at once forms the bulk of the chemical impurity that the process will separate from water, and is the material whence the ingredient for effecting the separation will be obtained.

"In water, chalk is almost or altogether insoluble; but it may be rendered soluble by either of two processes of a very opposite kind. When burned, as in a kiln, chalk loses weight. If dry and pure, only nine ounces will remain out of a pound of sixteen ounces. These nine ounces will be soluble in water, but they will require not less than forty gallons of water for entire solution. Burnt chalk is called caustic lime, and water holding caustic lime in solution is called lime-water. The solution thus named is perfectly clear and colourless. The seven ounces lost by a pound of chalk on being burned, consists of carbonic acid gas—that gas which, being dissolved under compression by water, forms what is called soda-water.

"The other mode of rendering chalk soluble in water is nearly the reverse. In the former mode, a pound of pure chalk becomes dissolved in water in consequence of losing seven ounces of carbonic acid. To dissolve in the second mode, not only must the pound of chalk not lose the seven ounces of carbonic acid that it contains, but it must combine with seven additional ounces of that acid. In such a state of combination, chalk exists in the waters of London—dissolved, invisible, and colourless, like salt in water. A pound of chalk, dissolved in 500 gallons of water by seven ounces of carbonic acid, would form a solution not sensibly different, in ordinary use, from the filtered water of the Thames, in the average state of that river. Chalk, which chemists call carbonate of lime, becomes what they call bicarbonate of lime when it is dissolved in water by carbonic acid.

"Any lime-water may be mixed with another, and any solution of bicarbonate of lime with another, without any change being produced: the clearness of the mixed solutions would be undisturbed. Not so, however, if lime-water be mixed with a solution of bicarbonate of lime: very soon a haziness appears; this deepens into a whiteness, and the mixture soon acquires the appearance of a

well-mixed whitewash. When the white matter ceases to be produced, it subsides, and in process of time leaves the water above perfectly clear. The subsided matter is nothing but chalk. What occurs in this operation will be understood, if we suppose that one pound of chalk, after being burned to nine ounces of caustic lime, is dissolved, so as to form forty gallons of lime-water; that another pound is dissolved by seven ounces of extra-carbonic acid, so as to form 500 gallons of a solution of bicarbonate of lime; and that the two solutions are mixed, making up together 540 gallons. The nine ounces of caustic lime from the one pound of chalk unite with the seven extra ounces of carbonic acid that hold the other pound of chalk in solution. These nine ounces of caustic lime and seven ounces of carbonic acid form sixteen ounces, that is, one pound of chalk, which, being insoluble in water, becomes visible, at the same time that the other pound of chalk, being deprived of the extra seven ounces of carbonic acid that kept it in solution, reappears. Both pounds of chalk will be found at the bottom after subsidence. The 540 gallons of water will remain above, clear and colourless, without holding in solution any sensible quantity either of caustic lime or of bicarbonate of lime." (pp. 4-5.)

Nothing can be easier than to subject the water in the reservoirs of the different companies to this mode of purification.

In the pamphlet before us it is calculated that the daily supply of water to the metropolis is about 37½ millions of imperial gallons, and that if subjected to the patented process they would deposit no less than 24 tons of solid chalk, making an annual amount of 8000 tons! It is to be recollected that all this solid matter is such as *no filter can separate.*

To say nothing of the probable effects of this purified water on health, (the author does not advert to this subject,) the following are some of its more obvious economical advantages:

1. *The water will be much softened.* Not to mention the advantages hence resulting in regard to cookery, cleanliness, &c. the saving of soap and soda (for washing) will be enormous. This can be shown in the simplest and most striking manner,—*a similar lather being produced in the purified water by about one third of the quantity of soap that is required by the same water before it is purified.* In a word, *very hard water* is instantly converted into *very soft water* by the subtraction of the hardening ingredient, and without the addition of any new ingredient. The author calculates the probable consumption of soap and soda in London at £640,000 per annum, and reckoning the saving from the purified water only at 20 per cent. (a most reasonable estimate) we have here a saving of £128,000. "It is not, however," as he well observes, "alone in soap and soda that a saving arises from the use of soft water in washing. The labour in washing clothes is much increased by the use of hard water, and the wear and tear in consequence is probably a more expensive item than the additional soap."

2. *All Fur in boiling will be prevented*—a most important advantage to those who employ steam, it being well known how all machinery in which boiling water is employed, suffers, in various ways, from the deposition of the earthy matter from water.

3. *Vegetating and colouring matter will be separated.* This is proved by experiment. "The reason is probably twofold: first, the lime-water, which exists in the mixture for a short time as lime-water, may destroy the germs of vegetation; second, the chalk as it forms has a cleansing effect on water, from the property it then possesses of incorporating itself with diffused mud or colouring matter in the water."

4. *The water-insects will be destroyed,* probably by the same means as the vegetation.

The cost of the purifying material is quite insignificant, being little more than that of the burnt chalk required: Mr. Clark calculates this at less than £10 per day for all the water companies of London.

In conclusion, we would express our conviction that this is one of the most valuable applications of science to economical purposes that has been made for a long time, and we think that the inventor has thereby entitled himself to the gratitude of his country.

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ART. XVI.—*Tic Douloureux, or Neuralgia Facialis, and other Nervous Affections; their Seat, Nature, and Cause; with Cases illustrating successful Methods of Treatment.* By A. H. ALLNATT, M.D. A.M.—*London*, 1841. 8vo, pp. 184.

WE are much at a loss to know what can have induced Dr. Allnatt to publish the present volume. He must be well aware that it contains not a particle of novelty in doctrine or practice; and since the appearance of Dr. Rowland's very excellent Treatise on Neuralgia generally, there is no want of a compendium of our actual knowledge of the subject, as far as this is possessed by Dr. Allnatt. Had he, indeed, been acquainted with M. Valleix's recent most elaborate *Traité des Nevralgies*, and given some interest to his barren pages by transferring to them some of the new facts and deductions to be found therein, he might have advanced a better claim to the notice of the profession. As he has not done this, we shall ourselves give a full analysis of M. Valleix's treatise in our next Number; and in the meantime would recommend Dr. Allnatt not only to study it, but to study much longer the great book of nature, before he again ventures to commit his reputation with the public in a treatise on neuralgia.

Dr. Allnatt informs us that he is "firmly of opinion that Tic Douloureux arises, *in every instance*, from an unhealthy condition of the digestive apparatus," particularly of the liver, which unhealthy condition is elsewhere stated to be an "irritation," inducing, "in cases of long standing, hyperæmia." This irritation, of course, acts on the cerebro-spinal axis, and gives rise to the neuralgia at the peripheral extremities of the nerves. Dr. Allnatt's treatment, in accordance with this old and well-known theory, is precisely that of Abernethy and Sir Charles Bell, more especially of the latter author, as croton oil is his grand dependence. "I have found," he says, "the free use of aperients of *unfailing efficacy*, and I give a decided preference, over all others, to a pill combining a small quantity of croton oil with stomachic aperients." That this treatment has been successful, in a certain proportion of cases, has been shown by Sir Charles Bell and others, and that it is very proper, in certain circumstances, we do not attempt to deny; but we venture to pronounce, from our own experience in this distressing malady, probably somewhat longer than Dr. Allnatt's, that ere many years have passed over his head, he will be less ready to speak of its "unfailing efficacy." Dr. Allnatt's book might have been acknowledged to be a respectable thesis at graduation, but he has assuredly been ill-advised in claiming public attention to it.

PART THIRD.

# Selections from the British and Foreign Journals.

## I. THE FOREIGN JOURNALS.

### ANATOMY AND PHYSIOLOGY.

*On the Organic Causes of the Motion of the Heart.*
By Dr. J. Heine, of Germersheim.

This paper contains a remarkable case of occasional arrest of the heart's action, the appearances found after death, and the physiological deductions which the author believes they may justly warrant.

A man, thirty-six years old, was admitted into the Vienna hospital, complaining chiefly of fits, which he said depended on stoppage of the heart. It was soon found that at different times, to which there was no clue, the heart ceased to move for an interval of from four to six beats, its cessation of action giving rise to the most fearful sensations of anxiety, and to acute pain passing from both sides of the chest up the neck to the head, and there remaining fixed for some time after the fit. These attacks had already occurred frequently for a considerable time; they became more numerous after the patient's admission, and the pain of the neck and head becoming constant, he soon died comatose.

At the examination after death there were found considerable effusion of serous fluid into the cerebral ventricles without any traces of inflammation of the arachnoid, numerous small tubercles on the left hemisphere of the cerebellum, earthy and softened tubercles at the apices of the lungs, inclosure of the great cardiac nerve in a black bronchial gland as large as a hazel-nut, the same condition of the bronchial branches of the left vagus, and a remarkable increase of size of the cervical portion of the spinal marrow. All these however, with the exception of the tumour compressing the great cardiac nerve, having been often met with without the preexistence of any affection of the heart, the author ascribes to this tumour all the peculiar symptoms of the case, and draws from it the following among many deductions:

1. The great cardiac nerve is proximately the source of nervous influence for the action of the heart. 2. The interruption of its continuity destroys the heart's action, (at least its sensible action.) In the present case this interruption was only occasionally complete in consequence of the occasional enlargement and increased pressure of the tumour. 3. The muscle, even of the heart, is therefore not, as Haller supposed, independent of nervous influence. 4. Respiration, though it has an evident influence on the heart's action, has yet only a secondary influence, and is dependent for the exercise of this on integrity of the nervous power. 5. Since the occasional interruption of the continuity of the cardiac nerve in man destroys the action of the heart, and since with this interruption a shock is conveyed upwards, while at the same time the heart remains fixed, we must assume that the nerve acquires the power necessary for the maintenance of the heart's action from some part higher up. 6. The cardiac nerve has therefore, in these respects, the same relations as a cervical nerve. 7. Since this is not the only nerve going to the heart, and yet interruption of its continuity destroys the heart's action, there is no reason to believe in any communication of nervous fibres. 8. The pain in the neck and head experienced during the attack must be referred to a recoil of the nervous influence prevented

from discharging itself at the periphery, and therefore turning backwards and producing a kind of shock. 9. The ganglionic system, so far as it is connected with the heart, and therefore probably in all other parts is not independent of the central organs, but is in immediate connexion with them, and receives its motor power from them, and especially from the spinal marrow.

*Muller's Archiv.* Heft iii. 1841.

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*On the Stroke of the Heart.* By Dr. KUERSCHNER, of Marburg.

THE results which the author here publishes he states to have been obtained from many years' observation and experiment. He draws particular attention to what Haller* and some others have cursorily remarked, but which has been generally disregarded, namely, that in the systole of the heart it not only has its apex elevated or advanced forwards, but that at the same time the apex is always pushed somewhat towards the right side, and the whole organ partially rotated on its axis, the left ventricle, which in diastole (when the animal is on its back) is directed backwards and nearly invisible, being turned forwards so that a considerable portion of its front wall is exposed to view. He also points to the fact, that very often on opening and removing the pericardium, when the animal lies on its back, the rocking motion of the heart cannot be observed, though it is contracting vigorously, and this he ascribes to the fixed position which the heart takes against the vertebral column, and which is such that it cannot recede as it ordinarily does in diastole; for he found that on raising it by the pericardium from the spine, the rocking antero-posterior motion, with the rising and falling back of the apex became again perfectly distinct. Led by this fact to the belief that the elevation of the apex is the consequence, not the precedent, of its depression, he was induced to undertake a course of experiments to determine why the apex of the heart sinks or moves backwards in diastole.

These experiments, which were again and again repeated, consisted of artificial injections and imitations of the currents of blood flowing into the heart, managed so as to produce the same motions as the heart naturally makes. They were performed on dead foxes. These were placed on their backs; their hearts were exposed by the removal of the pericardia, and through the apex of each a thread was fixed, and then carried over a pulley to a scale carrying weight sufficient to raise the heart to its natural distance from the vertebral column. Injections were now forced in different directions into the trunks of one or more of the great veins, so as to fill one or both of the auricles and ventricles; then, the piston of the syringe being raised, the fluid was again withdrawn from these cavities, and again impelled into them; and so on alternately, imitating as nearly as possible the flow of blood into, and its removal from, the auricles and ventricles.

However the experiments were varied, and into whatever vein or veins the fluid was impelled, the same general result was observed; the scale was raised by the apex of the heart moving backwards towards the spine, and the whole heart turned round with force upon its axis from right to left, so that the left ventricle was directed completely towards the spine, and the right ventricle only presented forwards. On the other hand, when the fluid was withdrawn, the contrary movements took place; the apex was elevated and the scale fell lower, and the heart turned round its axis from left to right so as to expose a portion of the left ventricle. Only in the experiments in which the left pulmonary veins were injected was anything different from these noticed; for when this was done, the heart turned during the injection from left to right, and in the opposite direction when the fluid was withdrawn. The results were the same in whatever position the body of the animal was placed; and even when it was placed with the apex of the heart directed downwards, it still moved towards the spine when fluid was impelled into it from the veins.

* Elementa Physiologiæ, vol. i. p. 389.

From these facts the author deduces, that the sinking or movement backwards of the apex of the heart is dependent on the streaming of the blood into the ventricles; and that the direction in which the blood flows from the venous trunks to the auricles determines the direction of the heart's rotatory motion. The venous current being directed principally from right to left, gives this motion the same general direction, which is that which is observed during diastole in all living animals.

The elevation of the apex is regarded by the author as secondary to its sinking, and as the result mainly of the elasticity of the great vessels attached to the base of the heart. These, when the apex is carried backwards by the current of venous blood, must, by their position, be stretched; and therefore when the ventricles contracting close the auriculo-ventricular orifices, and prevent the further influx of venous blood, the great vessels return from their condition of tension, and again carry the apex of the heart forwards. They could not however do this with a force sufficient to produce a perceptible impulse against the front of the chest, unless the ventricles were, as they are, coincidently hardened and rendered firm by their contraction, and in some measure forced forwards by their recoil from the blood which they are impelling backwards and upwards. The same forces of elasticity and recoil give the heart its second rotatory motion; for the blood moves through the arteries chiefly from right to left, and the force exerted by the left ventricle is greater than that of the right, so that its recoiling force predominates, and the heart moves, as it always does in systole, from left to right.

*Muller's Archiv.* Heft i. 1841, *p.* 103.

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*Direct Experiments to determine whether Portions cut off from Leeches are reproduced.* By STEFANO GRANDONI.

BOSE, by whom Buffon's Natural History was continued, positively asserts the reproduction of divided leeches; in the Dictionnaire des Sciences Naturelles the contrary is stated. This contradiction with other circumstances induced the author to submit the question to the test of experiment. He divided 20 leeches between the 5th and 6th segments of their bodies, and placed half of them in glasses containing water with little stones at the bottom, and the other half in glasses containing a thin stratum of clay moistened with water. All possible care was taken to maintain them in health. After three weeks one of those that lived in the clay exhibited on its truncated extremity two white and somewhat convex gelatinous corpuscles, in the centre of which there was a more vividly coloured and transparent point; the rest of the section was somewhat rounded and covered by a very delicate membrane. These signs seemed to render it probable that the experiment was about to succeed; but a month after the leech died, and apparently from the disease of the truncated extremity. Three months after being divided, the sections of the other leeches were found covered by a dense rounded gelatinous mass, which after some time became a very fine and transparent investment, and made the surface of the divided part quite smooth. After about six months the leeches in the water with the pebbles all died one after the other, without any regeneration of the part that had been cut away. Their weight was neither diminished nor increased. At this time six of the remaining leeches were taken from the clay, in which they had till lately constantly lain buried, and put in water. These, as well as the three that were left in the clay, were then looked at several times every day; the condition of their trunks was from time to time examined with the aid of powerful lenses, and the number of their segments was counted, but no increase was observed. At the tenth month from the commencement of the experiment only the three leeches in the clay remained alive; these also died four months afterwards but without the reproduction of even the most minute ring. It was thus decided that divided leeches are not regenerated.

*Commentarij dell' Ateneo di Brescia*, 1837-8.

*Annali Universali di Medicina, Ottobre*, 1839.

*Microscopic Examination of Lymph.* By Professor Bischoff, of Heidelberg.

The fluid examined was taken from two large lymphatics in the neck of a dog. It was quite clear and pellucid, and after some time coagulated, but without assuming a reddish colour. It contained some yellowish glistening globules of no great size, having an average diameter of from $\frac{3}{1000}$ to $\frac{2}{1000}$ of a Paris inch, the largest being $\frac{4}{1000}$, the smallest $\frac{2}{1000}$. A nucleus and envelope could not be distinguished in them. They were not all quite round like the blood-globules; nor were they granular and nodulated. They were not altered by water, acetic acid, or ether; but in caustic potash they vanished immediately. Similar globules with the same reactions present themselves together with innumerable very small granules in the white contents of the thoracic duct and in the chyle.

*Müller's Archiv.* 1839, Jahresbericht, p. cxxxix.

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*Microscopic Experiments on Blood, Plastic Lymph, Pus, and Milk.*
By Dr. Letellier, of Saint Leu.

The following are the conclusions at which the author arrives as the results of his experiments: 1st, It is not possible to prove by the microscope that the red globules of human blood are formed of a nucleus and pellicle; 2d, But the microscope and chemical agents prove that these globules are formed of an envelope probably fibrinous, and of a transparent full nucleus; 3d, This nucleus offers the chemical properties of albumen coagulated by an acid; 4th, The albumen is evidently formed of transparent grains, becoming opaque where they are precipitated upon each other by alcohol or acids; 5th, Plastic lymph which runs from wounds, carries all the elements of the blood but the red colour of the globules; 6th, Pus principally contains, (*a*,) a great number of globules of blood deprived of colouring matter, and rendered opaque; (*b*,) a small quantity of vesicles of very varied forms and dimensions, formed by cells of fibrine; (*c*,) the débris of fibrine. *L'Expérience. Oct.* 1, 1840.

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*On Spontaneous Vomiting after Division of the Nervi Vagi.*
By Dr. J. Hoppe, of Berlin.

After a just criticism of the rather vague opinions of Valentin respecting the cause of this phenomenon, the author gives as the ground of his own decision regarding it, the following facts: 1st, He has never seen any animal vomit spontaneously after division of the nervi vagi, which had not the round stomach of carnivora, which, as Schulz observed, is so prone to the action. 2. He has very rarely seen it occur after the division of only one nerve. 3. The vomiting ensues immediately after the operation, and persists till the time of death with intervals of uncertain length. 4. The contents of the stomach were first vomited, and then mucus and gastric juice as soon as a sufficient quantity had accumulated. 5. After the animal had vomited once, it usually went on only retching till sufficient substance to be vomited had collected again. 6. The vomiting always took place during expiration, and ceased when that act was completed; and the straining continued longer only when the mucus was hard to get rid of. 7. The vomiting ceased when the muscles of the abdomen were divided. 8. There was nothing but straining when the stomach was empty at the time of dividing the nerves. 9. There was no vomiting when the animal breathed in any other way than is usual after division of the vagi. 10. And it ceased when this peculiar mode of breathing was altered by the faintness or collapse of the animal, or when by any circumstance expiration was performed without the jerking motion of the abdominal muscles. 11. The peculiar respiration of animals whose nervi vagi are divided consists of an infrequent, slow, long-continued, very forced and deep or rather great movement of the chest, which ends with a short jerking expiration. Dogs and cats during the inspira-

tion open their mouths so wide that the whole pharynx is raised; and in expiration they close it till they have vomited or retched, and they then open it wide again. The expiration is extremely violent; the arch of the chest is forced downwards, and at the same time pulled back by the muscles of the abdomen, with such force that air passes through the nose with a loud noise. During such an expiration vomiting always takes place, but *never* at any other time, 12. The inspiratory action is so powerful that the air passes into the œsophagus. so as to distend it; but it never gets into the stomach.

From these facts there can, the author holds, be no doubt that the vomiting is merely the mechanical result of the violent contraction of the abdominal muscles, with which expiration is performed after the division of both nervi vagi, assisted as their influence is by the wide-opening of the mouth in inspiration, by which the pharynx is raised up high and the œsophagus becomes distended with air.

*Casper's Wochenschrift. November* 14, 1840.

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*On the Development of Heat during the Incubation of Oviparous Reptiles.* By M. VALENCIENNES.

AT the sitting of the Royal Academy of Sciences, 19th of July, 1841, M. Valenciennes communicated the result of the observations he had made upon the female striped serpent in the *Jardin des Plantes*. It has not hitherto been positively determined if the animal heat of serpents is augmented during incubation. It would appear that the serpent in question, a native of the isles of the Indian Archipelago, would not require this increase of heat, the elevated temperature of the regions it inhabits heating the eggs without the necessity for maternal incubation. The facts established by M. Valenciennes have contradicted these presumptions. On the 5th of May last this serpent laid fifteen very large eggs, and immediately afterwards gathered herself into a spiral form, so as to surround the eggs in every part; and she did not cease to cover them during almost two months, never changing her position. When she was approached she put herself on the defensive, and appeared inclined to protect her eggs. She had made a meal of a living rabbit and a large portion of meat, but throughout the whole period of incubation she took no food, and on the 25th of May alone, about two glasses of water. On the 5th of July, the first serpent broke its egg, and in the space of twenty-four hours all the others appeared, making fifty-six days of uninterrupted incubation. At birth the young ones were about a foot in length, and the mother who had previously been so careful, abandoned them as soon as they were hatched, and paid them no more attention.

The cages in which the serpents are kept, and the coverings with which they are surrounded being artificially heated, it was not easy to distinguish the heat produced by incubation from that artificially developed, but notwithstanding this difficulty, M. Valenciennes is assured by twenty-four observations made with the greatest care, that while the temperature of the cage and coverings was 22° (72° Fah.), that of the body of the serpent covering the eggs was constantly 41° (106° Fah.) of the centigrade thermometer. It is not possible then to deny the unusual development of heat which accompanies incubation in reptiles of this class.

*Gazette Médicale de Paris.* 24 *Juillet*, 1841.

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*On the extreme Ramifications of the Minute Arteries and Veins in the Coats of the Intestines.* By Dr. GADDI, of Modena.

DR. GADDI has instituted some researches on the mode of termination of the arteries and origin of the veins in the coats of the intestines, by injecting pure water coloured with cinnabar for the arteries, and indigo for the veins, in children from two to five years of age unaffected by any intestinal disorder.

The arteries were filled by a general injection from one of the carotids, the current being directed towards the heart; the veins from one of the mesenteric. When the injection was successful, Dr. Gaddi has observed constantly that the

trunks of the intestinal arteries were guided by the peritoneal duplicature to the external layer of the muscular coat of the intestinal tube; they turn around this tunic, and penetrate it, dividing into an infinite number of anastomotic arches, more and more delicate, which traverse the second muscular coat to arrive at the submucous cellular tissue. There they all terminate in a thick tuft of almost imperceptible arterioles, not one of which ever penetrates into the mucous membrane, or terminates by a free orifice. The veins on the contrary always arise on the free surface of the mucous membrane by three, or at most four veinules, which in most cases have a visible funnel-like orifice, and immediately after penetrating the substance of the membrane, they converge together and unite in the submucous tissue, in a vesicle from which a very fine venous trunk arises, which latter soon associates with the trunk of the artery; passes with it to the muscular layers, where it divides; reunites into trunks of larger and larger size; and lastly leaves the intestinal walls, and passes between the folds of peritoneum.

The tuft of very minute arterial ramusculi just described envelopes the venous vesicle ["in a sort of atmosphere,"] so that it is there that the artery discharges itself. The vesicle is the point where the arterial takes the character of venous blood; and it is also the point which puts the two systems in communication, as Dr. Gaddi was assured in the rare cases where the arterial injection penetrated to the venous trunks, to the vesicle, to the branches, and even into the cavity of the intestines. On removing and examining with the greatest care the mucous membrane alone, he has never seen an artery nor venous vesicle, but only the venous radicles in radii. This vascular disposition is constant throughout the whole length of the intestinal tube, from the cardia to the rectum; but it offers some varieties: thus the mucous membrane of the stomach is better provided with venous radicles, especially towards the pylorus, than that of the duodenum; and they always diminish in number towards the rectum, the quantity being much less in the larger than in the small intestines.

It follows therefore that the generally admitted opinion of anatomists that the arteries and veins communicate by means of an intermediate capillary system is not correct as regards the intestines; that intestinal absorption is sufficiently explained by the capillary attraction of the venous tubes; that this anatomical disposition accounts for the rapidity with which certain substances penetrate into the circulatory system, and which could not take place through the long circle of chyliferous lymphatics; and lastly, it also explains the occurrence of hæmatemesis and melæna, which are nothing more than venous hemorrhages of the gastro-intestinal mucous membrane.

*Revue Médicale. Juin*, 1841. (From *Memoriale della Med. Contemp.*)

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*On the Presence of several Cysticerci in a Tumour having the appearance of a Boil.*
By Dr. Fournier, of Craon.

A child, six years old, had a tumour of the size of a hen's egg on the superior and lateral part of the neck, which had only appeared four days. It was red, hot, painful, of a conical form, and circumscribed. On examining it with care there appeared a small hole towards the base, in the middle of which a small white point was prominent, which had an almost imperceptible motion. A very limpid aqueous fluid flowed on pressure, and a particular but feeble sensation of *fremissement* was perceived. A species of clash (collision,) was distinguished by the ear, and the tumour, although red, hot, and painful, was soft and fluctuating, so that the presence of hydatids was diagnosticated. One of these worms was pressed out, and seven or eight more were removed by a small incision. They were afterwards recognized as cysticerci having a very small roundish head, supported by a contracted neck. The body was formed of imbricated rings perfectly visible to the naked eye; it was terminated by a small swelling, a kind of vesicle containing matter apparently albuminous. All performed some undulatory motions. The cure was complete on the seventh day.

*Journal des Connaissances Médico-Chirurgicales. Mai*, 1841.

*On the Temperature of Marine Invertebrata.* By VALENTIN and WILL.

THE authors have made observations on the proper temperature of seventeen kinds of marine invertebrata, comprising Polyps, Medusæ, Echinodermata, Helices, Cephalopods, and Crustacea. They found that all of them had a peculiar temperature varying with, but always somewhat surpassing, that of the medium in which they lived. The greatest difference amounted to 1°, the least to 0·1°; the former was observed in Pelagia denticulata, the latter in Aplysia leporina. With regard to the several classes the differences were among

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| Polyps, on an average | +0·205 |
| Medusæ | +0·27 |
| Echinodermata | +0·40 |
| Helices | +0·46 |
| Cephalopods | +0·57 |
| Crustacea | +0·60 |

Which numbers prove an increase of proper temperature directly proportioned to the ascent in position in theanimal kingdom.

*Muller's Archiv.* 1840. Jahresbericht, S. xcviii.

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*Experiments on the Development of Tubercles, and on the Action of Ferruginous Bread in preventing them.* By M. COSTER.

SIX rabbits of the same age (three weeks) and condition, were brought up from the 1st of April, 1838, in the manner described in the following numbers, 1, 2, 3, &c.

No. 1. Nourished in the open air, with a variety of herbs, as in its natural state, during one month.

No. 2. Nourished in a humid and dark cellar, the temperature varying only from 0 to 4°. All external motion impossible, the animal being inclosed in a very confined cage. Food: potatoes, turnips, and clover during five consecutive months.

No. 3. Placed in the same conditions with No. 2, but nourished alternately with the same vegetables, and with wheaten bread, with which carbonate of iron had been incorporated in the proportion of half a drachm to a pound of bread. A quarter of a pound of this bread was given in the day.

No. 4. Same external conditions as No. 1, but nourished as No. 3.

No. 5. Same conditions as No. 3, but instead of iron in the bread, iodide of potassium was employed in the proportion of 144 grains to the pound.

No. 6. Same as No. 2.

All were killed towards the end of August, when No. 1 was found healthy throughout. The tissues of the Nos. 2 and 6 were extremely flaccid, many glands engorged; tubercles existed to the number of three in the liver of one, and of two in the other; one in the spleen and many in the lung. Nos. 3 and 4 were healthy and well developed. No. 5 very lean, but without tubercles.

M. Coster states that he has experimented upon dogs, rabbits, guinea-pigs, and fowls, which he has submitted to the most injurious hygienic influences, and has separately combated these influences with iron, baryta, iodine, bromine, mercury, and tannin, and that hitherto the ferruginous bread has always prevented the production of tubercles.

*Bulletin de l'Académie Royale de Médecine. Avril* 15 *et* 30, 1841.

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*On Spontaneous Combustion of the Human Body.* By Dr. JACOBS, of Eupen.

FROM twenty-eight cases of spontaneous combustion collected and analyzed by the author, he concludes:

1. That spontaneous combustion always occurs in living human beings, never after death nor in the lower animals. 2. The subjects were generally very old, the two youngest being fifty and twenty-nine years of age. 3. Women are most frequently the subjects, it having only occurred in *two* men. 4. It was once preceded by jaundice, once by a malignant ulcer on the head. 5. All the per-

sons were alone at the time of the occurrence. 6. They led an idle life. 7. All were very fat, except three very lean females. 8. Almost all were intemperate. 9. Most frequently a light or some ignited substance was in the neighbourhood at the time of the accident. 10. The combustion proceeds very rapidly, and finishes in seven, three, and two hours, and even in one hour. 11. The flame, difficult to be extinguished by water, was very mobile, only destroying the objects placed very near, or in immediate contact with the burning body. 12. The room in which the combustion took place was usually filled with a thick vapour, and the walls covered by a black carbonaceous substance; the floor, ashes, and bones imbued with fat and fetid moisture. 13. The trunk was most frequently completely destroyed, some parts of the head and extremities usually remaining. 14. This combustion has occurred with only two exceptions during a cold temperature, in winter, and in the northern regions.

*Bulletin Général de Thérapeutique. Mai* 15 *et* 30, 1841.

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*Injections of the Iris.* By Professor Grimelli, of Modena.

The author has made some experiments to support the opinions of Dr. Fario upon the vascular erectile structure of the iris. The substances which answer best for very fine injections of this organ are olive or walnut oil variously coloured, which penetrates into the most delicate vascular ramusculi without transuding through their coats, and preserves for a long time the parts which they impregnate. In injecting the bodies of children, Dr. Grimelli observed, that from being soft and much dilated, the iris became turgid, and contracted more than half its diameter, in the same manner as when the retina is affected by light during life. This fact appears to prove that the iris is composed of a union of vessels forming a disc, in the centre of which is the pupillary aperture, and the circumference of which is attached to the ciliary ligament. By the aid of the lens and microscope, we see that the very fine vessels which constitute the iris are disposed between the pupillary and ciliary circles, under the form of rectilinear and curvilinear radii, curved upon themselves and zigzag; agglomerated and united in an inextricable manner. We observe also some ramifications disposed in circles between the pupillary and ciliary circles, more or less near each other, and always few in number. It results from this disposition of the minute vessels, fixed towards the larger circle and moveable towards the lesser, that the sanguineous afflux and turgescence unfold the iris and contract the pupil, and that the return of blood and diminution of turgescence, fold again or wrinkle the membrane and dilate the pupillary aperture. Thus, contrary to the generally admitted opinion on the muscularity of the iris, as it appears to the author, this membrane is composed of a turgescible or erectile vascular tissue, in which arterial vessels predominate, and Dr. Grimelli is led by analogy to conclude that the muscles of the small bones of the ear are constituted in the same manner.

*Revue Médicale. Juin,* 1841. (From *Memoriale della Med. Contemp.*)

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*On the Oxidation of Gelatine.* By M. Persoz.

M. Persoz has determined that when gelatine is submitted to an oxidizing agent it is susceptible of being transformed into hydrocyanic acid, ammonia, and carbonic acid, and a small quantity of one of the fat volatile and odoriferous acids, the existence of which was established by M. Chevreul. M. Persoz remarks that this fact leads to analytical researches to discover whether among the products of normal or abnormal cutaneous secretions, we cannot discover ammonia, hydrocyanic acid, or some one of its derivatives, as those composed of cyanogen and formic acid. He thinks that in certain cases of suppuration hydrocyanic acid may be formed, and states that M. Nonat has seen bandages and charpie tinted greenish-blue after having been for a long time in contact with the purulent matter of an abscess. This colouring may be attributed to Prussian-blue, but to verify and explain the fact, it will be necessary to observe the effect the pus of different wounds produces on pieces of linen impregnated with a salt of iron.

*Gazette Médicale de Paris. Juillet* 24, 1841.

*On the Motions and Sounds of the Heart.* By Professor CRUVEILHIER.

A CHILD which had been born nine hours was brought under the notice of M. Cruveilhier The heart was outside the chest, having escaped through a perforation in the superior part of the sternum; it was thus as completely laid bare as though the sternum had been removed and the pericardium incised. Its surface was dry and so pale that M. Cruveilhier thought at first that it was still covered by its fibrous envelope. This opportunity for studying the motions and sounds of the heart was not neglected, and the following are the results.

A. *Motions of the Heart.*

1. Both ventricles contract simultaneously. Both auricles do the same.

2. The contraction of the ventricles coincides with the dilatation of the auricles, and the projection of blood into the arteries. The dilatation of the ventricles coincides with the contraction of the auricles, and the collapse of the arteries.

3. There are but two periods in the motions of the heart: the period of contraction and the period of dilatation; the period of repose, admitted by authors, is completely wanting. (Qy.—Would this be the same in a new-born as in an adult animal?—*Trans.*) Contraction immediately succeeds dilatation, and dilatation contraction.

4. It appears that the contraction and dilatation of the ventricles and auricles result from two opposed forces always active, which prevail alternately in a necessary and invariable order, like the alternate movements of a pendulum, or of a balance-wheel in perfect equilibrium.

5. The duration of the contraction of the ventricles is double that of their dilatation. If we divide into three equal periods the whole duration of the ventricular systole and diastole, we have two for contraction and one for dilatation. The same holds good with regard to the auricles.

6. During the contraction of the ventricles their surface becomes rugous, strongly folded, and as if shrivelled. The superficial veins swell; the fleshy columns of the right ventricle are delineated; the curved fibres of the summit (sommet) of the left ventricle, which alone constitutes the apex (pointe) of the heart, become more manifest.

7. During their contraction the ventricles contract in every diameter; the phenomena of shortening is the most sensible, but this is owing to the vertical diameter being the greatest. The summit of the left ventricle, or in other words the summit of the heart, describes a spiral movement from right to left, and from behind forwards.

8. It is to this slow, gradual, as it were successive, spiral contraction that the forward movement of the summit of the heart is owing, and the consequent percussion of the summit against the thoracic parietes. The ventricular systole is not accompanied by a projection of the heart forwards, it is the spiral contraction which exclusively determines the approach of the summit of the heart and the thoracic parietes.

9. The dilatation or diastole of the heart occurs in an abrupt instantaneous manner; it is so rapid and energetic that it appears to be an active motion of the heart. One cannot form an idea of the force with which the dilatation triumphs over pressure exercised upon this organ. The hand closed upon it is opened with violence.

10. The ventricular diastole is accompanied by a projection of the heart downwards. This motion was at its maximum when the child was placed vertically, and was very strongly marked.

11. The dilatation of the auricles is as abrupt as that of the ventricles; but its duration is marked by the duration of the ventricular systole. The contraction of the auricles is as brief as the systole of the ventricles.

12. During its dilatation the right auricle appears ready to burst, so much is it distended and so thin are its walls. The left auricle, smaller, longer, and thicker, does not exhibit the same phenomena, at least in so evident a manner.

B. *The Sounds of the Heart studied with regard to the Motions of the Heart, and the cause which produces them.*

1. The ear applied to the heart, either bare or covered by a piece of fine linen, recognized the double sound of the heart, of which the first was much more feeble than when it is heard through the thoracic parietes. It is evident therefore, on the one hand, that the cause of the double sound is inherent in the heart itself; and on the other hand, that the first sound is increased by the thoracic parietes. The feebleness of the first sound cannot be accounted for by the feebleness of the strokes of the heart, for they were very vigorous, and the infant extremely lively.

2. This double sound increased in proportion as the ear was carried from the apex of the heart towards the base, and *vice versa*. Thus it is at the base of the heart that we must seek for the cause of these sounds.

3. The finger applied to the origin of the pulmonary artery, (which is situated before the aorta, and completely conceals the latter,) experienced a perfectly distinct vibratory *fremissement*, which corresponded with the collapse of the artery and the dilatation of the ventricle. It was but feebly perceived at the moment of the dilatation of the artery and contraction of the ventricle.

4. As it was impossible to apply the ear directly to the perforation of the sternum, the finger was used as a stethoscope, and on applying the ear to any part of this finger, a sound of clapping (*bruit de claquement*) was heard very clearly. The same result was obtained on applying the ear to the angle formed by the junction of the second metarcarpal bone, with the first phalanx of this (the index) finger.

5. The double sound was sought in vain; there was only one sharp sound, as brief as the second period. This sound coincided with the collapse of the artery, and consequently with the fall of the sigmoid valves, refolded by the column of blood. The use of the finger as a stethoscope was advantageous in combining the notions given by the sense of touch, with those by the ear.

6. The cause of the second sound therefore was very evidently in the vibratory thrill of the sigmoid, pulmonary, and aortic valves, thrown back by the column of blood.

7. In order to determine the cause of the first sound, and impressed with the idea that this sound had its source in the movements of the auriculo-ventricular valves, the finger was carried over the whole circumference of the base of the ventricles, in order to determine if there did not exist at the situation of the mitral and tricuspid valves some vibratory thrills analogous to those at the situation of the sigmoid valves. But none could be perceived, nor could any sound be heard by using the finger as a stethoscope over every accessible point of the base of the heart. Thus we were convinced that the mitral and tricuspid valves afforded no sound.

8. But if the first sound has not its seat in the auriculo-ventricular valves, if all the other parts of the heart are by themselves equally devoid of sound, and if the sounds which they communicate to the ear of the observer are sounds of transmission, is it not possible that the first sound has the same cause with the second, and results from the projection of the sigmoid valves by the column of blood, as the second does from their retrograde movement? This idea appears to be converted into a demonstration by the following conclusions:

A. In this child the maximum intensity of the first sound was in the same situation with the maximum intensity of the second.

B. The first was exactly of the same nature as the second, except as to the intensity which was less, and the duration which was greater.

C. If the seat of the two sounds be the same, they would both be altered in all diseases of the sigmoid valves, which M. Cruveilhier says is the case in all the cases he has seen.

D. We should not be astonished that no sound accompanies the action of the auriculo-ventricular valves, for they are not free, but retained by the *chordæ tendineæ*. The ventricular contraction takes place in a successive manner, the

apex of the heart moving towards the base; the elevation of these valves is consequently made in the same manner, and therefore without vibration. It is evident that when they are thickened, they will become more or less vibratile, and the resulting sound be confounded with that of the sigmoid valves.

E. It may be objected: if the sigmoid valves are the seat of the first sound, how is it that the greatest intensity is at the apex of the heart, and not at the base, the situation of these valves? The maximum of the first sound is at the apex of the heart when the ventricles contract with vigour, so that this point is strongly struck against the costal cartilages; but when the ventricles contract feebly, and consequently the percussion against the costal cartilages is feeble, M. Cruveilhier is convinced that it is behind the sternum, at the situation of the sigmoid valves, that the first sound is most intense. It is behind the sternum only that he has been able to recognize it in cases of dropsy of the pericardium.

The first sound is composed of two distinct phenomena: 1, the valvular sound; 2, the impulse of the apex of the heart against the costal walls. It is thus that in anæmia, chlorosis, and some organic diseases of the heart, the first sound is sonorous to such a degree as to have something of a metallic character, sometimes masking the second sound.

If the diaphragm were cartilaginous or osseous, resonant on percussion, it would be over the xiphoid cartilage, at the moment of the ventricular diastole, that the second sound would be most intense, in consequence of the abrupt projection downwards of the heart during the diastole.

[Without offering any opinion as to the validity of M. Cruveilhier's conclusion respecting the cause of the sounds of the heart, we may observe that it obviates one difficulty which we have always felt in assenting to all the other theories which attributed the two sounds to the action of different parts. This objection we stated many years ago in the following words: "In reference to this opinion of M. Bouillaud [that both sounds are owing to the play of the valves of the heart], as also to that of Dr. Hope [an early opinion, that the first sound was caused by the ventricular systole and the second by the ventricular diastole], I would observe that they both possess a degree of probability in my mind over all those which attribute the two sounds to *two* different causes. Although certainly characteristically different, yet the two sounds have so great a similarity, and are so allied in time and place, that I cannot readily bring my mind to believe that they do not both depend upon one and the same cause slightly modified, or, at least, on the different play of the same parts."—*Note to the Fourth Edition of the Translation of Laennec.*—ED.]

*Gazette Médicale de Paris.* 7 *Aout*, 1841.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

*Cases illustrating the Pathology of the Spinal Marrow.* By M. PRUS, Physician to the Salpétrière.

THIS paper is an account of a number of cases highly important in illustration of a class of diseases hitherto very imperfectly understood. We can merely give the chief features of each case, referring our readers for details to the original paper.

CASE I. Contraction of the extensors and flexors of both inferior extremities; preservation, but alteration of the sensibility of these parts. Progressive paralysis of the sphincters of the rectum and bladder. Death. Gelatiniform softening of the two inferior thirds of the spinal marrow.

CASE II. Sudden loss of speech; deviation of the mouth to the right; progressive diminution in the voluntary power of the left arm; incomplete para-

lysis of motion in the left side of the face without any alteration of sensibility; involuntary discharge of saliva; dyspnœa carried to commencing asphyxia; death; superficial softening of the whole circumference of the medulla oblongata.

Case III. Pricking and loss of sensibility of all the limbs, especially of the inferior; incontinence of urine; sloughs on the sacrum; death; slight softening of the posterior columns of the spinal marrow in the portion constituting the lumbar swelling.

Case IV. Loss of motion in the inferior extremities; sensation preserved; acute pains continued during many years in the lateral parts of the chest; purulent expectoration; hectic fever; softening of one portion of the spinal marrow; spinal meningitis; caries of the seventh, eighth, and ninth dorsal vertebræ; abscess communicating with the bronchiæ by means of numerous fistulæ; compression of the spinal nerves at their passage through the foramina of the carious vertebræ.

Case V. Progressive paralysis of motion only in all the limbs; paralysis of the sphincters of rectum and bladder; spasmodic respiration; sudden death in going to stool; double softening of the spinal marrow, that is, in the cervical and lumbar portions; the gray matter of the cord invisible in the diseased parts, almost so in others.

Case VI. Paralysis of motion only in the four limbs, which are slightly contracted; progressive paralysis of the sphincters of rectum and bladder; reddish softening in the middle lobe of right hemisphere of cerebrum, and in anterior lobe of left hemisphere; spinal marrow perfectly healthy.

Case VII. Paraplegia during three years; exostosis of the left clavicle; treatment by mercury and sudorifics; successive disappearance of the exostosis of the clavicle and of the paraplegia. *Revue Médicale. Décembre*, 1840.

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*On the Puerperal Fever observed in the Hôtel Dieu at Paris in* 1840.
By M. Bourdon.

The principal interest of this paper is in the account of the morbid appearances. In one case the only appreciable lesion was a slight effusion of reddish serum into the peritoneal cavity, holding in suspension small flocculi. In another there were all the anatomical alterations which characterize uterine phlebitis, but the most minute examination failed to discover the slightest quantity of pus deposited in the serous or synovial cavities, or infiltrated into the cellular tissue of muscles or of any organ. In a third case there were all the degrees of softening of the uterus described as gangrenous metritis, or putrescence of the uterus. Besides this gangrenous softening of the internal surface of the uterus, a perforation through the whole thickness of its walls had taken place. There were also traces of severe peritonitis, with fetid blackish effusion, the serous cavity communicating externally by perforations of the uterus. The subperitoneal cellular tissue and the subjacent muscles were infiltrated with pus of the same nature. Most of the organs were softened, and the small intestines were ulcerated without traces of surrounding inflammation. Another case offered all the characters of the pyrogenic fever of M. Voillemier. Pus was infiltrated in the cellular tissue of the limbs at their posterior part, and under the peritoneal covering of the uterus, without trace of phlebitis, inflammation of the uterine lymphatics, or metastatic abscess in the parenchymatous organs. In a fifth patient all these lesions coexisted, pus being united in a focus, or diffused in the subperitoneal cellular tissue of the pelvis, almost all the organs being softened, but especially the right lung, which broke up or tore like the tissue of the liver, without losing its property of floating in water.

Some alterations were constant in all the autopsies; a detritus, blackish or of the colour of wine lees, of a sickly odour, forming a layer of variable thickness over the internal surface of the uterus, the quantity being sometimes so great as

to cause considerable enlargement of the uterus. It may be considered as the coagulated product of the lochial secretion more or less putrefied, as the layer of false membrane which sometimes covers the internal surface of the uterus may be regarded as a peculiar exudation, independent of all inflammatory action. The blood was remarkably fluid in the heart and vessels, and in every case there was softening, more or less marked, of the parenchymatous organs and of the heart.

The only means which appeared useful in the treatment were methodical compression of the abdomen, and ipecacuanha. The author thinks that the latter does good, not only by exciting vomiting, but that it also acts in a "special unknown manner." Camphor in large doses, mercurial embrocations, bloodletting, and other usual means, were tried without good effect. Injections into the uterus appeared to be of service by removing the collections of fetid detritus.

*Revue Médicale. Juin*, 1841.

During the last few weeks puerperal fever has been epidemic in Paris; and though at first of slight severity, it has become rapidly so intense that M. Dubois has been obliged to clear his wards.

*Bulletin Général de Thérapeutique.* 15 *et* 30 *Juillet*, 1841.

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*On the Contagiousness of the Pseudo-membranous Inflammation of the Mouth, Pharynx, Œsophagus, and Stomach.* By Dr. Kessler, of Samter.

On the 1st of December, 1840, a robust woman, twenty years old, after exposure to cold, was seized with the usual signs of this disease, which continued till the thirteenth day, when the false membranes began to be thrown off, and the whole condition of the patient improved. After some days more she completely recovered.

Up to the fifth day of her illness she was nursed and watched by one of her relations, a woman named F.; she however became ill, and then a woman forty years old, and of rather a weak constitution, was attacked with the same disease, with a remarkable predominance of the nervous symptoms. The place of both these was then taken by an unmarried woman, S., twenty-seven years old, who had been reduced by a very laborious life, and she also fell ill with the same symptoms on the 9th of December.

At the same time another woman, G., who lived in the neighbourhood, and had often waited on the preceding, sickened with the same disease. In her it assumed many of the characters of typhus abdominalis, and she died on the 20th of December.

Another woman, twenty-eight years old, of a rather weak habit, and very excitable, now came to attend on F. and S. and waited on them. She became ill on the 15th of December with the same symptoms, and the disease in her took the same course as in them, the nervous symptoms being more than commonly predominant.

Her husband, a man fifty years old, who had drunk hard, and now attended steadily on his sick wife, fell ill with nearly the same symptoms on the 20th of December, and in him the disease deviated from its usual course into an attack of delirium tremens.

There were thus six cases of this membranous angina which in their mode of spreading evinced a contagious nature, or which, at least, seemed to spread only by contagion The patients were of different ages and constitutions, occupied different positions, led different kinds of life, and, for the most part, inhabited different houses.

*Medicinische Zeitung. Mai* 19, 1841.

*Ileus from Hypertrophy of the Pancreas.* By Dr. Holscher, of Hanover.

The subject of this case was a strong and apparently robust man, forty-eight years old, in easy circumstances, and the father of a numerous but scrofulous family. He had occasionally lived freely; but during the last six or eight months of his life, whenever he made a full meal, he always felt a great oppression at the stomach and umbilicus, and was obliged to drink a large quantity of water, as much sometimes as two or three quarts. His bowels were very costive, but still he scarcely thought himself unwell till the 20th of July, when he stated that, four days previously, after having eaten and drunk a good deal at a friend's house, he thought he had caught cold. A surgeon had given him an emetic on the following morning, and he had vomited; but since that time, though he had taken various purgatives, his bowels had never been open. He suffered from a constant desire to discharge fæces, and was always thirsty, but as often as he filled his stomach he vomited its contents again. He had but little pain but his anxiety was inexpressible. Pressure on the precordia gave no pain, and there was no trace of inflammation, but the abdomen felt rather tense. He was in a cold sweat and his pulse was 130; he had hiccup, and frequently vomited a clear, watery, acid-smelling, almost colourless fluid. In spite of a variety of means the patient died on the 22d.

At an inspection thirty-six hours after death, the abdomen exhibited neither effusion nor any trace of inflammation. But it was found that the pancreas had grown to the size of the head of a four-months' fœtus, and had so enclosed the duodenum for about three inches of its length, that its canal would not permit a goose-quill to pass. Anterior to this constriction the duodenum from the pylorus onwards was so distended as to form a kind of second stomach, in which for a long time past the food must have always undergone a considerable dilution before it could pass through the stricture. The hypertrophied pancreas had lost its normal granulated character; it had become softer, more succulent, and more fleshy, but when cut through it exhibited neither tuberculous matter nor any appearance like cancer or fungus hæmatodes.

*Hannoversche Annalen.* 1840. Heft ii.

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*Intestinal Strangulation produced by the Peduncle of a Fatty Tumour developed in the Cellular Tissue of the Mesentery of a Mare.* By M. Leblanc.

This mare died after having suffered for fifteen hours with violent colic; and at a short distance from the stomach the small intestine was strangulated by the elongated peduncle of a fatty tumour developed in the cellular tissue of the mesentery. The peduncle completely surrounded the intestine, arising on the left side of the mesentery, and being again attached by the fatty tumour which was appended to it.

*Bulletin de l'Académie Royale de Médecine. Mars* 31, 1841.

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*On Hemeralopia which was Epidemic in the Department Bouches-du-Rhône.* By Dr. Frechier.

Epidemic hemeralopia has often been observed. In the present instance it was first observed at Mausanne in the commencement of March last, affecting pregnant women especially, but sparing neither age, sex, nor temperament. In some it simply enfeebled vision after sunset; others were completely blind at night, although their vision was perfect during the day; and in others vision was imperfect even at mid-day, although it was not entirely abolished at night. The duration of the affection was about seven or eight days; the constituent parts of the eye were unaltered; indeed, except in the cases in which pregnancy was concomitant, the affection was absolutely isolated. The cause was evidently general and diffused, but its nature is subject for conjecture.

*Bulletin Général de Thérapeutique.* 15 *et* 30 *Avril,* 1841.

*On the Composition of the Urine in Pregnancy and under Disease.* By M. Donné.

At the sitting of the Royal Academy of Sciences, May 24, 1841, M. Donné presented a memoir on the above subject. The urine during pregnancy contains less uric acid and phosphate of lime than in the natural state, a difference easily comprehended on considering the elements necessary for the formation of the bones of the fœtus. The crystallization of the salts of the urine is thus modified in a manner so remarkable, that by simple inspection, without examining the females, M. Donné has in more than thirty cases recognized the state of pregnancy at its different periods.

In a healthy man the urine contains iron; in chlorosis it cannot be detected by the ordinary reagents, but it is discovered after the administration of ferruginous preparations, and thus the examination of the urine affords valuable indications as to the necessity for these medicines. Thus in certain forms of chlorosis, the urine contains iron, and in such cases iron will not prove serviceable. We may know also that the cure of chlorosis is complete if the urine contain iron several days after its internal administration has been suspended.

In typhoid fever the crystals of the urine present a radiant pearly aspect, and assume the appearance of crystals of phosphate of ammonia; but this character is not very important, for it is met with in other diseases, as in pneumonia.

M. Donné also announced that he had injected milk into the serous cavities of dogs, and even into the veins, and he is convinced that when the milk is pure, it produces no bad effect, but circulates in the vessels as the blood.

*Gazette Médicale de Paris.* 29 *Mai*, 1841.

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*On the Use of Oxalic Acid.* By Dr. Nardo.

Dr. Nardo has employed this acid in inflammations of mucous membranes, and finds that its antiphlogistic action is more marked than that of any other vegetable acid, possessing the property of instantly calming the severe pains which frequently accompany inflammation of the mucous tissue. He has used it with success in acute and chronic affections confounded under the name of angina, in different inflammations of the mouth, aphtha of newborn infants, gastritis, and gastro-enteritis.

The following is the formula he prefers:

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| Solution of gum arabic | 3 ounces, (94 gram.) |
| Oxalic acid | 1 to 2 gr. (15 to 30 cent.) |
| Gooseberry syrup | 1 ounce, (32 gram.) |

A tablespoonful (cuillerée à bouche) to be taken slowly at short intervals.

*Journal des Connaissances Médico-Chirurgicales. Juin* 1841.
(From *Observatore Medico.*)

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*On the Treatment of Scrofulous Affections by the preparations of Walnut Leaves.* By G. Negrier, Professor at the Preparatory School of Medicine at Angers.

M. Negrier has used the above remedy in seventeen scrofulous children, nine of whom had osseous enlargements with caries; seven ulcerated glands; one several swollen cervical glands with scrofulous ophthalmia of both eyes. Each patient took daily two or three cups of infusion of bruised walnut leaves sweetened with sugar or honey, and a four-grain pill of the extract of the leaves, or a spoonful of a syrup prepared with eight grains of the same extract to ten drachms of syrup. All the sores were washed with a strong decoction of the leaves, and covered with linen compresses steeped in this decoction, or poultices made with flour and the decoction. Seven of the patients were completely cured after six months of this treatment, and five nearly so; and M. Negrier concludes that the walnut leaves are superior to all other antiscrofulous remedies.

*Bulletin Général de Thérapeutique.* 15 *et* 30 *Mai*, 1841.

*Intussusception and Separation of a part of the Ileum.* By Dr. Forcke, of Goslar.

A mechanic, forty-nine years old, had been in the campaigns of 1812-13-15, and from that time had very often suffered from colic and other signs of abdominal disorder. In March, 1838, when first seen, he had not left his bed for five months; he was excessively emaciated, pale, and broken down by irritative fever; during all this time also he had suffered from costiveness, gripes, and vomiting. On examining the abdomen a large, long, hard, and sensitive tumour was felt on the right side, which occupied the usual position of the cæcum and ascending colon. Various antispasmodic means produced but little effect on the symptoms from which he suffered. The only ease he received was derived from his bowels being opened, and from the use of enemata containing opium. The pains in the tumour and in the abdomen rising to a fearful height, the patient was ordered calomel and opium, and emollient poultices over the whole abdomen. These diminished the pain, and produced free evacuations, rest, and sleep, but the tumour remained unaltered. On the 7th of April a severe hemorrhage from the intestines occurred, and was followed by the discharge of pus, which however ceased when the patient took large quantities of lime water.

On the 28th of April the patient had an enormous discharge from the intestines, which made him feel as if something had been torn out of his abdomen, and induced him to examine the evacuation. He found therein in the midst of fæces and blood, a portion of ileum which measured two feet nine inches and a half along its convex border; it was dark and livid, but had a firm texture, and was still connected with a portion of mesentery. Its discharge was followed by a slight hemorrhage and a considerable secretion of pus, which however again ceased after using lime water. The patient was almost speechless with exhaustion, but the pain and the tumour had nearly disappeared. Tonics were administered, the fever diminished, and the strength rapidly increased. In the following October he had regained all his former strength and was hale and hearty, except that, on severe bodily exertion, the sensation of tension around the umbilicus, and of dragging of the stomach, would still return.

*Hannoversche Annalen.* Bd. iii. Hft. iv.

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*Case of Deficiency of the Uterus.* By Dr. Wehr, of Kassel.

The patient was a little woman, who was born of weakly parents, and died aged fifty-four. At the age of ordinary puberty her breasts did not enlarge, nor did she either then or ever after menstruate, though at almost regular monthly periods she had pains in the limbs and other parts of the body. Throughout life she enjoyed moderately good health, and suffered from no singular symptoms; she died of pneumonia.

After death the clitoris was found very much developed, but there were scarcely any traces of nymphæ, and but little hair about the external organs. The vagina was very small, the hymen perfect, the orifice of the urethra large. The form of the uterus was preserved in the arrangement of the peritoneum, but very little of its substance was found in what corresponded to its body and cornua, and only a little more in its cervix. In the left broad ligament was a small tubercle. The left ovary resembled only an elongated flattened transparent cyst full of fluid; the right ovary was in nearly the same state, but appeared connected by a little tubercle with the rim of the pelvis. The Fallopian tubes were of about the normal length, but their size was small and proportioned to that of the uterus and ovaries, their abdominal orifices were small and surrounded by a few imperfect fimbriæ. The vagina was small; its walls were thin and but slightly wrinkled internally. The orifice of the uterus was flaccid; the cervical part of the cavity was wide, and was continued through the body into two separate cornua.

*Casper's Wochenschrift. Mai* 8, 1841.

*On Syphilis in Pregnant and recently-delivered Females.* By M. HUGUIER.

M. HUGUIER has made a long series of observations on this subject, and has arrived at the following conclusions:

1. The sole primitive phenomenon of syphilis is sometimes a pustular eruption at the vulva. This is often the commencement of chancre. 2. Purgatives are not proper for pregnant females tainted by syphilis. 3. Abortion in syphilitic females is rather in consequence of the exhibition of mercury than of the disease itself. 4. The venereal disease has not the grievous influence on pregnant females which is attributed to it. In twenty-seven females in this condition, only three are dead, and probably their death resulted from lesions unconnected with syphilis. 5. The mercurial treatment does not always preserve the infant nor protect from relapses, and often produces serious results. 6. The sublimate is the most dangerous among the mercurials, and mercurial frictions are preferable to it. 7. Syphilis is very often hereditary; and it ordinarily declares itself from three to thirty days after birth. This last remark is important, for if the child be examined on the first or second week, it is declared healthy and sent out to nurse, probably with the effect of infecting the female.

*Archives Générales de Médecine. Aout*, 1840.

## SURGERY.

*Absence of the Nasal Duct, and its Artificial Formation.* By M. BÉRARD.

A MAN, twenty-one years of age, was admitted into the hospital Necker on account of congenital fistula lachrymalis. This fistula discharged a limpid transparent fluid, and caused continual epiphora. On pressing on the angle of the eye in the morning a muco-purulent liquid flowed from the fistulous orifice and from the puncta. The nostril of the same size was habitually dry; stimulating powders, such as snuff, becoming dry without exciting the secretion of the pituitary membrane. A stylet introduced into the fistulous orifice in the direction of the nasal duct would not pass, nor was it possible to penetrate its nasal orifice. No doubt could exist, therefore, of its congenital absence, and M. Bérard made an artificial nasal duct by piercing the os unguis after the manner of Woolhouse.

The inferior border of the internal portion of the tendon of the orbicularis being laid bare by incision, M. Bérard directed a trocar downwards, backwards, and inwards, perforating the internal wall of the orbit. The trocar was immediately replaced by a silver canula about half an inch long, enlarged at its two extremities, and on closing the mouth and nostrils of the patient, the air passed through the canula, showing that it was well placed. Three days after the operation the small wound had cicatrized; no bad symptoms followed. In two months, the patient having neglected the directions of the surgeon, returned with epiphora, when M. Bérard changed the canula, and in two months the epiphora had completely disappeared, and the patient was quite well in February last.

*Bulletin Général de Thérapeutique. Juillet* 15 *et* 30, 1841.

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*New Artificial Leg.* Invented by M. LEPAGE, a Stonemason of Paris.

THIS artisan having had the right leg crushed by an enormous stone submitted to amputation, and being poor was obliged to follow another trade and became a shoemaker. The ordinary wooden leg without joint did not suit his new condition; he broke several, and he therefore contrived a leg with two joints, the one corresponding to the knee, the other to the hip. By means of a button applied to a spring, which descends laterally from the superior to the inferior articulation, these two joints are easily accessory to different movements, especially those of sitting down and rising up. The contrivance is very ingenious. The cost is about two guineas.

*Bulletin de l'Académie Royale de Médecine.* 15 *Juillet*, 1841.

*On the Curative Influence of Galvanism in some of the Organic Diseases of the Eye.* By Dr. Lerche, of St. Petersburg.

[The following is the report made by Dr. Lerche, one of the principal physicians of St. Petersburg, on the application to opacities of different parts of the eye of the galvanic action which Dr. Crusell, a physician of Finland, was said to have employed with great success for the removal of other kinds of thickening, &c. consequent on inflammation.]

To Dr. Crusell of Finland is due the honour of having employed galvanism in a new and peculiar manner for the cure of organic diseases. Having been attracted by the treatise on the subject which he had sent to the Imperial Academy of Sciences, I had proposed to repeat the galvanic experiments instituted by him on the diseases of the lens in animals. The necessary apparatus was not yet completed when Dr. Crusell himself arrived at St. Petersburg; we therefore determined to experiment in his presence and with his assistance. For the subject of the first experiment we chose a complete leucoma of the cornea, which was deemed incurable, and in which, therefore, nothing could be lost, and perhaps something might be gained. The patient, a boatman sixty-eight years old, was in our hospital for chronic inflammation of the other eye. Our apparatus, according to the directions of Dr. Crusell, consisted of a simple galvanic circle of one zinc and one copper plate, both immersed in dilute sulphuric acid. The wire proceeding from the copper-plate, which we named the copper-pole, was placed in contact with the leucoma; that coming from the zinc-plate, the zinc-pole, was made to touch the patient's tongue, and the galvanic current was maintained for two minutes. The patient did not suffer in the least degree, neither did any unpleasant symptoms follow the operation; on the contrary, the chalky dulness at the margin of the cornea appeared somewhat thinner and clearer. The operation was repeated after three days, and we saw again a partial alteration in the state of the leucoma; and the patient, on his part, asserted that he had an increased perception of light.

The idea was now not remote to apply galvanism for the cure of opacities of the internal tissues of the eye. The results obtained by Dr. Crusell, in his experiments on the eyes of animals, strengthened our expectation of doing good, and we had opportunities enough of repeating them on eyes which had been regarded as almost or quite incurable. For safety's sake, however, we determined first to see the effects of the operation on some animal, and a young pig was chosen for the purpose. A fine cataract-needle fixed on the zinc-pole was passed through the cornea into the crystalline lens of the right eye, and the wire from the copper-pole was stuck into the ear. After thus remaining for four minutes the pupil began to grow turbid, and the operation was discontinued on the right but repeated on the left eye. Some days after we found completely-formed cataracts in both eyes, and the pig was quite blind.

Now, according to the theory, the artificially-produced dulness of the lens ought to be removed by a reversed galvanic action, and before we could feel justified in undertaking similar experiments in human eyes it was necessary to obtain the proof that such would be the case. After ten days, therefore, we proceeded to this second operation. In three minutes, (the galvanic current being maintained through the wires which were now so placed that the copper-pole was connected with the eye and the zinc with the ear), there appeared bubbles of gas in the pupil, and the process of solution seemed to be going on; the experiment was therefore at once brought to a close. The pupil appeared smoky and less dull. In four days it had almost regained its natural clearness; and the sight, so far as we could judge from the animal's action, was restored. In the cornea there was an opaque spot around the aperture made by the needle. This result at once determined us to make an experiment of a similar kind on the human eye.

We chose an old copper-smith from Finland, who had a long time previously been successfully operated on for cataract in the left eye. The right eye had presented a firm yellowish-brown capsulo-lenticular cataract, which had adhe-

sions to the iris. It had been once depressed but it rose again; and in another attempt to break it up, nothing more could be done on account of its hardness, than the making of one vertical cut through its substance. This, however, rapidly closed, and for two months afterwards a variety of means were employed for the solution of the cataract without the least benefit. It was now very large, and lay close behind the widely-dilated, somewhat irregular, and immoveable pupil. The patient had a perception of light through it.

The galvanic operation was commenced in the presence of Dr. Crusell, in the forenoon of the 11th of November. It was very surprising to us to see, when the fine cataract-needle fixed on the copper-pole was inserted into the lens, and the zinc-pole laid on the patient's tongue, how, in less than a minute, the cataract swelled up, increased in size, seemed to press against the cornea, and then suddenly burst into three pieces, of which one was carried upwards and inwards, and another towards the temple, and the third projected downwards through the pupil into the anterior chamber. The triangular fissure left between them appeared clear and black. We now thought it best, as this was the first experiment of the kind on a living man, to stay the galvanic action. The patient could already see sufficiently to discern, with his right eye alone, both a finger held before it, and the face of a person standing in front of him. The operation lasted a minute, and neither produced pain or was followed by inflammation or any other bad symptoms.

Here the history ceases; the authors thought they had better leave the case with a limited amount of amendment, than run the risk of marring the advantage they had gained, and of throwing unnecessary discredit on the operation by going further. The facts, however, seemed to merit record as proofs that the plan is without danger, and offers some prospect of effecting a permanent cure of the disease.

*Medicinische Zeitung. Juni* 16, 1841.

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*M. Ricord's New Urethro-plastic Operation.* By M. HELOT.

F. J. M. a shoemaker, twenty-six years old, came under the care of M. Ricord on the 16th of June, 1840. He said that when seven years old he took it into his head to tie up his penis with a thread, and after tying it tightly a little way in front of the scrotum, there followed on the next day a considerable swelling of the adjacent parts, in the midst of which the thread disappeared, cutting its way through the skin. To the swelling and division of the integuments, which cicatrized almost as fast as they were divided, there was added a retention of urine which lasted, the patient says, fourteen days. At the end of this time the urethra was divided by the thread, and a large quantity of urine came away; after this severe symptoms ensued, which, however, in six weeks entirely disappeared, leaving only a kind of constricted cicatrix around the whole circumference of the penis, with the two opposite ends of the divided urethra exposed. The patient was also affected with phymosis.

From this time, says the report, the patient's infirmity has scarcely troubled him. The emission of urine takes place in him as in those affected with hypospadias; during erections, which are complete, the penis remains perfectly straight, though the glans does not seem to participate in the erection so much as it should. Neither is this portion of the organ the seat of any of the sensations which are peculiar to it in its perfect state.

On the 1st of January this man was attacked by gonorrhœa eight days after connexion. The disease commenced in the posterior portion of the urethra, and it was not till four days afterwards that (the two orifices being in contact) the anterior part of the canal was affected. Then the discharge went on at once through the posterior aperture and both the anterior ones. When the patient came into the hospital the discharge was abundant and very purulent; the posterior or vesical portion of the urethra was the seat of pain in the passage of the urine; the other part was only a little painful on pressure.

The acute symptoms having yielded to reducing treatment, a daily dose of

twenty-four grains of powdered cubebs was ordered. After a few days the discharge had considerably diminished in the posterior part of the urethra, but in the anterior it was unaffected; after six days' use of the cubebs it had entirely ceased in the former situation, but in the latter it still continued.

During this time the two portions of the urethra had been carefully kept from contact; but on now permitting them to come together the posterior part which had been cured was again infected by the anterior, and had again to be treated by cubebs. The anterior part was at the same time treated and cured by injections of nitrate of silver.

After this it was proposed to endeavour to cure the organic defect of his urethra by a plan somewhat similar to that which M. Segalas had once successfully employed. The patient was (on the 3d of November) placed as for lithotomy. A large catheter having been introduced by the opening of the posterior part of the canal into the bladder, an incision, commencing immediately behind the situation of the bulb, was made in the middle line of the perineum and into the membranous portion of the urethra, through which, with some difficulty, a female catheter was then passed into the bladder. This being fixed, M. Ricord next operated for the phymosis, that he might have more yielding integuments behind the glans; and then having revivified the edges of the apertures in the urethra he brought them together (after all bleeding had ceased) and fixed them by sutures. A bougie had, before drawing the sutures, been passed through the two parts of the urethra down to the catheter in the membranous portion.

After the operation the patient was kept very quietly, with his lower limbs somewhat raised, and in the evening he was bled. Next day the urine had flowed freely through the catheter in the perineum; the wound was wet with urine, and its edges were swollen; on the third day the sutures came out; there was no union, and the urine escaped by the fistulous opening as it did before the operation. The edges of the fistula, however, preserved a transverse direction, and such a thickness as to lead one to hope that fresh trials at union might be more successful. On the 15th of November a considerable abscess which had formed in the perineum was opened at the scrotum, and discharged for some time pus and urine; but after it had healed fresh attempts were made to induce all the urine to pass through the opening purposely made in the perineum by introducing into it each day a larger catheter. This method was at last successful, and on the 19th of January a new union of the aperture was made by twisted sutures over a bougie introduced to a shorter distance beyond the aperture than the first had been. Three days after this operation the two external pins were removed, and the union of the parts near them was found complete. Next day the two others were removed, and there remained but a very small aperture through which a little urine and pus flowed. This was touched with nitrate of silver, and in a week (on the 1st of February) it had closed; another then appeared which remained for some time, but was also ultimately closed.

On the 12th of February, three months and some days after the first operation the perineal catheter was withdrawn and a small one introduced through the whole urethra. Through this latter the urine now all passed, and the wound in the perineum rapidly healed. At the time of the report the patient was perfectly well; the penis had regained its form and discharged all its functions normally.

*Annales de la Chirurgie. Mai*, 1841.

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*On the Application of Oil-varnish to the Immoveable Apparatus, in Fractures of the Limbs of Children.* By M. Tavignot, House-surgeon of the Hospital of Children's Diseases.

When the common immoveable apparatus is applied to the fracture of the lower limb of a young child, at the end of two or three days it becomes perfectly moveable by saturation with urine, and therefore totally incapable of maintaining the fractured parts in apposition. After some trials with glazed silk and

spirit-varnish, which were soon permeated by the fluid, M. Tavignot found that oil-varnish answered the purpose extremely well. The best mode of using it is to apply the starch-bandage first, and this having become completely dry a layer of the varnish is to be smeared over its whole external surface, and also over its superior border and part of its internal surface by a fine brush. The limb is then to be exposed to the air in warm weather, and to artificial heat in winter. Another layer of varnish is to be applied in about two hours, being applied especially on the parts of the limb most exposed to the action of the urine. When the layer has dried the ordinary coverings of the limb may be replaced, and the urine then glides over the surface of the bandage with great facility without affecting it in the least. M. Tavignot has placed a bandage thus prepared in constant contact with urine, by putting it into a vessel full of this fluid without altering its consistence in any degree.

This is not the only advantage, for the apparatus can be removed without cutting the bandages, by placing the child in a warm bath, for the water insinuates itself between the bandage and the limb, and so softens the former by dissolving some of the starch, that the limb is readily unrolled.

Paper steeped in this varnish appears likely to prove highly useful to the surgeon in protecting parts exposed to the action of acrid or irritating fluids.

[The composition of this varnish is not given. It is probably the common size-varnish of our oil-shops.]

*L'Examinateur Médical. Aôut* 1, 1841.

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*Successful Case of Excision of the Head of the Humerus, after Compound Fracture.* By Dr. HELLO, of Cherbourg.

AN English boy, fifteen years of age, fell from a height on the 16th of April, 1841, and sustained a fracture of the humerus at the situation of its surgical neck. The lower fragment tore the integuments between the great pectoral and deltoid muscles, and appeared through the clothes. There was considerable hemorrhage at the moment of fracture, but the fracture was almost immediately reduced, and the bleeding ceased. On passing the finger into the wound it was found full of clots; the inferior fragment of the humerus was denuded of its periosteum to the extent of about six centimetres; the superior fragment, very short, offered a number of small splinters which were almost attached to the articular capsule. I at once determined to lay bare the shoulder-joint, with the double purpose of resecting the humerus and extracting the head of the bone. A semicircular incision was made, commencing a little below the coracoid process, following the line which separates the great pectoral and deltoid, turning backwards at the insertion of the latter which was cut to the bone, and remounting to the acromion along the external border of the deltoid. The flap thus formed was turned upwards, and the arteries nourishing it tied. The superior part of the humerus was then carried from the wound, (which was easily done by seizing the condyles to direct the bone,) the periosteum surrounded with the bistoury below the situation where it was torn, and after having placed a piece of pasteboard between the flesh and the bone, the denuded portion of the latter was removed by an ordinary saw. The posterior circumflex artery which bled freely was tied; and being cut so near the axillary trunk it is not improbable that this might render secondary hemorrhage frequent after this operation, the formation of the clot being prevented. The axillary artery was so superficial at the bottom of the wound that I should have passed the ligature around it, would not this have hazarded the success of the means employed to save the limb. The musculo-cutaneous nerve which had been torn was excised, the flap reapplied, three sutures inserted, and the parts were then kept in apposition by adhesive plaster, and covered with compresses of charpie, the whole being maintained by a bandage crossing under the opposite axilla. The forearm and hand were placed in a sling, and the arm was separated from the chest by a soft cushion of charpie. The patient went on without a bad symptom until

the 19th, when there was some fever, with fetid and abundant suppuration. On the 23d there was less fever, and the suppuration more healthy. Two ligatures came away. Sutures removed. Internal part of wound cicatrized. All the ligatures had come away by the 29th. On the 1st of May a well-marked intermittent set in, for which quinine was given, and continued for several days. The wound had perfectly cicatrized by the 31st. The patient could seize small objects with the hand, and having recovered his health wished to return to England immediately.

This case is a very interesting one, showing that, under very favorable circumstances, a useful limb can be secured to the sufferer, and pointing out a very easy mode of performing the operation. The author states that he followed the same proceeding on a Turk at Marseilles, but he was sent to Smyrna on the fifth day, and the result has not been ascertained.

*Annales de la Chirurgie. Juillet* 1841.

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*On a new variety of Hernia: Hernia destitute of Peritoneal Sac.* By M. Dumeaux.

The following remarkable disposition of parts was observed in the body of a man between 55 and 60 years of age, who had a large scrotal hernia on the right side. After removing all the coverings of the tumour, the hernial sac, or what appeared to be the sac, instead of being a uniformly resisting membrane, presented very distinct muscular fasciculi, and the posterior wall of the cæcum was immediately recognized. On examination by the abdomen it was found that the cæcum instead of occupying the iliac fossa, was placed on the abdominal wall, and covered by peritoneum on one of its surfaces only. Its adherent wall was engaged in the inguinal canal forming part of the hernial tumour. The free wall, covered with peritoneum followed, being as it were invaginated in the former, and forming towards the abdomen a cavity or true hernial sac, in which seven or eight inches of intestine were engaged.

This singular species of hernia has never before been described. The preparation was presented to the Anatomical Society.

*Annales de la Chirurgie. Juillet*, 1841.

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*Two Cases of Strabismus cured without Operation.* By M. Beydler.

A woman, thirty-three years of age, regular in menstruation, had been affected with diplopia for six weeks. There was convergent strabismus of the right eye, apparently owing to paralysis of the external rectus. It manifested itself after noises in the head. Half a grain of strychnine was dropped into the eye daily, which was followed by amelioration; and complete reestablishment of the sight succeeded six applications of electricity, twenty or thirty sparks being disengaged at the external angle of the eye at each application.

A man, thirty years of age, after acute mental suffering caused by his trial in a court of justice, suddenly squinted with the left eye. He was acquitted, and after fifteen days the two eyes became again perfectly parallel.

*Annales de la Société de Médecine de Gand. Septembre*, 1840.

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*On the Radical Cure of Spina Bifida, by a New Operation.* By M. Dubourg, Physician to the Hospital of Marmande.

Having observed that the ordinary method of treating spina bifida was far from successful, and reflecting that when the soft parts over bones separated by arrest of development are brought together by suture, they have considerable effect in approximating the edges of bone, as in cleft palate, M. Dubourg concluded that by bringing together the soft parts over the defective spot in the spinal column, a radical cure might be effected by producing permanent approximation of the edges of the bony opening. The first case in which he put his ideas into execution proved unsuccessful, the child having died two days afterwards, but in two other cases the success was complete.

CASE I. In the spring of 1837 I was called to a female infant eight days old, who had a congenital pediculated tumour on the lumbar region, about the size of an apple. Its colour was livid from the development of a venous network, giving it the appearance of a vascular fungus. It was evident on examining the vertebræ that they were defectively formed, the edges of a bony opening being discovered. The tumour was opaque, its walls much thicker than usual, and it appeared that the arrest of development was confined to the last lumbar vertebræ, the child being otherwise healthy and well formed. Having the cautery in readiness in case of severe bleeding from the vascular coverings, and being prepared to prevent the sudden escape of the spinal fluid, an elliptical incision was made around the base of the tumour, when a quantity of reddish serum escaped, and the excision of the sac was readily effected. The finger passed readily into the spinal canal. The edges of the wound were brought together, and four needles being passed, the twisted suture was applied as in a case of hare-lip. The threads were twisted so as to exert as much traction as possible on the contiguous parts, and small compresses were placed at the extremities of the needles to protect the skin. The child cried sharply at the commencement of the operation, but as soon as the fluid escaped from the spinal canal it fell for a few minutes into a state of stupor. It cried again as the needles were applied, and by the time the dressing was completed it took the breast as though nothing had occurred. The needles and sutures were removed on the fourth day, when the edges of the wound were found to be united. Adhesive plaster was applied, and in fifteen days a strong cicatrix, forming a sort of solid button filling up the opening in the vertebræ, was all that remained of this reputed incurable disease. The examination of the removed tumour demonstrated that it was a cyst distended by fluid, communicating with the spinal marrow, bounded behind by the common integument and an expansion of the arachnoid and dura mater. The cavity was not in proportion with the volume of the tumour, for there were several layers of cellular and adipose tissue between its external and internal surfaces.

CASE II. The next case was similar, except that the swelling was situated over the junction of the last cervical and first dorsal vertebræ. The same operation was performed except that the sac was not perforated in the first incision, a lateral flap being formed with a narrow bistoury from within outwards, and the sac being then removed and the other flap formed by a second incision. The needles and sutures were applied as soon as possible to prevent the loss of any considerable quantity of the spinal fluid, and the access of air. The child recovered without an unfavorable symptom and continues well, eighteen months after operation.

The following are the conclusions drawn from these cases by the author, some of which, at least, are premature:

1. Some cases of spina bifida are susceptible of a radical cure.
2. Instead of abandoning to their fate most children born with this malformation, those should be selected in which this operation might be efficacious.
3. Although it is impossible absolutely to establish limits of incurability, every child born with a spina bifida, of which the opening of communication with the spine does not exceed an inch in diameter should be submitted to operation.
4. Ablation of the tumour and the twisted suture constitute the best operation.
5. It is well to open the spinal canal as late as possible in the operation, and to make the opening narrow.
6. When the sac is formed by the meninges, only the skin being atrophied over the tumour, the skin must be dissected on each side of the vertebræ, and the edges of the integuments united as in hare-lip.
7. The probability of reunion is proportionate to the size of the osseous opening and the general state of the patient.
8. The spinal canal may be opened, the spinal marrow laid bare, and a considerable portion of the fluid which bathes this important organ lost with impunity. *Gazette Médicale de Paris. Juillet* 31, 1841.

*Improved Method of Scleroticonyxis.* By Dr. Ruete, of Göttingen.

The first part of this operation is performed in the same way as in the ordinary proceeding for the depression of a cataract. But in the second part, the author does not endeavour, as most operators do, to pass the needle into the posterior chamber between the iris and the capsule (because this is neither a safe nor an easy proceeding), but he carries the needle, with its convexity turned forwards and its cutting edges upwards and downwards, about half a line from the uvea, and then when it has passed for a line or a line and a half into the eye, he makes with its cutting edge a vertical section through the capsule, and carries it on between the capsule and the lens till its point appears at the middle of the pupil. Then he turns the point of the needle forwards, strikes it through the anterior wall of the capsule, and cuts the latter from above downwards and from within outwards. The remaining stages are performed in the ordinary manner of depression, reclination, or breaking up.

The advantages of this method are stated to be as follows: The section through the lateral part of the capsule and the zonula Zinni prevents the tearing and stretching of those parts which in the ordinary method of operation is scarcely avoidable. There is no danger of injuring either the ciliary processes or the iris. The anterior wall of the capsule can easily be cut through, and there is no risk of capsular cataract remaining, for the four portions into which it is cut shrink up, and when the pupil is dilated with belladonna may be seen forming a grayish-white circle behind the ciliary body. All the injury inflicted being done by cutting, and none by tearing, it is very rarely that the operation is succeeded by inflammatory reaction.

The author has practised his new method successfully in twenty-eight cases.

*Holscher's Annalen.* Bd. iii. Hft. iv.

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*Case of Pruritus Scroti cured by fresh Lemon Juice.* By Dr. Oppler, of Tarnourtz.

This was an extremely distressing case, that had resisted all internal and external means for ten weeks, depriving the patient of sleep, and producing incessant distress. The pruritus extended to the penis, and was accompanied by no primary rash, nor any perceptible local alteration except what was produced by the friction. A wash of diluted lemon-juice gave immediate relief, and after a few applications produced a perfect cure.

*Berlin Med. Zeitung. June* 30, 1841.

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*On the Condition of the Nails in Fractures of the Limbs.* By Dr. Troschel.

Dr. Pitschaft mentions, in Hufeland's Journal, St. viii, the remarkable fact that the nails do not grow during the union of a fracture in any of the extremities, and that the commencement of their growth is a proof that the fracture has completely united. Dr. Lester is mentioned as the first observer of the fact. In the cases which have since occurred to me, I have been able to confirm its truth. In the first weeks after the injury one might be inclined to attribute the want of projection of the nails of the injured limb to the swelling of its fingers or toes; but this swelling not unfrequently continues for some time after the fracture is united, and then it may be seen that the nails again begin to grow, and that their having before projected less than those of the uninjured limb was actually the result of an arrest of their growth.

[We suspect that the defective growth has only an indirect connexion with the fracture. The growth of all the horny tissues bears a direct proportion to the degree of waste to which they are exposed; when a limb is fractured it is kept quiet, and its nails being guarded from the friction and other sources of waste to which they were before exposed, grow less rapidly than those on the other limb which are exposed as much or more than before. The subject, however, deserves notice.]

*Medicinische Zeitung. April* 28, 1841.

*On the Cure of Ozæna.* By Dr. DETMOLD, of Hanover.

THE author says he has never failed to cure ordinary ozæna (by which he means the chronic coryza accompanied by a stinking discharge from the nose and a flabby relaxed state of the Schneiderian membrane) by the use of an injection composed of one or two drachms of chloride of lime rubbed up in a glass mortar with thirteen ounces of decoction of rhatany root, and strained off after standing for half an hour. About half an ounce of this must be injected into the nose three or four times a day with a syringe whose point is sufficiently long to carry the fluid high up into the nasal passages. The use of the remedy should be accompanied by the occasional administration of purgatives. It is very beneficial also in cases of chronic otitis with offensive discharge from the ear.

*Holscher's Annalen*, 1840. Bd. v. Hft. i.

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*On Hairs within the Eye.* By Dr. STILLING, of Cassel.

A blacksmith, a healthy man thirty-four years old, had the end of a wire driven with considerable force through the cornea and to the depth of half an inch into the interior of the right eye. Next day he had acute inflammation of the whole globe, and a high degree of fever, which were reduced by antiphlogistic remedies. Within a few days the wound in the cornea completely closed so as to leave but a small cicatrix; the chambers, which had been completely evacuated, were again filled, but a partial capsular cataract had formed. The sight, which had been entirely lost, improved considerably, and though at first it was double (apparently from the axis of vision being altered by an adhesion of the iris), it soon again became single and slowly increased in clearness.

About six weeks after the injury the attendant surgeon first discerned in the posterior chamber some hairs of different lengths, of which the author gives the following account from an examination made about four years after the accident: The left eye is perfectly healthy; in the right the iris is discoloured as if from a former attack of chronic inflammation. The pupil of the latter eye is irregular, and there is a partial capsular cataract. One hair commences a line below the horizontal diameter of the pupil and extends in an almost vertical direction (forming a slight arch, like that of the inner eyelashes) to the outer edge of the iris, where it comes upon the cornea. It is about five lines long and pointed at both ends; it is light yellow at its lower extremity and dark brown at the upper. The second hair begins near the vertical diameter of the pupil, on the lower and inner part of the pupillary margin of the iris, and extends obliquely upwards and outwards to the distance of a line and a half beyond the outer margin of the pupil. It is three lines long, and is pointed at one and probably at both ends; it is of a dark brown colour. A third hair lies just below that last described, and seems to come from the same root; it extends in the same direction, has a pointed extremity, is about three lines long, and of a yellowish-white colour. The appearance of two other hairs proceeds from some small black points attached to the capsule of the lens, and probably resulting from particles of uvea affixed to it and adhering with fine filaments of fibrin passing from it to the iris. The small cicatrix is still visible in the middle of the cornea, and is marked by a black point, which is probably the consequence of a small portion of uvea having been left in the wound when the wire was drawn out directly after the accident. The eyelashes and eyebrows are of the same colour as the hairs within the eye. The patient suffers no pain in the affected organ, and its sight is not more impaired than by the opacity of the capsule of the lens.

The author discusses the question whether these hairs were really produced within the eye as they sometimes are from growths on the conjunctiva, or whether they were carried into it by the wire, and had subsequently become adherent. He adopts the latter view, grounding his opinion chiefly on these facts: that there was no appearance of a growth from which they could originate; that

they did not proceed from the same point; that they exactly resembled the man's eyelashes; that they did not grow; and that such a circumstance as the production of hair within the eye is not only unheard of but most improbable. [On the other hand, it is surely scarcely less improbable that a wire should carry eyelashes through the cornea and leave no cicatrix on the lid, and that these should become adherent, and yet not be remarked by a careful observer daily examining the eye for nearly six weeks.]

*Holscher's Annalen.* Bd. iv. Hft. iii.

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*On the Employment of Nitrate of Silver in White Swellings.* By M Jobert, Surgeon to the Hospital St. Louis.

By a series of accurate and conclusive observations M. Jobert has shown that the best and most prompt means of overcoming articular pains in cases of white swelling, and to make the turgescence of the tissues disappear, consists in the external employment of an ointment of nitrate of silver. We have watched on fifteen patients the action of this remedy, in the wards of M. Jobert, and have been astonished at the prompt effects in long-continued and previously rebellious diseases. The treatment consisted in frictions on the diseased articulation with an ointment composed of thirty parts of lard to four of nitrate of silver. If the action of this be insufficient M. Jobert uses eight or twelve parts of the salt to thirty parts of lard. These ointments, designated by the numbers 1, 2, 3, constitute the whole of the treatment. Twelve or fifteen hours after the first employment of the ointment, and generally after the second friction, an eruption of small accuminated pustules appears, presenting a black point in their centre, and surrounded at their circumference by a small rosy areola. The liquid contained in the vesicle at first resembling thick milk, and rapidly assuming a yellowish-white appearance, afterwards becoming true pus. Each friction is accompanied by pains which last three or four hours. About the second or third day the skin becomes of a violet colour, and smarts acutely. The frictions must then be suspended, and not renewed until the parts are calmed. We do not enter into further details, as a full memoir on the subject is promised.

[It is very extraordinary that such accurate pathologists as the French should so generally continue to class as "*tumeurs blanches*" the very different diseases to which the joints are subject. We are thus left in doubt whether the above cases were scrofulous enlargement of the articular extremities of the bones, ulceration of cartilage, disease of synovial membrane, or of parts external to the joint.]

*Bulletin Général de Thérapeutique. Juin*, 1841.

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*On Ecchymosis of the Eye and Eyelids as a means of Diagnosis in Injuries of the Head.* By Dr. Maslieurat Lagémard.

After the recital of several interesting cases the author arrives at the following conclusions:

1. That sanguineous effusion on the exterior of the cranium in the sub-aponeurotic cellular tissue, and before a line passed transversely from the posterior border of one external ear to the other, may appear on the exterior, determining ecchymosis in the eyelids, but not in the cellular tissue of the conjunctiva or of the globe of the eye.

2. That bodies which act directly on the eye, and which by their form are capable of compressing it from before backwards, may rupture the surrounding capillary vessels, and produce ecchymosis of the conjunctiva and eyelids.

3. When there is a fracture of the base of the cranium which is denoted externally by ecchymosis owing to extravasation of blood into the cavity of the orbit, this ecchymosis *appears first on the ocular conjunctiva*, afterwards reaching the eyelids in some cases but not in all.

*Archives Générales de Médecine. Juillet*, 1841.

*On Dislocations of the Sternal Extremity of the Clavicle backwards.* By M. V. MOREL.

THE author has had three opportunities of examining this rare accident at La Charité and La Pitié, and from a comparison of these cases with the only three others published, has drawn up an essay, of which the following is an abstract:

The causes of this dislocation are forces acting *indirectly* by pushing the shoulders violently forwards, or by suddenly pulling the arm forward while the trunk is fixed, or *directly* by sudden blows on the front of the clavicle. The latter cause is by far the most rare.

The symptoms at the moment of the accident are extreme pain at the base of the neck, with syncope or a tendency to it, and generally more or less difficulty of breathing. The position of the limb is nearly the same as in fractures of the clavicle.

When the dislocated bone has passed backwards and downwards, its scapular extremity is always more than naturally prominent, and the clavicular end is lost sight of; but, if the patient be thin, and the parts not swollen, it can be felt pushed in behind the sternum. A depression more or less painful to the touch indicates the vacancy at the sternal fossa, and the external portion of the sterno-mastoid muscle is obviously turned backwards and inwards; but none of these last signs can be discerned if the patient be fat or if the injured parts be much swollen. The shoulder does not readily move, but if the whole strength of two men be applied to pull it backwards and outwards, the parts regain their natural relations, and the luxation is reduced; but the instant the extension is remitted, a rubbing sound and the reappearance of the deformity announce the reproduction of the displacement.

The dislocation backwards and downwards is very often by the efforts at reduction or other forces, converted into one backwards and upwards. In this case the end of the clavicle instead of being fixed is moveable, and, projecting inwards and upwards, forms above the sternum a small hard round tumour which moves with every motion of the shoulder, and may be easily reduced and pushed out of the joint again.

The reduction of the dislocation backwards and upwards then is perfectly easy; that of the dislocation backwards and downwards is best effected by pulling the upper part of the arm horizontally backwards and outwards while the elbow is held against the side of the chest. But the main difficulty is to keep the head of the bone, when reduced, in its place, and this may be best accomplished by M. Velpeau's bandage, which fixes the arm with its elbow on the front of the chest and the hand on the opposite shoulder, and is itself retained in place by a second bandage similarly applied and thoroughly starched. It must be worn for forty or fifty days.

*Annales de la Chirurgie. Juin*, 1841.

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*Case of Internal Intestinal Strangulation.* By M. GAUTRIC.

AT the sitting of the Anatomical Society, July 7, 1841, M. Gautric presented the pelvis of an old woman who had never had anything abnormal in connexion with the abdomen until some days before death, when she presented all the phenomena of internal strangulation. On examination it was found that the left ovary had contracted adhesions by its external extremity to the point where the peritoneum leaves the corresponding lateral surface of the bladder, thus forming a species of arch large enough to admit three fingers, and so disposed that the inclined plane of the internal iliac fossa leads directly towards it. A portion of small intestine about two feet in length was engaged under this arch and there strangulated. This adhesion was evidently produced by an old local peritonitis, for around the sigmoid flexure of the colon and between the uterus and bladder there were other filamentous adhesions. The fimbriated extremity of the fallopian tube was free.

*L'Examinateur Médicale. Juillet* 11, 1841.

*Traumatic Pneumothorax, produced by violent Pressure on the Chest.*

Pneumothorax from disease of the lung and the production of pleuro-bronchial fistula is not uncommon, but it is very rarely produced by violence applied to the thoracic parietes. Hence the following case which lately occurred in the hospital of la Pitié is of some interest. A man, thirty-eight years of age, of robust constitution, was standing with his back to a post, between which and the wheel of a carriage in motion he was forcibly compressed, the wheel being applied to the anterior part of the chest. He fell senseless, and some hours after the accident presented the following symptoms: General prostration, intense dyspnœa, hard pulse, tympanitic resonance throughout the right side of the thorax, enlargement of that side with elevation of the intercostal spaces. A dry sibilant metallic râle occupied the middle part of the lung fixed at the same point, and recurring at each inspiration: scarcely any respiratory murmur; amphoric resonance towards the roots of the lungs. The whole external injury was ecchymosis with excoriation over the inferior angle of the right scapula. On the left side, where the pressure appears to have been less violent, the resonance of the chest and the nature of the respiratory murmur were not sensibly modified. There was considerable ecchymosis under the conjunctiva at the internal angle of the eyes. Metallic tinkling was very well marked on the next day. Seven bleedings were practised in five days from the time of the accident, the first to a pound, the second a little less, and on the four following days from half a pound to twelve ounces. Abstinence was rigorously enforced. The effusion was gradually absorbed and the patient is doing well.

The condition of the patient is explained by rupture of the lung and pleura pulmonalis, pneumothorax, inflammation of the pleura and formation of pus, with obstruction to the return of the blood from the head at the time of the accident; which would doubtless have produced rupture of the vessels of the pia mater and apoplexy, had the pressure been more violent, or continued for a longer time.

*Bulletin Général de Thérapeutique. Juin*, 1841.

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*Report on M. Louvrier's Treatment of Anchylosis by sudden and forcible Extension.* By MM. Thillage and Bérard.

The following is the substances of the above lengthy report. M. Louvrier's machine has been employed on twenty-two patients, of whom only three have experienced ill effects, all the others having escaped injury. Most of the patients suffered excessive pain at the moment of operation. In no case has the anchylosed articulation recovered entire freedom of motion. Those patients most successfully treated are obliged to use a staff in walking; one only walks without a stick, but the restraint is evident.

With regard to the unfortunate cases: in one female, in whom the anchylosis of the knee was complete and the limb fully flexed, the application of the machine was followed by a very considerable rupture of the skin, luxation of the leg upon the posterior part of the thigh, and abundant suppuration which terminated in death three weeks after the operation. At the necropsy the articular cavity was found full of pus, the popliteal artery intact, the popliteal vein full of pus and its coats thickened. Many muscles were ruptured and softened; the anterior crucial ligaments softened; one of the posterior softened, the other ruptured, attached by one extremity to the tibia and terminating at its free extremity by an osseous portion, which was evidently part of the condyle of the femur, fractured at the moment of operation.

Another patient suffered excessively acute pains at the moment of operation, and remained during some time in a sort of delirium occasioned by the suffering. Gangrene commenced on the next day, but was limited by the efforts of nature alone, and the patient is actually cured.

In a third case, that of a young woman in whom the anchylosed limb was fixed at a right angle, the straightening was incomplete. M. Louvrier applied a piece

of wood to the anterior part of the knee by means of which he hoped to press the limb into its natural position; but an eschar formed on the next day and the patient died in six weeks.

In another patient who died from other causes, the articular extremity of the tibia was found to be luxated upon the posterior part of the femur, the internal condyle of the latter being fractured.

The number of these accidents, however, being small [!], the opinion of the reporters would be less unfavorable, were the ill effects balanced by real advantages; but as the limb after operation is as immoveable as an artificial support, they conclude:

1. That the application of the machine of M. Louvrier is followed by an instantaneous straightening (*redressement*) of the anchylosed limb.

2. That it is not ordinarily followed by any severe symptom, primary or consecutive.

3. That when these accidents are produced they are frightfully severe, and are ordinarily followed by death.

4. That none of the patients operated upon by this method have entirely recovered the free motions of the anchylosed articulation. We therefore report to the minister that the machine of M. Louvrier, although ingenious, is dangerous in application, for it will be always impossible to determine the nature of the anchylosis and to foresee the conditions which would offer some chances of success for its employment.

[The barbarousness of this most heroic chirurgery needs no comment from us to prevent its adoption. The facts, however, are well worthy of record.]

*Bull. de l'Acad. Roy. de Méd.* Nos. xiii-xiv-xv-xvi. *Avril* et *Mai*, 1841.

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*On the Action of the Muscles of the Eye, and the cure of Myopia by division of these Muscles.* By MM. PRAVAZ, BONNET, GUERIN, &c.

I. The division of the great oblique in certain cases of strabismus having diminished the myopia which ordinarily accompanied this deformity, M. Phillips has been led to conclude that this muscle elongated the antero-posterior diameter of the eye, an elongation necessary for distinct vision at a short distance. M. Bonnet, who has cured several cases of myopia by dividing the lesser oblique, has assigned to the latter the same function. M. Pravaz assures us that they can have no such action, and proceeds to state that hitherto it has been supposed that myopia was owing to one of three causes and perhaps to the three united; an increased convexity of the cornea, an active or passive depression of the crystalline lens (according to its transverse *limbe*), or too great a distance existing between the lens and retina. M. Guerin, however, supposes that in vision at a short distance the eye is flattened from behind to before by the action of the recti muscles, while M. Pravaz concludes that the power of both recti and oblique, with the elasticity of the envelopes of the eye, perfectly accounts for all the phenomena of the appropriation of the refractive surfaces of this organ to variations of distance, and reconciles in theory the two methods which have been employed in the cure of myopia. The simultaneous contraction of the recti compresses the eye laterally, and also draws it from before backwards, the latter action being much the more powerful, but antagonized by the contractions of the oblique which thus indirectly tend to elongate the globe. The supposition that the recti cause myopia by diminishing the antero-posterior diameter of the eye is contrary to all optical principles, and its fallacy is shown by the success of the operations of Phillips and Bonnet; for according to this hypothesis the section of the oblique muscles would augment the myopia by removing one source of resistance opposed to the action of the recti.

*Archives Générales de Médecine. Mai*, 1841.

II. The case of M. Bonnet alluded to in the above paper was related at the sitting of the Academy of Sciences on the 29th of March last. He only performed the operation once, and with very little success. The case was that of a

young man who had been short-sighted for two years, and suffered from some symptoms of amaurosis, and the only good effect of the operation was that the patient did not see sparks before his eyes as he had done before.

III. The following letter (abridged) on the cause and surgical treatment of myopia was addressed to the Academy of Sciences by M. Guerin, March 15, 1841: 1. There exist two species of myopia, the mechanical or muscular, and the optic or ocular. The mechanical results from the primitive shortness or active retraction of the muscles of the eye. 2. In the mechanical myopia the shortened muscles are the four recti together or two or three only. 3. When only one rectus is feebly contracted, or two or more irregularly contracted, strabismus coexists with myopia. 4. The characters of mechanical myopia are furnished by the form and movements of the eye. The anterior half of the globe is conical, the cornea representing a segment of a sphere the radius of which is much smaller than the segment of the eye which it fills up. The lateral parts of the globe are depressed, flattened in the situations of the shortened muscles. The movements of the two eyes are more or less limited upwards, downwards, inwards, or outwards, according to the number of muscles affected and their degree of shortening. 5. The treatment of mechanical myopia consists in the subconjunctival section of the short or retracted muscles. I have practised this operation several times in cases of simple myopia, and in myopia complicated with strabismus. Among the most remarkable cases I will cite that of a man fifty years of age, affected with slight divergent strabismus. He could read with No. 3 glasses; three days after the operation he could read readily without glasses the characters of the *Moniteur*. In another case a young man, aged eighteen, whose mother and grandmother had been shortsighted, before operation could not distinguish Roman characters at the distance of about five inches, but he read them readily at a greater distance with glasses No. 7. Three days after the division of the two internal and external recti he began to read without glasses, the same characters at the same distance, and could distinguish at the distance of ten yards objects which, before the operation, he could not see at all. On the ninth day from the operation the patient could read without glasses roman characters at the distance of eighteen inches, and large roman capitals at the distance of a yard, but they appeared smaller to him than before the operation. He distinguishes pretty clearly at the distance of 100 yards large objects, as a dog or a statue, although he cannot see the same objects with glasses No. 7, and but very confusedly with No. 13. The eyes as yet do not accommodate the focus to all intermediate distances, and this circumstance coincides with a reunion and contraction still incomplete of the divided muscles. M. Guérin then goes on to expound the theory we have before alluded to.

*Journal des Connaissances Médico-Chirurgicales. Mai*, 1841.

[M. Cuvier has practised with success the operation proposed by M. Guérin in four cases, and in one in which the patient offered signs of contraction of the inferior oblique, this muscle was divided. (*Annales d'Oculistique, Avril*, 1841.) M. Bonnet has performed his operation in three other cases, and although in two there was some improvement, in none was the cure complete. We believe, however, that our countryman, Mr. Adams, claims priority in applying myotomy to the cure of myopia, having performed his first operation in a short-sighted patient in August, 1840. See a notice of his book in the present Number.]

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*Results of Operations for Stammering in France.* By MM. Velpeau and Bonnet.

The following extracts will confirm the justice of the observations made in our last Volume on the lately-introduced and lately-discarded operations for stammering. At the date of M. Velpeau's paper (June 1st), nearly 200 patients had been operated on in France; the modes of operating were already ten in

number; the beneficial results obtained by others were even less than in M. Velpeau's patients.

I. *Experience of M. Velpeau.* The first patient cut was in three months in the same state as before the operation. In the second there was but very little amendment. The third and fourth remain cured (at the date of his paper). The fifth, who was at first much benefited, now stutters again very much. The sixth is improved but still stammers. The seventh, in whom the dorsum of the tongue was tied, and who was for some time benefited, now stammers as much as ever. The eighth, in whom the arches of the palate were cut, is in the same plight. The ninth, a physician, whose genio-glossi were divided, was in three weeks as bad as ever. A tenth after remaining well for twelve days, returned in a fortnight again stammering. The eleventh and twelfth gained but a very little advantage by the operation. The thirteenth had severe hemorrhage and then vast abscesses in the neck, but still was not relieved of his infirmity. Four others in whom the anterior edges of the genio-glossi were divided, and in whom the stammering previous to the operation was not very bad, considered themselves completely cured, and in fact did speak much better than before.

The dangers of the operation are hemorrhage and subsequent acute inflammation. [Both have occurred with sufficient frequency and severity to render it unjustifiable to proceed to an operation which does not afford a good prospect of benefit to the patient. From the reports in other journals we learn that not less than three patients have died of one or other of these accidents.]

From his own observations M. Velpeau concludes that the excision of a V from the tongue offers but a slight chance of remedying the stammering; that the destruction of a transverse portion by ligature does not succeed better; that the section of the arches of the palate, or of the glosso-pharyngei muscles is equally unavailing; that the prolonged division of the frænum often succeeds, without exposing the patient to any real danger, in rendering the speech more easy and in partly removing certain varieties of stammering; and that the section of the genio-glossi near their attachment to the jaw is sometimes almost perfectly successful, but is often ineffectual, and often of only partial utility.

*Annales de la Chirurgie. Juin*, 1841.

II. *Experience of M. Bonnet.* Of forty-two patients in whom the genio-glossi muscles have been divided by the subcutaneous method, two had the speech so confused that it was difficult to understand what they said; in these no effect resulted. The same want of success followed in four cases where the impediment to speech resulted from defective inspirations or expirations; the patients did not repeat the same syllables, but they were immediately arrested at the commencement or the middle of the words, the air not passing to or from the chest. In such cases we can hope for or obtain nothing from division of the genio-glossi.

Thirty-six of the operations were on true stammerers, that is to say persons who repeat the same syllable several times. Of this number six were more than thirty-two years of age, and the results were completely negative. We therefore conclude that we should not operate on those who have confused speech, nor on those where impeded respiration is the cause of the difficulty of speaking, nor on true stammerers who have exceeded thirty-one years of age.

Of thirty stammerers under thirty-one years ten were completely cured, eleven extremely ameliorated, and seven experienced no advantageous result.

Of the ten who were cured one had returned to his former state from twelve to fifteen days after the operation. Another who stammered excessively, and whose cure was as it were miraculous, found his defect reappear very gradually towards the fifth week, but two months after the operation he may still be placed among those greatly relieved though not among those cured. The others continue perfectly well.

Of the eleven greatly relieved, two relapsed, but were improved after a time. The others also gradually improved.

*Bulletin Général de Thérapeutique. Juillet* 15 *et* 30, 1841.

*Application of the Subcutaneous Method to the Operation for Strangulated Hernia.*
By M. Jules Guerin.

In this case the hernia was a congenital epiplocele which had been strangulated for three days. The usual means of reduction had been applied, and the tumour had become hard, engorged, and the seat of commencing inflammatory action. After division of the two rings and of the antero-superior wall of the inguinal canal, the reduction was immediately effected. The wound did not inflame, nor did the slightest febrile symptom follow. The patient was able to rise on the eighth day, taking care to wear a bandage.

*Gazette Médicale de Paris. 7 Aôut*, 1841.

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*Ligature of the Temporal and Facial Arteries in a Case of Epilepsy.*
By M. Velpeau.

A man, thirty-six years of age, who had been affected with epilepsy for seven years, which followed a fright, was admitted on the 29th of March last into the hospital of *La Charité*, under the care of M. Velpeau. The attacks had occurred eight or ten times in a month, but daily for the last three months. He had some very severe attacks in the hospital. On the third day from his admission, M. Velpeau, emboldened by some facts scattered here and there in science, and which have hitherto passed almost unnoticed, tied the two temporal arteries. On the same day the patient had another fit, but slight, and on the following day he was perfectly tranquil. On the 4th of April M. Velpeau compressed the two facial arteries on the borders of the inferior maxilla. The fit did not return, and on the 5th of April the surgeon tied the two facial arteries. The patient quitted the hospital on the 15th of April, only having had one fit since the 5th, although for months he had not passed a day without at least one attack. He has promised to return to the hospital occasionally. The case is interesting, though further observation is of course necessary to show whether the ligature of the arteries was the cause of the cessation of the fits.

*Bulletin Général de Thérapeutique. Avril* 15 *et* 30, 1841.

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*Good Effects of the Extract of Belladonna in the Reduction of Paraphimosis.*
By Dr. Mignot, of Bordeaux.

A child, three years and a half old, was the subject of severe paraphimosis; the glans red, swollen, and tender; the prepuce strongly drawn back, forming a thick and apparently adherent ring, the constriction of which completely stopped the circulation. This state had lasted eight days, and the sufferings were excessive. Reduction being impossible, leeches were applied to the perineum and hypogastrium; cooling drinks, emollient enemata, cataplasms, lotions, and hip-baths were used, but they only gave slight relief and but for a short time. The strangulation became more menacing, and all the symptoms were aggravated; the glans was bluish and gangrene was threatened, when M. Mignot employed frictions around the glans, with an ointment composed of thirty parts of simple cerate to twelve parts of extract of belladonna. Under the influence of this remedy the circle of constriction relaxed, dilated, and the tissues gradually recovered their normal condition, without loss of substance or suppuration following.

The second patient had acute balanitis, brought on by a severe gonorrhœa, and followed by paraphimosis. The patient refused operation although gangrene was threatened, when the belladonna was applied, which induced relaxation and rapid amendment. It was also applied in a case of phimosis accompanied by chancres and a sympathetic bubo, and three days after its employment the dilatation of the preputial orifice was complete.

*Bulletin Général de Thérapeutique. Avril* 15 *et* 30, 1841.

*On the Development of Cryptogamia in the Tissues of living Vertebrata.* By MM. ROUSSEAU and SERRURIER.

IN a memoir on the Diseases of the Organs of Voice by the above gentlemen, it is related that a male paroquet affected with laryngeal and pulmonary phthisis died in 1834, and on examination there was found in the abdomen between the intestines and vertebral column a species of false membrane, on which there was a greenish and pulverulent mouldiness, which adhered so slightly that on blowing it disappeared as the finest and lightest powder. This mouldiness has been several times observed in different parts of the body; most frequently in the pelvis, between the kidneys and the viscera, on the principal vessels of the heart, between the ribs and the lungs. Pigeons and fowls are most particularly affected, especially those inhabiting cold and moist places, and in rainy seasons. Nevertheless this phenomenon has occurred in animals of a different organization, in a hind (*cervus axis*) and in the earth tortoise, a native of the Indies (*testudo indica.*) A similar fact has recently been communicated to the academy by M. Deslongchamps.

At the sitting of the Royal Academy of Sciences, July 5, 1841, M. Dumas stated that Dr. Gruby had discovered that one of the varieties of porrigo is owing to the development of a vegetable beneath the epidermis. M. Breschet remarked that the *porrigo favosa* was the form studied by M. Gruby.

*Gazette Médicale. Juillet* 17, 1841.

M. Kettner has since written to the academy to claim for Dr. Schönlein, successor to Hufeland at Berlin, the priority of indicating the existence of a vegetable in the crusts of *porrigo favosa*, a fact which was published by him in *Müller's Archiv.* M. Kettner states that Dr. Schönlein has established in many cases of *porrigo lupinosa* the vegetable nature of this affection, and therefore the discovery cannot be attributed to M. Gruby.

*Gazette Médicale. Juillet* 24, 1841.

[M. Gruby has made very minute observations on the physical and chemical properties of this vegetable, and has succeeded in inoculating it on cryptogamia, thus transmitting disease from man to vegetables, but only succeeded once in seventy-six trials. He failed to communicate the disease by inserting the vegetable into the arm. This subject is so interesting and important in connexion with the subject of animate contagion, that we shall wait with anxiety until the Report of the committee is published.]

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*On Contraction of the Vesical Orifice of the Urethra.* By M. CIVIALE.

THE object of the author, in a very long paper, is to describe a particular condition of the vesical orifice of the urethra, frequently met with, especially in old men, resulting sometimes from morbid states of the prostate; sometimes from a sort of semilunar valvular fold of mucous membrane, elevated from the inferior surface of the neck of the bladder, and constituting a barrier which opposes the flow of urine or the introduction of instruments. This valve presents different degrees of thickness and elevation, sometimes being eight, nine, or twelve lines in height. It appears to result from elevation of the mucous membrane in consequence of prostatic engorgements. The treatment consists in dividing the barrier, from its free border to the base, or puncturing the base and cutting to the free border. M. Civiale has constructed instruments for this purpose resembling those used by Mr. Stafford for the division of stricture.

*Bulletin Général de Thérapeutique. Mai* 15 *et* 30, 1841.

## MIDWIFERY, AND DISEASES OF WOMEN AND CHILDREN.

*Secretion of Milk after Menstruation.* By Dr. Wehr, of Cassel.

M. H. a labourer's wife, 30 years old, of ordinary size, menstruated first in her eighteenth year, having previously enjoyed good health. Her catamenia continued quite regularly to the time of her marriage in her twenty-eighth year. Ten months after marriage she had a healthy child which she suckled. Since that time, now six years ago, she has never been pregnant, but after menstruation, which occurs regularly every four weeks, she has always had a burning sensation in the breasts, both of which swell up, become hard, and secrete more or less milk. Some days before the reoccurrence of menstruation, the breasts again diminish, and the secretion gradually ceases.

*Casper's Wochenschrift. Mai* 8, 1841.

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*On softened, encysted Tubercles in the substance of the Uterus, as a cause of difficult Labour.* By Professor Osiander, of Göttingen.

The case here recorded differs from almost all others in which labour has been impeded by the pressure of tumours. Parturition was not impeded merely by their mechanical action, nor was the pelvic cavity contracted by their presence. They produced an injurious effect by paralyzing the action of the uterus and preventing the expansion of its fibres. The obstruction to labour thus caused was quite as great as if the pelvis had been contracted, and delivery could be effected only by mutilating the child and employing the blunt hook.

The patient was a woman, forty-five years old, who had suffered from scrofula in her infancy. She was a person of unhealthy aspect, had already miscarried twice, but had never given birth to a living child. When seen by Professor Osiander, she had been twenty-four hours in labour, her strength was much exhausted, and she was very low spirited. The head of the child was felt to be very high up in the pelvis, and the membranes were still entire, though the os uteri was freely dilated.

After waiting for some hours, during part of which time the uterine action had been energetic, the head came somewhat lower down, and Professor Osiander ruptured the membranes. The head, however, remained above the brim of the pelvis, where it presented in the oblique diameter with the posterior fontanelle directed towards the right sacroiliac synchondrosis. An attempt to bring down the head with the forceps was unsuccessful, and it was next sought to deliver the patient by turning. The hand of the operator could touch the ribs, but it was found impossible to reach the feet, for a sort of stricture in the middle of the uterus rendered all attempts to carry the hand as far as the abdomen of the child, unavailing. Changing the position of the patient and placing her on her knees did not diminish the difficulties, for the contraction seemed to occupy alike all the walls of the uterus, and neither the right nor left hand could penetrate beyond it.

A second unsuccessful effort was made to deliver with the forceps; and then, after waiting for half an hour, Professor Osiander proceeded to perform craniotomy, and to extract the child by means of the blunt hook. This was not effected without great difficulty. It was necessary to introduce the hand, in order to remove the placenta, which was not adherent, but merely retained by an irregular contraction of the uterus. No serious hemorrhage followed delivery, but the uterus never contracted properly, and the abdomen continued much distended. The patient lay in a listless condition, making no complaint of pain, but with a quick pulse and tumid abdomen which were thought to indicate the propriety of venesection. No relief followed its employment, the patient was soon afterwards attacked with vomiting, and died on the third day after delivery.

On a post-mortem examination, no traces of peritonitis were found; but the

whole right side of the abdomen was occupied by the enormously large uterus. The substance of that organ was nearly three fingers thick, beset with hard swellings like eggs, of a somewhat oval form, and filled with a yellow caseiform matter resembling pus, the liquid parts of which had been absorbed. These large tubercles, about nine or ten in number, were invested with a fibrous envelope. They projected on the posterior and external surface of the uterus, so as to render it uneven. Many smaller bodies of the bigness of cherries were imbedded in the uterine parenchyma, and on a section being made of them were seen to be made up of concentric fibres; thus resembling in structure the ordinary fleshy tubercles of the uterus. With the exception of partial ossification of the left ovary, the above was the only morbid appearance of moment, and to this state of the uterus the difficulty experienced in introducing the hand must be exclusively attributed.

*Hannoversche Annalen.* v Band. 15tes Heft.

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*On the Employment of Cold Affusion in the Treatment of Acute Hydrocephalus.*
By Dr. Münchmeyer, of Verden.

Dr. Münchmeyer observes that the medical world is greatly divided in opinion as to the value of this remedy; some persons greatly extolling its efficacy, while others regard it as altogether useless. He considers it to be a most important remedy, and one which will often save life when all other means have been useless. One great reason why cold affusion has met with so few supporters is to be found in the misconception which has prevailed with reference to the proper time for using it. It is certainly not always advisable to resort to it, and it should never be forgotten that its mode of action differs essentially from that of cold when kept constantly applied to the head. In the employment of cold affusion it is the secondary action of cold, as well as the sudden shock to the system produced by the mode of its application from which benefit is expected, while in the case of cold lotions to the head it is the primary action of cold which is obtained. Cold affusion then must not be looked upon as a directly antiphlogistic remedy, nor is its employment indicated during the early inflammatory stages of hydrocephalus, but rather when effusion, the consequence of inflammatory action has taken place, and a tendency to paralysis exists. After the subsidence of the violent symptoms of the disease, and when the patient has sunk into a comatose state, with a pale countenance occasionally suffused with a flush, dilated pupils, strabismus, and slow pulse, this remedy will frequently prove of excellent service.

In order, however, for benefit to be derived from it, it must be employed in an efficient manner. Dr. Münchmeyer directs that the patient should be taken out of bed, stripped of his clothes, and wrapped up in some simple covering (if waterproof the better), which leaves only his head exposed. He should then be placed in a sitting posture in a bath or tub, and the person who administers the affusion should mount on a chair and pour cold water upon his head, in a moderate stream from the height of five or six feet. This may be continued for a minute or two, and repeated twice or thrice. The patient should then be wrapped up in a warm sheet and placed in bed, where he should remain till it is thought proper again to have recourse to the remedy. At first, it will probably be requisite to repeat the affusion, in the course of an hour and a half or two hours; but as the patient improves the interval may be longer, so that at last it will not be necessary to employ it above two or three times daily.

The immediate effect of cold affusion is, that the patients awake from their comatose condition and begin to cry violently, which they continue to do so long as the water is poured upon them. They afterwards appear exhausted and pale, the skin is cool, the pulse small and very frequent. When placed in bed they usually fall into a dose, the pulse becomes more regular, and the warmth of the skin returns. By degrees, as with the repetition of the remedy the pa-

tients improve, they begin to have sound sleep, from which they awake in the possession of all their senses, recognize those by whom they are surrounded, and cease to squint. At the same time too a sweat, frequently of a critical nature, breaks out upon the whole body, and during its continuance the employment of cold affusion is very hazardous. The patient's sleep becomes more refreshing, and the comatose condition recurs at longer intervals; he begins to notice what goes on around him, the head regains its natural temperature, and the febrile symptoms disappear. The employment of affusion must, however, still be continued for some days, since relapses very frequently occur.

The paper is illustrated by five cases. In three the employment of cold affusion was perfectly successful, in one it produced temporary amendment, and the death of the patient was to all appearance owing to the apathy of the parents, who neglected to persevere in the treatment, while in the fifth convulsions and death followed affusion while the patient was perspiring profusely.

*Hannoversche Annalen.* Band v. Heft 4.

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*On the Immersion of Children apparently still-born in Cold Water.*
By Dr. Scholer, Assistant Physician of the Berlin Lying-in Institution.

Nothing more need be said of this paper than it contains two well-detailed cases, and alludes to several others, in which this measure was successfully adopted, after all the ordinary means had failed of reanimating the infant. The evidence adduced is certainly sufficient to warrant the adoption of the plan as a last resource after less violent measures have been tried in vain.

*Medicinische Zeitung. April* 28, 1841.

## MEDICAL JURISPRUDENCE AND TOXICOLOGY.

*Case of Death by Hanging, with numerous Wounds on the Person.*
By Dr. Krügelstein, of Ohrdruff.

The deceased was about forty-eight years old; she had borne several children, and enjoyed good health and spirits up to the time of her last accouchement. She then became morose and unhappy, and attempted to drown herself. One morning she was found hanging, quite dead, in a cowshed adjoining her bedroom.

On examining the room in which she slept, some coagulated blood was found near the bed on the floor, and there were marks of bloody fingers on the walls, but there was no trace of blood on the bed itself. From this room to the shed in which her body was found there were no traces of blood, but there were impressions of bloody hands on the walls of the shed; and the beam from which her body was suspended as well as the cord was slightly stained. On the floor beneath the body there were marks of the fæces having been discharged. The roof of the shed was low, but sufficiently high to allow a person to be freely suspended from the floor. There was some dried blood on the face and dress.

The following appearances were met with:

On the tuberosity of the os occipitis, a severe contusion of a livid colour and about two inches in circumference. On cutting into it coagulated blood was found beneath but the skull was uninjured. Over the left temple there were seventeen incised wounds about two or three inches long, some penetrating to the bone, but none involving the temporal artery. On the left side of the os occipitis were two superficial cuts, and on the upper part of the right and left parietal bones were many others of a similar character. On the right temple

there were so many deep incisions as to have completely destroyed the temporal muscle. Over the sagittal suture was a long wound penetrating to the bones.

The face was pale and free from any marks of violence; the eyelids were closed, and the eyes not projecting. The mouth was open; the tongue not prominent; there was no appearance of frothy mucus either in the mouth or nose.

Around the neck and above the larynx a tolerably deep depression had been produced by the cord. This extended beneath the angle of the jaw and the mastoid process on either side to the nape of the neck. Notwithstanding the cord had thus caused a deep and well-defined mark, there was no ecchymosis, if we except a slight bluish tint on the left side; nor had the skin acquired that parchment-like character which is often seen in the hanged. Neither the os hyoides, trachea, nor the vertebræ of the neck had sustained any mechanical injury.

The surface of the body was more or less covered with blood which had flowed from the wounds. Over the sternum and region of the stomach numerous but small incised wounds were perceived, which, from their appearance, had evidently been produced several days before those met with on the head. This was proved by the fact that some of them were passing into the suppurative stage.

On opening the cranium, splinters of the internal table were found corresponding to the wound in the region of the sagittal suture. These pressed on the dura mater, but had not injured it. There was no congestion, effusion, or injury to the brain, and the only morbid appearance was an hydatid cyst in one part of the pia mater. The lungs were of a bluish white colour, not particularly congested; a bloody froth escaped on making an incision into them. The heart and the abdominal viscera were in a normal state. The bladder was empty. The genitals were not turgid, as they are often described to be in females who have died by hanging.

The medico-legal questions to determine here were, as to whether: 1, the numerous wounds on the body had been self-inflicted; 2, whether the hanging was the result of suicide or murder.

The account given of the deceased showed that she had a predisposition to suicide. This is borne out medically by the discovery of hydatids in the pia mater.

In the shed where the body of the deceased was hanging an axe was found with which she had no doubt produced the injuries to her head. The edge of the axe was sufficiently sharp to account for the production of the numerous incised wounds found in the scalp, and the weapon was long enough to permit her to have produced the *severe contusion found on the os occipitis*. Had such a weapon been used by a murderer, one or two wounds only, and those of a much more severe character would have been produced. As it was there were no less than *fifty-five* wounds about the scalp, and most of these unimportant.

As negative proofs of this not being a homicidal act, may be mentioned: 1, that there was no mark of violence on the person, discomposure of the dress, or disturbance of the furniture in the room; 2, the act must have been perpetrated at a time when any noise or struggling would have been heard by the neighbours; but nothing of the kind was remarked.

The conclusions of the inspectors in this case were:

1. That the deceased did not die from the wounds found on her person nor from the loss of blood.

2. That death was the result of hanging, operating partly through pressure on the nerves and partly through suffocation.

3. That it was an act of suicide.

[Remarks. This case is in many points interesting to the medical jurist. Had it occurred in England, it is scarcely necessary to observe that a coroner's jury would, under the circumstances, have deemed an inspection of the body useless, and as only giving rise to unnecessary delay and expense. Yet it is by the accurate observation of cases of this kind that materials can be collected,

which will enable a medical witness to prevent coroners' juries from falling into mistakes in future investigations where the circumstances are of a more doubtful character. Out of the hundreds of cases of suicidal hanging which occur in this country, how many bodies are inspected by the order of a coroner? Scarcely one per cent.! We do not believe that so complete and accurate an investigation of the subject of hanging could be produced from the records of coroners' inquests, although the opportunities for benefiting science by this kind of information have been numberless.

This case possesses more than usual interest: 1, On account of the great number of wounds found on the head; 2, from the *nature* of some of the wounds, thus those over the sagittal suture were sufficient to cause a separation of the inner table of the skull; 3, from the situation. One violent contusion was seen on the os occipitis, a most unusual situation for a suicidal injury. It is remarkable that from the two last-mentioned circumstances concussion of the brain, depriving the person of the power of subsequently hanging herself, had not resulted, since there was evidently violence enough to account for an immediate loss of sense and motion. On the whole, suicide seems to have been more established from *general* than from *medical* facts; and had the deceased been found thus suspended in an exposed place, a strong suspicion of murder might fairly have arisen in the minds of the inspectors.]

*Henke Zeitschrift für die Staatsarzneikunde.* i. 1840.

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*Poisoning by a Tobacco Enema.* B. M. Tavignot.

A strong man aged fifty-five, who had laboured for some time under dysuria from enlarged prostate, and more recently suffered from the presence of ascarides in the rectum, was subjected to the action of a tobacco injection, made by a decoction of 12 grs. of the tobacco in 6 oz. of water. Seven or eight minutes afterwards, the patient appeared in a slight stupor, with cephalalgia, and unusual paleness of the face. He complained of pain in the abdomen, and his answers to questions were troubled. Two purgative injections were successively administered. A stimulant potion was given; sinapisms applied to the inferior extremities, and blood taken to the amount of three *palettes.* Paleness became more and more marked, respiration more and more laboured, stupor, intelligence altogether lost; convulsive movements of the arms, then of the legs, and afterwards of the whole body, which progressively augmented during six or seven minutes, and were succeeded by extreme prostration. The patient fell into a comatose state and died.

*Revue Médicale. Décembre,* 1840.

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*Medico-legal Question on the Criminal Production of Abortion in case of an Acephalous Monster.*

On the 2d of June last at the Assizes at Drôme a girl was accused of procuring abortion by criminal means. The fœtus when it came into the world was acephalous; there was no vertebral canal to the spinal column; the spinal marrow and nerves were in a rudimentary state and immediately under the skin. One of the lungs was but half the size of the other, and both sunk in water The forehead was completely absent, the chin was almost confounded with the chest, and there was scarcely any neck. The fœtus was of about six months. It was proved by MM. Morin and Biguel that it had never respired, and that its life ceased with gestation; but they also remarked on its head a wound and a small ecchymosis which they regarded as a probable result of a criminal manœuvre, executed by the aid of a pointed instrument shortly before abortion, and they thought that the death of the fœtus was the consequence, whilst in the womb of the mother.

The most important question which presented itself, and which was seized by

the counsel for the prisoner was, in a case where the material fact of abortion provoked by the accused was demonstrated, could the penalty apply for delivering herself of a monstrous fœtus, incapable of life, and could a deed be considered as a crime which had caused no prejudice to society nor to an individual? (individualité.) The defender after having distinguished abortion from infanticide, which cannot be a crime when the infant is not viable; and alluded to the remarkable fact peculiar to abortion, that the law does not punish the intention, even when it is manifested by the commencement of execution, argued that it is not the endeavour that the law punishes, but the injurious deed alone. "The destruction of a monster incapable of life cannot injure it. The fact, then, cannot constitute the crime of abortion. If it be the deed prejudicial to generation that the law arraigns, it must be said that in this case there existed no prejudice, no crime, and no penalty could be awarded." The jury acquitted the prisoner, notwithstanding the opposition of the *ministre public*.

*Gazette Médicale de Paris. 3 Juillet*, 1841.

## ANIMAL CHEMISTRY AND PHARMACY.

*On the Chemical Composition of the Brain.* By M. Fremy.

According to the analyses of M. Fremy the human brain is formed of a considerable quantity of water, and of matter insoluble in æther, which he calls albuminous matter. It contains also a part soluble in æther which is formed by three matters: 1, white matter, which he calls cerebric acid, he having discovered it to possess well-marked acid properties; 2, a liquid fatty matter, which has the composition and all the properties of the oleine of human fat; 3, cholesterine; also very slight and variable traces of oleaic and margaric acids, and cerebrate of soda. The albuminous matter of the brain has the property of transforming oleine into oleaic acid.

*Journal des Connaissances Médico-Chirurgicales. Janvier*, 1841.

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*On the identical Composition of Fibrin and Albumen.* By M. Liebeg, of Giessen.

M. Liebeg states that he has been able to dissolve pure fibrin in a saturated solution of nitre, at a temperature from 50 to 56 degrees. The fibrin becomes gelatiniform, merely leaving a few flocculi, which are insoluble. The filtered fluid possesses all the properties of albumen, and the composition of both is exactly the same. It is as follows:

| | |
|---|---|
| Carbon | 48 |
| Hydrogen | 74 |
| Nitrogen | 14 |
| Oxygen | 11 |

M. Liebeg has also precipitated albumen under the form of globules, by adding a sufficient quantity of water to serum which had been neutralized by an acid, and he has likewise obtained fibrin from the blood-globules, by the proceeding described by M. Denis. Lastly, by adding a little caustic potash to albumen, M. Liebeg has precipitated it under the form and with the properties of casein, by means of alcohol.

*Gazette Médicale de Paris. 3 Avril*, 1841.

## II. THE AMERICAN AND COLONIAL JOURNALS.

*Origin of the Operation for Strabismus.*

[It would appear from the following communication from S. Y. Atwell, Esq. of Providence, to Dr. Rivers of the same city, that an American Physician, and not Dr. Stromeyer, has the merit of having first suggested this operation:]

In the years 1812-13, I attended courses of surgical and anatomical lectures delivered before the Medical School of Brown University, by William Ingalls, M.D., of Boston, then the Professor of Anatomy and Surgery in that institution: being subject myself to this infirmity (strabismus), Dr. Ingalls took frequent opportunities to explain to me the method of its surgical cure; he did this by dissection of the eye itself, explanation of the power and disposition of the several muscles appertaining to that organ, and showed me how, by a division of one or more of them, the eye might be brought to its proper place. In my own case I know he proposed to divide the *rectus internus*. So strongly was I impressed with the practicability and success of this operation, that I strongly urged my father to permit me to submit to the operation; but upon the nature of the operation being explained to him, he declined the permission, because he feared the effect might be to turn the eye the other way.

*Medical Examiner. Feb.* 20, 1841.

*Cases of Poisoning by Arsenic—Treated by the Hydrated Peroxide of Iron.*
By T. T. Smiley, M.D., and J. M. Wallace, M.D.

On the 3d day of January, 1840, the family of Mr. Constant Gigon, residing at No. 148, Pine Street, Philadelphia, was poisoned by eating a pudding made of Indian meal, in which arsenic had been mixed, probably for the purpose of destroying rats. The precise quantity of arsenic contained in the pudding could not be ascertained; but, judging by the effect produced, must have been very great. The whole family, consisting of eight persons, was affected with symptoms more or less violent. An hour after the pudding had been eaten, on finding a general sickness pervading the family, a dose of ipecacuanha had been administered to each of the individuals affected, by Mr. Zollickoffer, an apothecary residing in the neighbourhood.

On the arrival of Dr. Smiley, who was first called in, poisoning from arsenic was at once suspected,—and on that supposition copious draughts of warm water, with sweet oil, were administered, and followed by whites of eggs in large quantities. During the operation of the foregoing remedies, given with a view to evacuate the stomach, and remove the poison as thoroughly as possible, Mr. Zollickoffer was requested to prepare a large quantity of the hydrated peroxide of iron, for the purpose of being administered as an antidote to that portion of arsenic which always in such cases remains in the stomach, and cannot be entirely evacuated by vomiting. As soon as the hydrated peroxide of iron could be prepared, it was administered in tablespoonful doses, at short intervals, to all the patients, and as often as rejected by vomiting, was immediately repeated. In nearly all the patients an almost entire subsidence of the most violent symptoms soon followed, and the burning sensation of the œsophagus and stomach greatly diminished. Two of the patients died.

In the two cases which proved fatal, death appears to have ensued from the direct effects of the poison on the nervous system. It might even be doubted whether the injury sustained by the stomach in either case was sufficient to have produced death by the subsequent inflammatory action, and possibly both these patients might have recovered, provided the direct shock to the system could have been obviated, and reaction produced.

It is worthy of observation that every one of the patients who were able to retain the hydrated peroxide of iron on the stomach speedily recovered, and were restored to perfect health, with the entire exemption from any symptom

indicating the existence of chronic imflammation of the stomach which has heretofore so uniformly followed poisoning from arsenic in the few cases which have not proved immediately fatal.

The great and almost immediate alleviation of the urgent symptoms which followed in every instance in which the hydrated peroxide of iron could be retained on the stomach, affords the strongest evidence that the beneficial results ascribed to that remedy in such cases have not been overrated. Fortunately, few instances have occurred in which the virtue of that antidote could be tested on so large a scale.

Note.—The contents of the stomachs of the two patients who died were submitted to a careful chemical examination by Dr. Rogers, and the existence of arsenic in them placed beyond a doubt. A large quantity of metallic arsenic was produced both from the contents of the stomachs and the remains of the pudding.

*Medical Examiner. October* 24, 1841.

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*Anomalous Case of Periodical Suspended Perception.*
By Joseph James Ridley, m.d.

[This seems a case of catalepsy, of which the most curious feature is the loudness of the eructations. Were not the case authenticated as it is, it would seem fabulous: but as the French proverb has it, truth is not always truthlike.]

Harry, a man servant of Mrs. W., of N. C., was from his youth of a strong and vigorous constitution, plethoric habit, and in the intervals of his paroxysms of good general health. In about his thirty-fifth year, Dr. R. was requested to see him in a paroxysm simulating apoplexy. The remedies for apoplexy were used in vain. He lay perfectly torpid and insensible; the symptoms became hourly more threatening until recourse was had to cleansing the primæ viæ. The administration of a laxative restored him to sensation, and with the restoration of his perceptions all symptoms of disease disappeared. In a few days he had a second attack; the symptoms as before. The same treatment relieved him. His mistress, not famed for abundance of the "milk of human kindness," suspected his honesty, and cruelly scourged and burnt him in his paroxysms without making any impression upon his perceptions. His paroxysms periodically returned, and became more frequent gradually. He was likely to prove an expensive piece of property; and his mistress, resolved to incur no more expense, sold him at a reduced price to Dr. R., who, upon taking him home, essayed every power of the medical art to ascertain and remove the disease. He discovered that in his paroxysms the abdomen was uniformly extremely tense; the colon was, to all appearance, the seat of the disease. Upon making use of friction upon the abdomen, a vast quantity of flatus was eructated, and the urgent symptoms gradually gave way. After discharging an inconceivable quantity of gas, his senses returned; his paroxysms gradually became quotidian. Dr. R. was forced to take him with him whenever he was called from home, about the periodical recurrence of the paroxysm. The longer the paroxysm continued, the more difficult was it to restore him to sensibility. A most remarkable fact was the circumscribed spot upon which friction afforded relief; a five franc piece would cover the space on which it was necessary to rub; friction on any other part of the abdomen was certain to aggravate the symptoms fourfold. This spot was, I think, about four or six lines to the right of the umbilicus.

Another remarkable fact connected with this case was the incredible force of his eructations: when properly rubbed, the flatus would escape with such force *as to be heard one or two miles*[!] I am aware, Messrs. Editors, that this seems rather a Munchausen story; it transcends the bounds of credibility, I know. It is difficult to believe a statement conflicting so palpably with the results of our observation. The lungs had no part in the production of the report; it was exclusively from the stomach and intestines. The statement, in its general out-

line, would be avouched by a whole neighbourhood, and several physicians, to whom the case was one of inexplicable difficulty and interest. When the paroxysm was coming on, before the power of locomotion was suspended, he would wander about, regardless of those things that affect the senses. Upon one occasion he was in the act of jumping into a well, and was most narrowly and opportunely rescued. Upon the return of sensibility, he was like a man suddenly aroused from sleep, before his perceptions are fully brought into exercise.

The disease lasted several years and finally exhausted itself.

*Medical Examiner. January* 9, 1841.

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*On the Presence of Kiesteine in the Urine as a Test of Pregnancy.* By W. M. M'Pherters and J. C. Perry, Resident Physicians at Philadelphia Hospital.

We have instituted a series of experiments for the purpose of ascertaining the existence of kiesteïne in the urine of pregnant women. They have been conducted after the manner pursued by Dr. Golding Bird, as published in Guy's Hospital Reports, of April, 1840. Daily observations were made, and every change as it presented itself carefully noted. The results of our observations are as follows: The urine of twenty-four out of twenty-seven women, in different stages of pregnancy, from the second to the ninth month of utero-gestation, on the second day began to lose its transparency; on the third, became quite opalescent. From the second to the fourth day, a whitish scum made its appearance, which, as described by previous observers, might aptly be compared "to the layer of greasy matter which covers the surface of fat broth when it has been allowed to cool." This continued to increase until the fifth or sixth day, when, in each case, it was so unequivocally marked as not to be mistaken. From this until the fourteenth day, when the observations closed, the pellicle became gradually thinner by the detachment and subsidence of flocculi, though it never entirely disappeared. The odour, which was peculiar, could not be recognized by us as "cheesy," although our attention was particularly directed to this point. The filtered specimens of the same urine underwent similar changes.

Of the three remaining cases, in which the pellicle did not appear, one was labouring under peritonitis, and subsequently had an attack of puerperal fever, and another had incipient phthisis. We are unable to say whether the third was labouring under disease or not.

The urine of one suckling woman, two weeks after delivery, presented all the characteristics of kiesteïne. That of two others, twelve months after delivery, whose children were still at the breast, underwent no change.

So far as we observed, the presence or absence of milk in the breasts, previous to delivery, appeared to exercise no influence over the formation of the scum.

An elevated temperature of from 60° to 80° appears most favorable to the speedy and perfect formation of the kiesteïnic pellicle, whilst one low enough to freeze the urine occasionally prevents or very materially retards the process. This may perhaps account for some apparent discrepancies between our observations and those of Dr. Bird, as some of our experiments were conducted at rather a low temperature.

By way of instituting a comparison at the same time, the urine of twenty-seven unimpregnated women was submitted to examination, but underwent no changes indicative of kiesteïne. Of these, twelve were regular and healthy, nine were labouring under gonorrhœa, and were generally irregular. Three had chronic leucorrhœa, two indolent ulcers on the leg, and one was in the last stage of phthisis. The six last mentioned also had amenorrhœa of long standing.

The urine of four healthy men, and of two labouring under catarrh of the bladder, underwent no change.

From the nature of our situation, we had an opportunity of tracing the history of each case examined. The urine, in every instance, was that first voided

in the morning, and that of no one was taken who did not remain long enough in the house either to be delivered or to place their state beyond a doubt.

From what has hitherto been published on this subject, as well as from the further observations made by ourselves, it will be seen that this matter is worthy of further investigation; and that whilst it cannot be relied on as an absolute test of pregnancy, yet when taken in conjunction with other symptoms, it forms a valuable aid to diagnosis.

*American Medical Library and Intelligencer. March* 15, 1841.

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*On the Use of the Chloride of Silver.* By J. C. Perry, M.D. one of the Resident Physicians of the Philadelphia Hospital.

It must be universally acknowledged, that the nitrate of silver administered by the mouth can never act as nitrate of silver on the system, since any dose which could be ventured upon must, immediately on entering the stomach, be converted into the chloride of silver, from the chloride of sodium of our food, or the hydrochloric acid of the gastric juice.

The chloride must then, à priori, be considered as efficacious equally with the nitrate, while it will be found less uncertain in its effects, more convenient for exhibition, less liable to decomposition, and free from its nauseous taste. It may be given, too, in any dose thought necessary to produce the alterative and tonic action of silver without danger.

In less doses than thirty grains no irritating or manifest effects result. Thirty grains given at once will generally produce emesis. The best form of exhibition is in pill. To children it can be given as a powder suspended in syrup.

Twelve grains administered daily for three months have produced no unpleasant symptoms, and none of my numerous and long-continued courses of the remedy have been followed by discoloration of the skin.

In epilepsy, three grains, given four or five times daily, produced effects similar to those of nitrate of silver, but more marked.

In chronic dysentery, half a grain to three grains thrice daily produces immediate diminution in the number of stools, and tormina, with amelioration in the character of the stools and other symptoms.

I have not ventured to use it freely in acute dysentery, but in a few cases in which it has been administered, its effects seemed equally beneficial.

In chronic and colliquative diarrhœa, benefit was derived from its use in the same doses, but not so markedly, uniformly, or permanently.

The catamenia suspended for years have, without any adjuvant treatment, returned after its free exhibition for two or three weeks.

One case of secondary syphilis (the only one in which it has been tried) improved rapidly under it.

*American Medical Library and Intelligencer. February*, 1841.

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## III. THE BRITISH JOURNALS.

*Memoir of the Case of a Gentleman born Blind, and successfully operated upon in the eighteenth year of his age; with Physiological Observations and Experiments.* By J. C. August Franz, M.D. M.R.C.S.

The young gentleman who is the subject of this memoir had been affected from birth with strabismus of both eyes; the right eye was amaurotic, and the left deprived of sight by the opacity both of the crystalline lens and of its capsule. At the age of seventeen, an operation for the removal of the cataract of the left eye was performed by the author with complete success. On opening the eye for the first time, on the third day of the operation, the patient described his visual perception as being that of an extensive field of light, in which every thing appeared dull, confused, and in motion, and in which no object was dis-

tinguishable. On repeating the experiment two days afterwards, he described what he saw as a number of opaque watery spheres, which moved with the movements of the eye, but when the eye was at rest remained stationary, and their margins partially covering one another. Two days after this the same phenomena were observed, but the spheres were less opaque and somewhat transparent; their movements were more steady, and they appeared to cover each other more than before. He was now for the first time capable, as he said, of looking through these spheres, and of perceiving a difference, but merely a difference, in the surrounding objects. The appearance of spheres diminished daily; they became smaller, clearer, and more pellucid, allowed objects to be seen more distinctly, and disappeared entirely after two weeks. As soon as the sensibility of the retina had so far diminished as to allow the patient to view objects deliberately without pain, ribands differently coloured were presented to his eye. These different colours he could recognize, with the exception of yellow and green, which he frequently confounded when apart, but could distinguish when both were before him at the same time. Of all colours, gray produced the most grateful sensation: red, orange, and yellow, though they excited pain, were not in themselves disagreeable; while the effect of violet and of brown was exactly the reverse, being very disagreeable, though not painful. Brown he called an ugly colour: black produced subjective colours; and white gave rise to a profusion of *muscæ volitantes.* When geometrical figures of different kinds were offered to his view, he succeeded in pointing them out correctly, although he never moved his hand directly and decidedly, but always as if feeling with the greatest caution. When a cube and a sphere were presented to him, after examining these bodies with great attention, he said that he saw a quadrangular and a circular figure, and after further consideration described the one as being a square, and the other a disc, but confessed that he had not been able to form these ideas until he perceived a sensation of what he saw in the points of his fingers, as if he really touched the objects. Subsequent experiments showed that he could not discriminate a solid body from a plane surface of similar shape; thus a pyramid placed before him, with one of its sides towards his eye, appeared as a plane triangle.

Two months after the above-mentioned operation, another was performed on both eyes, for the cure of the congenital strabismus, by the divisions of the tendons of the recti interni muscles, which produced a very beneficial effect on the vision of the left eye; and even the right eye, which had been amaurotic, gained some power of the perceiving light, and, from being atrophied, became more prominent. Still it was only by slow degrees that the power of recognizing the true forms, magnitudes, and situations of external objects was acquired. In course of time, the eye gained greater power of converging the rays of light, as was shown by the continually increasing capacity of distinct vision by the aid of spectacles of given powers.

*Proceedings of the Royal Society.* 1840-1841. No. 46.

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*On the Preparation of Vinum Ferri.* By M. Donovan, Esq.

The virtues of this preparation, the first metallic medicine on record, have been known to mankind since the time of the Argonautic expedition: its medicinal character has therefore stood the test of experience during 3100 years—and it is now more in use than it has been for a long period. The will of the profession retains it, although it is expunged from the three British Pharmacopœias; and a medicine that has withstood such vicissitudes cannot be destitute of virtue. Why, then, is it expunged? why is there so much difference of opinion? The answer is, because there are such differences in its strength, and in the modes of preparation.

I have, by a very simple process, removed all uncertainty from this preparation, and now obtain it always of the same strength, containing an effective quantity of iron, of an agreeable flavour, and still possessing all the aroma of

the wine. The process is this:—Take of the best hock one pint; common rust of iron of the shops well levigated, two ounces. Introduce both into a matrass, which plunge into a water bath maintained at the temperature of 100°. Constantly agitate the matrass for an hour; then remove it from the water, and the next day filter. The colour of this vinum ferri is a very deep greenish brown, almost black when the volume is great: its taste is ferruginous, agreeably and highly vinous; it produces a pleasant warmth in the stomach, and never sickens. In its effects it must be tonic, diuretic, emmenagogue, anthelmintic, and carminative. It does not, in a moderate dose, excite. No other wine than hock will afford a preparation possessing these virtues. The dose for an adult may be three or four drachms thrice a day; in smaller doses, it is of little use. If it is to be exhibited in combination with a bitter, it agrees well with colombo or gentian. By this method, in one day, we obtain a far better preparation than is procurable by the processes of the pharmacopœias in two months. The iron exists in it chiefly in the state of protoxide.

*Dublin Medical Press. June* 23, 1841.

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*Hints to Prescribers of Sulphate of Quinina.* By M. Donovan, Esq.

The agreeable taste, odoriferous smell, and elegant colour of the acid infusion of roses have rendered it a favorite vehicle for the exhibition of more active medicines; and the choice is the more fortunate when the medicinal or physical properties of the infusion assist those of the remedy for which it is the selected medium. On these accounts the infusion of roses is frequently employed as a solvent for sulphate of quinina. The latter being nearly insoluble in water, and while in the insoluble state being much less active, it is rarely prescribed in liquids merely aqueous: sulphuric acid is therefore always employed to acidulate the water, as well as to add to the tonic powers of the salt. Infusion of roses contains the necessary quantity of sulphuric acid, and if it possess any medical efficacy, this coincides with the powers of the sulphate of quinina. Hence it is a common practice to prescribe infusion of roses with sulphate of quinina; and it is supposed that the two medicines form an elegant, efficacious, and compatible mixture.

I believe that the supposition is ill-founded. The mixture is not elegant; for it is no longer red and transparent, but becomes muddy and disagreeable in appearance; it is not efficacious; for much of the quinina is withdrawn in an insoluble state; and it is not compatible; for there are two sources of decomposition. Rose-leaves contain both gallic and tannic acids: hence gallate and tannate of quinina will be formed; both are insoluble in cold water; and they will remain floating through the liquid, notwithstanding the presence of sulphuric acid, which, so far as these salts are concerned, affects no good purpose as it does not dissolve the new salts formed.

Sulphate of quinina is also prescribed in conjunction with compound infusion of orange-peel as a vehicle and adjuvant. I have seen this much practised both in England and Ireland, but believe the formula to be liable to the same objection, for a precipitate is copiously deposited.

It might be supposed that it is little matter in what state the quinina is administered, whether as a sulphate, gallate, or tannate: but if the sulphate require the addition of sulphuric acid to hold it in solution, and if the state of solution be necessary to the exertion of its full medical efficacy, it must be improper to conjoin with it any agent which eliminates it in the solid form. Besides, it has never been proved that either the gallate or tannate of quinina possesses medical powers.

It seems much better to prescribe the sulphate of quinina simply dissolved in water, or in camphorated mixture, by the aid of a little sulphuric acid. If adjuvants are required, they may be administered by themselves, in some time after the exhibition of the sulphate. It is unsafe to combine or modify such delicate articles, as it is difficult to foresee where injury may be done. Who,

except one that had witnessed the effect, would suppose that there could be any impropriety in conjoining sulphate of quinina with preparations of cinchona bark? yet alcoholic solution of sulphate of quinina is copiously precipitated by tincture of bark, and insoluble tannate of quinina is soon separated. I have reason to believe that the precipitate which falls when the decoction of bark is allowed to cool, consists chiefly of tannate of quinina.

*Dublin Medical Press. July*, 1841.

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*On the Chorda Dorsalis.* By MARTIN BARRY, M.D. F.R.S. L. & E.

THE author of this communication, after pointing out the similarity in appearance between an object noticed by him in the mammiferous ovum, and the incipient chorda dorsalis described by preceding observers in the ova of other vertebrata, mentions some essential differences between his own observations and those of others as to the nature and mode of origin of these objects, and their relation to surrounding parts. Von Baer, the discoverer of the chorda dorsalis, describes this structure as "the axis around which the first parts of the fœtus form." Reichert supposes it to be that embryonic structure which serves as "a support and stay" for parts developed in two halves. The author's observations induce him to believe that, instead of being "the axis *around which* the first parts of the fœtus form," the incipient chorda is the last-formed row of cells, which have pushed previously-formed cells farther out, and that, instead of being merely "a support and stay" for parts developed in two halves, the incipient chorda occupies the centre out of which the "two halves" originally proceeded as a single structure, and is itself in the course of being enlarged by the continued origin of fresh substance in its most internal part.

The author enters into a minute comparison of the objects in question; from which it appears that the incipient chorda is not, as Baer supposed, developed into a globular form at the fore end, but that the linear part is a process from the globular; and that the pellucid cavity contained within the latter—a part of prime importance, being the main centre for the origin of new substance—is not mentioned by Von Baer. Farther, that the origin of the "laminæ dorsales" of this naturalist (the "central nervous system" of Reichert) is not simultaneous with, but anterior to, that of the chorda.

The author then reviews the observations of Rathke and Reichert on the chorda dorsalis, which contain internal evidence, he thinks, of a process in the development of fishes, reptiles, and birds, the same as that which he has observed in mammalia, namely, the origin of the embryo out of the nucleus of a cell.

And it is his opinion that this observation may assist to solve a question on which physiologists are not agreed; for it shows that if the nucleus of a cell is a single object, the first rudiments of the embryo are not two halves. The author thinks that unless the very earliest periods are investigated, it is in vain that we attempt to learn what that is of which the rudiments of the embryo are composed. From not attending to this, physiologists have supposed their "primitive trace" to arise in the substance of a membrane, which the author, in his second series on the embryo, showed could not be the case. To the same cause he thinks is referrible an opinion recently advanced by Reichert, that the first traces of the new being are derived from cells of the yelk.

*Proceedings of the Royal Society.* 1841. No. 46.

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*On the Corpuscles of the Blood.* By MARTIN BARRY, M.D. F.R.SS. L. & E.

THE observations recorded in this memoir are founded on an examination of the blood in every class of vertebrated animals, in some of the invertebrata, and in the embryo of mammalia and birds. The nucleus of the blood-corpuscle, usually considered as a single object, is here represented as composed, in some instances, of two, three, or even many parts; these parts having a constant and determinate form. In the substance surrounding the nucleus, the author has

frequently been able to discern, not merely "red colouring matter," but cell-like objects; and he points out an orifice as existing at certain periods in the delicate membrane by which this substance is surrounded. In a former memoir he had differed no less from previous observers regarding "cells." He had shown, for instance, that the nucleus of the cell, instead of being "cast off as useless and absorbed," is a centre for the origin, not only of the transitory contents of its own cell, but also of the two or three principal and last-formed cells, destined to succeed that cell; and that a separation of the nucleus into two or three parts is not, as Dr. Henle had supposed in the case of the pus and mucus-globule (the only instances in which the separation in question had been observed), the effect of acetic acid used in the examination; but that such separation is natural, apparently common to nuclei in general, and forming part of the process by which cells are reproduced. The author had farther shown the so-called nucleolus to be not a distinct object existing before the nucleus, but merely one of a series of appearances arising in succession, the one within the other, at a certain part of the nucleus, and continuing to arise even after the formation of the cell. These views he now confirms; and in the present paper shows that they admit of being extended to the corpuscles of the blood.

He then compares appearances observed in the latter with those he had traced in the ovum. These relate to the number of parts of which the nucleus is at different periods composed; the nature of the nucleolus; the communication between the nucleolus and the exterior of the cell; the formation of the contents of the cell out of the nucleus; the final division of the nucleus into the foundations of a limited number of young cells, destined to succeed the parent cell; and the escape of the young cells for this purpose. It follows from these investigations that the corpuscles of the blood are generated by a process essentially the same as that giving origin to those cells which are the immediate successors of the germinal vesicle, or original parent cell; it being also by a continuation of the same process that the corpuscle of the blood divides itself into the minuter objects figured by the author in his former paper on the blood.

He adds, that in its form and internal state the blood-corpuscle found in the adult of certain animals very much resembles that existing only in the fœtal life of others. It is incidentally remarked that the fœtal brain, at certain periods, appears to consist almost entirely of objects very much resembling those which, in some stages, form the nuclei in the fœtal corpuscles of the blood.

The author concludes by expressing his opinion that the mode of evolution of the minute mammiferous ovum is deserving of close attention, in connexion with some of the processes by which nourishment is communicated, and the growth of the body effected at all future periods of life.

*Proceedings of the Royal Society.* 1841. No. 46.

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*On the Action of certain Inorganic Compounds when introduced directly into the Blood.* By James Blake, Esq., M.R.C.S.

After some preliminary remarks on the mode in which the experiments were conducted, and on the assistance derived from the hæmadynamometer of Poiseuille (or instrument for measuring the pressure of the blood circulating in the vessels), the author gives a list of the various saline substances of which he noted the effects when they were severally injected either into the venous or the arterial systems, arranged according to the nature of those effects. He finds, in general, that all the salts having the same base exert similar actions when introduced directly into the blood. He carefully enquires into the phenomena apparently arising from the direct contact of each of the substances above enumerated with the animal tissues; and more particularly into the effects produced on the heart, on the muscular and the nervous tissues, and on the pulmonary and systemic capillaries.

The first series of experiments related are those on the action of the salts of

magnesia: these are found, when introduced in any quantities into the blood, to arrest altogether the action of the heart; but a still more remarkable effect which results is the complete prostration of muscular power. The salts of zinc have a similar operation under the same circumstances, but produce the same effects in smaller quantities. The action of the salts of copper, of lime, of strontia, of baryta, and of lead, are considered successively in the order in which they are more closely related by their physiological actions. The author particularly notices the peculiar action which the salts of the three last-named substances exercise on the muscular tissues, occasioning contractions in them during many minutes after death produced by their introduction into the blood. These muscular movements were, in some cases, observed forty-five minutes after the cessation of the heart's action. Experiments with the salts of silver and of soda are then detailed: substances which exhibit a remarkable similarity in their actions on the pulmonary tissue, on the heart, and on the systemic capillaries: for while, in the case of all the other salts already mentioned, death seems to be produced by the destruction of the irritability of the heart, the fatal result with the salts of silver and of soda is the consequence of their action on the tissue of the lungs. The physiological actions of the salts of ammonia and of potass were found by the author not to correspond with any of the preceding. Although agreeing perfectly with one another in their action upon the heart and systemic capillaries, they differ extremely in their effects on the nervous tissue; ammonia being particularly distinguished from all inorganic compounds in this respect, and being very analogous to poisons derived from organic products, which it also resembles in its chemical properties.

The general conclusion which the author is led to draw from these researches is, that there exists a close relation between the chemical properties of the substances experimented upon, and their physiological effects; his experiments tending to prove that, when introduced into the blood, substances which are isomorphous exert similar actions on the living tissues. He notices, however, two exceptions to this law; namely, the similarity of the actions exerted on the pulmonary tissue by the salts of lead with those of silver, although these salts are not isomorphous; and also the action on the nervous tissue of the salts of ammonia being different from that of the salts of potass. But he remarks that the oxide of lead bears a close analogy to the oxide of silver in its relation to organic compounds. The general fact previously announced by the author in his memoir read to the Academy of Sciences at Paris, namely, that salts with the same base have analogous actions, may be considered as a corollary of the above law.

*Proceedings of the Royal Society.* 1840—1841. No. 46.

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*Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiar uniform mode of their Organization and Nutrition.* By Joseph Toynbee, Esq. Member of the Royal College of Surgeons.

In the introduction to this paper, the author first speaks of the process of nutrition in the animal tissues which are pervaded by ramifications of blood-vessels; pointing out the circumstance, that even in them there is a considerable extent of tissue which is nourished without being in contact with blood-vessels. The knowledge of this fact leads us to the study of the process of nutrition in the non-vascular tissues; which tissues he divides into the three following classes; namely, first, those comprehending articular cartilage, and the cartilage of the different classes of fibro-cartilage. Under the second head he comprises the cornea, the crystalline lens, and the vitreous humour; and, under the third, he arranges the epidermoid appendages; viz. the epithelium, the epidermis, nails and claws, hoofs, hair and bristles, feathers, horn and teeth.

The author then proceeds to show that the due action of the organs, into the composition of which these tissues enter, is incompatible with their vascularity. In proof of the non-existence of blood-vessels in these tissues, he states that he

has demonstrated, by means of injections, that the arteries, which previous anatomists had supposed to penetrate into their substance, either as serous vessels, or as red-blood vessels too minute for injection, actually terminate in veins before reaching them; he also shows that around these non-vascular tissues there are numerous vascular convolutions, large dilatations and intricate plexuses of blood-vessels, the object of which he believes to be to arrest the progress of the blood, and to allow a large quantity of it to circulate slowly around these tissues, so that its nutrient liquor may penetrate into and be diffused through them. The author states that all the non-vascular tissues have an analogous structure, and that they are composed of corpuscles, to which he is induced to ascribe the performance of the very important functions in the process of their nutrition, of circulating throughout, and perhaps of changing the nature of the nutrient fluid which is brought by blood-vessels to their circumference. The author then brings forward facts in proof of the active and vital properties of these corpuscles, and concludes his Introduction by stating, that it appears to him, that the only difference in the mode of nutrition between the vascular and the non-vascular tissues is, that in the former, the fluid which nourishes them is derived from the blood that circulates throughout the capillaries contained in their substance; whilst, in the latter, the nutrient fluid exudes into them from the large and dilated vessels that are distributed around them: and that in both classes, the particles of which the tissues are composed derive from this fluid the elements which nourish them.

The author then enters on an examination of the structure and mode of nutrition of the several tissues of each of these three classes. In considering the first class, he commences with articular cartilage, which he describes at great length in the various stages of its development, and at the different periods of life. He gives in detail the account of numerous dissections of the ovum and fœtus illustrating the first stage, during which he shows that no blood-vessels enter into the substance of any of the textures composing a joint; but that the changes its component parts undergo, are effected by the nutrient fluid from the large blood-vessels, by which, at this stage, each articulation is surrounded. In the second stage of the development of articular cartilage, the author shows, by numerous dissections, the process by which the blood-vessels are extended into the substance of the epiphysal cartilage, and converge towards the attached surface of articular cartilage, and how, at the same time, blood-vessels are equally prolonged over a certain portion of its free surface. He shows that none of these blood-vessels enter the substance of the articular cartilage, and he points out that in them the arteries become continuous with the veins; first, by their terminating in a single vessel, from which the veins arise; secondly, by their forming large dilatations from which the veins originate; and, lastly, they become directly continuous with the veins in the formation of loops of various characters. In the third stage, that which is exhibited in adult life, the epiphysal cartilage is converted into osseous cancelli. These contain large blood-vessels, which are separated from the articular cartilage by a layer of bone composed of corpuscles, and the author believes that the principal source of nutrition to this tissue is the nutrient fluid which exudes into it from these vessels, by passing through the articular lamella just noticed. The free surface of adult articular cartilage is nourished by vessels which pass to a slight extent over it. The author points out the presence of fine tubes which pervade the attached portion of adult articular cartilage, to which he ascribes the function of transmitting through its substance the nutritive fluid derived from the vessels of the cancelli. He also advances the opinion that the articular cartilage becomes thinner during the whole of life, by being gradually converted into bone.

Fibro-cartilage constitutes the second tissue of the first class. The author first enters upon an examination of its structure; and in order to arrive at some definite conclusions on this subject, whereon anatomists of all ages have so much differed, he made numerous dissections of fibro-cartilages in the different classes of animals at various periods of their development, the results of which he

details. He arrives at the conclusion that this tissue is composed of cartilaginous corpuscles and of fibres; the latter preponderating in adult life, the former in infancy; and that during life the corpuscles are gradually converted into fibres. He enters at length into the question of the vascularity of these cartilages; and from a careful study of many injected specimens of man and animals at various periods of their development, the particular results of which he relates, he believes that blood-vessels are contained only in their fibrous portion, and have the function of nourishing that which is cartilaginous, and which, on account of its being subject to compression and concussion, does not contain any.

Among the second class of extra-vascular tissues, the cornea is first treated of; and its structure is described as being very lax, and as containing corpuscles only in a small quantity. The opinions in favour of its vascularity are combated; and it is shown that the blood-vessels which converge to its attached margin, and which are the principal source of the fluid that nourishes it, are large and numerous, and that at the circumference of this tissue the arteries, without any diminution of their caliber, return in their course, and become continuous with the veins. A second set of vessels, devoted to the nutrition of the cornea, is also described; they extend to a short distance over the surface of the tissue, but do not penetrate into its substance.

The crystalline lens is described as being composed of corpuscles, of which the radiating fibres are constituted. The arteria centralis retinæ is described as ramifying over the posterior surface of the capsule, where it forms large branches; these pass round the circumference of the lens, and reach its anterior surface, at the periphery of which they become straight: the arteries terminate in loops frequently dilated, and become continuous with the veins. With respect to the vascularity of the vitreous humour, the author states that although many anatomists have, in general terms, represented the arteria centralis retinæ as giving off, in its course through this organ, minute branches into its substance, still those who have paid especial attention to the subject, have not been able to find such vessels. He believes that the nutrition of this structure is accomplished by the fluid brought to its surface by the ciliary processes of the choroid, which fluid is diffused with facility through its entire substance by means of the corpuscles of which its membrane is composed, assisted by the semifluid character of the humour.

The third class of extra-vascular tissues comprehends the epidermoid appendages. The author describes them all as composed of corpuscles, which are round and soft where they are in contact with the vascular chorion, compressed and flattened where they are farther removed from it. He points out, in the substance of the hoof of the horse, the existence of fine canals, which he supposes to conduct fluid through its mass; and he states that the perspiratory ducts of the human subject possess a structure analogous to the spiral vessels of plants. The author describes each of the tissues of this class at length, and shows that the various modifications presented by the vascular system with which each is in contact, have the sole object of enabling a large quantity of blood to approach and circulate slowly around them. He also points out, in connexion with this subject, the remarkable vital properties which are possessed by these non-vascular tissues.

In concluding this paper, the author states that his object has been to establish as a law in animal physiology, that tissues are capable of being nourished, and of increasing in size, without the presence of blood-vessels within their substance. He shows the analogy which is presented between the extra-vascular animal and the extra-vascular vegetable tissues. He expresses a hope that the application to surgery of the above law, with reference to the prolongation of blood-vessels into the extra-vascular tissues during disease, and to pathology in the investigation of the nature of morbid structures, particularly of those classes which contain no blood-vessels, will be not devoid of interest, and will be productive of some advantage.

*Proceedings of the Royal Society.* 1841. No. 48.

## PART FOURTH.

# Medical Intelligence.

## TABLE OF MORTALITY FOR LONDON,

FOR THE SECOND QUARTER OF 1841:

Showing the number of Deaths from all causes registered in twelve weeks, from the 27th March, 1841, to the 24th June, 1841.

| Causes of Death. | Total Deaths. |
|---|---|
| CLASS I. | |
| Smallpox | 247 |
| Measles | 142 |
| Scarlatina | 120 |
| Hooping-cough | 548 |
| Croup | 102 |
| Thrush | 54 |
| Diarrhœa | 68 |
| Dysentery | 12 |
| Cholera | 1 |
| Influenza | 78 |
| Typhus | 258 |
| Erysipelas | 55 |
| Syphilis | 7 |
| Hydrophobia | 1 |
| Total Epidemic, &c. | 1693 |
| CLASS II. | |
| Cephalitis | 173 |
| Hydrocephalus | 467 |
| Apoplexy | 214 |
| Paralysis | 158 |
| Convulsions | 650 |
| Epilepsy | 49 |
| Insanity | 9 |
| Delirium tremens | 20 |
| Dis. of brain, &c. | 128 |
| Total Dis. of Brain, &c. | 1868 |
| CLASS III. | |
| Quinsy | 20 |
| Bronchitis | 142 |
| Pleurisy | 21 |
| Pneumonia | 735 |
| Hydrothorax | 47 |
| Asthma | 229 |
| Consumption | 2039 |
| Dis. of lungs, &c. | 201 |
| Total Dis. of Lungs, &c. | 3434 |

| Causes of Death. | Total Deaths. |
|---|---|
| CLASS IV. | |
| Pericarditis | 7 |
| Aneurism | 7 |
| Dis. of the heart, &c. | 219 |
| Total Dis. of Heart, &c. | 233 |
| CLASS V. | |
| Teething | 200 |
| Gastritis, enteritis | 192 |
| Peritonitis | 20 |
| Tabes mesenterica | 56 |
| Ascites | 4 |
| Ulceration | 18 |
| Hernia | 21 |
| Colic or ileus | 29 |
| Dis. of stomach, &c. | 55 |
| Hepatitis | 13 |
| Jaundice | 27 |
| Dis. of the liver, &c. | 109 |
| Total Dis. of Stom. &c. | 744 |
| CLASS VI. | |
| Nephritis | 4 |
| Diabetes | 3 |
| Stone | 5 |
| Stricture | 3 |
| Dis. of the kidneys, &c. | 58 |
| Tot. Dis. of Kidneys, &c. | 73 |
| CLASS VII. | |
| Childbed | 72 |
| Ovarian dropsy | 6 |
| Diseases of uterus, &c. | 42 |
| Tot. Dis. of Uterus, &c. | 120 |
| CLASS VIII. | |
| Rheumatism | 23 |
| Diseases of joints, &c. | 31 |
| Total Dis. of Joints, &c. | 54 |

| Causes of Death. | Total Deaths. |
|---|---|
| CLASS IX. | |
| Ulcer | 3 |
| Fistula | 2 |
| Diseases of skin, &c. | 7 |
| Total Dis. of Skin, &c. | 12 |
| CLASS X. | |
| Inflammation | 57 |
| Hemorrhage | 51 |
| Dropsy | 390 |
| Abscess | 35 |
| Mortification | 55 |
| Scrofula | 39 |
| Carcinoma | 93 |
| Tumour | 23 |
| Gout | 15 |
| Atrophy | 69 |
| Debility | 250 |
| Malformations | 11 |
| Sudden deaths | 166 |
| T. Dis. of Uncert. Seat. | 1254 |
| CLASS XI. | |
| Old age or nat. decay | 711 |
| CLASS XII. | |
| Intemperance | 12 |
| Privation | 4 |
| Violent deaths | 283 |
| Total by Violence, &c. | 299 |
| CLASS XIII. | |
| Causes not specified | 42 |
| T. Deaths from all causes Males 5374, Females 5163 | 10537 |

### TEMPERATURE DURING THE QUARTER.

| | Weeks ending April | | | | May | | | | | June | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 3d | 10th | 17th | 24th | 1st | 8th | 15th | 22d | 29th | 5th | 12th | 19th | 26th |
| Highest | 60 | 56 | 57 | 59 | 72 | 65 | 72 | 76 | 79 | 82 | 69 | 80 | 87 |
| Lowest | 38 | 38 | 37 | 40 | 44 | 50 | 46 | 48 | 54 | 52 | 45 | 47 | 51 |
| Daily mean | 49 | 48 | 46 | 48 | 59 | 58 | 58 | 59 | 65 | 66 | 54 | 61 | 64 |

Quarterly Maximum .... 87° Minimum .... 37° Daily Mean .... 55°

## Royal College of Surgeons in London.

[It appears from the following circular which has been transmitted to us for insertion, that the College of Surgeons is at length arousing from its literary lethargy. When will it open its eyes to its more vital interests, and become in fact as well as in name the College of English Surgeons? Were it not that the history of every country exhibits the fruits, or rather no-fruits, of the *vis inertiæ* of non-representative Corporations of every sort, we should be extremely surprised that a body of learned and enlightened men, like the Council of the London College of Surgeons, should, year after year and for many years, be content to see themselves—or, at least, to be seen—in their collegiate capacity, lagging so far in the rear of improvement. To us and to all men of liberal views not within such mesmeric influence, it seems truly marvellous that the simple truth, approved by the plainest reason and demonstrated by all experience, has not yet forced itself upon their minds—that weak, powerless, and without influence as they know and feel they are in their exclusiveness and isolation, they would be strong and powerful and influential in the fair association, union, and communion of their members. We have heard much, of late, of projects being on foot to accomplish so desirable a change; we know such a change cannot be very long delayed; and we are willing to see in the present feeble glimmer, a promise and harbinger of the day that is at hand. We are the more led to indulge this hope from our knowledge of the great talents, enlightened views, generous sentiments, and energetic character of the present President. We cannot but think that if Mr. Guthrie seriously contemplates the reconstitution of the College on more liberal principles, he has sufficient influence with his colleagues to bring it about: of this, at least, we are certain, that a finer field than that which he now occupies can never present itself for the exercise of the noblest ambition. To the earliest champion of such a cause, even failure will be greater glory than the success which crowns the final effort,—

Victrix caussa Deis placuit sed victa Catoni.

Were the College what it ought to be, the encourager, fosterer, protector,—the true, equal, loving and beloved *alma mater* of all the well-educated surgeons of England, what a different series of Transactions might we expect from that which is now commencing!]

"The Council proposing to publish, in the course of the ensuing year, a volume, to be entitled 'Transactions of the Royal College of Surgeons in London,' invite, from the Members of the College and other scientific persons, communications relating to the improvement of anatomical and surgical Science. The subjects proposed to be included in this Publication are specified in the following extract from the Ordinances of the College: 'The Transactions shall consist of Original Communications on Surgical subjects; Collegial and Jacksonian Prize Dissertations, deemed of sufficient originality and merit; Original Memoirs on Human Anatomy; Original Memoirs on Comparative Anatomy; Anatomical Monographs of rare Animals, dissected in the Museum of the College; Explanations of, and Commentaries on, important Preparations in the Museum, with illustrative Plates; Statistical Reports from Hospitals.'

"It is requested that Papers intended for publication in this volume may be transmitted to the President at the College, on or before the 1st of May, 1842.

"Edmond Belfour, *Secretary*."

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## The Metropolitan Convalescent Institution.

As we have reason to believe that this most excellent institution is much less known to the members of the profession than it ought to be, we beg to call the attention of our readers to the following brief notice of its nature and objects, extracted from the Society's First Report just published. The importance of such an establishment is self-evident; and as we can vouch for the high respect, ability and disinterestedness of the managers of it, we venture to hope that many of our metropolitan readers will be induced to contribute to its funds and

also to make known its character to benevolent persons not in the profession. Funds are much wanted to carry its objects into effect.

"The object of the Metropolitan Convalescent Institution is to provide an Asylum in the Country for the temporary accommodation of the convalescents who have been dismissed from our Hospitals and Dispensaries, and for the Sick Poor generally, for whose recovery a short change of air is found to be essential. There are numerous persons who, on leaving the hospital or sick room, are in so destitute and helpless a condition in consequence of the effects of exhaustion and disease, as to be totally unfit to return to their accustomed employments, without their health being more fully established by means of a temporary change of air; whilst there are many others, particularly among the poor, who are labouring under a class of diseases which admit of but little relief from the highest resources of medical skill, and whose only hope of recovery exists in the invigorating influence of country air. Most of these persons are from poverty deprived of the means of obtaining this necessary remedy, and being in consequence of their debility rendered incapable of providing for themselves and families, have no other resource but to become burdens on society, by being the frequent patients of an hospital, or the inmates of a workhouse. It is to provide the means of restoration of health to these persons, many of whom have at one time discharged important duties in life, that the Metropolitan Convalescent Institution has been established.

"The necessity for such an Institution has long been felt by those who have the best opportunity of estimating its importance. A convalescent Institution, indeed, must be regarded as absolutely essential to the completion of our present system of hospitals, and will materially aid in relieving these establishments from the burden of many patients, who having never had the means of acquiring sufficient strength before engaging in their accustomed occupations, become subject to continual relapses, and are frequently obliged to return to the wards of the hospital.

"The Committee have succeeded in obtaining for the Institution very eligible and commodious premises, most agreeably situated in the neighbourhood of the healthy village of Carshalton, in the county of Surrey. The building has garden and other grounds attached, with every necessary convenience, and can accommodate from 50 to 70 persons. Thus, calculating that 50 is the number of patients, and that the average residence of each is one month, there would be upwards of 600 persons annually relieved by means of this Institution. The Committee, however, need scarcely remark, that the extent of relief must altogether depend on the amount of contributions; and they trust that the benevolent will liberally aid them, especially at present, when the furnishing and other necessary expenses will almost exhaust the funds of the Institution.

"Subscriptions will be thankfully received by Messrs. Snow and Co., Bankers, Strand; The Hon. Secretaries, J. C. Pott, Esq., 19, Lincoln's-inn-fields, T. Monro, Esq., 87, Harley-street; Messrs. Mortimer and Haselden, 21, Wigmore-street; and Mr. Nelson, the Collector of the Institution.

## BOOKS RECEIVED FOR REVIEW.

BRITISH.

1. Eighth Annual Report of the Trustees of the State Lunatic Hospital at Worcester (United States).—Boston, 1841. 8vo, pp. 100.

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