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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL
REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL. XXX.

JULY — OCTOBER, 1862.

LONDON:

JOHN CHURCHILL, NEW BURLINGTON STREET.

MDCCCLXII.

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

CONTENTS OF No. LIX.

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PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Lehrbuch der Physiologie für Akademische Vorlesungen und zum Selbstudium.* Von Dr. Otto Funke, Professor der Physiologie an der Universität Freiburg. Dritte Umgearbeitete Auflage.—*Leipzig*, 1860.

Text-book of Physiology for Academic Lectures and for Private Study. By Dr. Otto Funke, Professor of Physiology in the University of Freiburg. Third Revised Edition. Three Volumes.

2. *Untersuchungen über die Physiologie des Electrotonus.* Von Dr. Eduard Pflüger, Privat-docent an der Universität zu Berlin. —*Berlin*, 1859. pp. 500.

Researches on the Physiology of Electrotonus. By Dr. E. Pflüger.

3. *Bericht über die Fortschritte der Anatomie und Physiologie im Jahre* 1858. *Bericht über die Fortschritte der Anatomie und Physiologie im Jahre* 1859. Herausgegeben von Dr. J. Henle und Dr. G. Meissner.

Reports on the Progress of Anatomy and Physiology in the Years 1858 *and* 1859. By Drs. Henle and Meissner.

During the last ten years no chapter in biological science has been cultivated with greater assiduity than that which relates to the general properties and functions of nervous tissue. The study has attracted men who, from a previous profound acquaintance with physical laws, are able to apply a physical mode of inquiry and reasoning to an organic structure. To wring from nature by the most severe physical observation and deduction the mysterious secret of the mode in which are conducted those internal nervous mechanics necessary, on the one hand, to bring the conscious mind into relation with the external world, and on the other, to inform the mind of the changes which the material universe undergoes, is, indeed, a problem worthy of the noblest efforts of intellect. The interest of the subject is commensurate only with its difficulties. It is our desire to acquaint the English reader with the general results of those laborious investigations which have of late so much engaged the attention of Continental physiologists.

For a text, the three works at the top of this article have been selected. A few words regarding each. First on the list is the third edition of a work which, in our opinion, is the most complete physiological text-book of the day. In its preface the author remarks, that the feature which chiefly distinguishes this from the preceding edition, is the great enlargement of the chapter on General Nerve Physiology. In his attempt to give a digest of the voluminous and scattered literature of this subject, we think the author has been highly successful, and in what follows, let it be understood that we have closely followed the order adopted by Funke.

The second work above-mentioned is of an entirely different character. It embodies the results of immense research. It constitutes an epoch in the science of animal electricity. It stamps its author as one of the most profound physiologists of modern times.

The 'Yearly Reports on Anatomy and Physiology for 1858–9,' by Henle and Meissner, have been cited because they contain tolerably good accounts of papers which otherwise could not be made available. Some of the most valuable contributions to nerve physiology have been made in the form of short papers, widely scattered through the German journals. These are collected and reported by Professor Meissner in his annual Bericht.

With such sources of information before us, we shall now attempt to give an outline of what is at present known of this interesting, but abstract subject. Nervous tissue, in its two distinctive varieties, grey and white, presents for examination structural, chemical, and physical peculiarities. Besides these, which we may regard as properties, nervous tissue possesses certain more distinctive and more complex peculiarities known as its physiological attributes. It is upon some of these that recent inquiry has exhausted itself, and the data which have resulted, have led to the formation of the laws of nerve-excitation, of nerve-irritability, and of nerve-conduction. With other of the attributes of nerve-tissue—such, for instance, as belong to the functional mechanism of grey matter—we are as yet entirely unacquainted. The psychical connexions, which place this tissue above the rank of all others, seem equally to remove it beyond the reach of the keenest physical examination.

It will be convenient to consider our subject under the following heads:—1st. The structural peculiarities of nervous tissue—its histology. 2ndly. Its chemical properties. 3rdly. Its electro-motory characters. We shall then pass to the consideration of what is known of the functions of nervous-tissue, and as yet this part of the subject may be considered under the laws of excitation, of irritability, and of conduction. Lastly, from the data ascertained in the examination of these laws, an ingenious theory to explain the mechanism of nervous excitation and conduction has been proposed by Professor Pflüger. An exposition of this theory will conclude the article.

I. *Histology.*—On the structure of nerve-elements—fibres and cells—little need be said. The nerve-fibre consists of an external membrane of the white nerve-marrow, and of the central axis cylinder. Regarding the axis cylinder three opinions are held—1. That it is a more or less solid fibre present in living nerves (Kölliker). 2. That it is a tubular structure (Remak). 3. That it is a post-mortem product, due to a species of coagulation, or rather to a decomposition of the white marrow (Funke). The latter view appears very improbable. The axis cylinder is composed of a material similar to, and continuous with, the contents of the nerve-cell. This we have seen beautifully displayed in the spinal ganglia of the skate. In carmine preparations the axis cylinder takes on the carmine colour (Lister and Turner), in the same manner as the nerve-cell. As to the tubular nature of the axis cylinder, we once thought we had convinced ourselves of this in the anterior nerve-roots of the ox. But on examining our preparations, in which the axis cylinder gave the appearance of having a double contour with a finer microscope, the tubular appearance could not be made out, and the axis cylinder, under a very high power, appeared to consist of a finely-granular homogeneous substance. We have now little doubt that the axis cylinder is a solid or semi-solid fibre, continuous with the contents of the nerve-cell. A short time ago, Stilling gave a most extraordinary account of the nerve-fibre and cell, describing in both a system of fine anastomosing tubules, which not only connected together every part of the fibre and cell, but even united contiguous fibres. Funke, who has examined the subject carefully, has convinced himself of the falsity of Stilling's observations; and Mr. Lockhart Clarke has shown that in his observations on the white substance of the fibres, Stilling has been deceived by appearances due to the artificial mode of making his preparations.

The particular kind of fibre which occurs in almost all grey nerves, known as the Remakian fibre, has been a subject of much dispute. Remak, the discoverer of these fibres, considers them to be nervous, as does also Henle and Hyrtl. On the other hand, Valentin and Kölliker hold that they belong to the class of connective tissues. Further observations are wanted to decide this point. The nerve cell, regarded functionally, is the most interesting anatomical element in the body. Numerous observations on the central organs of the nervous system, prove that all nerve-fibres either originate or terminate centrally in nerve-cells. An a-polar nerve-cell or a cell without processes, is now known to have no existence, and Wagner and others are inclined to doubt the existence of uni-polar nerve-cells; so that nerve-cells are mostly bi-polar or multi-polar. In structure they are simple, possessing a cell wall with contents more or less granular in character, often containing pigment granules, a nucleus, and a nucleolus. Cells vary in size and in form; and a Russian observer, Jacobowitsch, has recently asserted that, according to size and form, cells may be distinguished functionally into motor, sensitive, and sympathetic cells. Were this true it would be exceedingly interesting; but we feel convinced, from our own observations, that just as in gland-cells so in those of nerve organs, no morphological peculiarity exists by which we can determine their functional peculiarity. Indeed, so far from being able to determine specific function, histologists have not yet discovered any certain means of determining whether a cell is nervous, or whether it belongs to the connective-tissue elements now shown to enter largely into the composition of nerve organs, more especially by Bidder and others, of the Dorpat school. Physiologically important is the fact that a nerve-cell not only receives and originates nerve-fibres, but also sends off processes which serve to connect it with other nerve-cells.

Space does not permit of our referring at any length to the peripheral relations of nerve-fibres, but the conclusion to which much laborious inquiry on this difficult subject has led is, that the nerve-fibres have *free* ends. The idea which Valentin originated, that at the periphery nerves formed plexuses, and returned again in the nerve-trunks, is almost entirely disproved. The fibres often *do* form plexuses, but these are not terminal, since out of them may be traced fine, marrowless fibres, which do not form loops, but have free endings. This mode of ending is most evident in those curious structures found on the nerves of the palm of hand, sole of foot, glans penis, &c., the corpuscles of Pacini. At the upper part of the central cavity of a Pacinian corpuscle, the pale, marrowless fibre, almost like an axis cylinder, is seen to split into two or three branches, which end in free, lightly-granulated knobs (Kölliker*).

In the muscles of the lower animals the observations of Wagner, Kölliker, and Luschka prove that nerve-fibres terminate in free extremities; and in Kölliker's opinion, it is probable that in man and the higher vertebrata the muscular nerves, after dividing and anastomosing, end *free* (Kölliker†). A similar mode of termination is found in all the organs of special sense; but in these the nerves undergo those peculiar peripheral modifications which are necessary to receive the particular impression to be conveyed to the nerve-centre. Much histological research has been spent upon these wonderful terminal structures. Here we can only roughly indicate their nature.

The nerves which convey tactile impressions, as also those of taste, are comparatively simple in their terminal structures. The skin-nerves enter the papillæ, to end in the peculiar bodies termed touch corpuscles, by forming knobs (Krause‡). The nerves of the tongue terminate in the points of the papillæ by forming club-like, blunted or pointed free ends (Fixsen§). Billroth supposes that the fibres are connected with the non-ciliated epithelial cells of the larger papillæ of the tongue. A similar mode of termination has been assigned to the

* Gewebelehre des Menschen, p. 333. 1859.
† Kölliker, loc. cit., p. 194.
‡ Henle und Meissner's Bericht, p. 81. 1858.
§ Kölliker, loc. cit., p. 370.

fibres of the olfactorius (Eckhard, Ecker, Schultze, Kölliker). Mr. Lockhart Clarke, our distinguished English histologist, has lately examined the nasal mucous membrane, but he has not seen a direct communication of nerve-fibres and epithelial cells; he, however, supposes there may be an indirect connexion.* In the retina the fibres of the optic nerve terminate through the medium of a nerve-cell, in those remarkable structures the rods and cones (H. Müller, Kölliker).

In the ampullæ and vestibule of the internal ear, Schultze and Kölliker have traced the fibres of the auditory nerve into the layer of epithelium which invests these parts. According to the former observer, just as in the nasal mucous membrane, so here there are two forms of epithelial cells—one cylindrical or conical and yellowish, the other pale, spindle-shaped, with a rod-like appendage on their external end, and a fine non-varicose filament on their internal extremity. With these latter thread-like cells Schultze supposes the nerve-fibres are connected, though in no instance has a direct connexion been seen. Kölliker thinks this termination probable. On the other hand, in the cochlea the peripheral relations of the nerve-fibres would seem to resemble those of the optic nerve in the retina, in being connected with those peculiar structures the rods of Corti (Kölliker), and with cells in their neighbourhood (Schultze†).

Another instance of the free mode of termination of nerve-fibres, is found in the electrical organs of the Silurus, Gymnotus, and Torpedo. The so-called electrical plates are really nothing else than peculiar terminal appendages of nerve-fibres into which the latter can be traced either directly, as in the Silurus, or after the formation of a fine anastomosing network, as in the Gymnotus and Torpedo.

Sufficient has been said to indicate the structure of nervous elements, and to show their general relations in the nerve centres and at the periphery. Numerous monographs have recently been written upon the special anatomy of the various nervous centres; and although upon matters of detail wide differences exist, there are certain laws of structure upon which all are agreed. Physiologically, by far the most important is, that the ganglionic cell is the central origin or terminus of every nerve-fibre in the body.

II. Into the *chemical properties* of nerve elements there are few temptations to lead us to enter, seeing that our knowledge on this subject is very imperfect, and that what we do know has as yet so little physiological importance attached to it, that it does not seem necessary that we should trouble the reader with what would be but dry detail. We shall therefore pass at once to the consideration of the physical properties of nervous matter.

III. *Electro-motory properties of nerves.*—Whether we consider electrical forces merely as accidental accompaniments of physiological activity

* Ueber den Bau des Bulbus Olfactorius und der Geruchsschleimhaut. (Reprint from Zeitschrift f. wissen. Zool.)

† We would here refer the reader to the Micrological Reports in previous numbers of this Review for the accounts of various histological researches into the mode of termination of nerves.

or as the conditioning agents of such, a study of what has been ascertained regarding them is extremely interesting. In the researches of Du Bois-Reymond, we have a demonstration of the extent to which an organic structure can be subjected to physical examination. A perusal of the whole or of a part of Du Bois-Reymond's great work on animal electricity will produce in the mind of the reader a feeling of amazement at the amount of literary research and of learning, at the critical acuteness shown in separating the true from the false, the apparent from the real, and at the mechanical dexterity and physical ingenuity exhibited in devising the experiments on which all that we know of the electro-motory properties of nerves is founded. His work may be said to constitute the first era in animal electricity, for before its publication all was doubt and obscurity.*

Although this part of our subject is not novel, we propose to give our reader a sketch of what has been done by Du Bois-Reymond in order that he may the more readily comprehend the researches of the physiologist whose work on electrotonus is now before us. Whilst Du Bois' work must be regarded as the foundation of the science of animal electricity, that of Pflüger constitutes a most important, perhaps *the* most important addition to the superstructure.

The instrument by which the presence of an electric current in a nerve is ascertained is termed a galvanometer. It consists of a coil of wires, within which an astatic magnet needle is suspended. When an electric current traverses the wire, a variation of the needle is observed; the *amount* of variation depending upon the intensity, and the *direction* of the variation depending upon the direction, of the current. Du Bois-Reymond contrived an arrangement by which a piece of fresh nerve could have its electric relations tested by a sensitive galvanometer, and the following are the results of his investigations upon a piece of nerve removed from a living animal.

Every piece of fresh excitable nerve is the source of electric currents, as may be proved by placing the two ends of the wire of a galvanometer in contact with it, but it is found that the position of the nerve is of great importance in the result produced. 1. If the nerve be placed so that the wires touch two points of its surface at an equal distance from the central point (equator) of the nerve, no deviation of the needle takes place. 2. When the wires are placed so that one touches the transverse section of the nerve, and the other a point on its longitudinal surface, a strong variation of the needle ensues, the current going from the external longitudinal surface to the transverse. The external surface is therefore positive (+), towards the transverse, which is negative (—), in its electric relations. If both wires be placed on the surface of a nerve, one nearer the centre point (equator) than the other, a weak current is produced, going from the point nearest the equator towards the point at a distance from it. A point near the centre point of a nerve is therefore + towards a point at a greater distance from it. In muscle the same relations are found to hold

* See Müller's Physiology, 1840.

in two points of a transverse section—*i.e.*, the one nearest the central axis is + towards the one nearer the periphery.

From these few experiments we learn, 1st, that white nervous matter in the fresh state possesses an electro-motive power. 2. That this electro-motive power acts according to a definite law, which may be shortly stated as the law of antagonism of the longitudinal and transverse sections, the former being positive, the latter negative. In the longitudinal section, every point near the equator is positive towards a point at a distance from it, and in the transverse section an analogous relation probably exists, although it has not been proved by experiment.

The theory of Du Bois to explain these effects may be thus stated:—Every minute particle of nerve acts according to the same law as the whole nerve. A nerve consists of a number of di-polar molecules, one-half presenting positive, the other half negative electric properties. These di-polar molecules are arranged in couplets, and so that the positive poles are turned towards each other, their negative poles to the extremities of the nerve. Each couplet of molecules produces currents going from the central positive poles to the negative poles. The accompanying figure will render this clear. A nerve is always in the condition of a closed current, and when the extremities of the wires of the galvanometer are applied, a part merely of the current is abduced.

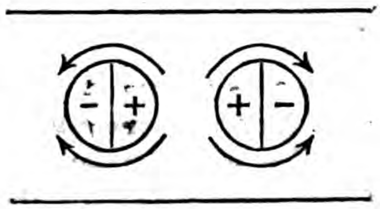

Fig. 1.

The facts and theory just mentioned apply to a nerve when it is in a state of rest. When a constant galvanic current is made to pass through part of a nerve, an electrical change takes place, to which the name electrotonus is applied. Supposing that whilst a piece of nerve is placed on the cushions of a galvanometer, and a constant deflection of the needle is produced, a constant current be passed through this piece of nerve, in the same direction as the nerve current, then an increase of the deflection of the needle is observed. This is termed the positive phase of electrotonus. If a constant current is applied in a direction opposite to that of the nervous current, the deflection of the needle is decreased, and this is termed the negative phase of electrotonus. If a nerve is placed with its equatorial point exactly between the cushions, so that no deflection is produced, and if a constant current be now applied to the nerve, a deflection of the needle will take place, its direction being determined by that of the applied current. Such are the principal facts of electrotonus, and the explanation of them furnished by Du Bois is the following. When any portion of the length of a nerve is traversed by an electric current, besides the effect of the original nerve current, a new electro-motive action takes place, which has the same direction as the applied current. This new current is *added* to the original nerve current if the direction is the same, but *subtracted* from it, if the direction is opposite. Thus is explained the increase and decrease of deflection in the needle when the applied current is sent in the same or in the opposite direction to that of the nerve current. The effect of the applied or exciting current is found to depend upon various circumstances, such as the distance between the point of its application, and the part of nerve included in the circuit of the galvanometer, on its intensity, and on the extent of nerve which it traverses. If in the natural quiet state of a nerve we suppose the electro-motive molecules to be arranged with their similar poles turned to each other (see fig. 1), some change in their arrangement must occur in the electrotonic state. Du Bois has rendered it probable that the polarization of a nerve is an electrolytic process. The molecules arrange themselves so that their negative poles are turned to the point at which the current enters the nerve, the positive poles to the point at which the current leaves the nerve. We should then have, instead of the arrangement in fig. 1, one in which the opposite poles are turned to each other, just as they are in Volta's pile (see fig. 2). To effect this change a rotation of 180° is required in some of the molecules. This pile-like arrangement must occur not only within the electrodes, but external to them, as the nerve is electro-motory in the sense of the exciting current throughout its length. But the question occurs, if all the molecules assume the arrangement of a perfect Voltaic pile, how is the original nerve-current (which is also present in the electrotonic state) produced? Du Bois answers this difficulty by assuming that the pile-like polarization is more or less imperfect; that is, some do not rotate 180°, but remain more or less in their original position, and this the more the greater the distance from the polarizing electrodes. When the exciting current is withdrawn, the molecules again assume their original position (fig. 1).

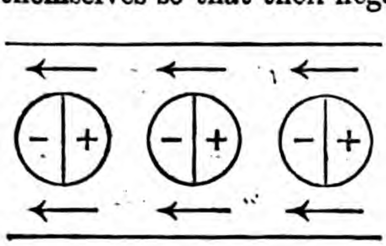

Fig. 2.

Is this condition of electrotonus the electrical state of an active nerve? There are analogies, but one decisive fact proves that a difference exists between the electrotonic state and the physiological condition of the active nerve. When a nerve with an attached muscle is irritated, we have in the contraction of the latter a proof of the physiological activity of the former. Now, when a continuous galvanic current is applied to the nerve of a muscle, the latter contracts on opening and on closing the circuit, but not during the interval, in which the nerve is in a state of electrotonus. If, instead of a continuous current, a series of interrupted currents be applied in very rapid succession, the contractions follow each other so quickly that the muscle has no time to regain its state of relaxation, and it remains in a state of continued contraction—tetanus. If the effect of such an interrupted current be examined by the galvanometer, the result will be found to be different from that of the continuous current. With the continued current, the positive phase of electrotonus is denoted by an increased, the negative phase by a diminished, deflection of the needle, which are termed positive and negative variations. The positive variation is greater than the negative. With an interrupted current, positive and negative variations of the needle likewise occur; but the positive phase appears less than the negative, and under certain circumstances is not marked, or is expressed by a negative variation instead. This would lead to the supposition that in tetanising a nerve, the original nerve-current undergoes a negative variation, which is added to the negative but subtracted from the positive phase of electrotonus. In circumstances in which no original current is present (e.g., when the nerve is placed in the galvanometer, so that its equator is central), this hypothetical negative variation fails likewise. The experiment which renders probable the idea of a negative variation of the nerve-current taking place when the nerve is in an active state, is that of passing alternating currents rapidly through a portion of nerve, so that every current is opposite in direction from that which preceded it. Now, if the electrotonic increase was exactly the same in the positive and negative phases, there would be no effect on the needle—equal forces acting in both directions produce rest. But, as the variation in the positive phase is greater than that of the negative, there ought to be a slight deflection to the positive side. Notwithstanding this, in electrotonus with alternating currents, the deviation is in a marked degree negative. In the tetanised nerve the characteristic phenomenon is expressed by a *negative variation* of the current. If the tetanised nerve is identical with the physiologically active nerve (and we are entitled to assume that it is), then a negative variation of the natural nerve-current is the physical expression of those further molecular changes which take place in a nerve when it produces a motion or a sensation.

Such is a brief, but we venture to hope a comprehensive, account of the discoveries of Du Bois-Reymond. Acquainted with them, the reader is in a condition to understand and to appreciate the researches of Pflüger, whose work on Electrotonus is now before us.

This book being purely physical in character, is dry and hard to read. In many parts it is critical. With such we shall not trouble the reader, but will content us with attempting to give a clear account of those researches on which the author founds a new theory of the molecular mechanism of an active nerve. Hitherto we have spoken of the physical properties of nerves manifested in their quiet and in their active condition. We must now examine the conditions under, and the means through which a nerve is rendered active; in other words, the laws of nerve irritation.

The experiments upon nerve excitation are all made with the limb of the frog—that "silent friend of the physiologist." The sciatic nerve is carefully dissected out, and may be subjected to any kind of excitation, the response to which is manifested by a muscular contraction, which can be accurately measured. In this way an estimate can be formed of the amount or degree of nerve excitation.

When the nerve of the prepared frog-limb is submitted in part of its length to a constant galvanic current, a muscular contraction follows on closing and on opening the circuit. In the interval the muscle is in a state of relaxation and remains so. If in the interval the current be *gradually* increased or diminished, no contraction takes place; but if the current be *suddenly* changed in intensity, contraction follows. Variations of intensity at a certain velocity produce tetanus.

These facts have been long known, but variously expressed. John Müller drew from them the conclusion, that "every change in the static condition of the electric fluid seems to become a cause of excitement to the nervous principle."* By Du Bois-Reymond the law of nerve excitation by the electric current is thus stated: "The excitation of a nerve is not produced by a current of an unvarying magnitude, but by changes of this magnitude from one moment to another." Thus, on closing a current we produce in the nerve a sudden elevation of electric tension, a positive variation of the current; on opening the current, a sudden depression of tension, a negative variation is produced. Both cause contraction of the muscle because each fulfils the conditions of nerve excitation by changing at a certain velocity the internal molecular constitution of the nerve.

Whilst such is the rule, exceptions render some modification of it necessary. Du Bois observed that very powerful constant currents produce a series of contractions which continue during the application of the current, and even after it. He ascribed these contractions to a destructive influence of the current, and to consequent electrolysis of the nerve.† To this subject Pflüger has given particular attention, and his discovery of some entirely new facts has rendered a modification of the above law necessary. Pflüger examined the effect of constant currents of every strength, beginning with the weakest possible, and going on to the strongest. He found that the weakest currents possible do not produce any other effect than a contraction on closing the circuit; with stronger currents, but so weak as not materially to deflect the needle of a galvanometer, tetanus is produced; but, strange to say, when the strength of the current is increased beyond a certain degree, the tetanus no longer occurs. It would appear that constant galvanic currents within a certain range of intensity, and that a low one, have the faculty of producing changes in the molecular constitution of the nerve, which lead to a continuous contraction of the muscle. A consideration of these new facts has led Pflüger to propose the following modification of Du Bois-Reymond's law of nerve excitation:

"Although the excitation of a nerve by a constant current depends above all other things on variations in the density of the current traversing the nerves, yet the latter react upon the current in continuous quantity. Whilst the latter dependence is of such a character that the function attains a maximum, in order then again to diminish, yet in the meantime the more exact law of the other relation is unknown." (p. 453.)

The further consideration of this subject leads us from these general expressions of the conditions of nerve excitation to the examination of the precise effects of electric currents upon the functions of the motory

* Müller's Physiology (Baly's edition), vol. i. p. 672.
† Du Bois-Reymond, Band i. p. 258.

nerves—in other words, to the study of the law of muscular contraction. At first sight such a study would seem far removed from the object of all these inquiries—a knowledge of the internal mechanism of an active nerve. But this is not the case, as it will appear that the theory which Pflüger has proposed to explain the operation of the active nerve is founded upon a study of the laws of muscular contraction. Until the appearance of Pflüger's writings, the greatest confusion prevailed, and no law could be said to exist. The difficulties of the subject being great, the merit of having substituted order and law for confusion and error is the more signal.

It has been already stated that a muscle contracts on opening and on closing a constant current. Were this invariably true, the law of contraction would be a very simple one; but it has long been known to physical inquirers that, although, generally speaking, contraction occurs *either* on opening or on closing the circuit, a constancy of the occurrence does not exist. Amongst those who have tried to determine the circumstances which cause the presence or absence of contraction, may be mentioned Pfaff, Ritter, and Nobili. Into their observations we shall not enter. Suffice it to say that, although they numbered amongst the conditioning circumstances of contraction, first, the degree of irritability of the nerve, and secondly, the direction of the current, they omitted to take into account one most important element in the question—viz., the strength of the current employed. Heidenhain and Pflüger simultaneously discovered that the strength of the current is most material to the result. The observations of Pflüger have been made with more refined instruments than those of Heidenhain, and are therefore more reliable. At p. 454 of these researches, Pflüger states that the law of contraction is, in the fresh nerve, a function of the strength of the current, and he distinguishes three degrees of strength. The following table expresses Pflüger's law of contraction for the fresh nerve:

Strength of current.	Centripetal current.	Centrifugal current.
With a weak current	Closing—contraction ...	Closing—contraction.
	Opening—rest ...	Opening—rest.
Moderately strong current	Closing—contraction ...	Closing—contraction.
	Opening—contraction...	Opening—contraction.
With strong current	Closing—rest ...	Closing—contraction.
	Opening—contraction...	Opening—Weak contraction (?).

It need scarcely be said that by centripetal is meant a current directed *from* the muscle; by centrifugal, a current directed *towards* the muscle. The table requires no comment; and, having ourselves witnessed the experiments of Pflüger in the University of Berlin, we can bear testimony to the accuracy of the facts he has tabulated. His experiments have been repeated and confirmed by Bezold, Rosenthal, and Funke.

The explanation of these facts, and the theory of the law of contraction, is founded upon a knowledge of the changes of irritability produced in a nerve by the constant current. When such a current is passed along a portion of nerve, a change of irritability takes place both within and for some distance to the outside of the electrodes. In the neighbourhood of the negative electrode there is produced an increase, in the neighbourhood of the positive electrode a decrease of irritability, and this proportionate to the strength of the current—i.e., with the strength of the latter the degree of increased irritability at the negative pole and decreased irritability at the positive pole increases. Again, the relative extent of the two zones of irritability varies, so that with weak currents almost the whole of the intra-polar tract is in a condition of exalted irritability. Pflüger designates the condition of increased irritability which occurs at the negative electrode (*Kathode*) of the constant current, "Katelectrotonus;" that of depressed irritability occurring at the positive electrode (*Anode*) he terms "Anelectrotonus." It must further be premised that the conducting power of a nerve is altered in these circumstances, and in such a manner that the anelectrotonic part is rendered unfavourable, the katelectrotonic part is greatly favourable for conducting irritation. Further, Pflüger discovered that "one and the same irritant applied at two different spots of a nerve does not irritate the muscle in equal degree, but so that the irritation acts more powerfully the further removed the spot of application is from the muscle." (p. 141.) To all of these subjects we shall have again to refer. What has been said is sufficient to enable the reader to understand the explanation of the law of contraction. It is given in Pflüger's words, as follows: "A given tract of nerve is irritated by the constitution of katelectrotonus and the disappearance of anelectrotonus, but not by the disappearance of katelectrotonus and the constitution of anelectrotonus." (p. 456.) In other words, this rule implies that in passing a constant current along a portion of nerve, the irritation on closing the circuit takes place at the negative electrode, on breaking the circuit at the positive electrode. Viewed by the light of this law (for the truth of which we shall afterwards give Pflüger's proofs), the phenomena of the law of contraction are easily explained. Let us take first the centripetal current (see preceding table). With a weak current a contraction takes place on closing the circuit, which afterwards with stronger currents disappears. Why? Irritation of the nerve is produced by katelectrotonus of the portion most distant from the muscle and above the anelectrotonic part of the nerve. With a certain intensity of current the latter part of the nerve becomes so depressed in irritability that it fails to conduct the irritation above it (in the region of katelectrotonus) to the muscle. The opening-contraction occurs with a stronger current, and is caused by the disappearance of anelectrotonus when the circuit is opened. The reason that it occurs later than the closing contraction is that, in conformity with the law above given, a point of nerve is more irritable the greater its distance from the muscle—besides, the disappearance of anelectrotonus is found by Pflüger to be in itself a less powerful stimulant than the production of katelectrotonus. The same rules as easily explain the phenomena of the centrifugal current. With a weak current contraction occurs first on closing the circuit, because the katelectrotonic portion is next the muscle. The opening contraction *may* sometimes occur *before* the closing contraction, because the irritation (caused by the cessation of anelectrotonus) acts upon a higher and more irritable portion of nerve. That the opening contraction should diminish and disappear before the closing contraction is thus explained. On opening the circuit the irritation at the anode must pass along the intra-polar portion of nerve and the part which has been in the katelectrotonic state. Now this portion of the nerve, on opening the circuit, falls at once into a state of depressed conducting power, and is unable to convey to the muscle the irritation produced by the disappearance of anelectrotonus.

Such are the facts, and such the explanation of the phenomena which follow electric irritation of the motory nerves. Other agents have been experimentally used as excitants; the various chemical, thermal, and mechanical modes of irritation have all been examined in relation to their effects on motor nerve-fibres. It would lead us beyond the limits of our space to enter upon the already extensive literature of these subjects, but we have recorded underneath the most important recent contributions.* One remark only may be made; and as it affects the *modus operandi* of all the three classes of excitants mentioned, it is important. Prof. C. Eckhard, in his essay on the 'Chemical Irritation of Nerve,' advances the theory that chemical agents operate by producing immediate death of the part of nerve to which they are applied. It would seem, we think, much more consistent with experimental facts to suppose that the essential source of all kinds of nerve-irritation consists, not in an entire and immediate suspension, but in a certain velocity of molecular movement.

Hitherto we have spoken of the law of excitation in the motor nerve only. How is it with the sensitive nerve? No morphological difference has yet been observed between the two kinds of fibre. Is there any difference in the phenomena attending their excitation? To serve physiological purposes the *modes* of excitation are different, and the results are so also; but the phenomena of action, so far as the fibres are concerned, appear to be the same. It is obviously a much more difficult inquiry than in the case of the motor nerve, when we have in the muscular contraction an objective result capable of being

* *Chemical Irritation of Nerves.*—C. Eckhard, Die Chemische Reizung der motorischen Frosch nerven: Zeitschrift f. rat. Med., N.F., Band i. p. 303. Kuehne, Ueber die directe und indirecte Muskelreizung mittelst Chem. Agent.: Arch. f. Anat. und Phys., 1859, p. 1. Kölliker, Ueber die Vitalität der Nervenreize der Frosches.: Verh. der phys. med. Ges. zu Würzburg, Band vii. p. 145. Harless, Die Muskelkrämpfe bei der Nerventrocknung.: Zeitschr. f. rat. Med., Band vii. p. 219.

Thermal Irritation of Nerves.—Eckhard, Ueber die Einwirkung der Temperaturen des Wassers auf die motorischen Nerven des Frosches: Zeitschr. f. rat. Med., Band x. p. 165. Schiff, Lehrb. d. Phys., Band i. p. 99. Rosenthal, Ueber den Einfluss höherer Temperaturen auf motorische Nerven.

Mechanical Irritation of Nerves.—Harless, Ueber Muskelkrämpfe bei der Nerventrocknung: Zeitschr. f. rat. Med., Band vii. p. 219. Heidenhain, in his Physiologischen Studien, Berlin, 1856, p. 129; also in a paper, Ueber ein mechanisch Tetanomotor für Vivisectionen: Moleschott's Unters. zur Naturl. des Menschen, Band iv. p. 124.

as accurately measured. An interesting experiment by Pflüger* renders it probable that in sensitive nerves the law of excitation is analogous to that of the motor nerve. A frog was poisoned with strychnine in order to increase its susceptibility of reflex movement, and so fastened as to escape disturbance of every kind. Previously the sciatic nerve had been isolated, and all the textures of the upper part of the limb divided so that no connexion existed between upper and lower part of the limb except through the nerve. The electrodes being now placed on the nerve, a strong centrifugal current was applied. On closing the circuit, contraction of the limb only occurred; on opening it, *no* contraction of the limb, but strong reflex convulsions of the body of the frog. With the centripetal current the results were reversed, reflex convulsions being present on closing the circuit, but absent on opening it. These facts would seem very clearly to prove that in the sensitive nerve the causes of irritation are the same as in the motor—viz., the constitution of katelectrotonus and the disappearance of anelectrotonus. Of course, as the results are manifested in opposite directions, we must expect (what the above beautiful experiment proves) the inverse sequence of phenomena with the two directions of current—that is to say, in the sensitive nerve we should expect the same phenomena with the centripetal current, as in the case of the motor nerve would occur with the centrifugal current, and *vice versâ*.

The capability of responding to electrical and other external sources of irritation implies the possession of a property which in its variations under natural and unnatural conditions affords a subject for careful study. We have spoken of the laws of excitation. There remain to be considered the laws of irritability. These may be examined in the living nerve, and in the dying nerve. They may also be examined in relation to the effect which the various external excitants produce upon the irritability of the fresh nerve-fibre. As with the law of excitation, so here the only objective sign by which the irritability of the nerve-fibre can be measured is the degree of muscular contraction which follows the application of an excitant under given circumstances.

In the living nerve three conditions are requisite for the maintenance of its irritability: 1. Nutrition by arterial blood. 2. Connexion with the central organ. 3. Interruption of rest by activity. Much research has been bestowed on these topics, yet many questions remain unanswered. It is yet undetermined whether, in the regeneration of nerves, the old nerve-sheaths fill again (Schiff, Lent), or whether entirely new fibres are formed (Waller and Bruch). On cutting through a nerve, the fatty degeneration which occurs in the peripheral portion is probably due to the want of some influence (nature unknown) coming from the nerve-cell (Waller), or from a part close to the anterior roots (Schiff). Here, as in the case of all other tissues, activity is necessary

* Pflüger, Vorläufige Mittheilung über das Gesetz der electrischen Empfindungen: Allgem. medic. Centralzeitung, 1859, No. 69; also Henle und Meissner's Bericht für 1859, p. 454.

for the maintenance of function. The reason why, we cannot exactly tell, though we may surmise that the peculiar chemical changes which accompany physiological action are amongst the requisite conditions for adequate nutrition. The most important contributors to our knowledge of the normal conditions of nervous irritability are J. Müller, Waller, Schiff, Lent, Kuettner, Bruch, and Brown-Séquard.

To ascertain the changes of irritability in the dying nerve, it is requisite, first, to ascertain the degree of irritability possessed by each part in the course of an intact living nerve. It was till lately supposed that the irritability of a point on a fresh nerve is greater the nearer the point is to the muscle in which the nerve is distributed. Budge was the first to guess the true relations (1852), and Pflüger, by the nicest possible physical experiments, has made out the following law: "One and the same irritant, which is applied successively to two different points of a nerve, does not irritate the muscle in the same degree, but the irritation which is applied at the greater distance from the muscle acts the more powerfully." (p. 141.) Of various hypotheses which might explain this law, Pflüger thinks the most probable is that "the excitation increases in an avalanche-like manner (*lawinenartig*), and this is the more considerable the greater the portion of nerve over which it travels." (p. 155.)

Until very lately it was supposed that, in a dying nerve (i.e., in a nerve from which nutrition and the cerebral nervous influence is cut off), the irritability gradually sank from its normal state to 0. By the interesting researches of Rosenthal* (a pupil of Du Bois-Reymond's) it has been established that at every point of the nerve the irritability *increases* considerably from the moment of death, and then falls to zero; and this is the case, whether the nerve is cut out, or remains in connexion with the spinal cord.

Further, the manner in which the irritability rises and falls is not the same at every point of the nerve. The total course of these changes occupies the less time the further removed the observed portion is from the muscle. If the nerve is cut through above the point at which the observation of its irritability is made, the course of these changes is hastened the nearer the section is to the point under observation. The effect of section of the nerve upon its irritability has been specially studied by Harless,† and some extraordinary hypotheses have been framed by the same observer, into which we cannot here enter. Rosenthal, in connexion with A. von Bezold, has made observations upon the law of muscular contraction in the dying nerve; and the results of their inquiries remove all doubt as to the course of the changes of irritability after death.‡ To save space, and for the sake of clearness, we have arranged their results in the following table:—

* J. Rosenthal, Ueber das sogenannte Vallischegesetz: Allgem. medic. Centralzeitung, 1859, No. 16, Henle und Meissner's Bericht für 1858, p. 436.

† Moleculare Vorgänge in der Nervensubstanz: Bericht für 1858; Funke.

‡ Ueber das Gesetz der Zuckungen: Archiv für Anat. und Physiol., 1859; also Henle und Meissner's Bericht, 1859, p. 431.

State of the nerve.	Centripetal current.		Centrifugal current.
In the fresh nerve	Closing—contraction	...	Closing—contraction.
	Opening—rest	...	Opening—rest.
At a later period.	Closing—contraction	...	Closing—contraction.
	Opening—contraction	...	Opening—contraction.
Still later . . .	Closing—rest	...	Closing—contraction.
	Opening—contraction	...	Opening—rest.

The explanation of this table is not difficult. Its meaning may be expressed by the following diagram, which we copy from Funke's work:

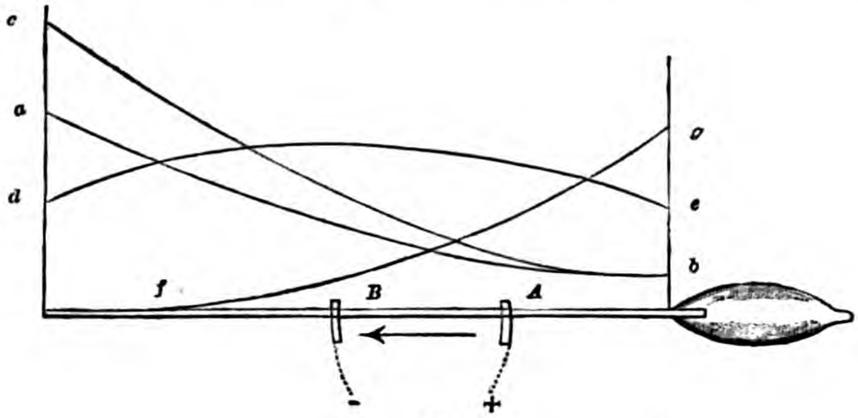

Fig. 3.

Suppose that a weak centripetal current is passed from A to B in a perfectly fresh nerve with attached muscle. The irritability of the fresh nerve is expressed, according to Pflüger, by the line *a b*. Contraction occurs on closing the circuit, because at the negative pole, B, the nerve is naturally more irritable than at the positive pole, A. After a short interval this closing contraction is more powerful, because the irritability of the nerve has increased, and may now be expressed by the curve *b c*. After another short interval, a weak opening contraction occurs; and soon after both contractions are equal in strength. The irritability of the nerve is now diminishing, at least towards the central end of the nerve. It is expressed by the line *d e*. At a later period, contraction occurs on opening the circuit only. The irritability is now low at B, but considerable at A, and is expressed by the curve *f g*. With the centrifugal current the phenomena are explained exactly in the same way.

In speaking of the excitation of nerves, it was necessary to assume some propositions regarding irritability of nervous fibre (see p. 12). The greater part of Pflüger's work is occupied with the precise determination and proof of the state of irritability of the different parts of nerve during and subsequent to the application of a constant galvanic current. In other words, Pflüger has endeavoured to ascertain special modifications of irritability during the electrotonic state. We shall endeavour briefly to indicate the chief conclusions to which his exact and almost mathematical experiments have led.

The general law of irritability, according to Pflüger, is as follows.

When a centripetal or centrifugal constant current is passed along a nerve, a change of irritability is produced, not only in the portion traversed by the current, but also before and behind it; and so that, in the neighbourhood of the negative electrode (*kathode*) there is exaltation; in the neighbourhood of the positive electrode (*anode*), there is a depression of the irritability. The former condition, which Pflüger designates katelectrotonus, takes place in the extrapolar part of nerves *before* the current, and also in that part of the intrapolar tract next to the kathode; the latter condition (anelectrotonus) takes place in the extrapolar tract immediately *behind* the current, and in that portion of the intrapolar tract next the anode. The extrapolar anelectrotonus and katelectrotonus are termed *centripetal* when they extend from the electrode towards the central end of the nerve, centrifugal when they extend towards the muscle. The point at which the two zones of irritability meet is called by Pflüger the point of indifference. Various circumstances influence the intensity of anelectro- and katelectrotonus: 1, the strength of the constant current; 2, the distance of the point of nerve, the irritability of which is tested, from the electrodes; 3, the length of the tract of nerve traversed by the current; 4, the time.

The effect of the first two of these conditions is illustrated in Funke's work by a diagram, which we take the liberty of transferring to our

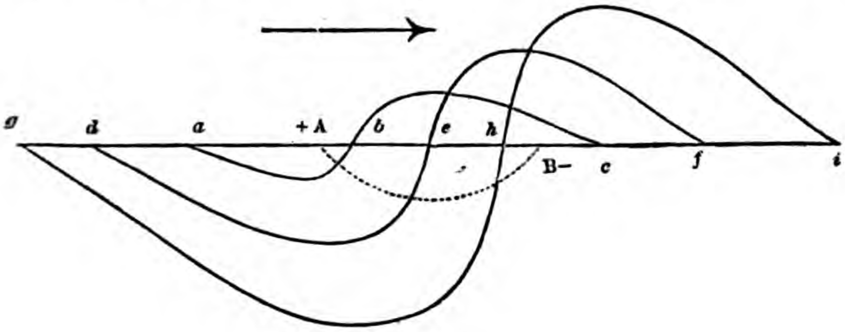

Fig. 4.

pages. Supposing the straight line to represent a nerve, and the points A and B to be the positive and negative electrodes of a constant current, placed so that the tract A B is traversed in the direction of the arrow, centripetal or centrifugal, as the case may be. If we now examine the irritability of this nerve when weak, moderate and strong currents are passed, and represent the effects by ordinates, we should obtain with weak currents a curve such as *a b c*, with medium currents the curve *d e f*, and with strong currents the curve *g h i*. Let us examine the curve *a b c*. The change of irritability in the extrapolar portions *a* A and B *c*, and in the intrapolar portion A B, consists in an elevation of irritability over the part *b c*, and a depression of irritability over the part *a b*. The point *b*, at which the curve bisects the absciss is the point of indifference at which the two zones of irritability meet. If we compare these three curves, it appears that, with weak currents, almost the whole of the intrapolar tract has its irritability increased;

with medium currents, the zones of altered irritability extend over a greater tract of nerve, are greater in intensity, and are almost equally divided in the intrapolar tract. With a still stronger current, the intensity and the extent of altered irritability is greater, and the zones are distributed so that the point of indifference is nearer the kathode, and the intrapolar tract is almost entirely in a state of depressed irritability.

To give some idea of the exactitude of Pflüger's mode of investigation, we shall extract his experimental observations on one only of the data on which the above general law of irritability is founded—viz., increased irritability before the centripetal current (centripetal extrapolar katelectrotonus).

A frog's limb being prepared, the electrodes are placed upon the nerve just above the gastrocnemius muscle, and a centripetal constant current is applied, of such strength as to produce a contraction of the muscle on closing. Above this, another current, centrifugal in direction, is applied, in order to irritate the portion of nerve under observation. The size of contraction being measured by a myographion, a comparison of the effect of the irritating current, with and without the polarizing current, gives the data expressed in the following table:

No. of the contraction.	Contraction in the non-polarized nerve.	No. of the contraction.	Contraction in the polarized nerve.
1.	0·9 mm.	2.	5·8 mm.
3.	2·3 „	4.	6·2 „
13.	0·9 „	14.	5·0 „
31.	0·5 „	32.	3·2 „

This table shows that, when a nerve is traversed by a centripetal current, and the portion of nerve before this current is irritated by another current, a much greater contraction of muscle ensues than when the nerve is not polarized. Another series of experiments is given, in which the irritating current was centripetal in direction, and the following are some of the results:

No. of the contraction.	Contraction in the non-polarized nerve.	No. of the contraction.	Contraction in the polarized nerve.
1.	1·1 mm.	2.	9·7 mm.
3.	1·2 „	4.	9·7 „
5.	1·9 „	6.	10·0 „
25.	0·9 „	26.	9·4 „
33.	0·9 „	34.	6·8 „

It thus appears, that with the use of a centripetal irritating current, the contraction which ensues when the nerve is polarized is greater than in the non-polarized nerve. Pflüger made use of chemical irritation, using a concentrated solution of salt, with results corresponding to those obtained with electrical irritation (p. 179).

Space forbids our going farther into the details of Pflüger's method of research; suffice it to say, that all his experiments are conducted with mathematical accuracy.

The after-effects of a current upon a nerve are of two kinds, and are designated by Pflüger dynamic and static. We shall consider the former.

Undoubtedly the first dynamic effect of a constant current is the opening contraction. Long ago Ritter made the distinction, that it does not depend on the current, but upon a succeeding change in the molecular condition of the nerve. Pflüger has corroborated and extended this observation, and has shown that the contraction may not follow for some moments after opening the current.*

Another effect of the constant current is the tetanus which occurs on opening a circuit which has been closed beyond a certain length of time. It has been called, after its discoverer, Ritter's tetanus; and the conditions of its existence have recently been minutely investigated by Rosenthal and Wundt.† Its discoverer supposed that this opening tetanus occurred with the centripetal current only, to which he accordingly ascribed an "exalting" influence; whilst to the centrifugal current, which he believed to produce no opening tetanus, he attributed a "depressing" effect. Volta founded upon his observations a law, which is known to physicists as that of "Volta's changes." This law affirmed that, for its own direction, a current depresses the irritability of a nerve; for the opposite direction, it elevates it.

From the observations of Rosenthal, made in the laboratory of Du Bois-Reymond, the following facts result. Opening tetanus is not, as Ritter supposed, peculiar to the centripetal current, but occurs with currents of both directions, which have been a long time closed; it is, however, less constant with the centrifugal current. Three stages of irritability may be distinguished by the form and intensity of the opening contraction. In the first, the opening of a current which has been a long time closed, calls forth a powerful tetanus, which lasts from a minute to a minute and a half; in the second stage, there is merely a strong contraction followed by slight shortening of the muscle, which lasts for some time; in the third stage, there is merely a contraction. The opening tetanus immediately stops when the current is again caused to traverse the nerve (Ritter), but reappears on opening. If a current is sent in a direction opposite to that which has produced the tetanus, the latter is intensified (and not suspended, as Ritter affirmed), but on opening this contrary current, it disappears. An opening tetanus which has been allowed to cease naturally may be again recalled by closing a current opposite in direction to the first, and this when a current in the direction of the first fails to reproduce it. Again, if the opening tetanus is intensified by an opposite current, and if this latter is kept continued, the tetanus which was at first increased, disappears, but is again produced on opening the circuit. From these facts Rosenthal concludes: "Every constant current exalts the irritability of the nerve for the opening of the operating current, and the closing of a current of an opposite direction, but diminishes it for the closing of the operating and the opening of the opposite current."

* Untersuchungen, p. 75.

† Quoted by Funke, p. 731; see also Henle and Meissner's Report, 1858, p. 422.

Pflüger has shown that Rosenthal's law is true only in regard to weak and to moderately strong currents. The opening tetanus produced by *strong* currents is weakened by the closing, but intensified by the opening of a second current of either direction. We shall see the importance of these facts when we come to Pflüger's theory of nerve-mechanism.

The *static* conditions of a nerve after a constant current were discovered by Pflüger. They consist in an exaltation of irritability in the katelectrotonic as well as in the anelectrotonic tracts, with this difference, that in the former there is a short interval of depressed irritability. Pflüger designates these changes as positive and negative modifications. They hold only in the extrapolar tracts.

Some exceedingly interesting and important researches as regards the relation of irritation of nerves to time, have been made by Von Bezold.* As these are corroborative of Pflüger's views, we shall here briefly refer to them. Bezold observed that the interval (of time) between irritation and contraction of a muscle, was shortest with the closing contraction of a moderately strong centrifugal current, and with the opening contraction of an equally strong centripetal current. Not far different was the time which elapsed with the closing contractions of weak centripetal currents. On the other hand, the closing contraction of strong centripetal currents began, *cæteris paribus*, always somewhat later than the closing contraction of the centrifugal current, and the difference in time corresponded to that which would be required to traverse the length of the tract. These results are just such as Pflüger's contraction law would postulate, and they go far to prove that the irritation on closing a circuit takes place at the negative pole, on opening a circuit at the positive pole.

Let us now turn to Pflüger's theory of the molecular mechanics of nerve. It is founded upon purely hypothetical premises, but it possesses the advantage of comprehending and arranging under a single point of view all the facts which our present state of knowledge discloses.

The fundamental idea of Pflüger's theory is, that the progressive molecular movement in a nerve sets free at all points of its course new forces, so that the sum of these forces is greater at the extremity of a nerve along which an irritation has travelled, than at the moment of excitation—the amount of force developed corresponding to the extent of nerve traversed. Pflüger designates his theory as the "evolution hypothesis" (*auslösungs-hypothese*). In the molecule system resides a force which is constantly tending to the production of movement, but this is opposed by a contrary force which controls such a tendency. During the quiet state of the nerve, these forces, which may be termed molecular tension and molecular control, are equally balanced, and the latter is kept in a certain position, to which it instantly returns when it has been removed from this position by other external forces acting upon it. Further, molecular control may be pushed aside in two opposite directions: if in the one, the result is the production of living

* Zur Physiologie des Electrotonus: Henle und Meissner's Bericht, 1858, p. 421.

force, the amount depending upon the extent to which the molecular control is pushed aside; if in the other, the result is an accumulation of tension force.

Pflüger illustrates his idea by the following mechanical contrivance. A cylinder bent at right angles, A B C, has placed in its horizontal limb a water-tight piston, capable of being pushed in the double directions *a b*, and *c d*. On one side of this piston, a steel spring, *e f*, fastened at *e*, presses and tends to propel the piston in the direction *a b*. On the other side a pressure is maintained by a column of mercury, the degree of pressure corresponding to the height of the column in the vertical part of the cylinder B C. This force tends to propel the piston in the direction *d c*. The piston comes to rest in a position at which the two opposing forces —i.e., the tension of the spring and the weight of the mercury—are equal. At one side of the piston is an opening in the horizontal tube, *g*. If the force of the spring is increased, the piston is pushed further from the opening *g*, but more of the fluid is propelled into the vertical limb B C, consequently the hydrostatic pressure is increased. If, on the other hand, we diminish the elasticity of the spring, the piston is pushed by the fluid in the opposite direction *c d*, until it passes over the opening *g*, through which the fluid runs out; living force is gained. With the exit of the fluid the hydrostatic pressure is again diminished, so that the force of the spring again pushes the piston over the opening, and prevents the further escape of fluid.

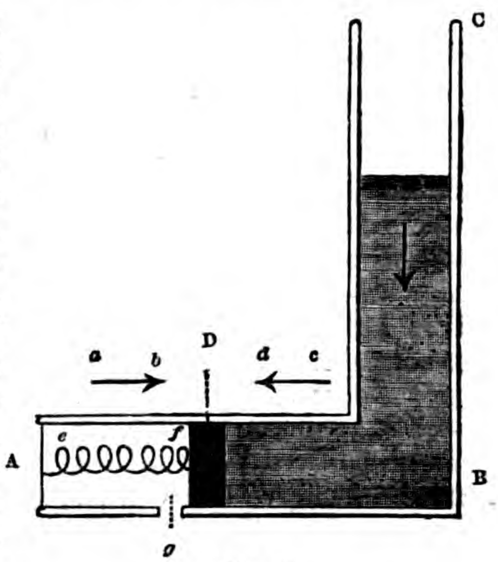

Fig. 5.

To apply this setting-free hypothesis to the explanation of the excitation and irritability of nerves, Pflüger requires that the following premises be granted. An electrical current which is made to traverse a portion of nerve, affects only the force of molecular control, not that of molecular tension. The change produced on the control force by the current consists in this, that in the region of anelectrotonus, this force is increased, in the region of katelectrotonus diminished. By referring to the diagram, this change is represented by changes in the pressure of the spring *e f*, which corresponds to the control force. If increased, there is an increase of the hydrostatic pressure, corresponding to the force of tension (anelectrotonus); if diminished, there is an outpouring of the fluid corresponding to the production of living force (katelectrotonus). A positive increase of the control force induces indirectly a positive increase of tension force, and reversely, a negative increase of the one, a negative of the other. In this way an increase of irritability in the katelectrotonic portion of a nerve, and a diminution in the anelectrotonic, is easily supposable.

It has been seen that with currents of weak strength an irritation will traverse the anelectrotonic tract of nerve (in which conducting power is reduced), just as in the natural state. Amongst Pflüger's facts, is that of a powerful contraction of muscle, when an irritation is applied *above* a weak centripetal polarizing current. To reach the muscle, the excitation must traverse that part of the nerve in which the power of conducting such excitation is diminished. How does Pflüger's hypothesis explain this? In the following manner. In the katelectrotonic tract the force of control is diminished, in the anelectrotonic the force of tension is increased. At the point where an irritation is applied, a certain amount of free force is produced, which pushes aside, in the next segment of nerve, the control force, and this sets free more force. In all portions of nerve the amount of force set free corresponds to the magnitude of the irritation. In the passage from segment to segment, as much of this free force is consumed as is necessary to the pushing aside of molecular control, and the greater the resistance offered by the latter, the greater is the consumption of the former. Pflüger gives a mechanical illustration of this, of which our space does not permit any description. (p. 483.)

At p. 12 it has been stated that in powerful electrotonus, the anelectrotonic tract loses its power of transmitting irritation. The theory explains this by supposing that the whole sum of free force resulting from the irritation is not sufficient to set aside the forces of control, which are greatly increased in the region of anelectrotonus.

In reference to the fundamental law of electric irritation the theory also holds. The law is, that on closing the current, contraction of the muscle is due to the constitution of katelectrotonus, whilst on opening the circuit the contraction depends upon the disappearance of anelectrotonus (see p. 12). In the region of the kathode the control force is diminished, the spring A (see fig. 5) allows the piston to be pushed over the opening, and the mercury—i.e., tension force—is allowed to escape. If the tension force is not replaced, the escape is momentary, and a contraction on closing the circuit only. But if the tension force is replaced, another escape of mercury occurs, and another, and another. This is what happens in tetanus on closing the current. On opening the circuit, the relations are reversed. Now, it is the control force in anelectrotonus abnormally increased, which returns to its natural position, but in doing so it does not come to rest at once, but, like a pendulum, it swings a little beyond the point of equilibrium; the piston is pushed over the opening *g*, tension force escapes, and a contraction of muscle follows. This is Pflüger's explanation of the opening contraction.

We have seen that the contraction of a muscle on closing a current is stronger than on opening it. Pflüger's hypothesis explains this fact very simply. In closing the circuit, the control force at the anode is pushed in the direction *a b* (see fig. 5), whilst at the kathode it is pushed in the direction $c\ d$—that is to say, two causes tend to push aside the control force, and a greater escape of tension force follows than in the region of anelectrotonus on opening the circuit. The contraction is, for this reason, stronger on closing than on opening the circuit.

In tetanus there is a continuous alternate escape of tension force at the anode and kathode, which continues as long as the metamorphosis of matter is able to repair the loss of force which each electric shock produces.

In relation to the law of Du Bois, according to which a nerve is excited by a *rapid* change in the density of a current, and not by a current of gradually changing or of uniform density, the theory of Pflüger gives this solution. When the current is made to increase very gradually, the control forces are slowly driven backwards, the force of tension escapes slowly, the mercury comes out drop by drop, and the force set free at any given moment is too small to occasion a muscular contraction.

It has been stated that after the application of a constant current to a tract of nerve, the extrapolar anelectrotonic portions undergo an immediate positive modification, but the extrapolar katelectrotonic parts are first negatively, and then positively, modified. Pflüger explains the positive modification by supposing that the constant current weakens the force of molecular control. It will be remembered that the fundamental proposition of his theory is that the current acts on the control force, and not at all on the force of tension. Supposing, then, that this depression of control force takes place, there will be less resistance offered to the conversion of the opposite force into living power, and the nerve will be more irritable than in the natural state; but how is the negative variation in the katelectrotonic part explained? In this way. In katelectrotonus there is, as we have already stated, a diminution of control force, and an escape of tension. In this momentary deficiency of tension lies the cause of the negative modification. A strong current causes, in the intrapolar tract, a depression of the irritability. Pflüger attributes this to an entire destruction of internal molecular relations.

One demand more upon the theory. What explanation does it afford of Rosenthal's law of opening tetanus? It has been stated that a constant current, if applied for a certain length of time, produces tetanus on opening the circuit. Pflüger ascribes the production of tetanus to a weakening of the control force in the region of anelectrotonus. The spring, $e\ f$ (see fig. 5), being thus weakened, the pent-up tension force pushes the piston in the direction $c\ d$, and an escape of tension force—denoted by the tetanic state of the muscle—is the consequence. The tetanus cannot be ascribed to the katelectrotonic part, because in this a consumption of tension force has occurred on closing the circuit, and the control force on opening is so strengthened as to increase the power in the direction $a\ b$, and thus prevent an escape of the fluid (tension force). A renewal of the current produces a positive modification of the control force in the anelectrotonic part—i.e., the increased force of the spring (see fig. 5) pushes the piston in the direction *a b*, and prevents an escape of the force of tension. This latter effect explains the cessation of tetanus, and accounts for Rosenthal's law of diminution of irritability on *opening* a current of the *same* direction as that which produced the opening tetanus. The fact, that a current of an *opposite* direction on being *closed* should increase the tetanus, Pflüger explains by the supposition that all the parts in which control force was weakened are now thrown into a state of katelectrotonus—i.e., that the control force is further decreased—more tension force is allowed to escape, and a positive modification of the tetanus is the result.

Enough has been said to convey some idea of the direction in which researches on the mechanism of nerve action are carried. The reader can now judge to what an extent physical experiment and physical reasoning may be applied to the most obscure and complex processes of organic life. Of Pflüger's facts we can scarcely doubt the correctness, having witnessed some of his most striking experiments. Of his theory we can only await the result of further inquiry to prove whether it is true or false. It is extraordinarily ingenious. The future alone can prove if it is true. It is now the subject of experimental criticism, and a mass of literature already exists for and against it. We have been careful to avoid entering upon the researches of any other than those of the Berlin school, which, with Du Bois-Reymond at its head, claims paramount consideration in all that relates to the general physiology of the nervous system.

Review II.

Mémoire sur l'Iodisme Constitutionnel, présenté à l'Académie Impériale de Médecine le 11 *Janvier,* 1859, *suivi d'un résumé de la discussion Académique et d'un Complément d'Observations.* Par Le Docteur F. Rilliet, Ancien Médecin en chef de l'Hôpital de Genève, &c. &c.—*Paris,* 1860. pp. 113.

Memoir on Constitutional Iodism. Presented to the Imperial Academy of Medicine, with Observations, &c. By Dr. F. Rilliet, Geneva.

The title given to his somewhat disjointed work by Dr. Rilliet is one which he should have preserved throughout. *Iodism*, we think, more clearly represents both the large subject of the influence of iodine on the human constitution, and the way in which it is manifested, than his other term, *Iodic intoxication.*

This expression appears objectionable on more grounds than one. Intoxication is an evanescent condition, dependent for its duration on continued supplies of the stimulating agent; but if Dr. Rilliet's facts are correct, constitutional iodism endures for a remarkably long period after the agent has ceased to be administered. Moreover, the influence of iodine (except in very large or poisonous doses) on the nervous system is secondary only, not immediate.

The statements made and cases related by Dr. Rilliet in confirmation of his views are certainly very curious, nor such as we are accustomed to witness in this country, and it is rather difficult to accept the explanation he gives of this admitted difference. The peculiar local affection so frequent in Switzerland, and quite sufficiently common with us, is no doubt one great reason why iodine enters so largely into the consideration of Dr. Rilliet and his compatriots.

It was the power of effecting absorption that first led the enterprising Coindet to apply iodine to the treatment of goître, and to publish his memoir on the new remedy against this disease in 1820. Aware of the influence of "Ethiop's vegetable" in the reduction of this frequent and hideous deformity, but ignorant of its connexion with the bladder wrack (*Fucus vesiculosus*), he yet imagined that iodine might be the active principle of both. He immediately acted on this analogy, and adopted the iodine treatment as one of great power, though not indeed "exempt from danger, even when administered by a careful physician, and with requisite caution."

It seems pretty clear that the principal ill effects in the early times of its administration were due to the form in which it was used, and though excessive emaciation, palpitation of the heart, nervous tremors, and morbid appetite, were among them, yet the gravest of all were those due to gastro-enteric irritation and ulceration. It is no longer the practice to give iodine, either pure or in tincture, or uncombined with a neutral base, and the decided causticity of pure iodine is so well recognised as to induce the attendant caution, and limit its application to the exterior of the body.

In this country iodine has its greatest reputation as a remedy in syphilis, especially in the tertiary forms of it, in struma, and those cachexiæ which are the result of both; but in Switzerland, over and above those diseases common to all civilized peoples, is added the great frequency of goître, and in goîtrous subjects it is very common to have no alteration of the general health whatever.

People of all ages and classes go about their ordinary avocations, more or less impeded doubtless, by the unseemly growth; but unless its weight or pressure on the trachea interferes with respiration and circulation, the owner does not consider himself or herself ill.

This must be borne in mind when estimating the influence of iodine on the system, and it will in some measure perhaps account for iodism being so much more frequent in the practice of Dr. Rilliet and his colleagues than with us.

Pushed to the extent of producing what the author calls *constitutional iodism*, or *iodic intoxication*, the action of iodine on the human economy presents itself in three degrees.

The first he calls

"Direct, primitive, or from without to within; occasioned by a local action on the digestive organs, of iodine or its compounds in too large doses. Its symptoms do not differ sensibly from those produced by other irritant metallic poisons; they are generally of brief duration and small moment. . . . . . This *empoisonnement* may manifest itself at any age, in any country, and under any condition of health, and predisposition exercises but a slight influence on its development."

The second, which he calls

"Secondary, indirect, or from within to without, is produced by the absorption of iodine given in general in large doses, and by its elimination through the secretions; and it is characterized by nervous symptoms. *Ivresse iodique*, followed or not by morbid conditions which indicate the action of the metalloïd on the secretory organs—such as ophthalmia, coryza, salivations, eruptions, &c. &c.

"Organic predisposition is requisite for the production of this condition, and it may be produced at all ages, in all countries, and all conditions of health, and has only been described since the time when iodide of potassium has become the remedy, *par excellence*, of tertiary syphilis."

The third he speaks of as absolutely specific, and has been described, since the discovery of iodine, under the name of iodine cachexia, and later under that of iodism.

The symptoms of the first degree he puts down

"As dryness, heat, and constriction of the throat, vomiting, diarrhœa, violent pains in the stomach, small pulse, anxiety, headache, and when the pain is very intense, an approximation to convulsions."

These are surely neither more nor less than the symptoms of metallic poisoning, and might be the result of mercury, arsenic, copper, and other similar active agents; there is clearly nothing more specifically iodic here than what belongs to an over-dose of an irritant metalloïd.

The symptoms of the second degree are notably

"Frontal and supraorbital headaches, disturbed vision, singing in the ears, stupor, convulsive movements; at the same time, or very soon after, other symptoms follow, announcing the action of the iodine on the secretions, such as ophthalmia, coryza, salivation, vomitings, diarrhœa, profuse micturition, various eruptions, ranging from a simple erythema to morbus maculosus.

"In addition to this, and a very important one it is, must be named the atrophy of the testicles and mammary glands, and this, too, without the general health being compromised.

"These symptoms are generally produced by iodine or its compounds, administered in large doses."

We again demur to this catalogue of symptoms as being purely and simply iodic, excepting the two last—viz., atrophy of the mammæ and testes.

There is not one that does not suggest itself as an effect of other medicinal agents, and the condition of the skin particularly mentioned, may fairly be referred to alkalization quite as much as to iodization; and unless Dr. Rilliet means to assert, and is prepared to prove, that the purpura hæmorrhagica of seamen is the result of their living in an atmosphere highly charged with iodine, it will be difficult to establish eruptions of this class as iodic.

The remarkable and calamitous effect in producing absorption of the testes and mammæ is another matter, and as it forms one of the most prominent symptoms of the third group, should have been confined to that class.

The third set of symptoms differs from the foregoing ones only by the rapidity of their appearance and their intensity.

"From the very beginning of the malady it is seen that they will be serious. The goître (if goître there be) diminishes or disappears in a few days, general emaciation comes on with a rapidity sometimes frightful, so that in a few weeks, even in eight or ten days, the patient becomes scarcely recognisable; he looks twenty years older, the marasmus is general, but more manifest in the glandular system; the breasts and testicles are atrophied, the pulse is quickened, small rather than full, and varying in rate. The skin of the face is pale-yellow or greenish, the countenance is anxious or sorrowful, the eyes very hollow and circled with black, sometimes they are fixed, sometimes wandering. The feebleness is great, the voice broken and husky. The patient is agitated, trembling, and breathless on the slightest exertion. The nervous symptoms are most painful; disquietude, fright, and causeless disturbance succeed to repose of mind, and nightmare replaces sleep."

Such is the sketch of the third degree of constitutional iodism, or iodic poisoning; for such, to all intents and purposes, it is; nor is it surprising that when things have reached such a point that death from extreme marasmus should take place.

Another symptom much insisted upon by our author as peculiar to iodism in all three stages, is great increase of appetite, amounting to bulimia.

Many years' experience of the effects of iodine in this country, and also frequent observations of the effects of iodized mineral waters, like those of Kreutznach, near Bingen on the Rhine, both on the spot and on those sent there for the use of them, have revealed no such results as those described here, nor warrant the conclusions arrived at by the author; the results, too, of French practice do not bear out Dr. Rilliet's statements. The only cases of clear direct iodism, or iodic poisoning, we have witnessed, have been those where solution of iodine has been injected into ovarian cysts and hydroceles; and again in one case where tincture of iodine was swallowed with suicidal intentions.

In the two former cases, but particularly in the ovarian injections, the train of nervous symptoms, the intense headache, feeble, rapid pulse, vomiting, sighing, cold extremities, were all witnessed; but this surely is due to the rapid introduction of an active agent into the circulation through an unusual medium, and not to any specific influence of iodine. The effects produced in the ovarian injections undoubtedly were nearest to the idea of iodic intoxication, but have never in our experience been followed by the prolonged series of symptoms described in the third category of Dr. Rilliet.

In the case of suicidal swallowing of tincture of iodine, the patient believing it was laudanum, all the symptoms were those of an actual irritant metallic poison, and were followed by acute gastritis, leaving the patient in a deplorable condition of dyspeptic suffering, but not attended by iodic marasmus.

The first objection to Dr. Rilliet's theory naturally arises on the question of dose, and many or most of these effects will be attributed either to the form, amount, or duration of the treatment. It has been already stated that iodine uncombined is now confined to external applications, and is rarely or never given internally, except in combination with a metallic or alkaline base. The quantity of iodine given is stated by the author to have *no direct* bearing on the effect produced, provided always that the dose has not been given in deadly amount, by mistake or design; in which case the symptoms are those of an irritant metallic poison. Some of his most remarkable instances of iodism are the result of extremely small doses. In one case, a lady took 10 drops of tincture of iodine in a glass of eau sucrée, and at the end of a few hours the effects produced were coryza, abundant salivation, contraction of the jaws, and grinding of the teeth, &c. (p. 17.) In another, the dose was $\frac{1}{30}$th of a grain of iodide of potassium in the day. Vertigo, bulimia, and great emaciation were produced very rapidly. The patient was seventy-four years of age. In the third case, the dose was $\frac{1}{24}$th of a grain per diem, the whole quantity consumed being only $12\frac{1}{2}$ centigrammes, or about 2·0868 grains; yet iodism was severe, and the recovery slow. (p. 54.)

It must not be supposed that Dr. Rilliet is an advocate for Hahnemannic attenuations, or a believer in the crude absurdities of his disciples; on the contrary, he guards himself against the imputation of such a disgrace, and gives a case in which a lady, fearing the results of iodine in allopathic doses, had recourse to an homœopathic quack, who prescribed a quantity greatly in excess of an ordinary dose. The patient believing the usual audacious assurance, that an homœopathic preparation *could do no harm if it did no good*, took the false potion, and soon was undeceived by the appearance of the whole train of effects.

In this country, and in France, iodine has established itself as an agent of no mean power. These symptoms are of rare occurrence; and though for twenty-five years we have witnessed its effects in every kind of case (struma, syphilis, goître, abscess, &c. &c.) in which its use was indicated, in patients, too, of every class, yet only one case of severe mischief ever came under our notice, and this case of fatal gastritis was the result of neglect as to the amount of the dose administered.

Dr. Rilliet, however, insists on the gravest forms of iodism being the result of quantities so small as to preclude the possibility of its being the product of an over-dose of a metal or its salts, and the result of a direct irritant.

The case most detailed, most interesting, and yet to our insular ideas most difficult to comprehend, is that in his seventh observation. (p. 59.)

There, in a summary alike remarkable for its perspicuity and its condensation, he says:

"A patient, aged forty-five. Robust constitution; habitual health good. Iodine used from the 26th of May to the end of July (in the shape of a salt of iodine mixed with common salt, and then entering into the composition of various articles of food), about 10 centigrammes, or about $1\frac{1}{2}$ grain of hydriodate of potash consumed: no results. Interruption of the medicine: resumed in August, and continued till January; about 20 centigrammes, or 3 grains of iodide of potassium consumed. Iodism: salt discontinued. Cure. Salt resumed in March and April, 6 centigrammes (0·9258 of a grain); no effects. Discontinuance of the use. Resumed in August: renewed iodism, far more intense than before; the salt discontinued. About the end of December 10 centigrammes = 1½ grain consumed. Cure. At the end of *two and a half years*, a month's residence at the sea-side brings on a very severe iodism."

This case is drawn with a vividness that makes it worth while to read it through, and though we cannot endorse the conclusions of Dr. Rilliet, we cannot help observing that a little more vivacity of description, and brighter word-painting, would, so long as the imagination has nothing to do with it, be a great improvement on the dry case-book style, which, begun in our student days, is scarcely improved upon in later years.

The following extract shows the deplorable condition into which this gentleman, who, after having ceased to use iodine for two years and a half, and which he then used in his food as a prophylactic against goître, was plunged by staying a short month by the sea-side.

"Quo tendis inertem
Rex periture, fugam? nescis heu perdite nescis
Quem fugias: hostes incurris, dum fugis hostem.
Incidis in Scyllam, cupiens vitare Charybdim."

It must be mentioned that he was a man in the upper ranks of society, enjoying good health, of vigorous mind, happy in his family, and a fine example of *mens sana in corpore sano*.

"Were I to live an hundred years, I shall never forget the sensation produced by the sight of this poor stooping old man (he was only forty-five), his eyes sunk and slightly wandering, his voice trembling and broken, a corpse-like emaciation, a shaking in all his limbs. His little daughter came constantly to gaze on him, while her face expressed at once her grief and her surprise. His servants one and all declared their master's days were numbered."

He did not die, however, but recovered on a regimen of country air, asses' milk, tonics, &c.

It is very difficult indeed to arrive at the same conclusions as our author, that this fearful condition of atrophy was the result of iodic salivation, produced by not more than seven grains of iodide of potassium, consumed in twenty-one months, and revivified by a month's residence at Biarritz.

Surely, without impugning the Doctor's sagacity, either some visceral mischief was undetected, or some constitutional taint existed, unrevealed by the patient. The description of the symptoms carries one back to the slow poisonings of the middle ages, and you see the gradual but irresistible wastings of secret mediæval poisoning. Another case is detailed at p. 63, where a lady who took no iodine at all medicinally, became the subject of iodism, purely from changing her residence from Pau in the Pyrenees, to Biarritz. It must be admitted that this case is pressed to serve under the Doctor's flag with rather undue violence.

We are familiar with Biarritz, and have a pretty full acquaintance with other sea-side sanitary stations, and though the waves of the Bay of Biscay do come rolling grandly on to the shores of pretty and now imperial Biarritz, its marine action is not appreciably more intense than that of Scarborough, or Cromer, or the west coast of Ireland.

The solution of this patient's condition must be sought in some functional or possibly organic mischief, and not attributed to so feeble an influence as iodine in the sea air.

Not less implicated than the ocean shores in producing iodism, are the natural iodized springs of Willdeg, Challes, and Coëze. Yet the strongest of these (Willdeg) contains only 1½ centigramme (2264th of a grain) of iodide of soda to a pint of water, so that a man must ingurgitate nearly five pints of water before he swallows a grain of the healing salt. Challes and Coëze are much less potent.

The waters of Kreutznach contain a much larger per-centage; but this is because some portion of the supply is derived from the Salines after evaporation has taken place in the process of salt-making, and what is known in salt-works as "bittern" has become concentrated; yet no case of iodism has come under our notice, either while at these baths, or among those who have gone there by our advice.

The last class of sufferers from iodism given in this work, are those who have used burnt sponge; but the cases given in illustration are not free from obvious sources of complication—such as gout, cardiac disease, nervous disorders, and the like.

The question of iodism is truly a large one, and embraces in fact the whole range of that very debateable territory, the action of medicinal agents on the human frame. It is very probable, that when this subject is considered fairly, and the descriptions given in this work examined, that some will find an explanation of a series of symptoms hitherto obscure in the action of iodine; but this work is not an investigation of the question, it advocates a particular theory, and the whole object of the work is to make this theory and the cases agree: there is no distortion of facts, nor unfair statements, but we do not think the deductions are logical from the premises laid down, and we are sure that iodism as here described is not a frequent result, either in this country or in France.

Can this immunity be explained satisfactorily? Not, we think, on Dr. Rilliet's theory—viz., that the air of Switzerland is particularly free from iodine, and hence the preternatural sensibility of his patients to the influence of it.

"Cette différence dans l'ensemble des constitutions soit évidemment tenir au climat, et il est bien probable qui les Genévois sont plus facilement influencés par l'iode parceque Genève est un pays dont l'air, l'eau, et les produits du sol, sont dépourvus de ce métalloïde."

In a note, he says that M. Chatin has proved the absence of iodine on the whole of the left bank of the Lake of Geneva; and that Professor Marignac could not find it in the water-cresses of that district, which he analysed at the request of M. A. de Candolle.

The more probable explanation of iodism, as discussed by Dr. Rilliet, is this: In almost all the cases he has recorded, iodine was administered either as a remedy for, or a protective against, the very prevailing affection, goitre. Its powers as an absorbent have been already shown to have led to its adoption for this purpose, and we also know how large a goître may become without injuring a person's health. Every traveller, especially medical travellers, are familiar with the vigorous peasants who walk daily from Martigny to Chamouni with heavy loads of fruit and vegetables, who are all more or less goîtreuses. With us, on the contrary, iodine is mainly used in cachectic disorders, notably syphiloïd and strumous, to which may be added rheumatism and glandular growths; but in any case the tendency to produce cacoplastic material exists.

Now, it is not saying too much to assert that the tolerance of a medicinal agent is in proportion to the necessity for its administration. Pain makes opium harmless; ague, quinine; anæmia, iron; primary syphilis, mercury; and so struma, secondary and tertiary syphilis, and rheumatism, considered as blood disorders, are all removable or alleviated by iodine.

A combat, if one may use the phrase, takes place between the two agencies, and the influence is expended on the disorder; hence iodism or poisoning is not manifested. Of course there are exceptions to this rule, and cases there are where iodine has disagreed just as mercury has done; but the cause is, doubtless, to be sought in the idiosyncrasy of the individual, or in the peculiar phase of the disorder, rather than in the remedy itself.

No one can have failed to observe gastric, intestinal, cephalic, renal, and glandular disturbances occurring from the use of mercury, iron, silver, lead, and other potent metallic agents; but this is surely to be attributed either to the peculiarities just mentioned, or to error in administration in form, quantity, or duration, rather than to the agent.

There remains, however, one set of results from the use of iodine as incontestable as they are ruinous, and but for which iodine would not have received so much consideration—viz., its absorbent powers, properly so called.

The instances of mammary and testicular wasting under the influence of iodine are too formidable in their results to be ignored, and no such action can be attributed to other absorbent remedies.

We are not sure that the distinction between the absorbent actions of mercury and iodine, as defined soon after the introduction of the latter, is not mainly correct—viz., that mercury produces the absorption of morbid deposits in certain structures, but that iodine produces absorption of the structure itself.

We have never seen the integrity of either mammæ or testes compromised by the use of iodine, but Dr. Rilliet alludes to it as frequent; he adds, however, that on the cessation of the iodism these structures are gradually restored.

He also speaks of the repeated returns of goître, which had disappeared under the influence of the medicine in proportion as the iodic poisoning diminished; this is a curious fact, and one deserving a closer investigation, and greater number of recorded observations, for it involves not only the question of the absorption, but also of the growth of goître. In estimating the influence of iodine in producing the effects described, the author lays great stress on the inordinate appetite or bulimia as a marked symptom. This he explains on the theory that as iodine causes the absorption first of all the superfluous, and then of all necessary tissues, so there is a demand made by nature to replace the removed structures, and an inordinate craving for food is created.

"La boulimie est l'indice de la réaction de l'economie contre la perte qui menace ou qu'elle a déjà éprouvée; elle tend à obvier la disassimilation, que outre passe déjà l'assimilation.

"A quelle cause faut-il attribuer l'amaigrissement? Depend-il de l'insuffisance de l'alimentation par suite de la lesion de l'estomac, ou bien de l'action exercée par l'iode sur la nutrition? Je ne hésite pas à adopter la seconde opinion, ce n'est ni la prétendu gastrite ni l'inanition qui occasionnent l'amaigrissement, car on l'observe en dehors de toute cause de cette nature chez les malades traités par les doses inoffensives et qui continuent de se nourir. Je ne veux pas pretendre cependant qu'un estomac délabré ne soit pas une cause aggravante de l'amaigrissement; mais très certainement dans la grande majorité des cas, ce n'est par dans l'irritation gastrique qu'il faut chercher son point du départ."

The theories of Dr. Rilliet and his Genevese supporters have met with considerable opposition in the French Academy of Medicine, and particularly from his formidable adversaries, Ricord and Trousseau, two men whose opinions are entitled to the greatest respect, based as they are on the largest experience of iodine in their respective *specialités.*

We cannot help the conviction that their opinions are correct—at least in the meridian of Paris and Greenwich; and there is in the gallant fight which Dr. Rilliet makes in defence of his own views a quasi-admission that latitude and longitude have something to do with the question: at least, he seeks to reconcile the discrepancies by a primary influence in an iodic or non-iodic condition of the localities.

This is a large and important question—one which meteorology might aid us in solving, and a series of observations made on the same plan as those for ozone, only on the inverse principle, might decide the point as to the existence of an appreciable iodic influence in the atmosphere. There are several other points in Dr. Rilliet's paper which demand a much larger series of observations before they can be admitted as proved, and we are sure that the circular issued by the author himself, asking for further and independent observations at the hands of his professional brethren, betokens a sense of incompleteness which is alike honourable to his candour and spirit of inquiry.

Dr. Rilliet is one of those very active spirits in whom an idea no sooner springs up than it bursts into a spread of print; *nonumque prematur in annum* is clearly no maxim of his, for he gives in the last page of his work a list of his publications, amounting to forty in number, in the space of twenty years.*

REVIEW III.

Verslag over den Staat der Gestichten voor Krankzinnigen in de Jaren 1857, 1858, *en* 1859, *aan den Minister van Binnenlandsche Zaken, ingediend door de Inspecteurs dier Gestichten.* (Bijdragen en Mededeelingen voor de Statistiek van het Koningrijk der Nederlanden, No. 1.)—*'s Gravenhage*, 1861.

Report on the State of the Institutions for the Insane in the Years 1857, 1858, *and* 1859; *presented to the Minister of the Interior by the Inspectors of these Institutions.* (Being No. 1 of Contributions and Communications respecting the Statistics of the Kingdom of the Netherlands).—*The Hague*, 1861. Royal 8vo, pp. 229. With numerous Tables.

In our twenty-fifth volume (p. 21) we reviewed at some length the admirable report of the Inspectors, C. J. Feith and Professor J. L. C. Schroeder van der Kolk, on the 'Condition of the Eleven authorized Medical Institutions for the Insane in the Kingdom of Holland during the years 1854, 1855, and 1856.' We have now before us the equally interesting and instructive report of the same inspectors for the triennial period 1857–1859. Although the asylum at Rotterdam is included in the present report, its formal recognition as one of the *twelve* asylums contemplated by the law of the 29th of May, 1841, did not take place until after the close of 1859.

Following the plan we adopted on the former occasion, we shall pass over the chapters in the report which are of merely local interest, such as those relating to the buildings, the furniture, &c., and shall confine our remarks to the sections containing information of a more general nature.

The state of health of the patients, apart from the physical causes of their insanity, was, during the three years to which the report refers, on the whole satisfactory.

"The influence of the great heat of summer is differently stated. In 1857, the unusual quietness of the patients at 'sHertogenbosch was attributed to the great warmth; while, during the heat of 1859, much greater excitability and restlessness were observed in the same institution. At Rotterdam, in one of the years remarkable for a warm summer, the small number of deaths was ascribed, perhaps not without reason, to the fact that the patients spent nearly the whole day in the open air." (p. 32.)

* Since this review was written, the career of Dr. Rilliet has closed. His busy brain, his fluent tongue, and nimble pen, are powerless, silent, still. The questions, however, he has raised, and the points of interest he has left behind him to be investigated and settled, are the best testimony that his life, though brief, has been both useful to society and honourable to himself.

The influence of epidemic diseases, more or less prevalent without the institutions, was occasionally felt also within the walls. The epidemic of intermittent fever, for example, which latterly prevailed throughout a great part of the kingdom, appeared most intensely in the asylum at Franeker, particularly in 1857 and 1859; in a very great degree at Meerenberg in 1857, and to a less extent also in 1858; moreover, in a remarkable manner, although less severely, in 1859, at 'sHertogenbosch.

In the institution at Rotterdam, a limited epidemic of catarrhal ophthalmia occurred in 1857, which was arrested by powerful antiphlogistic treatment in its commencement. In 1858, also, inflammation of the eyes appeared epidemically in the same asylum, especially among the women; as it did in 1859 at 'sHertogenbosch, among the men. On the other hand, the Rotterdam asylum continued quite free from the epidemic of measles, which in the last-named year prevailed to a great extent throughout the surrounding district.

With respect to Asiatic cholera, which, during the period comprised in the report, manifested itself in different localities, including some where asylums existed, it appears that only at Dordrecht was its influence felt within the institution, four men and three women having been attacked with slight, and only one woman with more obstinate, diarrhœa. None of these patients were, however, confined to bed in consequence of the attack. At Rotterdam a woman, who had for many years been insane, having been seized with cholera, was at the same time cured of her insanity.

During the prevalence, in 1858, of a severe epidemic of small-pox at Deventer, occurring in the immediate vicinity of the asylum, twenty cases appeared suddenly and simultaneously among the patients in the several wards. None assumed any dangerous characters, and all who were thus affected recovered. Recourse was, however, had to revaccination, both with the remaining patients and with the attendants, a measure which was thenceforward adopted in the case of every new admission.

At Meerenberg, in 1857, during a severe epidemic of ague, the males were principally affected to the extent of 105 out of 220, while of the women only 56 out of 279 were attacked, probably in consequence of the men being usually more occupied out of doors in the evening. Of the whole number of internes, including the attendants, and amounting to 616 individuals, 241, or 39 per cent., were affected in 1857.

In our former article (p. 23), we gave the results of a table, exhibiting the relative mortality from different causes during the five years 1852–1856, in nearly all the institutions. A similar table at p. 37 of the present report affords the following figures, corresponding pretty closely with those before obtained:—Apoplexy, 120 cases out of 801 (the total mortality in all the asylums), or 15 per cent.; marasmus, 305 : 801, or 38·1 per cent.; diarrhœa, 22 : 801, or 2·7 per cent.; consumption, 155 : 801, or 19·4 per cent.; other diseases, 199 : 801, or 24·8 per cent.

"We consider the detailed statements of the observations respecting the mode of treatment, and the use and action of remedies in different sorts of insanity, to be of the greatest importance. By their means we shall be able to make the experience of all the medical officers of the several institutions in the Netherlands generally available, thus increasing our knowledge, especially of the treatment and of the remedies with which we may hope to combat this disease." (p. 39.)

As in the former report, the beneficial effects of the removal of the insane to an asylum is first dwelt upon. These are most evident in cases depending on the abuse of intoxicating liquors, on poverty and want, or on domestic strife and discontent.

In a case of insanity caused by the patient being occupied in so-called table-turning, a cure was effected by at first strictly avoiding all mention of the subject, and by subsequently pointing out the injurious tendency of such things in a sensitive nervous system. (Zutphen.)

Some instances are communicated where the patients were convinced by reasoning; these are, however, very rare. Other cases are quoted where recovery was obtained by opposing depraved inclinations; or, where it was necessary, by isolation, by more or less restraint, or even humiliation, of which Dr. Roëll has recorded a remarkable example.*

"The more calmly, and at the same time mildly, a medical man speaks, the greater impression he usually makes on a highly excited or raving patient; and we often see the most striking results from adopting such a method. The example quoted by Dr. Roëll is highly illustrative of human nature. The case was that of a violent insane patient, who, after having trampled a lantern in pieces, caught the doctor by the legs and nearly threw him down. But Dr. Roëll tapped him gently on the shoulder, and in a friendly tone requested him to let him go, as he should otherwise fall; which the patient immediately did, violent as his rage had been against six servants. He subsequently acknowledged that that friendly request in the midst of so much severity on the part of the others had made him let go his hold.

"The controversy as to whether we should give the preference to psychical or bodily modes of treatment is a very unprofitable one; this depends entirely on the circumstances of each particular case. In general, it may be said that where the mental derangement is the result of a morbid material change in the brain—and this is almost always the case—the somatic treatment deserves the preference; the psychical is then auxiliary and subordinate. But in other instances, where the disturbances are more sympathetic, without permanent changes in the organ, the psychical should be preferred, although it also will be less efficacious if it be not supported by its sister.

"To this class, where psychical treatment occupies the first place, belong the unhappy objects who present themselves more or less as idiots or simpletons, with defective development of both mind and body. What may in such instances be effected by school education, combined with due attention to nourishment, exercise, and bodily health, is exhibited by many very remarkable examples supplied by several such unfortunate individuals who come forward as so many witnesses of the excellent instruction given to them at Meerenberg.

"The happy results attainable in this mode, and the striking proofs that a non-developed and completely slumbering mind may be aroused, as it were, out of darkness by unwearied patience and perseverance, are evidenced by the

* Statistical and Clinical Communications from the Asylum at Dordrecht for 1857 and 1858, by Dr. Roëll, in the Journal of the Medical Society (Tijdschrift der Geneeskundige Maatschappij), No. 21, pp. 5–7.

following cases quoted from the records of the institution at Meerenberg." (p. 41.)

Having quoted the cases, for the interesting details of which we must refer our readers to the admirable report of the inspectors, the authors add:

"Such examples prove incontestably the value of suitable school instruction in the institutions, in the majority of which the necessary arrangements have, however, as yet not been made; or of a special school for idiots, such as exists at the Hague. But also in non-idiots, whose insanity is the result of disease, a powerful impression on the mind may sometimes effect recovery, but in that case the material causes which have produced the disturbance have already wholly or partially given way." (p. 42.)

"Sometimes patients recovered, who had long been under treatment in an institution, simply by good feeding and occupation; occasionally such recoveries took place suddenly and unexpectedly. (Meerenberg.)

"Many, and some remarkable cases have been reported to us of recovery, sometimes from an apparently incurable state, in consequence of the supervention of intermittent fever." (p. 43.)

The authors observe that,

"Usually it is important, if such an attack of intermittent does not become dangerous, to employ no remedies against it. A very remarkable example of this was communicated to us from Meerenberg. The case was that of a young woman aged three-and-twenty, who had of late years suffered from intermittent fever. In 1857 she was seized with typhus, resulting in mania with great violence, and excitability, especially of the uterus. Treated alternately with preparations of iron, cupping, blisters to the neck, cold affusions, and finally moxa on the head, she was in 1857 admitted into the institution in a very debilitated but excited state. Under suitable treatment her health rapidly improved; her sleep became gradually better with the use of three grains of pure opium every evening, when an intermittent fever manifested itself, and at first irregularly, subsequently under the quotidian form, increased in violence. As she became quieter and calmer during the intermittent, nothing was done for it; she became mild, and tolerably clear in intellect. But as the intermittent affected her too severely, preventing her leaving her bed, rendering her legs œdematous, her face pale and puffy, and her pulse weak, it could no longer be left to itself. On the 7th of January quina was administered; the ague ceased, but the patient almost immediately became again as excited and boisterous as before. After some time the intermittent fever returned; it was left to itself. The patient's psychical state improved, but her constitution became again so seriously affected that it was necessary once more to have recourse to quina, which was again attended with the same result. When finally the ague returned for the third time, it was completely left to itself, attention being directed solely to supporting the strength, until at last in the beginning of April it ceased spontaneously. Subsequently the patient daily improved so much, that in the beginning of July she was able to leave the institution recovered. (Meerenberg.) In several cases it was observed what a beneficial effect ague may produce, sometimes under apparently hopeless circumstances. Occasionally the ague ceased spontaneously before the patient was perfectly recovered, and later, on a return of the intermittent, recovery ensued." (p. 44.)

On account of the importance of the fact, the authors add some additional examples of recovery supervening on the occurrence of ague.

The foregoing remarks apply, however, to *sporadic*, not to *epidemic* ague. Epidemic ague may produce mania, and its cure is beneficial in cases of insanity; while the reverse obtains in sporadic intermittent. Attention on the part of the physician is therefore requisite to prevent mischief resulting from his practice.

"As ague has its principal seat and starting-point in the cerebro-spinal system, it would appear to be excited by some change and special action therein, and may be, at least in part, the result of this change and natural cure. But if the ague be produced by a miasm, a fresh injurious morbid exciting cause exists, which by no means gives rise to such beneficial results. It would therefore appear not to be so very much the ague itself, as the altered condition of the cerebro-spinal system producing the ague, which has a salutary action in insanity." (p. 45.)

The report is so full of valuable cases and sound practical remarks, that we scarcely know how to make a selection. We shall therefore for the present proceed consecutively, abridging details as much as possible.

"A remarkable case is recorded of a woman, who for twenty-eight years had been in a constant state of melancholy depression, had to be fed by force, resisted dressing and undressing, and whose moral feeling appeared to be completely blunted. This woman was attacked with cholera, and we succeeded, although with great difficulty, in bringing her through the paralytic stage, the result of which was that a complete change speedily took place in her mental powers, she recovered perfectly from her insanity, and was able to leave the institution (Rotterdam). Hence it is clearly seen, as is satisfactorily confirmed by numerous cases, how long a prejudicial cause may continue to act upon the brain, without producing an incurable degeneration; a fact much more frequently demonstrated in women, however, in whom there is more sensibility than vascular action than in man, where chronic inflammation and incurable disorganization are more rapidly developed. Some such cases occur, nevertheless, in men also. Thus, mention is made of a man of sanguineous temperament, who had been for five years insane, during four of which he had been ineffectually treated in the institution; moreover, he stammered, but this was independent of his malady. Without any assignable cause he suddenly got well, and his recovery was permanent ('sHertogenbosch). A very remarkable case is recorded, of which the following is an abstract. It is one of the most singular which have occurred. It is that of a young Israelite. Born in 1838, he was, after having led in poverty a dissipated life, in 1854 admitted into the hospital at Veenhuizen, with nearly complete paralysis of the legs, supposed to be the result of rheumatic myelitis, together with such exalted sensibility that, although he could not move a toe, he was intolerant of the slightest touch there. After some antiphlogistic treatment with leeches, cupping, and internal remedies, the myelitis was subdued, the exalted sensibility was diminished, but the paralysis continued. Subsequently strychnia was given, but he could not bear the powders, which he took irregularly, as they produced spasms, distress, and cries on the approach of any one, which phenomena ceased on the administration of morphia. But on continuing the latter in large doses, his state again got worse, sleep was obtained only in snatches, and ended in loud cries. Hereupon he was, in the end of July, transferred to the institution for Israelites at Amsterdam, where we were ourselves witnesses of his very singular condition.

"He was of short stature, affected with paralysis of the legs, so that he could not stand, much less run. He sat gathered together, and even when sitting had to be supported and fastened on a chair, to prevent his falling off.

His two hands were so strongly closed by contractions of the flexors, that the fingers could not be opened by any degree of force. The power of speech was wholly wanting; he made only a barking sound, exactly resembling the barking of a dog, which animal he imitated in everything. He lapped his drink like a dog, and bolted his food, which he held between his contracted hands. Strangers or persons who provoked him he growled or bit at, while to those who were known or agreeable to him he manifested his pleasure by licking them. When he saw a cat he endeavoured to attack her, and had to be kept back by the attendants. These phenomena were the only indications of sympathy, or feeble traces of any mental powers, which he exhibited. In other respects he was in a perfectly apathetic state. His peculiar dull look and features, the shaking of his head, and his mouth almost always employed in mastication, gave him, in fact, such an animal appearance, that his form was scarcely human. All his excretions were involuntary. In consequence of the incompleteness of the above quoted report of his state at Veenhuizen, there was much obscurity as to the causes and earlier circumstances of his disease. On account of his extremely weak and cachectic condition, a milk and animal diet was prescribed, with tepid baths, and infusion of arnica with decoction of bark. He now increased in strength, but his state continued in other respects the same. Chalybeates likewise proved useless. An attack of intermittent fever yielded to quina, without producing any change in his mental state. In the beginning of 1855 slight epileptic attacks supervened, which however diminished after the insertion of a seton in the neck, and the administration of flowers of zinc in ascending doses, and finally ceased. The seton was kept open for nineteen months, and as he was in the same lamentable state in 1856, his case might be termed one of cynomania; his appearance alone was improved. Opium was now given in large doses, up to ten grains, under which treatment some improvement seemed to take place, inasmuch as the patient manifested more interest in what passed about him. This treatment had, however, to be given up, in consequence of the supervention of colliquative diarrhœa. When this was subdued, his lost strength was restored by the exhibition of bark and preparations of iron, after which, in the summer of 1857, a fresh trial was made with opium, but its continuance was again prevented by the return of the diarrhœa. But on recovery from the latter he began to pay more fixed attention to everything. His secretions and excretions became normal; he took an interest in watching the sports of his fellow-patients, and he was attentive to his clothing. He was now taking arnica and chalybeates alternately. In 1858 his improvement proceeded more rapidly. He began to stand firmly upon his feet, and to jump and run; gave signs of approbation or of disapprobation at the domino-playing of his fellow-sufferers; his hands were still contracted, and the paralysis of the tongue continued; his barking diminished, and at last he commenced himself to play, to draw with a pencil held between his closed fists, and, though very badly, to write some lines. An attempt was now made to open his hands after having previously etherized him, but its success was very imperfect, incipient spurious anchylosis having apparently set in. On this account his medical attendant caused him, painful as it was, to place a little ball between the fingers of each hand, while the joints were rubbed with mercurial ointment, the ball being enlarged every two days—a plan which was attended with such decided success, that, at the end of four weeks, he had obtained the free use of his fingers and hands. The paralysis of the tongue continued, and a trial having been made with an electro-magnetic machine, the latter acted so strongly on his nervous system that its use could not be repeated. It was worthy of note that during these four years his hair and nails had not grown. On opening his hands, which had all that time been closed, the nails appeared as if they had just been cut. At length this remarkable patient was able, on the 12th of April, 1859, to leave the institution recovered, and went to America. But the affection of his tongue, which rendered him speechless, still continued.

"Subsequently his physician received an account, that in a violent storm he had regained the power of speech. It is very remarkable that he did not retain a trace of recollection of any part of the long course of his illness, and that the entire period is to be regarded, so far as he is concerned, as a deep sleep. A letter, written by himself on the 14th of April, 1859, to the governor of the institution, proved the completeness of his recovery. He had previously, on the 22nd September, 1858, with his still closed hands, written a letter to his parents, expressing his surprise at finding himself in the institution at Amsterdam, and stating that he well recollected that on leaving them before his illness he had entered upon an evil course of life. He expressed his satisfaction at the great improvement which had taken place in his state, but lamented that he was still deprived of his speech and of the use of his hands." (p. 48.)

"Of such a case," add the inspectors, "we shall probably never be able to give a physiological explanation."

Remarkable instances of spontaneous recovery occur from time to time, as proofs that we must never be too ready to despair. Having exemplified this in the case of a man, aged fifty-three, whose insanity had lasted for eleven years, the authors observe that—

"Such cases, of which several, though not of so long standing, have been reported to us, are extremely important. They prove that tolerably well-marked organic changes in the brain may take place, which nevertheless may subsequently disappear; for such long-continued disturbance of the imagination and of the understanding cannot, in the present state of science, be supposed to exist without simultaneous organic changes in the brain, which changes are, as we shall subsequently see, if they be only carefully looked for, constantly discovered on post-mortem examination. Perhaps it is also possible that an organic change in the brain, if limited to a small spot, after the first state of irritation has passed off, may cease to communicate its injurious influence to the other parts, and being now disorganized, and no longer acting, the mental powers cease to be disturbed; just as it is well known that portions of the brain may be lost without any impairment of the mental powers—even, as has been observed in more than one case, where the parts removed have been such as are most closely connected with the exercise of the higher intellectual powers." (p. 49.)

"Several cases have been communicated, especially from Zutphen, of mania with lofty ideas (which phenomenon always depends upon irritation or chronic inflammation of the anterior lobes of the brain), in which the perverted ideas continued long after the disappearance of the irritation, until at length they ceased spontaneously."

In such cases, as well as in cases of onanism, bodily labour is one of the most efficacious remedies.

Many interesting and valuable cases are brought forward, illustrating the beneficial effects of tartar emetic, iodide of potassium, digitalis, opium, lupulin, camphor, henbane, laxatives, &c.; but as these are chiefly corroborative of the results we have briefly quoted in our abstract of the preceding report of the inspectors, we need not here dwell upon them.

In speaking of opium, the authors observe:

"In mania, too, opium has been given, administered in a particular manner.

Very recently it has been specially recommended by Le Grand de Saulle.* This physician begins by giving one grain of opium daily, and increases the dose to ten grains in the twenty-four hours; when under its use, the maniacal excitement usually increases, in which case he suddenly interrupts the treatment, and now, according to his statement, recovery very rapidly ensues: this method he has advantageously employed, particularly in incipient cases. Dr. Ramaer, too, has tried it in four men and twelve women. In seven of these patients, with whom various modes of treatment had before been adopted, and who were affected either with dementia, or with excitability and hallucinations, this plan had no result. In six women—namely, two labouring under hysterical mania, two under periodic mania, one under paralysis agitans, and one under moria—it produced the violent excitement described by the writer, which, however, after giving up the medicine, only in one case passed into recovery. In two others it produced a sedative effect, subsequently passing into convalescence; in one there was improvement." (p. 58.)

The inspectors add the following well-grounded caution:

"This method would appear to require to be employed with great caution in mania; and where the latter is the result of meningitis, might very easily be injurious. Only in cases of more nervous excitement ought it to be tried, and in such opium often produces very good effects."

Respecting the use of opium, they add:

"That the sexual impulse is rather excited than depressed by the use of opiates is beyond all doubt; on which account digitalis, camphor, and lupulin appear to deserve a preference in certain cases. Dover's powder seems to be too little used; it is an excellent antispasmodic, and is in this respect perhaps to be preferred to morphia. Dover's powder and opiates are also to be highly recommended as emmenagogues.

"Chloroform, too, is occasionally advantageously employed in cases of sleeplessness. In a case of idiopathic inflammation of the brain, the vigilance, but not the maniacal phenomena, was removed by chloroform; after which the other symptoms yielded to the use of tartar emetic with nitre (Zutphen). Sometimes chloroform produces only temporary alleviation." (p. 59.)

Having referred to the cases of a lad of sixteen, of two hysterical women, and of a girl, in which the administration of anthelmintics led to the expulsion of worms and the speedy recovery of the patient, the authors remark that:

"In the insane, worms very seldom appear to be connected with the disease; they seem much more easily to act injuriously in the more sensitive system of children. In adults, the presence of ordinary lumbrici is very often not observed, except in going to stool." (p. 63.)

"The number of cases reported to us is very great, in which chalybeates, bark, and arnica had effected a cure, or had established the strength of patients already convalescent.

"But it must be borne in mind, that in all these cases, mostly in consequence of poverty and want, or of the abuse of strong drink, or other causes, debility and cachexy have occurred. Now these ultimate causes in different constitutions produce totally different results; in one, mania; in another, melancholy, with or without religious fanaticism; in others, dulness. When one of these forms is present from weakness and a depraved state of the fluids, the strength must be increased, sanguification improved; in some, too great

* Annales Medico-psycholog., 1859; and in the Nederl. Tijdschrift voor Geneeskunde, 1859, p. 251, by Dr. Ramaer.

vivacity must be moderated by opiates; in others, the too blunted sensibility must be excited by arnica, and sometimes by nux vomica. Yet all this may occasionally be effected without these remedies, by good feeding, exercise, and occupation; and herein lies the key to the fact, that many patients, when admitted into asylums, recover without medicines, because the food is so much better than in their wretched abodes; nevertheless, this does not prevent our attaining the same beneficial results much more rapidly by good food and occupation, when the latter are accompanied with the use of steel or bark, arnica, phosphoric acid, &c.

"But if the exciting cause—the abuse of strong drink, for example—can be removed without the use of medicine, and the system be at the same time strengthened by more nourishing food, recovery generally ensues very rapidly." (p. 65.)

"The effects of work, and of the remedies just mentioned, in strengthening the system, are not the same. If we can induce a patient to undertake fatiguing work, and if a good appetite is combined therewith, it will be much more beneficial than opium, steel, and bark: it promotes sleep, it very much increases the metamorphosis of tissue in the body, and there is nothing which so powerfully diverts the mind. But patients, at least those of the higher class, cannot always be prevailed on to work hard." (p. 65.)

"It is of the utmost importance to investigate the true nature of the affection and the state of the patient's system, and to modify the remedies accordingly. To expect to be able to cure sleeplessness by means of opiates alone, would be to take a very one-sided view of the question. In some, camphor is a narcotic; in others, as we have already exemplified, sleep follows the administration of tartar emetic. In these maladies the physician must endeavour perpetually to enlarge his stock of remedies, so as to be able to make a suitable choice in each particular case." (p. 66.)

"It is very necessary not to confound dulness with dementia, the latter being a form and result of meningitis, while dulness is the effect of pressure in the brain, whether caused by œdema or great congestion of the blood. Sometimes great exhaustion gives rise to insensibility, inactivity, or semi-paralysis of the brain, which is too often looked upon as incurable silliness." (p. 67.)

The influence of cutaneous eruptions or of external local lesions is sometimes remarkable. Thus, an anthrax in the neck may, through the irritation communicated to the medulla oblongata, become a cause of insanity, of which an example was furnished by the asylum at 'sHertogenbosch. On the other hand, a case of violent mania, which had passed into the chronic form, and threatened to end in incurable paralysis, and had resisted all treatment, was cured by the development of a number of furuncles, supervening on a state of general dyscrasia, characterized by an extremely itchy eruption on the legs, passing into ulceration, and by the formation of a traumatic abscess on the under lip (Meerenberg).

A case is related where intense melancholy, with tendency to suicide, seemed to be produced by strangury, the result of tuberculosis of the kidneys. The authors remark that diseases of the kidneys and affections of the bladder very rarely occur as causes of insanity.

The evil effects of general bleeding, dwelt upon in their former report, are again referred to. Even the local abstraction of blood is now much more rarely resorted to than formerly. The authors suggest that in cases where the ears especially are very red, and spinal irritation exists in the upper part of the neck, cupping to the nape appears in general to deserve the preference. But where the nose, cheeks, or forehead are high-coloured (excluding the passive blue colour of the first), where we have increased irritation of the brain and strongly pulsating carotids, leeches to the nose are preferable; by the latter mode more blood is taken from the branches of the cerebral carotid arteries distributed to the nose as ethmoidal branches, while by cupping the neck, the supply is drawn chiefly from the vertebral arteries and the occiput. As it has been shown by post-mortem examinations, that in mania proper, with derangement of intellect, the anterior lobes are principally affected, while in morbid states of the disposition and in melancholy, the upper and posterior lobes are chiefly engaged, it is important to attend to this distinction.

Many cases are reported, especially of spinal irritation, in which issues, setons, and stimulating external applications contributed largely to recovery. Very frequent mention is made also of the value of both local and general baths and douches.

The number of epileptics in the asylum at Meerenberg continued very large. In 1858, 99 out of 478 patients in the institution, or one-fifth of the entire number, laboured under epilepsy. In 1859, the number was reduced by deaths and the admission of a smaller proportion of such cases to 73.

As the autopsies recorded in the present report tend in general to confirm the results obtained on the preceding occasion, of which we have given a tolerably fully abstract in our former notice,* we need not at present dwell upon them.

The following sections of the report are devoted to the subjects of the domestic administration of the institutions; of quiet, order, and cleanliness; of public worship; of instruction; of labour; of amusements; of means of restraint; of food; of clothing and bedding; of warming and lighting, &c.

To the report are appended the usual full and elaborate statistical tables. The inspectors feeling, however, the difficulty of ascertaining with precision the proportion of cases ending in death and in recovery, as well as of those found to be incurable, in consequence of the fact that the number of recoveries, for example, and of admissions in any given year, refer to different persons, have prepared a table deduced from their elaborate "consecutive tables of the results of the treatment of the insane prior to 1844, and in each of the years 1844–1859, admitted into the medical institutions for the insane in the Netherlands," which affords the most certain statistics we as yet possess upon the subject. Having from these authentic data found that all the men and women admitted in certain years, the earliest of which is 1844, into certain of the institutions, had, on December 31st, 1859, ceased to be inmates of the asylums, they ascertained what had become of these individuals, amounting in the aggregate to 550, of whom 363 were men, and 187 womem. From their calculations it

* Vol. xxv. of this Review, p. 28.

appears that of the men thus admitted during sixteen years, 46·3 per cent. had died, 12·7 per cent. had been dismissed uncured, while 41 per cent. had recovered. Of the women, 38·5 per cent. had died, 16·6 per cent. had been dismissed uncured, and 44·9 per cent. had recovered. Of both sexes together, 43·6 per cent. had died, 14 per cent. were dismissed uncured; while 42·4 had recovered.

Such a mortality as 43·6 per cent. may appear large, as, for example, in England it is stated to be much less. But this is the result of a different and less certain mode of drawing up the statistics. Nowhere has the proportion of recoveries as yet reached 50 per cent., whence it follows that more than half the total number of patients have either died or have been removed uncured; and of the mortality of the patients after leaving the asylums we have, of course, no account.

On the numbers thus obtained the authors remark:

"This important result, that of 100 admitted, 42·4 have recovered, is the more favourable, as among those dismissed uncured are some who, at the time they left the institutions, were more or less improved; of whom a number, not indeed accurately known, but still not very inconsiderable, became after dismissal either greatly improved or completely recovered, as we have positively ascertained with respect to some.

"Further, it appears from the foregoing that more men have died than have recovered, while on the contrary more women have recovered than have died; a result completely agreeing with what we have stated in our report for the years 1851–1853."

It is seldom that it would occur to royalty to visit such an abode of misery as a lunatic asylum. An interesting feature in the present report is presented to us in the following paragraph:

"In conclusion, we may state that on the 11th of August, 1858, the asylum at Meerenberg received the high distinction of a visit from Her Majesty the Queen. Her Majesty was pleased to inspect the entire institution with the greatest interest, to signify her great satisfaction with the same, and to converse most graciously with several of the patients and officials. In our immediately preceding report we were enabled to communicate a similar proof with respect to the institution at the Hague, of Her Majesty's benevolent sympathy in the fate of our unhappy insane. Such proofs are, by the many patients whose minds are not yet closed to all noble sentiments and impressions, really valued and long remembered."

In the present notice we have again been led, notwithstanding the promise we gave in the concluding paragraph of our last review on the same subject, to confine ourselves almost exclusively to the more purely medical portions of the report. Into doing so we have been irresistibly led by the great interest of the subjects there brought forward. These portions of the report, it is scarcely necessary to observe, are the production of the medical inspector, Professor Schroeder van der Kolk. When we consider the vast amount of practical information which this distinguished man has now from time to time placed on record in his successive reports, we cannot avoid giving expression to our earnest hope that his present serious illness, alarming accounts of which reach us as we write, may issue in recovery, and that he may long be spared, not only to complete his contemplated work on the pathology and treatment of insanity, but to continue the brilliant series of investigations upon various subjects, with the results of which he has already so extensively enriched his profession and the scientific world at large.*

Review IV.

1. *A Practical Treatise on the Diseases of the Sexual Organs of Women.* By F. W. von Scanzoni. Translated from the French of Drs. H. Dor and A. Socin, and annotated, with the approval of the Author, by A. K. Gardner, A.M., M.D., Professor of Clinical Midwifery and the Diseases of Women in the New York Medical College.—*New York.* pp. 669.
2. *A Practical Treatise on Inflammation of the Uterus, its Cervix and Appendages, and on its Connexion with other Uterine Diseases.* By James H. Bennet, M.D., &c., &c. Fourth Edition.—*London,* 1861. pp. 600.
3. *Amputation of the Cervix Uteri.* By J. Marion Sims, M.D., Surgeon to the Woman's Hospital, New York.—*New York,* 1861. Pamphlet.
4. *The Principles and Practice of Obstetrics.* By Gunning S. Bedford, A.M., M.D., Professor of Obstetrics, &c., in the University of New York.—*New York,* 1861. pp. 731.
5. *Placenta Prævia; its History and Treatment.* By William Read, M.D.—*Philadelphia.* pp. 340.
6. *Essay on the Use of Anæsthetics in Midwifery.* By J. Fordyce Barker, M.D., Professor of Midwifery and Diseases of Women in the Bellevue Hospital Medical College, New York. (Reprint from Transactions of New York Academy of Medicine. Read Nov. 20, 1861.)

Of living German gynæcologists, the name of Scanzoni carries with it peculiar authority. Yet, till within a very recent period, he was but little known to English practitioners, and this is the first of his works, we believe, which has been translated into the English language. Though enjoying so great a reputation on the Continent—so great, indeed, that he was brought to St. Petersburg to assist at the accouchement of Her Majesty the Empress of Russia—still it does not seem that this high fame has been acquired by the discovery of any startling novelty in doctrine or practice. Judging of the man by the work before us, we should say that he is more characterized by caution and prudence than by any of that love of novelty and of bold innovation which has made the character of some eminent men in our own day. And certainly at a time like the present, when there seems to be a strong tendency to over-estimate the value of surgical and mechanical

* As in the case of Dr. Rilliet, whose work we referred to at p. 33, we have in like manner to record the death of S. van der Kolk, which has taken place since the above notice of his labours came from the printer, and of whom a short obituary notice will be found at the end of the Review.

treatment in the diseases of the uterus, and to depreciate the use of medical and general measures, we are truly glad, for the sake of rational medicine, to see a man in his influential position giving expression to moderate and qualified views on the controverted points of gynæcological therapeutics. There can be no possible doubt but that the uterus has now to bear the blame (formerly laid upon the liver) of many disorders of females, with which disorders it is only secondarily connected; and the active topical treatment employed for the subjugation of these supposed uterine diseases, has in countless instances laid the foundation of new diseases, mental or bodily, or both combined.

The work before us by Professor Scanzoni, may to a certain extent be regarded as the joint production of *four* different physicians. In the first place there is the treatise of Scanzoni, which appeared in German about the year 1856—at least, his preface bears date of August 20th of that year. Of this the original treatise, a French translation with notes by Drs. Dor and Socin, revised by Scanzoni, appeared in the summer of 1858. And lastly, we have this American edition, translated from the French, and "annotated, with the approval of the author," by Dr. Gardner, of New York. The notes and additions of the several translators occupy but a very small space, however, in the volume. As to the general scope of the work, the American translator has somewhat exaggerated when he says that "in the etiology, pathology, and therapeutics of female diseases, with all the improvements which have been realized during the last twenty years, this volume is exceedingly rich." The author himself does not claim anything like so much. He says:

"This work must in some respects complete our 'Treatise on Accouchements.' It, therefore, must not astonish any one if we have not entered into the numerous details upon the subject of the affections of the sexual organs of woman peculiar to pregnancy, labour, and the lying-in."

And again:

"In the present work we rely, above all, upon our own observations. We do not at all desire to make a simple compilation; therefore, as much as possible, we have avoided citing and judging the opinions of other authors."

The work is all the more valuable in our estimation that it is not a compilation, but a full exposition of the opinions and experience of its talented author.

As Scanzoni's original work has been already reviewed in this journal (vol. xxi.), we are saved the trouble of making any lengthened notice of it on the present occasion. As embodying the views and the practice of so high an authority, we are truly glad to see a translation of it in our own language. It will at once take rank as a standard work of reference; and Dr. Gardner's notes, interspersed throughout the volume, tend to augment its value. In the chapter on Vaginal Fistulas, he introduces a full description of Marion Sims' mode of operating for the cure of vesico-vaginal fistulæ. As this is now so well known, we need not further allude to it.

This same surgeon—Marion Sims, of New York—has published the account of a new mode of performing the operation of amputation of the cervix uteri. This is effected by cutting obliquely upwards and inwards towards the cervical canal, from a little below the insertion of the vagina into the cervix. This he prefers doing with a scissors. Particular care is, of course, required to avoid wounding the peritoneum. The two—anterior and posterior—raw surfaces are then brought into apposition, and confined there by a few sutures of silver wire, a roll of lint or bit of bougie being introduced to preserve the patency of the cervical canal. Dr. Sims details ten cases in his paper (the title of which is given in the heading of this article) where the cervix uteri was excised in this manner, and with uniform success so far as the immediate effect of the operation was concerned. Upon this point the evidence contained in his paper is conclusive, and proves this plan of operating to be superior to any other. But what the ulterior results of the operation were—that is to say, whether the symptoms these women laboured under, and for the cure of which they submitted to this formidable procedure, were completely and permanently removed thereby—is a question upon which we would be glad to have fuller and more distinct information; for, after all, this is the true criterion by which to test the utility of any such operation. In four out of Dr. Sims' cases we are left in doubt as to the ulterior effects of the operation upon the patient's condition. It is chiefly for hypertrophy of the os and cervix uteri, with or without prolapse, that Dr. Sims has recourse to amputation. In this respect he follows Huguier. Upon the general propriety of this operation we must as yet altogether abstain from expressing any opinion. The French translators of Scanzoni thus speak in a note upon this subject, referring to Huguier's practice: "The cases observed up to the present time are not sufficiently numerous to form the basis of a definitive judgment as to the value of this treatment."

Of the elaborate treatise of Dr. Bennet we need do no more than mention the fact of a *fourth* edition having been required. He tells us he was for two years revising this for the press, and that in its present state it may be considered to embody the matured experience of the many years he has devoted to the study of uterine disease. In reference to the practice which has just been considered (viz., amputation of the cervix uteri for chronic hypertrophy), one passage may be quoted from Dr. Bennet's work, as showing his opinion upon the point. He observes:

"In my opinion, amputation should be restricted to the cases in which congenital or hypertrophic elongation of the cervix resists all other means of treatment, and is a cause of permanent and irremediable distress, or of matrimonial incapacity, or of probable sterility. Thus limited, it will rarely have to be performed. I do not recollect having met with more than three or four cases of hypertrophic elongation in which amputation could have been rationally performed on anatomical grounds, and even in these exceptional cases, when I saw the patients, the necessity for an operation did not exist on the above grounds. Under other social circumstances, or at a later period of life, however, I believe that in these very instances it might have been advisable, or have become advisable."

A very large acquaintance with uterine diseases leads us to coincide most fully in the above observations of Dr. Bennet.

The work of Dr. Gunning Bedford, which we now proceed to examine, professes to be, as its title implies, a complete systematic and practical treatise upon obstetrics, brought up to the existing state of science, and embracing the anatomy, physiology, signs, symptoms, and diseases of pregnancy, parturition, and childbed. All these subjects, with physiological disquisitions arising out of some of them, are discussed in forty-six lectures. Assuredly so comprehensive a course was never perhaps before delivered. It is illustrated with numerous woodcuts of a plain kind, and with a few lithographs (somewhat too highly coloured) of the mammary areola and nipple, at different periods of pregnancy, copied from the plates of Montgomery.

Dr. Bedford is favourably known to us as the author of an English translation of 'Chailly's Midwifery,' and of a volume of clinical lectures on the 'Diseases of Women and Children,' a review and a notice of which appeared in this journal (Nos. for Jan., 1856, and Oct., 1860). The work before us is evidently the result of much labour and research. It contains a vast deal of information, and that of a recent kind, upon nearly all the subjects connected directly or indirectly with midwifery. It is supplied with a copious table of contents, a full and carefully prepared alphabetical index, and an alphabetical catalogue of the authors referred to, and the pages at which they are quoted. It is therefore provided with every means to enhance its value as a book of reference. But still, in our estimation, it does not bear the stamp of a practical work, and we believe all our readers understand what this means. To produce such a book was one of the objects the writer had in view; "to present," he says, "to the profession a practical book—one which will develope the phenomena of parturition in their various phases as they occur in the lying-in room." Further on he tells us: "The great object in teaching is to simplify, as far as it may comport with the nature of the subject discussed, so that the chief end of all instruction may be accomplished—viz., to be useful." We most heartily echo this sentiment. It should be the ruling maxim of the physician in all he says or does in the lecture-room, at the bedside, and at the desk. When tested by this standard, however, we find the work to be somewhat deficient. One cause for this is the defective classification which the author pursues. It is too artificial; the subjects are not disposed of in any sort of natural connexion or sequence. He divides all labours into two great classes—*natural* and *preternatural*—the eutocia and dystocia of Merriman. The first of these classes includes presentations of the vertex, face, breech, the feet, and the knees, because in any of these Nature can by her own resources accomplish the expulsion of the child, if nothing arise to complicate the delivery. The second class he divides into two orders, according as the delivery is accomplished by *manual* or *instrumental* assistance. Such a division, it is obvious, can be of no use whatsoever in the lying-in room, for this simple reason—that, till the labour be completed, we cannot know to what division it belongs. This classification is only adapted for a clinical report, where a number of cases are to be distributed under distinct heads. It is true it will take in all the subjects to be treated in a work like the present, and in doing this its purpose may perhaps be considered fulfilled. But more should be aimed at, and may be attained. A classification may be "useful" as well as methodical. How admirable, for example, is that of Denman; for, whilst having a natural foundation, it is also eminently practical. It comprehends, too, all the subjects which require to be discussed in a system of midwifery.

Another drawback to the practical utility of this volume is the disproportionate amount of consideration bestowed on several matters; some, of the weightiest importance and of frequent occurrence, being treated very cursorily, whilst others, of a speculative kind or extreme infrequency, are discussed at great length. For example, mammary inflammation and abscess, "one of the banes of the lying-in room" (to use Dr. Bedford's own words), "inflicting upon the patient intense suffering, and oftentimes leading to tedious and protracted convalescence," is disposed of in about three-quarters of a page; whilst inversion of the uterus, an accident of extreme rarity, occupies ten pages. Rigidity of the os uteri, an everyday cause of protracted labour, is very imperfectly described, and its treatment anything but satisfactorily laid down; but lengthened and minute directions are given for the application of the forceps for extracting the head in breech and footing cases. A detailed account of the symptoms developed in the course of a difficult or tedious labour—which symptoms constitute the most valuable index of the patient's condition and the most reliable guide to her management—is nowhere given; though the different positions of the fœtus when presenting with the back, belly, or chest—presentations which the experience of the Dublin Lying-in Hospital shows never to occur at the full term—are given with great minuteness. The important subject of rupture of the uterus is dismissed in a few lines, but Cæsarean section has thirty pages devoted to it; and a physiological disquisition on the "determining cause of labour" extends to twenty pages. The author's researches upon this point lead him to the conclusion "that there is a necessary connexion between this first spontaneous movement in the muscular walls of the uterus, and, if I may so term it, a matured development of the muscular structure of the organ itself." That, in fact, as he elsewhere expresses it, "the *primum mobile* of uterine action, when gestation has been completed, is a physiological necessity." Now we put it to the candid reader interrogatively, what the author does affirmatively, "Whether this theory does not prove less satisfactory than the opinion of Avicenna—'that at a fixed time labour takes place by the grace of God !'" (?)

One of the most important questions to decide in the management of difficult labours, is whether the fœtus be still alive or not, as this must always exert a most material influence upon our practice.

"The decision of this question is one of no ordinary import, and it, therefore, is the duty of the accoucheur to exercise a full measure of discretion, in order that he may reach the truth; and, above all, let him be cautious not to suffer himself to be led to a hasty conclusion from the mere love of bringing the child into the world piecemeal. Whether it be really a love for this kind of thing, or an indifference to the shedding of innocent blood, I will not undertake to determine; but of one fact I am quite confident—the perforator and crotchet are oftentimes employed in this metropolis with a recklessness altogether startling to those who suffer conscience to have its share of influence in the doings of the lying-in chamber."

It is now admitted, we believe, by most British accoucheurs, that in auscultation we can have, as a general rule, a reliable index of the death or vitality of the fœtus during labour; and that, except in some rare cases, it is the only sign upon which dependence can be placed for the solution of this question. It redounds to the infinite credit of the Dublin School of Midwifery that it was among the first to appreciate the full importance of auscultation in practical midwifery, and that in the great Maternity Hospital of that city the use of the stethoscope in the management of labour is taught to every pupil, and resorted to in every case. The value of such a means of diagnosis can scarcely be overrated. It was therefore with much surprise that we found Dr. Bedford appropriating only three lines to the examination of this most important and pre-eminently practical subject. To some we may appear exaggerating the value of this means of diagnosis, but a close attention to this subject throughout a pretty extensive experience justifies us in speaking as we have done. It may be safely laid down as a general proposition, that a person familiarized with obstetric auscultation can hear the sounds of the heart of a living fœtus after the rupture of the membranes in every case of labour at the full term. But an objector at once says, Is it possible to fail of hearing the sounds? for, if so, their inaudibility in any given case is valueless as a test of the child's condition. This question we answer in the affirmative, but we deny the corollary therefrom. That is to say, we admit that on some rare occasions the sounds though present may not be discoverable; but it does not at all follow from thence that we are never to regard their absence as proof that the fœtus is dead. There are certain circumstances which may obstruct the transmission of the sounds; if any one of these be present, of course the negative result of auscultation is without value; but in the absence of any of these hindrances, the inaudibility of the fœtal heart by a competent auscultator, on careful examination after the escape of the liquor amnii, may be received as evidence of the child's death. Nor do we think it absolutely necessary for arriving at this conclusion, that comparative observations shall have been made during the course of the labour, as some writers have laid down, though this gives additional confirmation to the inference. The principal obstructions to the conveyance of the sounds are these—viz., a distended bladder; a tympanitic state of the lower belly (a very common symptom in labours protracted in the second stage); continuous or frequently recurring pains, so that a sufficient interval of rest is not allowed to permit of a proper examination; extreme restlessness also on the part of the patient may prevent a sufficient exploration of the uterus; and lastly, the head being very low in the pelvis, in which case the cardiac sounds, if weak, will not be heard in the usual situation, but in the median line, and immediately above the symphysis pubis.

The author's directions for the management of twin labours betray some little inconsistency, though the practice inculcated is, on the whole, accordant with that laid down by the most experienced authorities. As many sad accidents have happened to patients, and great obloquy has arisen to practitioners from the mismanagement of these labours, it is of great moment that the true principles on which to conduct them should be clearly understood. The average length of the interval between the births, in Dr. Collins's large collection of cases, being a very short one, Dr. Bedford infers it as "incontestably true that the general rule is that nature, if left alone, will speedily cause the second child to follow the delivery of the first." But he seems to forget that these cases were nearly all subjected to treatment, and not left to nature; and it was part of Dr. Collins's plan of treatment (as we learn at p. 311 of his 'Practical Treatise') to rupture the membranes of the second child when half an hour had elapsed from the birth of the first. And this rule of practice, based on the soundest physiological principles, is followed by accoucheurs of the highest experience. Dr. Bedford, however, is rather opposed to it. According to his "own experience, it is far better practice to commit the entire management of the second child to nature, all things being equal." And he asks, "is not a twin labour, in strict construction, two successive parturitions developing the same phenomena, and consummated by the same means?" To this negative interrogation we must reply in the negative. The birth of the second child is not a complete process of parturition, because there is no first or dilating stage; it is a labour *minus* the first stage, and therefore all the successive phenomena belonging to one complete parturition are not developed. In reference to the cases where delay does occur in the expulsion of the second fœtus, he observes:

"There can be no objection to the employment of ergot in these cases, provided always that the child presents naturally; for the uterus is apt, through previous effort, to become more or less defective in action, and the influence of ergot will oftentimes be very marked in evoking its contractility. Should, however, these means fail in producing the expulsion of the fœtus, it will be proper, after waiting two hours, to introduce the hand, and bring down the feet; or if the head have descended into the pelvic excavation, the forceps should be had recourse to. The necessity as well as the economy of this mode of practice are abundantly sustained by the important fact that, according to accurate observation, the second child will usually be sacrificed if more than two or three hours elapse after the birth of the first.

"In twin labours it is important that the accoucheur should not leave the room of his patient until the delivery of the second child has been completed; this, as a general rule, should be scrupulously observed. As I have mentioned to you, there are occasionally some exceptional cases in which a compliance with this precept would not be practicable, for there are instances on record in which the second child has not been expelled for two, three, or more weeks subsequently to the birth of the first. Therefore, while in the observance of the general rule, it will be well to bear in mind the exceptions."

It appears to us that the practice laid down in the former part of this quotation, and which we consider highly judicious, would, nevertheless, effectually preclude the possibility of the occurrence of one of the "exceptional cases" mentioned in the latter part of the quotation. At all events, it was evidently incumbent on the author to have informed us how to recognise a case in which art is not to interpose for the accomplishment of the second labour. Had he done so, he would clearly have the merit of introducing a new principle altogether in the management of plural births.

The cases of labour where the funis prolapses have long been an odium against midwifery, especially those cases in which re-position is found to be impracticable, and the head has not yet come within safe reach of the forceps. Dr. Bedford notices the "postural treatment" of prolapsed funis, lately recommended by Dr. T. Gaillard Thomas, of New York, but he does not appear to have made trial of it. We happen to have done so on two or three occasions, and can safely say it will be found a useful auxiliary to the taxis, but by no means a substitute for it: indeed, Dr. Thomas did not claim this much for his plan. Version, though praised by Denman and Dewees, and highly so by Mauriceau, Boivin, and La Chapelle, has found little favour among British accoucheurs of our own time. It is just one of those points where theology and practical midwifery come in contact, and upon which, consequently, the religious tenets of the parties concerned must exercise more or less influence. The following are the observations of Dr. Bedford upon this point:

"The ostensible and only justifiable argument in favour of version in cases such as are now under consideration, *is that it will afford the child the best means of safety*. But while, on the other hand, we are prompted to do so much for the child, we are not to forget that the safety of the mother has claims equally urgent, which cannot be lightly regarded by the accoucheur. How often is the life of the mother involved in peril in the operation of version, and how often, alas, does this peril terminate in her death! You see, therefore, that in selecting the alternative, you must be governed, not by the abstract fact that the funis is prolapsed, but by a due consideration of all the surrounding circumstances. You are to consider, whether, in full view of all the facts of the case, *turning* presents the greatest promise of safety to the child, without compromising the life of the parent. If my own opinion is worth anything on this question, I should advise you, no matter how imminent may be the danger to the child, never to have recourse to version, except under the following conditions: 1. The head at the superior strait not having descended into the pelvic excavation. 2. The mouth of the uterus soft and dilatable, readily permitting the introduction of the hand. 3. The pains must not be characterized by great vigour, for this would not only be a serious obstacle to the introduction of the hand, but would prove a substantial ground why version should not be attempted, for the reason that efficient and regular contractions would be likely to terminate the delivery more rapidly than it could be done by turning. 4. There should be no pelvic deformity, or at all events very slight."

From the concluding sentence of the above extract the reader will doubtless be led to infer that Dr. Bedford is not favourably disposed to the operation of turning, on account of pelvic contraction. And such an inference would be perfectly correct. For on referring to his observations upon this subject, we find that he entertains very nearly the same objections to the proceeding that Professor Meigs does, and prefers trusting to nature, as long as circumstances will justify it, and then having recourse to the forceps, "if the antero-posterior diameter do not measure more than three and an eighth inches." Indeed, he calls in question the accuracy of some of the fundamental principles on which the practice is based. We are left in doubt whether the author, or Dr. Meigs, has had any experience of the operation in this particular class of cases. If they had, it is much to be regretted they did not bring forward its results in support of the opinions they have expressed.

The author is a strenuous opponent of the operation of craniotomy, and he tells us that "in the fulness of his faith," he has no hesitation in saying, "that if the child be alive, the woman at the completion of her pregnancy, and if it be made manifest that the maternal passages are so contracted as to render it physically impossible that a living child can be extracted *per vias naturales*, I should, between the two resources—craniotomy and the Cæsarean section—not hesitate to decide in favour of the latter." The crotchet he designates as being "in more senses than one a murderous instrument;" and that as it is never resorted to except in cases where craniotomy is indicated, "its chief danger, therefore, regards the mother." The child being always dead when the crotchet comes to be used, we should have thought that its sole and only danger was to the mother; how great this danger may be altogether depends on the unskilfulness of the operator. Following in the track of Simpson, Tyler Smith, and others, he measures the danger of the operation of craniotomy by the mortality which has *followed* its performance; wholly overlooking the material influence which the existing circumstances of the patients at the time of the operation, in a large proportion of cases, must necessarily have had in bringing about a fatal result. In the last edition of his 'Midwifery,' Dr. Churchill shows the gross average mortality of craniotomy cases to be about 1 in $5\frac{1}{2}$. Separating the cases which occurred in private practice, he finds the mortality to be very much less among them, namely, 1 in 29. Much though this operation is to be condemned, when not rigidly confined to the cases justifying its employment, still, let this be done on the real and true grounds, and not the assumed one of its being necessarily and inherently more dangerous to the mother than other modes of artificial delivery.

There are several other parts of this volume that we intended quoting or commenting upon, but our typographical limits will not allow us. We hasten on to the last chapter, devoted to the subject of Ætherization. Whilst engaged in its perusal, Dr. Fordyce Barker's pamphlet reached us, in the course of which he makes extracts from the chapter before us, and thinks that the teaching of Dr. Bedford is calculated to leave an impression "of great doubt and uncertainty as to the propriety of anæsthetics in midwifery." We hardly think Dr. Bedford's observations go so far as this. He admits there is much diversity of sentiment among accoucheurs in reference to the particular circumstances justifying anæsthesia. With some in America as well as here, it is the rule to give chloroform in all cases of labour, how auspicious or natural soever they may be. This Dr. Bedford thinks is "really abusing a good thing;" and we believe him to be right, though the reasons he adduces are not such as would carry home conviction to our own mind. We do not see, for instance, any force in the argument that, "the female, at the most interesting period of her life—the time of labour—should, all other things being equal, have her mind unclouded, her intellect undisturbed, her judgment fully adequate to realize and appreciate the advent of a new and important era in her existence—the birth of her child." This is, no doubt, very fine writing, but we can see no force in it, and we presume neither does Dr. Barker, as, though he quotes the passage, he yet does not name the author.

The obstetric profession in New York is still divided as to the relative advantages of chloroform and ether inhalation. Professor Bedford tells us he has, "some time since, abandoned the use of chloroform, and has recourse exclusively to sulphuric ether, which he has always found safe and reliable." In this selection he is sustained by the report of the committee specially appointed by the Boston Society for Medical Improvement, to inquire into the alleged danger of ether inhalation; which report states that "there is no recorded case of death known to the committee, attributed to sulphuric ether, which cannot be explained on some other ground equally plausible, or in which, if it were possible to repeat the experiment, insensibility could not have been produced and death avoided." Dr. Barker, on the other hand, very much prefers chloroform to any other anæsthetic, on account of (1) its more agreeable odour; (2) its greater activity and more rapid effect; and (3) from its allowing us to regulate the degree to which we may desire to carry anæsthesia with a certainty and security that is not possible with ether. He further states, that there is an absence of all proof that in any single instance has death resulted among the thousands of midwifery cases where chloroform has been given. Both these authors agree that it is only in exceptional cases, or when anæsthesia is carried to its maximum, that the uterine action is materially interfered with, and the labour process lengthened. Dr. Barker has seen it retard the labour more frequently in the second than first stage, and this is concurrent with general experience. Since the year 1848, he has administered chloroform to 786 midwifery patients. He describes three classes of cases in which he has found it to shorten the labour:—1st. In all cases where inefficient uterine action results from loss of sleep and exhaustion, in consequence of a prolonged first stage. 2nd. In rigidity of the os uteri and perinæum. 3rd. Where the pains are diminished or suspended by vivid moral impressions or hysteria, as Professor Murphy first pointed out; or by pains resulting from the coincidence of some malady, such as rheumatism of the uterus or other muscular tissues, gripings in the intestines, cramps, &c. On the whole, his conviction is, that chloroform accelerates labour in a greater number of cases than it retards. We do not altogether comprehend the drift of his observations on its employment in the *removal of adherent placenta.* He writes—"I have been called upon to perform this operation but twice since I have been accustomed to rely upon the aid of an anæsthetic. Its value in these cases is beyond all controversy, and I shall not stop to dwell on this point." We presume he cannot possibly mean *morbidly* adherent placenta; but even in retention with normal adhesion of the placenta, we do not see what aid the chloroform can render, except in so far as it may lessen the difficulty of extraction.

The first of the general conclusions arrived at by the Boston Committee states that—"The ultimate effects of all anæsthetics show that they are depressing agents. This is indicated both by their symptoms and by the results of experiment." This brings before us a point of much practical importance as to the use of chloroform. Professor Barker thinks the above statement needs to be greatly modified in order to express a scientific truth, and he cites two cases to show that chloroform may, under certain circumstances and conditions, have effects quite the opposite to depression. Both these were patients about to undergo capital surgical operations: one, that of tying the external iliac artery (for aneurysm); and the other, amputation of the thigh on account of severe burn of the leg and knee-joint, under the immediate effects of which the man was still labouring. Both patients were in a state of great prostration, and in both marked reaction ensued after inhalation, the pulse becoming full and equable, and the surface warm. In the former of these patients the collapse was purely the result of fear and apprehension, so that we can easily explain how the chloroform acted in bringing about so marked an improvement. In the other case the collapse would seem to have been the result of nervous shock. Here the *modus operandi* of the chloroform is more difficult to understand. These cases are, no doubt, deserving of careful consideration; but still, we should be inclined to regard them as exceptional, the general rule being that anæsthetics are, *cæteris paribus*, depressing agents.

We now take up Dr. Read's work on 'Placenta Prævia,' which forms the twenty-third volume of "The Library of Practical Medicine, published by order of the Massachusets Medical Society for the use of its Fellows." The literary history of the subject of placenta presentation, and the clinical statistics of this anomaly, have but lately undergone such thorough investigation, that it showed no small degree of courage in Dr. Read to take the subject in hand, and to go over the ground that Lee, Simpson, Trask, Churchill, and Barnes, with many others, had so laboriously explored. Undeterred by such predecessors in the same path of inquiry, he has with infinite pains and care collected all the authentic facts relating to placenta prævia, and sifted them with great ability and discrimination. A prominent object with him, from the beginning to the end of his undertaking, was "to make use of no cases, and to quote no opinions, at second-hand, or through intermediate channels of communication; and to point out the source of every fact or statement quoted." By this course one common source of error in researches of this kind is avoided—namely, that arising from the vague or inaccurate reports of cases by translators or compilers. All the cases of which he could not consult the original reports are omitted in the tables, but are not excluded from the work, being thrown into an appendix, where they are tabulated together. To do all this imposed a vast deal of trouble and research on the author; but still it was the only proper way to insure accuracy. By this cautious selection of facts, and judicious collation of them, Dr. Read has rendered valuable service to his profession, and has considerably enlarged and strengthened the foundations of obstetric science. As this work is mainly one of compilation, and as the subject on which it treats is well-nigh exhausted, it is unnecessary that we do more than briefly notice some of the general practical inferences deducible from Dr. Read's admirable statistical tables.

The gross number of cases of placenta prævia that he has been able to collect amounts to 1628, and of these 380 died; in other words, 1 in 4½. This shows a smaller mortality than that obtained by Professors Simpson or Churchill; but as it is based on a much larger number of cases, it is probably a nearer approximation to the actual ratio than theirs. The average frequency of placental presentation he calculates to be once in twelve hundred cases. The first chapter is devoted to the history of our knowledge of this deviation, and gives a brief sketch of the opinions entertained respecting it by the leading obstetric authors, from Ambrose Paré downwards. The author is at issue with Dr. Robert Lee as to the views of Guillemeau, who, Dr. Lee asserts, was aware that the placenta in cases of placenta presentation originally adhered to the neck of the uterus, and that its separation was the cause of the hæmorrhage. In opposition to this, Dr. Read maintains that it is impossible to perceive how, by any torturing of the text, the conclusion arrived at by Dr. Lee can be drawn from it. The whole thing turns on the interpretation of a passage in Guillemeau's work, and we therefore leave the controversy with the philologists to settle.

The physiology of placenta prævia, its mode of production, and the special causes of the hæmorrhage, are questions which receive their full share of consideration in the second, third, and fourth chapters.

In Chapter V. he enters upon the important subject of "treatment;" and it is chiefly with a view of testing the success of the different modes of treating this complication of labour, that his tables are constructed.

Table I. gives all the particulars of 52 cases of *spontaneous separation of the placenta, the labour being completed by the natural efforts*; and it is most important to observe that all these cases recovered—at least, this fact is definitely stated of 43 of them, and in the others, the context plainly indicates that a favourable result took place. Of the children, 11 were born alive, eight being expelled along with the placenta, or by the next pain; 1 "very soon," 1 in five or six minutes, and 1 in less than twenty minutes." Table II. includes 26 cases of *spontaneous separation of the placenta, the labour being completed by* *artificial means.* Of these, 5 died, or a little over 19 per cent. Table III. contains a list of cases of *artificial separation of the placenta and natural delivery.* Of these cases, 31 in number, the per-centage of deaths is $6\frac{1}{2}$ nearly, very much below that of the former table. Table IV. gives all the details of 51 cases in which the *placenta was artificially separated and the labour completed by artificial means,* and in this class the mortality rises to $21\frac{3}{4}$ per cent. We must now let Dr. Read speak for himself.

"The four preceding tables comprise all the cases in which the placenta was entirely separated from the uterus before the birth of the child. They have been arranged in such a manner as to bring together those cases which resemble each other most closely, and have been analysed accordingly. Before entering upon the next division, it may not be amiss to group together the results already obtained, and re-arrange them for greater convenience of inspection. We accordingly find, that in the 160 cases enumerated,

141	lived.
18	died.
1	recovery very uncertain.

"This gives, after discarding the case in which no result is given, a per-centage of $11\frac{1}{4}$, or a proportion of deaths of 1 to 9, within a very small fraction. A mortality much greater than what Professor Simpson derived from the 141 cases published in his collected works, and which is there given as 1 in 14. . . . . If we add to the 160 cases in the four preceding tables, in which the result is stated, 107 cases with 7 deaths, quoted by Professor Simpson, and 18 cases, with 4 deaths, from Dr. Trask's, neither of which are included in these tables, for the reasons already specified, we have a grand total of 285 cases, of which 29 were fatal.

"Calculated upon this number, the per-centage of deaths is $9\frac{1}{4}$, or 1 in $10\frac{3}{4}$ nearly. This ratio is much nearer to that obtained from my own tables, than either Professor Simpson's or Dr. Trask's estimates; and when the number of cases is taken into account, may be assumed as the probable general average mortality to the mother under all circumstances, when the placenta is separated before the delivery of the child."

Did our limits permit, we should gladly follow Dr. Read through his analysis of these instructive tables; as it is, we have confined ourselves to one point—viz., the maternal mortality. With regard to the effect of separation of the placenta, whether spontaneous or artificial, upon the hæmorrhage, he finds, in the great majority of cases, this has ceased, but that there are exceptions enough to make it far from a universal rule. Dr. Read considers it proved by these cases beyond a doubt, that it is not the *separation* which puts an end to the flooding, "but that when this has been done, the uterus is put in a condition for its contractile power to operate to the best advantage; and if enough vitality remains in the system to insure condensation of its walls, the obliteration of the vessels cuts off the supply of blood, and the hæmorrhage is at an end."

A comparison of the very brief extracts above given of Dr. Read's first four tables, bring out this remarkable fact noticed by him—viz., that there are fewer deaths proportionately, where, after *artificial* separation of the placenta, the child is born by natural labour pains, than where, with spontaneous separation, assistance is required to deliver the child. He also demonstrates, by a reference to these same tables, that the course pursued in reference to the delivery of the child, is more important as having an influence upon the life of the mother, than what is done in reference to the placenta. In other words, whether the placenta has been spontaneously or artificially separated, is of small moment in comparison with the manner in which the delivery of the child is effected, whether by nature or art. Dr. Read gives three other tables which exhibit, among other points, the maternal mortality to be $11\frac{1}{3}$ per cent. in cases of *partial separation of the placenta and natural delivery*; 25 per cent. in cases of *partial separation of the placenta and artificial delivery;* and in the cases where the *placenta was perforated and child delivered,* the maternal mortality was $27\frac{1}{2}$ per cent. In this last group, the deaths were confined to the cases *artificially* delivered after perforation of the placenta, and amounted to 34 per cent. among them. Comparing the general results of *separation* of the placenta, and *perforation* of the placenta, with artificial delivery afterwards in either case, Dr. Read finds the difference is in favour of the former by 14 per cent. Of the entire number of cases of placenta prævia tabulated by Dr. Read, more than two-thirds come under the denomination of partial presentation—

"Showing most conclusively, that the method proposed by Professor Simpson will be available in only a small fraction of cases, and that the old practice of turning and delivering by the feet, without disturbing the connexions of the placenta any more than is necessary for the purpose, must, after all, be our main resort, and the method which in the mass of cases coming under our charge, will most likely be required."

Although we have thus been led to extract more of the numerical details of this treatise than we had originally intended, still we cannot close this notice without quoting one or two of the conclusions in the "General Summary."

"The danger to the mother is materially increased by artificial delivery. But the same statistics which show this result, also make it evident that this increased fatality is owing, *not so much to the operation itself,* as to the enfeebled and exhausted condition of the mother at the time; and that, with a favourable condition on the part of the mother, there is no more danger in resorting to it in placenta prævia, than in ordinary cases of difficult labour.

"When from the rapidly failing condition of the mother, or the presence of any cause rendering artificial delivery impossible, a resort to the foregoing (i.e., forced delivery) is forbidden, the placenta should be wholly separated from the uterus, and such remedies made use of as will recruit the strength of the mother, until, reaction having been established, she can be delivered in whatever way may be deemed best."

The copious extracts we have made from this work of Dr. Read's sufficiently attest our opinion of its merits. The impartial and philosophic manner in which he has used statistics for the purpose of deciding grave practical problems, is worthy of all praise and imitation.

Review V.

Public Health in relation to Air and Water. By W. T. Gairdner, M.D., &c. pp. 369.

Air and water may emphatically be said to be elementary in relation to health, and indeed to life itself, and Dr. Gairdner has well chosen them for the subject of his popular discourse. This was the first reflection we made on opening his book. From his preface we learn that the greater portion of it had been delivered in the form of lectures to a mixed class, "partly composed of students of medicine, and partly of persons otherwise interested in the subject of public health." An audience of this kind seemed of good omen, as promising real instruction, and not the *ad captandum* address, to which there is so great a temptation in lecturing to those who are not prepared by education to be enlightened by the *siccum lumen* of science. The perusal of the work has tolerably answered this our expectation; for, honestly, we cannot say that it has perfectly done so, the style here and there being somewhat too ornate and diffuse. We are disposed, too, to think that a religious tone has been unnecessarily and to disadvantage introduced, which is most apparent in the introduction. In matters of science, to which the topic of public health should strictly be relegated, the less that is said of Providence and of the Divine Being the better. Bacon wisely exhorted the non-commingling of science and of religion, insisting on the hazard from the commingling them of injury to both. In the present instance, a severe logic might lead to some conclusions, the consideration and adjustment of which would be inappropriate in these pages. Thus, going back with our author to the earlier times and to the Levitical law, we find enforced by religion strict rules for the preservation of the animal-man in his most vigorous state; that law, a sanitary code which, Dr. Gairdner says, ought, according to "a distinguished English Divine, to become the actual code of our modern cities." Now, this Levitical code, we know, was altogether set aside on the introduction of Christianity, and was accompanied not by an ameliorated, but by a deteriorated sanitary condition. How striking in this respect is the contrast, if we compare the Greek and Roman policy in sanitary matters with that of a later period of the Middle Ages, when the religious Christian spirit was the moving spirit of the times.

In the introduction the author gives a brief sketch of the history of sanitary science, restricting it to its latter and advanced period. The beginning of this he places little beyond a quarter of a century—viz., 1832, when, owing to the outbreak of cholera, so unexpected and alarming, public attention in England was first, he thinks, seriously directed to the subject, and real and material progress was made, mainly owing to the exertions in the cause of a few zealous men, chiefly of the medical profession. All honour, doubtless, is their due. Then unquestionably the greatest start was made in sanitary improvements, such as are still in course, and almost as much needed as ever.

In so limiting the period, he has, however, hardly been just. How well and with what excellent effect he might have noticed amongst our benefactors in the cause Captain Cook, that great navigator, who returned from a voyage which lasted three years and eighteen days, extending from 52° north latitude to 71° south, with the loss of only one man of his crew, and that owing to a complicated and lingering disease. The means employed which were so successful, are described by him in a paper communicated to the Royal Society, and published in the Society's Transactions for 1776. For it he was awarded the gold medal of that year. The sanitary circumstances he laid most stress on, and to which he attributed the uninterrupted health of his crew, were good ventilation, cleanliness of ship and men, the use of varied and wholesome food, including a certain proportion of vegetables, and an ample supply of water, and that as fresh as possible. His paper occupies barely three pages. Never, perhaps, in so few words, were there more important instructions delivered on the method to be taken for preserving the health of a ship's crew. They are applicable generally, and as much almost to landsmen as to sailors, to the inmates of houses as of ships.

We find from a passage in the introduction that Dr. Gairdner belongs to the strictest sect of contagionists, which we regret. Describing some of the destructive epidemics of the Middle Ages, connected with overcrowding in towns and the total neglect of sanitary precautions, he says:

"Then it was that a terrible idea, unhappily but too well founded, though exaggerated by fear and ignorance, acquired a new ascendancy over the minds of men; the idea of *contagious pestilence* (the italics are his), walking through the crowded streets, kneeling in churches, sitting at the social board, and everywhere dealing indiscriminate destruction. It was a natural idea; it was not, as some *doctrinaires* (again the italics are his) of later times have affirmed, an untrue or essentially mistaken idea. It was, on the contrary, essentially true, as regards the greatest number of these destructive plagues; and notwithstanding all the attempts made in these latter days to indoctrinate the public mind with the opposite belief, it is now more certain than ever that the sanitary reformer must face this terrible *fact*, and regulate his proceedings accordingly as he best may." (p. 12.)

Now, neither this statement, nor the language in which it is expressed, do we consider worthy of the author. There is a want of temperance in both; we cannot but deprecate such dogmatism, especially on the part of a lecturer of reputation addressing a mixed audience. It would be well were he to give a thought to the terrible evils which have resulted from the putting of his doctrine into practice, as witnessed in the system of quarantine, and the manner in which it has been conducted. Whenever there is a decided fear of contagion, there is a danger of that "frenzy" which he describes as pervading society in past times when there was an outbreak of epidemic disease. And the same fear, if unchecked, if supported by such language, must always have a like effect. In former numbers of this Review we have given examples of the kind; how under such a dread, for instance, of yellow fever, all the laws of humanity have been laid aside, and this within a very recent period. If there be one subject more than another that requires to be carefully considered and examined on the inductive, strictly scientific method, it is that of contagion; and if one more than another ought to be on his guard not to exceed the limits of truth, or of, in other words, what has been established by such induction, it is the public teacher. For our own part, we believe that those men whom Dr. Gairdner designates as belonging to the *doctrinaire* class, have been of the greatest service to society; such men as Mr. Chadwick, Dr. Southwood Smith, and other members of the Board of Health, and the Health of Towns Commissions, and not least in their very laudable inquiries regarding those diseases commonly believed to be contagious, endeavouring to arrive at practical results, not from abstract considerations, not from theoretical views, but from the examination of trustworthy evidence or facts.

Of the main body of the work we think most highly. Written by one who has evidently devoted himself very much to the subject, it contains a great deal of valuable information, and many suggestions likely to be useful if followed, as regards the administration of a public health service and its organization. To those who have not made hygiene a special study, we can highly recommend the book, in spite of a somewhat dogmatic style which here and there pervades it.

The author's great argument has been to show, that "it is not less the duty than the interest of the community to see that the means of personal and domestic cleanliness, the free use of the commonest and most indispensable of God's gifts—air and water—are within the reach of all, and that they are, within certain limits, properly applied." This he has amply shown; and to those who have any doubt on the matter, the details he has entered into can hardly fail to carry full conviction.

The limits to which we are restricted forbid our attempting an analysis of the book, or commenting to any extent on the many passages which we have marked as open to question; but we must not pass them by altogether.

Some points of his doctrine, etiologically considered, seem to us to be open to much objection. Water he holds to be the vehicle of the contagious matter of cholera; and it may be, he thinks, of yellow fever and of remittent fever. Atmospheric air tainted with a bad smell, he holds to be, *quoad* the offensive smell, an indication of malaria, and providentially designed to denote it, and put us on our guard against it. These are conclusions which do not appear to us to be borne out by the facts. There are decided facts proving that epidemic cholera has had its origin from another or other causes. How else is the sudden and great outbreaks of this terrible disease, which have so often happened, to be explained? or how their often sudden cessation? That water of a certain quality may predispose to the disease, it is easy to imagine; even mental emotion, fear of the malady, seems to have an influence of the kind. When the common cause is present, any predisposing circumstance may powerfully aid in promoting its spread. That a bad or unpleasant smell, or any peculiar smell, is not necessarily connected with malaria, almost all intelligent travellers in malarious countries are agreed. How frequently is the opposite the case? Where does the air seem purer than in the arid Roman Campagna? Where more grateful to the sense than in the Borgise gardens and the gardens of the Palatine, perfumed with the odour of flowers in the spring and early summer! If bad smells were to be considered the index that Dr. Gairdner dwells on, Edinburgh, his own town—at least, its old part—ought never to have been free from endemic fever; yet in the worst part of that old town, typhus fever has been only an occasional visitant. We remember the time, and for the space of three years, that typhus fever was almost unknown in its infirmary; the only cases admitted during that term being from the crew of a Russian ship of war that had put into Leith. Moreover, that happy time of exemption was too soon followed by an outbreak of the disease, which was almost, if not quite, as prevalent amongst the opulent and easy class of inhabitants of the New Town as in the poorest and most wretched of the Old. Regarding the causes of epidemics, must it not be confessed, that our positive knowledge is small; that the obscurity about them is great; and, consequently, that much caution ought to be exercised in treating of them; entering upon them in the spirit of inquiry, as a subject for further research, rather than in the opposite way, as if there were no problems belonging to them needing solution?

In that part of his book in which Dr. Gairdner insists on the necessity of a supply of water, we could have wished that he had been more definite and particular as to the kinds of water fit for use. This is a subject which deserves more attention than it has hitherto received; and from the circumstance of its being so suitable for chemical and microscopical examination, exempt from the difficulties and ambiguities in the way of the investigation of the nature of malaria, and the naturally fleeting causes of epidemic diseases, a very few of these excepted. By a certain class of officials, men half informed and with a smattering only of science, and a confidence in the inverse ratio of their knowledge, we have known really wholesome water condemned as unfit for use; and great costs in consequence incurred in procuring another supply, the main objection made to the condemned water being its hardness, that only in a moderate degree, and depending chiefly on the presence of carbonate of lime held in solution by carbonic acid, with which the water was impregnated.

Dr. Gairdner very properly insists on the propriety of diffusing knowledge amongst the people on matters of hygiene, especially as regards air and water. It would be well were the instruction to begin with the upper classes of society, those from whom our legislators are chiefly chosen, and the heads of the great departments of the public service. In a recent debate in the House of Commons, a member justly said, "our great men [our ministers and administrators] are very slow to be convinced, and unless the House of Commons urges them on [the subject under discussion was the merits of armour-protected ships of war compared with the old oaken fabrics], unless a lesson is taught on our shores by the burning of some of our own ships, they will be as slow as the men of the former times were," alluding to what he had before said of "the efficiency of the matchlock being doubted when it superseded the cross-bow; of the 'Brown Bess,' when it superseded the matchlock; of the Enfield rifle when it superseded the 'Brown Bess.'" We would add, it required the disastrous campaign in the Crimea to inaugurate an improved system in the army; and this not from want of information given, but from want of conviction in "the great men," the administrators-in-chief. Dr. Gairdner supposes that the fault lay in the defective knowledge of hygiene in the medical officers of the army. Let him consult the archives of the Medical Department, he will find ample proof that the imperfect state of ventilation of barracks and hospitals, and the improper quality of the soldiers' rations, with other matters bearing on the health of the troops, had been reported upon time after time, long before the siege of Sebastopol, but for most part in vain.* Nor was this surprising, considering how little science was valued by men in authority, educated as they mostly had been at our public schools, where, and at our universities, until recently, the physical sciences had been almost entirely neglected! What more could be expected, considering that the clothing of the army was exclusively directed by a board of general officers; and the erection of hospitals and barracks was left entirely to the officers of the Royal Engineers. The instruction, the education of the people in hygiene, as exhorted by Dr. Gairdner, is unquestionably a desideratum. But we should not be over-sanguine in our expectations

* Consulting these archives we find the following receipt for generating yellow fever written after the manner of Swift. Its author was the late Dr. Hugh Bone; it bears the date of Trinidad, 1818, where he was then serving as physician to the forces; we are tempted to copy it, as illustrating well what has been stated above:

"Take of soldiers lately arrived in the West Indies any number; place them in barracks in a low wet situation, or in the mouth of a gully, or on the bank of some dried river-channel, or on the summit of a mountain and to leeward of a swamp, or of uncleared ground, and where there is no water, or only bad water; give each of them only twenty-two inches of wall in their barrack room, built of boards, or of lath and plaster, and having neither galleries nor jalousied windows, but close window-shutters, and a hole or cellar under the flooring for containing mud and stagnant water, and holes in the roof for the admission of rain; and the windows only eighteen inches from the floor, that if open the men may be obliged to sleep in the draught of air; and let them have drill every morning on wet ground, when fasting, guard-mounting, and all kinds of fatigue, not in the morning and evening, but in the hottest time of the day; when on sentry no shed to keep off the direct rays of the sun; bad bread; putrid meat; few vegetables; plenty of new rum, especially in the morning; discipline enforced by terror and punishment, not by mind and preventions; an hospital similar to the barrack-room, always crowded, plentifully supplied with rum, scantily with water, and so ill regulated that they dread to enter it; a firm belief in the doctrine of contagion, and a horror of approaching any one affected with yellow fever; let these directions be attended to in Trinidad or even in Barbadoes, and especially when the air is stagnant or charged with noxious vapours subsequent to long droughts, the soldiers will soon die, some of them of yellow fever, some of them with black vomit, and those first in the rooms where my directions have been most carefully observed."

of any great and immediate good effect from it. Knowledge is one thing, practice connected with habits another. Where is the drunkard who is not sensible of his evil ways? That fresh air and plenty of it, and good water and cleanliness, are essential to health, are no new ideas. Who could have imagined a scene of recent occurrence, such as is described by Dr. Gairdner, in the Royal Infirmary in Edinburgh, at a time when epidemic typhus was prevalent, garrets crowded with cases of the kind lying on mattresses on the floor. "To visit them [Dr. Gairdner states] I had to walk along a narrow footpath-way between crowded beds in a room in which I could not walk without stooping; to feel their pulses and to look at their tongues, I had to kneel or sit down on the floor; and all the attention of the nurses was administered after the same fashion." p. 359. What a contrast this to the mode of providing for such cases most approved at the present time, and advocated by military surgeons, who, in the French service, and those men of the highest authority, such as MM. Larrey and Michel Levy, would have large hospitals abolished altogether, and the latter, even small hospitals, on the principle, founded on a wide experience, that congregation intensifies disease; and would have the sick and wounded treated in detached huts or tents, on the principle that segregation affords the best chance of recovery. It is gratifying to find how even some of the most terrible inflictions are not without their alleviations, how the destructive and the preservative power, as in the Hindoo mythology, are so often allied. Has it not been in the army and amidst scenes of war that more has been accomplished in the establishing the principles of hygiene, than in quiet, methodical civil life?

Amongst the measures suggested by Dr. Gairdner for the initiation of a right sanitary system, is that of inspection by persons with authority and competent for the duty. Such inspection, if leading to action, could not fail to be efficient. But without compulsory action, which he rather deprecates, we fear it would be of little avail. Take the instance of drainage and sewerage: in these matters, so essential to health, and now so much attended to in our prisons and barracks, why should our towns and villages be neglected? What a blessing it would be were every town and village throughout the country compelled to have its drainage in good order and its sewers of proper construction and clean. Left to the will of the people, the difficulties coming in the way of correcting sanitary evils are innumerable; there are so many clashing interests to contend with, so much ignorance, and not least, such uncertainty regarding the results of the methods commonly proposed. What we seem to want is authoritative interference exercised with judgment, based on science—that is, exact knowledge. We believe that in most of our villages and small towns, all the drains should be open, and that cesspools should be superseded by necessaries with receptacles formed of flags rendered water-tight by Roman cement, or other means. Were such used with the introduction of ashes, the abomination referred to would be got rid of, and our agriculture, threatened ere long with impoverishment, would be rendered more independent of imported manures, which are not inexhaustible.

We must hasten to a conclusion, resisting the temptation to discuss other interesting points, of which there are so many relating to hygiene, and which will be found more or less dwelt on in Dr. Gairdner's book. In parting from it, we will end with expressing the hope that it will be much read and critically, the subject being of the first importance as regards the welfare of society, and the manner in which it is treated needing, we may repeat, some hesitation of assent, especially in certain doctrinal portions of the work.

Review VI.

1. *Medical Climatology; or, a Topographical and Meteorological Description of the Localities resorted to in Winter and Summer by Invalids of Various Classes, both at Home and Abroad.* By R. E. Scoresby-Jackson, M.D., F.R.S.E.—*London*, 1862. pp. 509.
2. *Meteorological and Medical Observations on the Climate of Egypt, with Practical Hints for Invalid Travellers.* By Donald Dalrymple, M.D. Lond., F.R.C.S.—*London*, 1861. pp. 80.
3. *Mentone and the Riviera as a Winter Climate.* By J. Henry Bennet, M.D.—*London*, 1861. pp. 112.
4. *The Climate of Algiers in reference to the Chronic Affections of the Chest.* Being a Report of a Medical Mission to Algeria, presented to His Excellency the Minister of Algeria and the Colonies. By Prosper de Pietra-Santa, M.D., Physician to His Majesty the Emperor of the French, &c.—*London*, 1862. pp. 61.
5. *The Climate of Worthing: its Remedial Influence in Disease, especially of the Lungs.* By Walter Goodyer Barker, M.B. Lond. —*London*, 1860. pp. 71.
6. *The Climate of Brighton.* By W. Kebbell, M.D.—*London*, 1860. pp. 58.
7. *The Therapeutic Influence of the Southern Climatic Sanatoria, particularly with reference to Chronic Tuberculosis of the Lungs.* By Dr. Rullman, of Wiesbaden.—*London*, 1861. pp. 31.
8. *Bradshaw's Invalid's Companion to the Continent.* By Edwin Lee, M.D. Second Edition, with considerable alterations and additions.—*London*, 1861. pp. 416.
9. *Climates for Invalids; or, a Comparative Inquiry as to the Preventive and Curative Influence of the Climate of Pau.* By Alexander Taylor, M.D., F.R.S.E. Third Edition, condensed and considerably altered.—*London*, 1861. pp. 283.
10. *Bermuda: its History, Geology, Climate, Products, Agriculture, Commerce, and Government, from the Earliest Period to the Present Time; with Hints for Invalids.* By Theodore Gaudet, M.D.—*London*, 1860.

It is fortunate that the question, Where shall I go? so often put by the invalid who is about to expatriate himself, to his medical adviser, is less difficult to deal with than that awfully-sounding query of Crimean notoriety, Whom shall we hang? or even than that perplexing and under some circumstances important problem, Where shall we dine? which, if we are to draw any inference from the frequency with which it is obtruded on our notice in the advertising columns of the journals, presents to the bucolic mind when in London a source of daily distraction. That a satisfactory response to the former of the above questions is not so easily made as might at first sight seem possible, is indeed true; but any embarrassment which might be felt in shaping a reply, can hardly be attributed to lack of the materials from which to frame it. There is no department of medical literature which has of late years suffered under such a plethora of contributions, as the yet infant science of climatology. Every season—we had almost said every month—brings its quota of new works on climate, and though too many of them are little better than a *réchauffé* of well-worn and commonplace odds and ends, dressed up to suit the special exigencies of their purveyors, there are few which are totally destitute of interest either to the profession or the public. When there are so many to claim our attention, we cannot pretend to give more than a passing notice even to those whose merits are beyond the common average. On the present occasion we propose to glance at some of the more recent publications on this subject, and before doing so, to offer a few observations of a general character, which may serve in some respects to introduce them.

There are some branches of knowledge, the progress of whose development is necessarily slow, from the difficulties attendant upon the processes of observation which they entail; there are others in which the same tardiness is caused by the complexity of the phenomena contemplated, or by the difficulty experienced in distinguishing true from false facts, and in estimating the real value of the materials which individual observers have collected. Where, as in the case of the science of climatology, the obstacles to progress belong chiefly to the latter of these categories, our efforts will be considerably facilitated by our possessing a distinct conception of the conditions requisite for the attainment of the end we have in view. In the construction of such a system of climatology as shall be useful for medical application, these conditions are threefold. In the first place, it is necessary that we should possess accurate determinations of the meteorological and geological features of the various localities to which resort is had; their mean temperature, and the amount of daily and seasonal fluctuation which it exhibits; the gross annual and the daily fall of rain; the variations in barometric pressure; the exposure to prevailing winds; the nature of the soil and its vegetable productions, together with the general character of the surrounding country. These and other similar elements of information, if carefully collected and arranged, will enable us to form a pretty accurate estimate of the medical capabilities of each locality, and of its suitability for the evolution of definite physiological effects. In the next place, it is desirable that we should be provided with a sufficient number of reports of cases in which benefit has been derived from residence in the localities in question. These reports should contain precise statements as to the condition of the patients before and after resorting to them, as well as of the course of life pursued and the nature of the treatment, if any, adopted whilst there. Thirdly, we shall derive much assistance in our inquiries from a study of the rate of mortality which prevails amongst the native inhabitants of each locality, or amongst those who have become acclimatized to it, and of the diseases whose frequency in it may be supposed to indicate the influences which its meteorological and hygienic conditions exert. From each of these sources it is necessary that our information should be trustworthy and complete; uninfluenced by the too-frequent desire of the informant to give an undue colouring to the attractions of the place with which his own personal interests may be connected, and such as to enable us to estimate not only the average conditions of the locality, but the extremes within which those conditions may vary. Thus, statements as to temperature should not only include the *means* of the observations made, but the *extremes* from which they are deduced, and the rapidity with which the fluctuations between the mean extremes occurred.

Any information which omits to enlighten us on these points is radically defective, since the value of a given temperature as a therapeutic agent is directly proportioned to its freedom from sudden and extreme alternations, and a mere statement of its mean height can carry no argument in its favour, unless it can be shown that that height is on the whole a pretty constant one. In comparing the advantages of two localities in this respect we might be led into very serious error, if we were to assume that because the average temperature of the one was higher than that of the other, therefore the former would be a more desirable place of residence than the latter. For it might easily happen that in the one case the general temperature, though low, was uniform; whilst in the other the superiority of the mean might conceal extremes of fluctuation, which would be very disastrous to persons of a delicate constitution.

Of the sources of information to which we have referred, there can be little doubt that the first not only promises the most decisive and comprehensive results, but that it is the one which is least open to fallacy. For when we consider the difficulty which there often is in determining how much of the improvement obtained in any given case is due to therapeutic agencies which are comparatively simple in their operation, and whose application may be limited at will, we shall easily perceive how readily we may be led into error in estimating the relative influences which may have been exerted in a similar case by the complex moral, mental, and physical conditions which go to make up the sum of what we call climate. Nor should we exercise less caution in drawing any inferences as to the healthiness or unhealthiness of any locality from its death-rate, or from the prevalence or absence in it of certain diseases. The influences, whether favourable or unfavourable, to which these might be due, might affect only a certain class of the population, or might operate during but a limited season of the year. In this way a very unpromising aspect might be given to a locality which, under proper precautions, would be admirably adapted as a residence for invalids. Thus, whilst it was at one time alleged as an argument in favour of the suitability of Egypt for consumptive cases, that phthisis was almost unknown in that country, subsequent researches have shown that the disease not only exists, but is very prevalent in many parts of it. Are we, therefore, to conclude from this fact that the Egyptian climate is either unsuitable for, or predisposes to, phthisis? By no means; for the evidence of competent authorities has proved that the cases of consumption in Cairo and Alexandria occur principally amongst the Copts, Berbers, negroes, and other denizens of Southern Egypt who are imported to fill the harems or augment the forces of the Viceroy, and who are thus exposed to all the prejudicial influences which attend the exchange of a warm climate for a colder one, of a wandering out-of-door life for the unwholesome conditions of the seraglio or barracks, and, above all, of freedom for slavery.

Again, the suitability of a country for colonization is to be determined, amongst other things, by the rate of mortality which those portions of it to which emigrants have already resorted, exhibit. If we take Algeria, for instance, and compare the death-rate of Algiers with that of France, we find, according to M. Legoyt, that the maximum mortality of the latter country (which is found in the department of the Seine) is 27 per 1000; whilst, according to M. Pietra Santa, that of Algiers was, from 1852 to 1859, at the rate of 40·7 per 1000. The natural inference from this fact would be that the chances of death which a healthy man would incur in going to Algiers would be greater than those to which he would be exposed in the unhealthiest parts of France, in the proportion of 40·7 to 27. But the force of this inference would be considerably modified when it was known that the death-rate of Algiers had been swelled during the period referred to by several severe epidemics of cholera and small-pox, diseases of which the latter, at least, is entirely preventible; that four-fifths of the patients in the civil hospital of the town come from the interior of the province, and generally in a miserable state of health; and that a large portion, both of the native and immigrant population, is placed, through their own neglect, in the most unfavourable hygienic conditions, and such as strongly predispose to the attacks of disease. Taking these facts into consideration, there seems no reason to believe that Algiers is naturally more unhealthy than any other French town. The above illustrations, and numerous others might be quoted, will show with how much reservation any evidence as to the sanitary character of a locality as deduced from the rate of mortality, or the prevalence of specific diseases in it, must be received. We have adduced them, too, as indications of the difficulty which the climatological inquirer will often experience in drawing any positive inference from statistics of even apparently the most reliable character, and of the fallacies into which he may easily fall if he be too eager to embrace the conclusions which his facts may at first sight warrant.

When we consider the natural obstacles by which our attempts to form a comprehensive system of climatology are impeded, and the artificial ones which are superadded by the diffusion of the information necessary to this end over a multiplicity of works which are often neither very accessible, nor, when found, very concordant with one another, it is not surprising that the study of climate as a therapeutic agency occupies a less prominent place in the curriculum of medical education than its intrinsic importance would justify. Nor is it easy to suggest how an omission which we cannot deem other than serious is to be remedied. The number of subjects which are at present required for examination by most of the medical boards, is already sufficiently large to make it inexpedient to swell the list by the addition of any branch of knowledge that is not absolutely indispensable to make up the modest standard which constitutes the minimum of medical qualification. Much might unquestionably be done by lecturers on therapeutics, or by those who in some of our schools deal more exclusively with the subject of hygiene, to urge upon the student the importance of making himself acquainted with the relative capabilities of the principal varieties of climate. But the acquirement of this knowledge must, we fear, be in a great measure left to a post-studental period, and the verification of its acquisition be confined to the higher and more exclusive examinations which some few of the medical corporations maintain. There is, however, one class of students to whom a practical acquaintance with the subject of climate is imperatively necessary, and that is the class which supplies medical officers to the army and navy. A knowledge of the medical topography of the various countries to which our forces are distributed, of the principal forms of disease which prevail there, of the special modifications of diet and habit necessitated by a sojourn in them, and of the general hygienic precautions which are requisite for the preservation of health under sudden or extreme fluctuations of atmospheric conditions, is indispensable to the man upon whose daily consideration any one or all of these matters may be forced. In the regulations of the Army Board some arrangement is made for the instruction of assistant-surgeons in this department of their profession, and we have no doubt that the prelections of Professor Parkes do full justice to its claims. But we are not aware that any similar provision has been made for the medical officers of the navy. It is true that they are required before receiving their commission to pass an examination in "naval surgery and hygiene," but how far the latter may include an acquaintance with climatology, and if so, whether any facilities are given for its acquirement, we are completely in the dark. In the present unsatisfactory condition of the medical department of the navy, any attempt to impress upon the authorities the desirability of increasing the standard of qualification imposed upon candidates for admission into it would probably meet with but a cold reception. But when the service shall have been rendered sufficiently attractive to make it a matter of no difficulty to provide for its most pressing exigencies, we may hope that the same justice will be done to the sailor as is now shown to the soldier. Even as a matter of pecuniary economy, there can be no question of the desirability of making a general acquaintance with climatology imperative upon medical officers of the navy; for there can be little doubt that many of the severe visitations of zymotic disease under which our ships so often suffer when on foreign stations, are greatly, if not entirely, owing to that neglect of proper precautions which an ordinary knowledge of the laws of climate and hygiene would obviate. And whilst we are upon this subject, we may ask whether there is anything in the navy analogous to the admirable system for obtaining statistical information which was introduced into the army by Colonel Tulloch, and of which the climatological reports contained in the general ones presented to Parliament* are by no means the least valuable results? The regulations of the army require that every assistant-surgeon shall, prior to his promotion, forward to head quarters a "medico-topographical account" of the station in which he happens to be at the time quartered. We have no means of knowing whether a similar requirement is enforced in the navy, but we have no doubt as to its eminent utility both to the service and to the individual himself.† Although these reports may in some cases be founded upon crude or hasty observations, they must in the aggregate contain very valuable information which it would be difficult to obtain in any other way. Nor would the advantages derivable from the adoption of this system be confined to the advancement of an important branch of medical science. As is shown in the volume published by the Medical Department of the Army to which we have referred, such reports as these are the only means of efficiently checking the abuses which insensibly creep into every system at a distance from head quarters, and of making the most recent acquisitions of science available for the benefit of our forces in all quarters of the globe. Nor do we see any valid reason why the duties of a medical officer, whether of the army or navy, should not include the daily registration of all those phenomena, terrene, aquatic, or celestial, which constitute the climate of the place in which he may be located. The materials which would be thus collected, at little or no expense, would soon suffice to place the science of climatology upon a much more perfect footing than it at present possesses.

As we have before remarked, one of the great impediments to the study of climate lies in the diffuse and heterogeneous nature of its literature; and any attempt to arrange and condense into a compact form the information which is scattered over such a multiplicity of separate works must, even if not perfectly successful, be deemed worthy of very considerable praise. For this reason alone we were inclined to receive most favourably Dr. Scoresby-Jackson's treatise on 'Medical Climatology.' That such a work has been for some time a real *desideratum*, must have been felt on more than one occasion by every

* Army Medical Department: Statistical Sanitary and Medical Reports for the year 1859. Presented to both Houses of Parliament by command of her Majesty.

† We have since learnt, through the kindness of Dr. Kinnear, Deputy-Inspector of Hospitals and Fleets, that in addition to his certificates of conduct and servitude, all that is required from an assistant-surgeon R.N., previous to his passing of surgeon, is that he should keep a journal according to form.

medical practitioner whose library lacks the manual of Sir James Clark, or who, though possessing it, could not fail to perceive that it has fallen in some respects behind the progress of medical science. Dr. Jackson's qualifications for filling up this *hiatus* consist in a personal experience of the sanative influence of the principal winter resorts in the south of Europe and the north of Africa; in having passed three summers amongst the French, Swiss, and German spas; and in having availed himself of every work upon climate and cognate subjects upon which he could lay his hands. It is only fair to state that his book does full justice to the time and labour which he has expended on it. We cannot, indeed, say that it contains very much that is new—it would, perhaps, be more than we could expect that it should; but it is a very conscientious and painstaking *résumé* of the subject, with the additional merit of being pleasantly and often forcibly written. It is by no means the least of its recommendations, that it is conceived in a general spirit of impartiality, without any desire to unduly exalt one locality above another, and that it is intended more for the profession than the public. Not but that the unprofessional reader may peruse it with eminent advantage. Indeed, we hardly know a work which is likely to come much before the public in which the general advice on medical matters is more wholesome and unobjectionable, or in which a better tone is observed as to the relations which should exist between the medical man and his patient.

On the subject of hygiene, especially, Dr. Jackson's remarks are thoroughly practical; neither too concise to be intelligible, nor so minute as to leave nothing for the reader's common sense and experience to determine for himself. One in particular, which he repeats on several occasions, is so excellent that we cannot commend it too strongly to the attention both of medical men and their patients. When speaking of the advantage of choosing a residence which is well exposed to the influence of the solar rays, he observes that "invalids not unfrequently forget that the beneficial effects derivable from change of climate are to be found only in the sum of little things, and not in the specific action of a new atmosphere." If those who advise, and those who have recourse to, change of climate as a means of alleviating disease, would always remember that physical well being, like mental happiness, is dependent upon conditions that often appear so trifling as to be unworthy of attention, how much needless suffering and expense would be avoided, and how many opportunities of recovery now hopelessly lost, would have been economized and usefully employed!

In discussing the subjects of Food, Exercise, Bathing, and Choice of Residence, Dr. Jackson's remarks are generally so appropriate as to indicate that his acquaintance with the difficulties that beset the invalid in his search after health is not merely a theoretical one. In reference to the latter of these points, he gives the following useful caution:

"There is, however, one circumstance to be particularly remembered—namely, that in removing to what is understood to be a mild winter climate, at least in southern countries, the traveller will meet with houses which are built, not so much with a view of being warm in winter, as of being cool in summer; and hence, especially in Italy, they are constructed in such a manner that the sun's rays can rarely, if ever, gain access to the interior of the apartments, which are usually lighted from an inner court. Such houses are cold and gloomy in winter, and should give place in the invalid's selection to a warmer and more cheerful edifice."

And here we may take the opportunity of referring to another error which the experience of Dr. Jackson, and of all who have come much in contact with invalids abroad, concurs in showing to be one into which they are very prone to fall; and that is, the neglect to provide themselves, before starting, with a sufficiency of warm clothing, under the delusive idea that in the southern, and therefore *tropical*, regions to which they are going, such encumbrances are unnecessary.

"To these general remarks," says Dr. Jackson, "I need only add a word of caution to invalids who are about to spend a winter in a southern climate. It is often erroneously supposed that warm clothing, overcoats, shawls, and such-like articles of dress will not be required in the country whither they are going. This is a great and sad mistake to fall into. Be it the south of France, Italy, Algeria, Egypt, Madeira, or any of the other winter resorts to which the invalid is destined, he will find to his great disappointment, if he neglect to take a sufficiency of stout clothing with him, that there are cold, damp days, cold mornings and chilly evenings, even in the most favoured of these places, during which he would be very glad of his usual winter clothing."

The chapter on Mineral Waters is of a very cursory character. There are, however, so many works from which the fullest information may readily be obtained as to the merits of any particular source, that Dr. Jackson has done well not to overload his volume with further details on this subject. In his enumeration of the diseases in which change of climate is useful, the lion's share is, as might be expected, appropriated to phthisis. We fully concur in the author's opinion, that whilst uniformity of atmospheric conditions is a very desirable element in the choice of a climate suitable for consumptive patients, it is not by any means the only, or even the chief, object to be sought for. In weighing the claims of any locality, it is equally necessary to take into consideration its resources, both natural and artificial, for the occupation of mind and body, as well as its hygienic qualifications and social capabilities; and in forming our conclusion, we must also be guided by the habits and temperament of the patient, and the peculiar complications of his disorder. Above all things, the invalid should beware of attempting to combine the pleasures of sight-seeing with the pursuit of health. The two objects are, in the large majority of cases, absolutely incompatible. Rational occupation without excitement, exercise without fatigue, and the abundant enjoyment of fresh air and sunshine without exposure to violent alternations of temperature—these are what he needs, and what in the ardour of lion-hunting he will probably fail to obtain.

We feel some doubt in endorsing so fully Dr. Jackson's views on the value of sea voyages in the treatment of consumption. There are few more difficult questions to answer than one which is frequently put to the medical attendant with regard to the suitability of this remedy for a given case of phthisis. Short voyages, Dr. Jackson thinks, are rarely of any avail, and more frequently mischievous than useful; long ones, on the other hand, are generally advantageous. This is a statement which requires to be taken with some reservation. We have seen the course of consumption indubitably hastened by a voyage to Australia, whilst shorter ones have been followed by decided benefit. The monotony of a long voyage, the too frequent imperfection of hygienic and dietetic arrangements on board all but first-class ships, and the necessary exposure for a time to the debilitating influence of a tropical sun, are sufficient to account for the prejudicial result in the former case, without any recourse to the supposition which some have hazarded of the irritating effects of a marine atmosphere. These disadvantages may no doubt with care be more or less obviated, and under such circumstances a voyage to New Zealand or to the Cape is often eminently beneficial. But where there is no reason to dread the evils arising from sea-sickness, we should feel inclined, in an experimental point of view, to prefer trips of a shorter duration, and especially, in our own climate, during the summer.

The greater portion of Dr. Jackson's book is naturally devoted to the particular consideration of the various climatic resorts of Europe and Africa. He also discusses at some length the climatic characteristics of our principal colonies. The most interesting chapter in this part of the book is the one which treats of Algeria, both from the increasing repute which that country has of late acquired as a winter residence for consumptives, and from its embodying Dr. Jackson's personal experience. The report which he gives of the Algerian climate is on the whole very satisfactory, but he limits its applicability to the early stage of the disease, before softening has commenced. He states that it is mild and equable, at the same time that it possesses a certain amount of tonic and bracing power. It is true that the absolute fall of rain in the neighbourhood of Algiers is large, and that occasionally an exceptional season, such as that of 1856–7, for instance, may from this cause be rendered very inclement; yet, as a rule, the number of rainy days is small, and there are very few on which some amount of exercise in the open air is not possible. Moreover, in Algiers itself, and in the neighbouring country, which from the cheapness of carriage may be readily explored, the invalid will find an inexhaustible source of interest and occupation. Comfortable apartments and board may be obtained on moderate terms, and the increasing number of European visitors provides a reasonable amount of cheerful society. Before taking leave of Dr. Jackson's work, we would suggest the desirability of his adding to any future edition of it a classified list of climates, or of his at least arranging the various localities in such an order that those which most nearly resemble one another should be in some sort of contiguity. Its utility as a handbook of reference would thus be greatly enhanced, and the medical man would be able, after determining the *kind* of climate that was desirable in any given case, to cast his eye over the different resorts which possessed that climate, and to examine the relative merits of each for the special object which he might have in view.

In dealing with M. de Pietra Santa's brochure on the 'Climate of Algiers,' we must confess to some doubts as to the class of readers for whom it is intended. It professes to be a 'Report of a Medical Mission to Algeria, presented to his Excellency the Minister of Algeria and the Colonies,' in which case we presume that it would have been written in French. Why it appears in its English dress, and whether it is a literal translation of the original report, or only an abstract of such portions as may be considered interesting to an English public, we are left in ignorance. We can hardly suppose that his Excellency the Minister of Algeria and the Colonies requires to be reminded that "the name of atmosphere is given to the mass of air which surrounds the earth," or that he is so ignorant as to share the impression that "the term temperature is applied to the sensation produced upon the human body by the atmosphere according as it is more or less charged with heat," which *sensation* "may be estimated by means of the thermometer"! However, waiving these extravagances, which we will charitably assume to be due to the incompetence of the person who has given to the work its English form, as well as the slight aspect of having been "done to order" which it presents in some parts, we will deal with its merits in a strictly scientific point of view.

M. de Pietra Santa has divided his Report into three sections, which are devoted respectively to the consideration of the climate, the condition of phthisis in Algiers, and the influence of the climate upon that disease. The former of these comprises a readable account of the topography, geology, and meteorology of the neighbourhood of Algiers, which is sufficiently exact for popular, but is in many respects too incomplete to be relied on for scientific, purposes. The conclusions at which M. de Pietra Santa arrives with respect to the climate of Algiers, are, that it is intermediate between a tropical and a temperate one, and that it unites advantages for invalids which will be sought in vain on any of the other stations of the Mediterranean, being only to be met with in the island of Madeira. Phthisis, though far from being uncommon in Algiers, is less frequent than in some other parts of the Mediterranean, and considerably less so than in Paris or London, the death-rate from this disease being 1 in 14·15, as compared with 1 in 8 in London and Naples, 1 in 7 in Nice, and 1 in 5 in Paris. It is curious that whilst the general mortality of the Jew population of Algiers during a period of eight years bore to that of the Christians and Mahomedans the ratio of 2·75 to 4·22, the deaths from phthisis amongst the former were to those amongst the latter in the proportion of 7·22 to 7·06. The former of these facts hardly corroborates the views of M. Genty de Bussy with regard to the degeneration of the Jewish race, though it may be a question for consideration whether the latter of them is in any way dependent upon the very small quantity of animal food which M. de Bussy calculates is consumed by the Jew when compared with the Moor or European; the former only taking on an average 48 lbs. in the course of the year, whilst the two latter consume respectively 94 lbs. and 400 lbs. Amongst the causes to which M. Pietra Santa attributes the existence of phthisis in Algeria, are the abuse of the Moorish vapour baths (especially amongst the women, who remain in their hot and debilitating atmosphere for hours together), the influence of macadamized roads, and the spreading of venereal diseases. But out of all proportion to other agencies in the production of this disease amongst the native population of Algiers, is the baneful influence of captivity. The immunity which the Arab enjoys from phthisis whilst at large in the freedom of his nomade life is most painfully in contrast with the ravages which it commits amongst the native troops of the Dey and the native inmates of the prisons. We are told that "out of 600 natives confined in the prison of Nismes, nearly 250 died of consumption in a very short space of time;" whilst in the central house of El-Harrach, out of 153 deaths 57 arose from phthisis. And even this mortality, frightful as it appears, is as nothing compared with that which steadily annihilates the negro emigrants from Central Africa to the coasts of the Mediterranean, who die off under its influence so rapidly, that were it not for continual immigration the negro race in Algiers would soon become extinct.*

A portion of the medical division of M. de Pietra Santa's work is taken up by an attempt to refute the well-known views of M. Boudin on the mutual antagonism of ague and phthisis. The facts which he alleges do not in our opinion prove more than that phthisis and intermittent disorders are capable, under certain circumstances, of existing side by side; they do not appear to us to affect such well-marked instances as that cited by M. Jourdanet in his recent interesting work on Mexico,† in which he states that of the two neighbouring provinces of Yucatan and Tabasco, the one is almost free from ague, but is decimated by phthisis; whilst the other is the seat of intermittent fever in its most virulent form, but phthisis is almost unknown in it. And so well-recognised is the beneficial influence of the marshy grounds of Tabasco upon the progress of consumption, that the phthisical patients of Yucatan are regularly sent there, and with the greatest benefit. It is no disproof whatever of the asserted antagonism between these diseases, that "out of 789 deaths in the prison of El-Harrach, 19 were from marsh miasma, *or other noxious influences* (the italics are our own), 91 arose from low or typhoïd fevers, and 57 from phthisis," any more than a similar proportion existing in the obituary of one of our own hospitals would be. M. de Pietra Santa's work concludes with some sound remarks on the good results which a semi-tropical climate is calculated to effect in phthisis, and with some useful hints on the hygienic conditions which invalids who resort to one should endeavour to maintain. On the whole, though rather less precise than we should have expected to find a formal scientific report from a gentleman of M. de Pietra Santa's position to an important public functionary, his brochure is calculated to be of some service to that class for whose especial benefit we presume that it has made its appearance in English

* One of the Arabic names for consumption is *meurdh el abid*, or the slave's disease, from the frequency with which it attacks the negroes.

† Du Mexique au point de vue de son influence sur la vie de l'homme. Paris, 1861. Of this work we propose to present our readers with a fuller notice in a future number.

—those who as patients or physicians may wish to obtain in a compendious form an outline of the principal characteristics by which the climate of Algiers is distinguished.

Egypt may congratulate itself on having met in Dr. Dalrymple with an exponent who does full, but not more than full, justice to the claims of its climate. Condemned by his physicians to a temporary exile to some country where the conditions for recruiting mind and body might be found in greatest perfection, he turned his steps Nile-wards. Blending with the natural desire for the recovery of his own health, the still more commendable one to do what he could toward the recovery of that of others, Dr. Dalrymple instituted, during the four months of his excursion up the Nile, a series of meteorological observations, which, for accuracy and completeness, far surpass any hitherto made in that country. In addition to their having been taken with carefully-verified instruments, provided for the purpose by Messrs. Negretti and Zambra, the eminent scientific instrument-makers, they possess the by no means common merit of having been made under conditions as uniform as it was possible to obtain. The most important results of these observations is the very slight variability which they show the climate of Upper Egypt to enjoy, especially in regard to moisture. From Cairo up to the cataracts of the Nile, the usual limit of health-seekers, the barometric pressure varied less than one-third of an inch, and the dryness of the atmosphere as compared with that of our summer months for the last five years, three of which have been remarkable for their dryness, was as eighty-one to fifty-six.

"The all-absorbing topic of rain" (says Dr. Dalrymple), "that never-ceasing source of hopes and fears in other countries, here has scarcely an existence. During the time over which my observations extended, there were two or three rain showers, but the quantity was so small that none could be collected in the rain-guage. From the time I left Alexandria to the time I returned there, I never unfurled an umbrella against wet. Here the same conditions may be reckoned on year after year. There may be winters in which the cold or heat will be more or less, but the anxious hope of the invalid is never frustrated. The atmospheric phenomena are very constant, and in Egypt you hear little or none of that extenuation of the climate which in Italy, Spain, or the South of France so often meets the remonstrances of the disappointed health-seeker—'Oh, such a season as this never was known.' The mean result of all the observations I have collected that were made by others, and those made by myself, only give a difference of 1°·1, and yet one set of them was made ninety years ago."

Although the mean temperature would give the idea of extreme uniformity, only fluctuating from 58°·1 to 60°·4, the extremes were 90° and 40° in February. Yet a heat of 90°, which in a damp climate would be oppressive, produces no unpleasant effects in the dry atmosphere of the desert, which may not easily be counteracted by appropriate shelter and clothing. On the other hand, although the cold at night was never lower than 6° above freezing-point, the rapidity with which it makes itself felt so soon as the sun has set, renders great caution on the part of invalids necessary not to expose themselves to its influence. To those who, in their idea of a Nile voyage, may picture to themselves the unceasing monotony of the endless sands by which its banks are bordered, and the depressing influence which such a scene is calculated to exert upon the mind, we commend the reassuring descriptions by Dr. Dalrymple of the variety of scene and occupation which filled up days which he recals as "ever too short and never too long."

In phthisis, in renal disease, but above all in the chronic and debilitating form of bronchitis, which comes round for some invalids as regularly as the fogs of November, Dr. Dalrymple speaks in high terms of the Egyptian climate. Its beneficial effects in the second of these affections he experienced in his own person; but it is *par excellence* the country for bronchitics. The dry and bracing air, the cloudless sky, and the invigorating influence of a constant but not oppressive sun, build up the exhausted frame, give tone to the relaxed and irritable air-tubes, and allow a freedom of exposure which in other climates is impossible. To all these attractions there are but two drawbacks—distance and expense. The former, involving as it does a rather long sea voyage, must make Egypt to many invalids a land of promise which they can never enter; and the latter (upwards of one hundred pounds for the three months' excursion up the Nile alone) must exclude a still greater number whose *res angusta domi* forbids so expensive a luxury. Those to whom these obstacles may not be insurmountable, will find in Dr. Dalrymple's little work every information which they can require in the pursuit of health, comfort, and pleasure amidst the "sunny memories" of the Nile.

But, if Egypt contains balm for the wealthy *poitrinaire*, the less fortunate valetudinarian may console himself that he has in Mentone a retreat whose attractions will not lose much by comparison with the more favoured locality. Dr. J. Henry Bennet, who was led to reside in it during the winter of 1859–60, and, after a fruitless search along the Italian seaboard for a better climate, to return to it in that of the ensuing year, has given us the results of his experience. According to him, the characteristics of the Mentonian climate are—

"Absence of frost and fog; the paucity of rainy days; the clearness of the sky; the heat and brilliancy of the sun when it does not rain; a rather cool or chilly night temperature, and a bracing coolness of the atmosphere throughout the winter out of the sun's rays. When, on the contrary, the sun is obscured by clouds and rain falls, there is as miserable and chilly a state of things as in a drizzling day in November in England."

This latter statement is, perhaps, calculated to convey anything but a prepossessing impression of Mentone, and intending visitors may be anxious to know whether this obscuration of the sun is at all a frequent phenomenon there. We have much satisfaction in assuring them, on the authority of Dr. Bennet, that thoroughly cloudy days, and days of incessant rain, only "occur two, three, or four times during the winter." M. Brea, who has published the results of ten years' observations at Mentone, does, indeed, give eighty as the average number of its rainy days throughout the year, which is twenty more than the average of its neighbour, Nice; and Dr. Bennet also admits that sometimes "rain will fall for several days and nights;" still, the intervening days, "five out of six," are clear and bright, and when the sun does shine it does so to good effect. So powerful are its rays generally, even in December and January, that gentlemen, however reluctant they may be to adopt the fashion, find themselves compelled to carry parasols. Yet, at the same time, out of the sun it is cold and chilly, the temperature in the shade not exceeding 56°. As Dr. Bennet remarks, it is a powerful sun warming a chilly atmosphere. This is the character of the climate of the Italian coast generally in winter, and it is this which renders it so treacherous not only to invalids, but also to the strong and healthy. Hence it is only those who are able to take active exercise when in the open air, who can expose themselves with safety to even the more equable temperature of the sheltered and land-locked bay in which Mentone is situated. But, with all its advantages, the alternations of temperature here are often very sudden, and special care is requisite both to provide a sufficiency of warm clothing, and to avoid those sources of physical exhaustion by which the prejudicial effects of a chill are so easily intensified. Delicate persons, and those who neglect these precautions, are very liable to attacks of pneumonia and pleurisy, diseases which make up a large portion of the mortality of the natives of this part of the coast.

For the social resources of Mentone, and for further information as to all that can be said in its favour, we must refer our readers to the work itself. We have perused it with much interest; Dr. Bennet is a pleasing writer, and so long as he confines himself to his own observations and to the broad road of medicine, his remarks are sensible and to the point. But when he goes beyond this, he is not so trustworthy a guide. For instance, in speaking of the importance of solar heat as an element of climate, he thinks that—

"It will be better appreciated when we know that it takes at least six months for a thermometer to cool down after the glass tube has been closed by momentary exposure to the flame of a blowpipe. It is only after that lapse of time that the glass has regained a normal state, and that it can be graduated when scientific precision is in view; so retentive of heat are most solid bodies, and so long a period of time does it take for them to lose by radiation heat once acquired."

We do not know whether Dr. Bennet learned this astounding fact from Drew, whom he quotes hereupon, or whether it is his own inference; but if he will only take the trouble to consult any elementary work on physics he will see that the difficulty in graduating thermometers to which he alludes, has nothing whatever to do with the cause he suggests, and that the rapidity with which bodies radiate heat is proportionate to that with which they absorb it. We should like, too, to know his authority for stating that the deep blue colour of the waters of the Mediterranean is owing to their containing more salt than those of the ocean; and that the more salt is held in solution by water the bluer it is. Does he not know that the "blueness" of large masses of water is not confined to the sea, but that it is equally found in fresh-water lakes and rivers, and even in the ice of glaciers? If he will turn to 'Childe Harold' he may read Byron's description of

"The blue rushing of the arrowy Rhone;"

in a note to which the poet remarks that the colour of that river at Geneva "is blue to a depth of tint which I have never seen equalled in water, salt or fresh, except in the Mediterranean and Archipelago." The cause of the colour of the sea is very fully discussed by Arago in his 'Report to the Academy of Sciences,'* but the explanation which he gives is very different to that offered by Dr. Bennet.

Dr. Barker's work contains the substance of a paper read before the Medico-Chirurgical Society of London, and invites criticism on two grounds; firstly, as a description of the climate of Worthing, and its remedial influence, especially in diseases of the lungs; and secondly, from the statement by its author in the preface, that "he has embraced the occasion to give his opinions upon several subjects that have a relation to climate *generally*, which, so far as he is aware, have hitherto received but little attention." We must confess that we rather opened our eyes on being informed that one of these neglected branches of climatology was "the humidity of the atmosphere," and that "very erroneous ideas prevail on this subject; for there is scarcely a work that has been written, in which it has been referred to, that does not contain some grave error." We had imagined that this element of climate had received a very fair share of attention from most observers, and with the desire of correcting the delusion under which we had laboured, we turned with some curiosity to Dr. Barker's remarks upon the subject, to see what new light he could throw upon it. Judge, then, of our surprise on finding that this chapter "On the Humidity of the Atmosphere," contained absolutely nothing that can put forth the least claim to novelty of any kind, unless we may except the assertion made on the authority of "an intelligent farmer," that in the Weald of Sussex, "fogs are ten times as common as in this neighbourhood" (Worthing). We have some little acquaintance with the former of these localities, and we can only say that if the intelligent farmer did not over-estimate the infrequency of fogs at Worthing, they must be very rare meteorological phenomena there indeed. So far as we can make out what Dr. Barker's views on the influence of humidity really are, for his phraseology is rather obscure, they appear to be comprised in the following passage: "a cold, humid climate is often spoken of as pernicious, a warm, humid one as relaxing, and having an injurious influence, and a dry atmosphere as beneficial. My own observations, and a careful attention to the origin of diseases, *has* (*sic*) led me to entertain altogether contrary opinions to these." We regret that, as Dr. Barker's views differ so extensively from those which are generally held, he has not adduced a larger number of facts in their support

* See his Notices Scientifiques, 1839.

than he has done, and that he has thought it desirable to omit the "carefully recorded observations extending over a period of ten years" upon which they are based.

We learn, too, from the preface that the author's "observations on the influence of barometric fluctuations on the human system are altogether new, and which additional attention tends only to confirm." The value and novelty of these contributions to medical science may be appreciated, when it is known that they relate to the facts, that Dr. Barker having had the misfortune on one occasion to fracture several of his ribs found that a fall in the barometer was indicated by increased sensibility and pain at the injured parts, so that he could with certainty predict the state of the weather before rising in the morning; that after the subsidence of inflammation and occlusion of the Eustachian tube caused by an attack of sore throat, a diminution of atmospheric pressure always brought about an increase of deafness, which Dr. Barker attributes to "increased distension of the vessels of the tube closing the communication between the throat and ear," though he does not explain why the vessels of the Eustachian tube in particular should be so dilated; that "it is a common circumstance in phthisis pulmonalis for blood to transude from the ulcerated surfaces when the barometer is very low;" and that "the tendency to sleep is materially increased by diminished atmospheric pressure." We are afraid that in his discovery that morbid parts are peculiarly susceptible of atmospheric changes, Dr. Barker has the misfortune to have been anticipated, if by no one else, by the worthy Mr. Burchell, who was in the habit of invariably predicting rain by "the shooting of his corns." Without wasting more time upon these "observations," we will only remark that we have very great doubts as to the accuracy of the latter of them, for reasons into which it is unnecessary to enter here. A very considerable portion of the book is occupied by a discussion on the nature of ozone. Dr. Barker thinks that it is *an established fact* that the chief source of this principle is the ocean. Having arrived at this decision, upon evidence which appears to us anything but satisfactory, when we reflect upon the acknowledged difficulty which there is in accurately registering the presence of ozone, and the fallacies attendant upon even the best process for so doing, he proceeds to enunciate the hypothesis that ozone is *free chlorine developed by the decomposition of the chloride of sodium of the sea.* How this decomposition is effected he does not inform us, nor does he think it worth while to explain why, if free chlorine be present in the atmosphere, its presence cannot be detected by well-known and very delicate tests. "Professor Schonbein," it appears, "overlooking the grand source of this principle, denies altogether the existence of free chlorine in the atmosphere." If we may be allowed to offer a suggestion to Dr. Barker, it is that he should reconsider whether Professor Schönbein is not likely to be in the right, and whether this "grand source" of his be not a "grand delusion."

If we have devoted more space to the consideration of Dr. Barker's "observations" than they deserve, it is not so much because the merits of his work are such as to demand a lengthy notice, as to give our readers an opportunity of judging for themselves whether the very eulogistic notices which are appended to the advertisements of it are well founded, or not. We can conscientiously affirm that if there is no novelty in these "observations," there is none in any other part of the book. Worthing, though a stupid little town enough, enjoys some advantages which give it a fair title to rank as a favourable specimen of a south-coast watering-place; but we must protest against its being forced into publicity by such means as this. Its climate may be admirably adapted, as Dr. Barker tells us, "to establish the healing process in ulcerated lungs," but we must question the propriety of this method of making "the subject of *the air we breathe* intelligible to all."

Dr. Kebbell's book is a worse one than that of Dr. Barker, in proportion as a man who does indifferently a thing that is wholly uncalled for is a greater offender against propriety than one who has failed, through carelessness or inability, in accomplishing what was legitimately open to him. In Dr. Barker's case it might at least be alleged in mitigation of his offence as a book-maker, that the merits of Worthing are not very hacknied ones, and that having for a long time suffered under the black repute of its defective drainage, now that it has amended this, a little literary whitewashing, even if overdone, is a venial fault. But Dr. Kebbell can plead nothing of this kind in extenuation. In these days of half-a-crown trips, all the world knows what the climate of Brighton is like, or if it does not, there are plenty of sources from which all the necessary information may be obtained. Dr. Kebbell has no single new fact, and no new views of any old ones, to bring forward in excuse for his having encumbered medical literature with this publication, which, by the way, is an abridgment of a larger work. It has not even the negative merit of being decently written. Thus we have such sentences as, "The down air is the finest in the world; the scenery is beautiful; and on *their* (*sic*) summit, even during the hottest and stillest weather, there is always a breeze blowing."—"The summer season at Brighton at the present time—chiefly, probably, on account of its supposed heat—does not enjoy that reputation or attract that number of visitors which the real properties and advantages of its climate entitle it." It is not making an exorbitant demand to expect that when a writer has nothing new to tell us, he shall at least take some little trouble with the language in which he makes his communication.

Of course, as in most of these cases, there is great labour expended to show that Brighton is a very superior place of residence at all seasons of the year to its neighbouring competitors for popular favour on the south coast. And if it were a mere question of catching an extra breath of the *popularis aura* which were involved in the production of this class of publications, their authors might be well left to fight their battle for superiority out amongst themselves. But it is too bad that they should carry on their contentions under the mantle of science.

Of the remaining works included under our heading we need say but little. Dr. Rullman's paper was read at the Congress of Physicians of the Middle Rhine at Frankfort in 1860, and owes its translation to the well-known hand of Dr. Moore, of Dublin. It is a short but able *esquisse* of the comparative merits of the more common southern climatic resorts, showing what we do and what we do not know of their real influence on chronic tubercular phthisis. At its close, some useful tables are given, in which the various localities are classified according to their mean temperature, moisture, number of rainy days, and total rain-fall during the winter. Although most unpretendingly written, its tone is throughout critical and philosophic, and its author is practically acquainted with the subject of which he treats. We think that Dr. Moore has done good service in translating it.

We presume that Dr. Lee's 'Companion to the Continent' requires no commendation from us. Bearing as it does the imprimatur of that modern Colossus of Roads, the immortal Bradshaw, it may be considered as born to a popularity which other works of the kind only achieve after a lengthened struggle with the world. It is, however, but just to state that the present, although ostensibly only the second edition, is in reality the fourth, the work having originally appeared under the title of 'Memoranda in France, Italy, and Germany;' and that it fully maintains the reputation of its predecessors. The fact of Dr. Taylor's handbook on the climate of Pau having reached a third edition shows that it, likewise, "needs no bush." Those who propose visiting that most fashionable of winter retreats will find Dr. Taylor a very useful guide in all that relates to its various notabilia, not omitting Monsieur Patrick O'Quin, its present mayor, "whose political and administrative talents," we are told, "must ere long place him in a sphere of more extensive usefulness."

We can hardly expect that many of our readers are either likely themselves to visit, or to send others to Bermuda, for the sake of the climate alone. Although 'the vexed Bermoothes' still maintains its tempestuous repute by the prevalence during the greater part of the year of violent gales around its shores, the climate appears to be, except in the height of summer, even and agreeable. During the cold season, which lasts from November to March, the mean temperature is stated by Dr. Gaudet to be 60°, whilst the maximum elevation of the thermometer during four years was only 85°. Dr. Gaudet's 'Hints to Invalids' should be rather entitled 'Hints to Healthy Persons who wish to avoid becoming Invalids,' and his book is altogether adapted, and is well suited, rather as a guide to the intending emigrant or trader, than as a description of Bermuda and its climate from a medical point of view. Still, it is not without its merits, amongst which may be noted the vigorous way in which he exposes the mistake of the Colonial Government in bestowing upon this and other dependencies of Great Britain a representation in miniature of the constitution of the mother country, which is as well adapted for their wants as the garments of a full-grown man are for the limbs of a child.

Review VII.

Inversio Vesicæ Urinariæ og Luxationes Femorum Congenitæ, hos samme Individ. Iagttagne af Lektor Voss. Hermed 2de. farvetrykte Plancher.—*Christiania*, 1857.

Inversion of the Urinary Bladder and Congenital Luxations of the Femurs in the same Individual. Observed by Lector Voss. With Two Chromographs. Quarto, pp. 25.

The above deformities, though separately not very rare, have not, to the author's knowledge, before been observed combined in the same individual, at least, not in a young child. In the case which came under Lector Voss's observation, the little girl the subject of the lesions in question, had been exhibited to the Norwegian Medical Society on the 29th August, 1855, when at the age of eighteen months, partly in order to give the members an opportunity of examining the malformation of the bladder, and partly with a view to elicit a discussion as to what could or ought to be done, whether any operative interference was possible, or whether the treatment ought to be merely palliative. In the middle of October, the little patient was admitted into the Children's Hospital, suffering from cholerine, of which she died on the 20th. A post-mortem examination was made on the following day, when it was for the first time ascertained that the child had laboured also under partial congenital dislocation of both femurs—a lesion which had been completely concealed during life by the plumpness of the parts preventing any striking change of form. The following facts were elicited from the father: first, that the child had never been ill until she got the attack of cholerine, of which she died; secondly, that three months before her death she had begun to walk, but that her gait was tottering, and that though she could stand erect independently of support, she could not advance a step without leaning on some person or thing. The mother died of cholera three weeks before the child. Neither in her family nor in her husband's did malformations exist. Besides the subject of this notice, she had had five children, of whom one alone, a girl, aged six years, survives. Two of the others died of inflammation of the brain; and two, the youngest, twins, of cholera, one before and one after the mother. None of these five presented any abnormity.

The author adds a detailed description of the lesion of the bladder, through which it is unnecessary to follow him. Just before perforating the wall of the bladder, each ureter was dilated to form a cavity of between six and seven millimetres (quarter of an inch) in diameter. Hence the fact, that in the recumbent posture the urine passed in little gushes. By assuming the contractility of the ureters, by means both of elastic fibres and organic muscular fibres, this phenomenon is explicable even when the individual was in the erect position; for the muscular fibres of the wall of the bladder might act as a sphincter. The uterus was, as it were, impacted between these two enlarged ureters. The symphysis ossium pubis was wanting, nor did any medium which could be called a ligament unite the two bones. It appeared, in fact, as if the ossa pubis had been violently separated from one another; their horizontal rami were directed as if they would, if prolonged, meet about an inch and a quarter in front of the bladder. The ossa ilii and ossa ischii were also rather farther apart than is normally the case. Lastly, the os coccygis was curved at the base, almost at a right angle.

The hip-joints were deformed on both sides. The head of the femur was not received into the acetabulum, but lay on the cartilaginous lip of the latter, which was flattened externally and superiorly. The acetabulum was too small to admit the head of the bone; moreover, its cavity was quite filled with a fatty mass (*pulvinar adiposum*), and the ligamentum rotundum. The psoas major was tendinous throughout a greater extent than usual, and its tendon was very strong and rounded; it rested in a remarkably deep furrow under the antero-inferior spine of the ilium, while the tuberculum ilio-pectineum was rendered more prominent. There was no trace of preceding inflammation in the articular cavity; synovia was not present in any striking quantity. The abdominal organs, and particularly the kidneys, did not present any deviation from the normal state.

The author briefly describes two cases of inversion of the urinary bladder which he had previously seen. In one, the intestinal canal opened upon the upper part of the bladder, discharging a species of meconium. The right thigh, which was spontaneously amputated immediately above the knee, formed an atrophied rounded stump. In the middle of the right nates was inserted a fibrous cord, three inches in length, and as thick as a goose-quill, which probably was the remains of the band which had caused the amputation of the thigh. The child died four days after birth, but no post-mortem examination could be obtained. The other case was that of a countryman, aged twenty-one. The author next refers to some cases of congenital dislocations of the femur he has himself witnessed, and then passes to a review of the literature of both deformities; first considering the causes of malformations in general; secondly, those of inversion of the urinary bladder; and, thirdly, those of congenital luxations of the femur.

"Having thus briefly sketched the most prominent opinions respecting inversion of the urinary bladder and congenital luxations of the femur, and having weighed these in reference to the combined malformation in the present case, I have arrived at the conviction that both stand in this instance in causal relation to each other, so that what has given rise to the inversion must be assumed also to have co-operated in producing the congenital luxation, and that the commencement of each must be referred to the earliest period of fœtal development." (p. 14.)

As the consideration of the combination of the two lesions in the same individual is the leading feature of Lector Voss's work, we shall quote at length the reasoning by which he seeks to establish the view expressed in the foregoing paragraph. This is conveyed in the following words—

"We may in general be satisfied with Meckel's theory, that inversion of the bladder is the result of an arrest of development, in this case extending to the genitals, urinary bladder, osseous system, and skin. We see, in fact, that there is gradation in the arrested development. Symphysis ossium pubis may be wanting alone, without ectopia vesicæ, as in a case observed by Walter, quoted by Meckel, and described by Gurlt, with a drawing of the pelvis,* in a man, aged thirty, whose abdominal organs and genitals were normal. The muscular structures and skin covering the bladder and symphysis pubis may be wanting, and we thus have a true ectopia vesicæ, as in a case described by Vrolik. Again, inversion of the urinary bladder is produced where, in addition to the abdominal wall and symphysis, the anterior wall of the urinary bladder is at the same time deficient. Finally, more considerable deformities present themselves; the intestinal canal opens into the prolapsed urinary bladder, &c. If we compare the conditions presented by the history of normal development with those of abnormal, we shall be correctly inclined to consider the series of malformations as the development of these organs, and the various degrees of malformations as several steps in their development. Of originally coalesced organs, one separates in one individual at an earlier period from the rest, another in a second, a view which is shared by Meckel, St. Hilaire, and Vrolik. But what causes the arrest of development, they do not pretend to say, neither do they attempt to explain why the same deformity may be repeated with precisely the same features in a number of individuals. This seems evidently to be under the influence of a definite rule.

"But, as J. Müller has shown, inversion of the urinary bladder may be assumed to depend either on a rupture of the bladder caused by dropsy, or the opening in the urogenital sinus is prolonged upwards to too great an extent, if either of these abnormities exist in the earlier periods of fœtal life. If dropsy be supposed to have given rise to the rupture of the bladder, this must have occurred at a time when the abdominal integuments were still quite imperfectly developed; for there is not merely deficiency of the anterior wall of the bladder, but also of the abdominal integuments and symphysis pubis; in other words, the rupture must have taken place before the ossa pubis were united by fibro-cartilage, and before the abdominal integuments were united in the median line. This view seems to me to be very satisfactory; for on the same mechanical cause, the second malformation (the congenital luxation) may at the same time depend.

"I therefore suppose the process to be as follows: By the distension of the urinary bladder the horizontal rami of the ossa pubis are pushed from one another, which altered direction they maintain after the rupture has taken place. The altered direction of the ossa pubis has again an influence upon the form of the acetabula, whose circumference is diminished, an influence which is at first indeed very small, but is nevertheless sufficient to prevent a coaptation of the head of the femur with the articular cavity, which malproportion, moreover, gradually becomes better marked during the subsequent development. When the head of the femur begins really to exercise a definite pressure on the margin of the acetabulum, the position of the lower extremities in the fœtus, where the thighs are bent upwards towards the abdomen, so that the head of the bone constantly presses against the posterior and outer portion of the capsule, supplies a source whence the initiated abnormal position is maintained and increased. To the theory of rupture of the bladder, in consequence of dropsy, there is, however, an objection which I cannot overlook. The mechanical pressure must at the same time act behind the bladder, and yet

* Ueber einige durch Erkrankung der Gelenkverbindungen verursachten Misstaltungen des menschlichen Beckens. Berlin, 1854.

it has not caused any impediment to the normal development of the uterus, vagina, or rectum.

"If this explanation should prove to be correct, we may probably expect more frequently to meet with luxation of the femur in combination with inversion of the bladder, of which I am acquainted with no example except that above described. I would, however, suggest that the tottering gait which accompanies inversion, and to which the deficient symphysis ossium pubis is assigned as a cause, may probably in many cases depend upon congenital luxations."

To the foregoing the author appends a *résumé* of many of the cases on record, both of inversion of the bladder and of congenital luxation of the femur, and concludes his interesting memoir with a review of the methods which have been proposed to lessen the inconveniences connected with each of these conditions. The essay is illustrated with two plates, containing five well executed chromographic figures.

Review VIII.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Second Series. Vol. XXVI. —*London.* pp. 282.

Although of slighter dimensions than the volumes which the Medico-Chirurgical Society has put into our hands of late, the last one issued nevertheless contains several highly-interesting and practical communications. The majority are papers bearing on purely medical subjects, and besides a few on surgical ones, we find one or two devoted to the illustration of strictly physiological questions. The volume leads off with:

I. *A Case of Gastrotomy for Extra-Uterine Gestation.* By John Adams, F.R.C.S.—The patient, aged twenty-eight, had been married eight years, and became pregnant for the first time in 1859. At the supposed termination of gestation (end of October), she "began to feel sleepy, tired, and worn; and a week after this suffered from a sense of stiffness in her limbs, but had no distinct pains like uterine pains. A discharge took place from the vagina, and blood, varying in colour from dark to pink, and pieces of flesh-like substance, entirely inoffensive in odour, were expelled in gushes." From this time she gradually diminished in size. Menstruation recommenced and continued ever since, but the milk remained in the breasts until March following, when a hard, immovable, irregular, and oval tumour was found in the right side of the abdomen. There was no pain, nor was any disease of the uterus discovered on examination "per vaginam," and the patient could perform her usual domestic duties.

The case was also seen by Dr. Ramsbotham, who agreed with Mr. Adams as to the nature of the case and as to the necessity of gastrotomy, the patient being very anxious for the removal of the tumour. The operation was postponed until May 31, and after dividing the peritoneum, the tumour presented itself, having a glistening aspect and being only slightly adherent. "The cyst, which was about four inches in thickness and very firm, was opened, and a pint of a greenish yellow, transparent fluid escaped, with yellowish flakes of *vernix caseosa* and some hairs. As soon as the cyst was opened, a loop of the funis protruded." Subsequently the fœtus was extracted by the breech, and the placenta found adherent. After being sponged out, the walls of the cyst collapsed considerably, and in due time the wound was closed by interrupted sutures. The funis by degrees shrivelled up, and there remained for some time a small fistulous opening where the funis had escaped, discharging fetid pus. The note made Oct. 1 was that the "patient appeared quite well, and told me that the discharge had almost entirely ceased." Subsequently she suffered from a large ventral hernia. On examination by Dr. Bader the fœtus turned out to be a female. The lungs were pale and floated freely.

In his observations upon the foregoing, Mr. Adams remarks:

"There are points of serious importance in connexion with this case, on which the practice pursued has a forcible bearing: these are, first, the propriety of performing any operation whatever under such circumstances; secondly, the time at which the operation should be performed, if thought desirable; and lastly, the method of performing it."

Our space will not permit us to follow the author in his remarks upon these questions. One point only we shall further allude to, and that is, as to the method of dealing with the placenta in cases of gastrotomy for extra-uterine gestation. After stating that all the cases on record in which any rude attempts have been made to extract the placenta have proved fatal, he observes, that if it cannot be removed quite easily, it is better to leave it alone "with the hope that it will be separated and come away in the discharge; for while it remains, the wound will not close, and there is no doubt that the patient incurs the risk of pyæmia so long as the wound remains open."

II. *On an Operation for Pervious Urachus, with Stillicidium Urinæ.* By Thomas Paget, F.R.C.S.—It may be remembered that Mr. Paget related a case in the thirty-third volume of the Society's Transactions, in which he had extracted a "ring-shaped calculus, formed on a pubic hair as its nucleus," by the finger passed into the bladder through the navel and open urachus. Since then he has succeeded in permanently closing the opening at the navel in this case (the patient was a man aged fifty-five years), as well as in another (an infant) which he has met with, and the present communication refers chiefly to the details connected with the method of procedure resorted to for the purpose, which consisted in paring the edges of the umbilical opening and adapting them by a suture-pin and lint, as for hare-lip, or by curved needle. In both cases a catheter was introduced and fixed for a time, but Mr. Paget would in future only use it if it was directly called for.

III. *Contributions to the Subject of Compound Fracture; being an Analysis of* 302 *Cases.* By Thomas Bryant, F.R.C.S.—These cases all occurred in Guy's Hospital during the last twenty years; those which happened within the last seven years having come under Mr. Bryant's own observation.

Such a statistical paper, carefully drawn up, has great merits, but its nature quite precludes such condensation as would fit it for reproduction in our pages.

IV. *Analysis of Fifty-two Cases of Epilepsy observed by the Author.* (Second Series.) By Edward H. Sieveking, M.D.—In this communication the author takes the same number of cases as he did in his former paper on the same subject (in 1859), and observes the same order of inquiry, the sex, age, causes, premonitory symptoms, and treatment being all noticed in detail. With the closing words of this paper we fully agree.

"As I have neither discovered nor believe in the existence of a specific for epilepsy, and as I do not profess any method of procedure not already employed by physicians, I refrain from going into detail in this paper as to treatment. I adhere to my formerly-expressed view as to the necessity of an eclectic treatment, selected according to the features of each individual case. In epilepsy, specialism is to be particularly avoided, and I hold that the physician who enjoys the best general knowledge of his profession will also be best qualified to deal with this particular affection."

V. *On Pulsating Bronchocele.* By Joseph Bullar, M.D.—These cases are of unusual interest. The first was that of a gentleman, aged forty-five, who had

"Two pulsating tumours, one on each side of the trachea, attended by so much purring sound, as well as vibration communicated to the touch, that the disease at first sight was like double carotid aneurysm, for which indeed it had been taken. On careful examination, both lobes of the thyroïd gland proved to be enlarged, with violent pulsation of both carotids, and dilatation and pulsation of the thyroïd arteries, which arterial pulsation was communicated to the enlarged zones."

He had been generally out of health, and suffered from over-action of the heart and habitually quick pulse. Along with this enlargement of the thyroïd gland, an unusual prominence in the eyeballs had taken place; the heart's action was still very excitable, and there was great debility. Under the use of quinine and nutritious food, and the local application of iodine, the left lobe of the thyroïd considerably diminished; and subsequently the thyroïd gland so far decreased that only a small, hard enlargement of the right lobe remained. The pulsation entirely ceased.

The second case was that of a woman, aged fifty-two, in which both lobes of the thyroïd were much more enlarged than in the preceding case. Although the violent motion of the carotids and enlarged thyroïd arteries was communicated to the mass, yet its great size rendered the diagnosis more easy. There was much purring sound and thrill to the touch. There was also disease of the heart, and dropsy, and the eyeballs projected considerably. In this case diuretics, local application of iodine, and the use of steel quite removed the pulsation, and materially reduced the size of the enlarged gland, but the eyeballs remained somewhat prominent.

VI. *Remarks on the Cause of Closure of the Valves of the Heart.* By W. O. Markham, M.D.—In this paper the author subverts the ordinarily accepted view as to the mechanism of the closure of the auriculo-ventricular valve. He denies that the valve-flaps are raised up and brought into contact by the pressure of the blood during the diastole of the ventricle. Though, in an appendix, he does allow that, to a very limited extent, this force comes into play in their closure, yet he maintains that it is by the agency of elastic tissue entering into their composition that these valves are raised from the ventricular walls. From the dissection of bullocks' hearts, made by Mr. Broadbent at St. Mary's Hospital, Dr. Markham shows that the disposition of the elastic tissue in the valves is such that when its elasticity is called into play it must tend to draw the free borders of the valves towards their attached borders; that inasmuch as the elastic fibres are chiefly placed in the upper layer of the valves, they must necessarily tend to approximate the two borders of the valves in the direction of their auricular surface. He observes:

"*When the ventricles are emptied—i.e., at the end of ventricular systole, the valves are brought down and pressed against the internal walls of the ventricles,* and, as we must conclude, the elastic tissue put on the stretch. But during ventricular diastole, as the blood flows into the ventricles, the weight (specific gravity) of the valves is diminished by the fluid, so that the elastic fibres have power enough to draw the valves upwards towards each other, and to close partially the auriculo-ventricular orifice, the valves rising up in the ventricle *pari passû* with its dilatation. I may add that the elasticity of the endocardial membrane surrounding the auriculo-ventricular valve also assists in the elevation of the valves."

Dr. Markham then proceeds to show "that no other explanation of the fact is possible," inasmuch as when the auricle is removed from the ventricle, and water poured into the ventricle, it will be found that the valves not only gradually rise upwards towards the auriculo-ventricular orifice whilst the water is flowing into the ventricle, but that they remain in an upraised position when the stream is arrested, and that they will, if depressed, again rise towards each other when the pressure is removed. As the specific gravity of the valves is so much greater than that of blood or water, it is clear that they would not rise up or remain raised unless the force of elastic tissue were in operation. The elastic tissue in the semilunar valves performs the same office as in the case of the auriculo-ventricular valves.

Dr. Markham concludes by suggesting that the cardiac murmur which exists in certain cases in which after death no lesion of the valves is observed, may be owing to some pathological change in the elastic tissue of the valves, whereby its elasticity has been interfered with, owing to which the valves were rendered unable to rise up and approximate.*

In an appendix before alluded to, Dr. Markham remarks that the elasticity of the distended ventricle called into action by the distending force of the auricular systole, may play a part in the closure of the auriculo-ventricular valve, but only at "that instantaneous period

* We do not find that Dr. Markham alludes to the muscular fibre which is found in the heart's valves, and which is most likely concerned in their movement. An interesting instance of an exaggerated condition of their fibres may be seen described at page 109 of the ninth volume of the London Pathological Society's Transactions.

of time which intervenes between the conclusion of auricular systole and the commencement of ventricular systole," this elasticity reacting on the contained blood, and pressing it against the ventricular aspect of the valve-flaps.

VII. *Pathological Researches into the Diseases of the Ear* (seventh series). By JOSEPH TOYNBEE, F.R.S.—This paper is concerned with the subject of sebaceous tumours in the external auditory meatus—one which has received too slight attention from the profession. It is illustrated by the relation of six cases, with all details, accompanied by an account of post-mortem appearances, and by several engravings, and contains a tabular view of the symptoms and morbid appearances on dissection in eighteen cases of this disease.

The communication is one of very considerable interest, and shows how much more frequent this form of disease is than might be expected. Mr. Toynbee points out the formidable character of sebaceous tumours in the external meatus, their general tendency to produce disease of the petrous bone, and in some cases disorganization or abscess of the brain.

"It is a remarkable feature of this disease," Mr. Toynbee remarks, "that the tumour may pass through the substance of the petrous bone, causing a large aperture in it, without producing any visible effect upon the surrounding osseous tissue, the margins of the aperture being often as sharp and well-defined as if made by a chisel."

VIII. *Further Observations on the Structure and Treatment of Uterine Polypi.* By ROBERT LEE, M.D., F.R.S.—This contribution mainly consists of a "history of 105 cases of uterine polypi, reduced into a tabular form, in which the date, name, age, and social condition of the patients are recorded, and the symptoms, treatment, and results are given."

All the cases of polypus of the uterus which have ever come under Dr. Lee's observation, *fortunate and unfortunate*, are contained in this table.

In three of these cases, as they are tabulated, no operation was performed, and the result was not known. In two of the remaining 100 cases the polypi disappeared spontaneously, by some process the nature of which was not ascertained. Of the 98 cases which remained, 5 died before any attempt had been made to remove the polypi by ligature or by any other means. After deducting these 10 cases, there remain 93, of which 8 died and 85 recovered.

"Being fully aware," Dr. Lee observes, "that the greater number of large uterine polypi are fibrous tumours covered with the lining membrane and a portion of the muscular coat, and that these polypi have large arteries and veins distributed throughout their substance, in operating upon them I have not ventured to drag them out of the vagina with forceps of any kind, and divide their roots with the knife. The ligature has usually been applied in such cases with the bent rod, and the sloughing polypus has been removed when the ligature was long in dividing the root. By this means all the evil consequences which could be produced by the vagina being long filled with a putrid mass have been completely avoided; and to this course I do think may be attributed, in a great degree, the uniform success of operations performed by me of late. Even when the polypi have been of comparatively small size, and the ligature has been applied with the double canula, I have not considered it safe to leave the ligature many days around the root of the polypus, when the circulation through the substance had been destroyed, and the mass of the tumour was in a sloughing condition. By twisting the canula firmly round, the ligature has frequently passed through the roots, and the polypus has been removed several days before this would have happened if the ligature had been merely tightened. Even in cases where the root of the polypus has not yielded to this treatment, the ligature has been removed, and the dead polypus has come away harmlessly after three days, tepid water having been freely injected into the vagina. In all cases it has appeared of the utmost consequence to watch the condition of the uterine organs and of the general system, as it has been clearly proved that inflammation is the most common cause of death after the application of a ligature around the peduncle of a polypus."

IX. *Observations on the Growth of the Long Bones and of Stumps.* By George M. Humphry, M.D., F.R.S.—As regards the first part of this paper, the remarks contained relate chiefly to the mode of increase of bones *in length.* Following sundry and interesting remarks on the nature and changes effected in the cartilage which exists between the shafts and the epiphyses of bones, Dr. Humphry suggests that a difficulty exists with regard to the ordinary account of the general manner of growth of bones.

"If the addition," he asks, "to their length be made only at the ends of the shafts, and if the amount added at one end of the shaft be greater than at the other, why does not an alteration take place in the relation of the several parts of the bone to one another, as well as in the relation of the surrounding soft parts to the bone?"

For example, how is it that the entrance of the medullary artery, which in a young femur is about one-third from the lower end of the bone, maintains its relative position to the ends of the bone, although the shaft is elongated at the ends, but chiefly at the lower end? And so on with regard to the various points of attachment of muscular fibres on the surface of bones, how is it that the several fibres passing from the bone and all the surrounding soft parts, like the foramina and processes of the bone, retain the same relation to the ends of the bone at all ages of the bone?

In respect to this matter, Dr. Humphry says:—"There must be an interstitial growth in the whole length of the periosteum, and this must be attended with a continual shifting or sliding of the periosteum, and of the structures connected with it, along the surface of the bone at both ends, and more especially towards the end at which the growth proceeds most quickly." By this continual shifting downwards, for example, any portion of periosteum, and the structures connected with it, at the middle of the shaft retain their position in the growing bone. By this, also, the fact, first noticed by Berard, that the canal for the medullary artery always slants as it passes outwards towards that epiphysis, which remains separate from the shaft to the latest period, is explained. For as the "end of the shaft at which the epiphysis is last united is the end at which growth takes place most quickly," and as there is a continual shifting of the periosteum in this direction for the covering of the newly-added bone, a traction upon the medullary artery is exerted which—

"Causes it to slant downwards, in other words, to run upwards from the periosteum into the bone; and the canal which transmits it through the wall of the shaft in the medullary cavity takes, of course, the same direction. The proper relative *position* of the canal is maintained simply by the addition of new bone around its outer end, and by the absorption of bone at its inner end."

The second portion of Dr. Humphry's paper is devoted to proving, by the measurements of the stumps of persons who have undergone amputation in childhood, and by the results of experiments upon animals,

"That the growth of a stump usually is arrested—that is to say, that it does not keep pace with the growth of the other parts of the body; and that it accords with what the preceding remarks on the mode of growth of the bones would lead us to anticipate, inasmuch as the arrest is *most* marked in those instances in which the growth of the bone takes place chiefly at the lower end."

This paper is well illustrated by diagrammatic figures.

X. *Researches on Asphyxia; with Observations on the Effects produced by the Hot Bath in Asphyxiated Animals, and its Use in the Treatment of Suspended Animation.*—By A. T. H. Waters, M.R.C.P. —After alluding to the discrepancy of opinion as to the intimate pathological changes which take place in asphyxia, and as to the best mode of treatment in the more acute forms of the affection, the author of this paper gives the results of a series of experiments on dogs, cats, and rabbits, which he made for the purpose of determining (1) the length of time the heart continues to beat in asphyxia; and (2) the effects of the hot bath on asphyxiated animals; in the first place, after all respiratory movements have ceased and are not re-excited; and, in the second place, when respiration has been re-excited, and is being feebly carried out.

The conclusions arrived at by Mr. Waters are as follows:

"1. In dogs, rabbits, and cats, when asphyxiated by submersion, the ventricles of the heart do not, as a rule, cease to contract in a few minutes after the cessation of the functions of animal life; but in many instances their action continues for a very considerable period; in all probability the same remark applies to man, and serves to explain how recovery has taken place after lengthened submersion—a circumstance which has been by many attributed to the fact of syncope having occurred at the period of immersion.

"2. In cases of asphyxia where respiration has altogether stopped, the effects of the hot-bath are to produce an accumulation of blood in the lungs and on the left side of the heart, together with a tendency to coagulate on the part of the blood. It does not tend to prolong the action of the heart, but rather to paralyse its movements and diminish the duration of its contractions. It does not excite respiratory efforts, and prevents artificial respiration from being properly carried out.

"3. In cases of asphyxia where respiration has been re-excited, and is being feebly carried on, the hot-bath, although in some instances it seems to have an immediate bad result, yet has a tendency to produce a fatal issue some hours after its use, by causing extreme congestion of the lungs, together with consolidation and collapse of the pulmonary tissue."

The following are the practical inferences which Mr. Waters draws from these conclusions as respects the treatment of asphyxia:

"1. That efforts should be made to restore suspended animation in all cases when asphyxia has not been of a very prolonged duration.

"2. That, considering the effects of the hot bath, its prolonged use, whether respiration has ceased or is feebly going on, is not only inefficacious, but dangerous; and even to its temporary use—as, for instance, when the body is plunged into the bath and immediately withdrawn—there are serious objections, as it causes a loss of valuable time and produces no direct benefit. The object of the bath can only be to excite respiratory movements; these will probably be better brought about, so far as an influence of this kind can have any result, by alternately dashing hot and cold water over the body, or by the application of hot and cold baths, &c.

"3. That, judging from the increased mortality of the animals experimented on which were put into the hot bath, as compared with those not so treated, it is safer practice, and more likely to lead to a favourable issue, to omit all artificial aid in cases where respiration is feebly going on, than to make use of the hot bath.

"4. That in the treatment of asphyxia all our efforts should be primarily directed to restoring or continuing, as the case may be, the respiratory movements; and that all measures which have a tendency to load the lungs or embarrass the respiration should be avoided."

Dr. Waters looks upon Dr. Marshall Hall's method of producing artificial respiration as the best method we are yet acquainted with.

XI. *Letter relative to the preceding Paper on Asphyxia, &c.* From Sir B. C. Brodie, Bart., F.R.S.—This is a short letter concerning the period of the continuance of the heart's action in the so-called cases of asphyxia, in which Sir Benjamin Brodie remarks, that in the numerous experiments which he has made on full-grown warm-blooded animals he has "never known the heart to continue to contract after complete submersion, so as to maintain the circulation of the blood for more than a very few minutes, probably four or four minutes and a half at the very utmost." The contraction of auricles and ventricles which may be seen if the thorax of the animal be opened and the heart exposed to air, even at a much later period than that above mentioned, are "not to be confounded with the rhythmical contractions of the different parts of the heart in succession, which are necessary for the circulation of the blood."

Sir Benjamin believes that, except in cases of syncope (in which cases the left side of the heart is filled with scarlet blood), the rhythmical action of the heart can never be restored if it has once ceased.

XII. *A Contribution to the Pathology of the Pons Varolii.* By Hermann Weber, M.D.—Three highly-interesting and instructive cases, with all details, are given in this paper, and the following physiological inferences drawn by Dr. Weber from them:

"1. That the conducting fibres for the limbs passing through the pons, as well the motor as the sensitive, decussate below the pons. 2. That there are no sensitive fibres for the limbs in the lower or anterior part of the pons. 3. That the upper or posterior part of the pons contains sensitive fibres for the limbs. 4. That the intellectual functions of the brain are independent of the pons varolii. 5. That the nerves regulating the state of the pupils seem to be in close connexion with the pons varolii. 6. Extensive lesion of the pons seems to be associated with disturbance of deglutition, articulation, and also respiration; but it is possible that these phenomena do not depend so much on the alteration in the pons itself as on the influence exercised through it on the adjacent parts of the medulla oblongata."

We should have been glad had our space permitted an analysis of the cases themselves.

XIII. *On Disease of the Kidney, accompanied by Albuminuria.* (Second Paper.) By W. H. Dickinson, M.B.—This paper is in reality the completion of one published in a former volume of the 'Transactions,' in which the author endeavoured to show that there are two essentially different diseases which have passed by the name of Bright's disease—one in which the kidney is increased in bulk, remaining, however, smooth on surface, owing to an inordinate secretion within the tubes; and the other, in which the kidney is for the most part diminished in size, granular and uneven on the surface, owing to an affection of the inter-tubular structures. The present contribution deals with the clinical details as afforded by the post-mortem records and histories of cases accumulated at St. George's Hospital during ten consecutive years. These details should be themselves consulted.

We subjoin the following conclusions, which the author deduces from the observations brought forward:

"The smooth mottled kidney is the result of chronic nephritis. This disease may befal any person, the youthful being the most susceptible. It results from cold, from scarlatina, or some similar cause of renal hyperæmia. It begins with morbid symptoms, and usually terminates under a year. It is often temporary in its nature, or amenable to treatment.

"The granular kidney is the result of a degeneration which is peculiarly apt to take place in gouty or tuberculous persons.

"It is never seen except in adults, and is most frequent after the age of forty. It commences gradually, and is quite indefinite in its duration. This state of organ is necessarily irreparable.

"Chronic nephritis is characterized by dropsical effusion, a great liability to inflammatory attacks affecting the serous membranes and the lungs; diarrhœa, especially in the latter stages; pain in the loins, and finally coma, preceded by epileptic convulsions. The urine is scanty, often bloody, the albumen abundant, and the specific gravity comparatively high. Casts imbedding entire epithelial cells are characteristic of the disorder.

"Granular degeneration is to be recognised by a gradual failure of health, with sharpening of the features and discoloration of the skin, while dropsy is often absent or of small extent. Frequency of micturition is observed, especially at night, and there is great liability to bronchitis. There is also a tendency to valvular disease of the heart, as well as to extravasation of blood within the cranium. Epileptic convulsions sometimes occur, but a fatal state of semi-coma often comes on without any such antecedent. The urine is increased in quantity, except in the later stages, of low specific gravity, and often containing only a trace of albumen. The casts most commonly found are of coarse granular texture.

"One of the most conclusive arguments for the independence of the smooth mottled and the granular kidney—one which should have been more emphatically dwelt upon in the paper—may be found in tracing the results of scarlatina. That disease is familiar to everybody, as giving rise to the large white kidney, whereas the author has never heard or read of a case of granular degeneration which could fairly be traced to this cause."

XIV. *On a Case of Aneurysmal Varix in the Upper Part of the Thigh, following the Employment of Pressure for the Cure of an Aneurysm of the Posterior Tibial Artery.* By Oliver Pemberton, M.R.C.S.—The particulars of the case show that the aneurysmal varix was purely the result of the artificial pressure; the disease occurring ten months after the cure of the aneurysm, and within a few lines, either above or below, of the spot where the compressing pad was applied for upwards of nine months. The author discusses at some length the literature of the disease, but neither into this interesting part of the subject, nor into the account of the progress of his case, will our limits allow us to enter. The description of the case is well illustrated by two engravings, showing the arterio-venous communication.

XV. *On a Case of Aortic Aneurysm, in which a Communication with the Pulmonary Artery was recognised during Life by means of Physical Diagnosis.* By Willoughby F. Wade, M.B.—The cases brought forward by the author "as being the first in which such a lesion has been discovered during life by physical means," and "as establishing the rational diagnosis of similar lesions in future."

XVI. *Account of a Case in which the Corpus Callosum and Fornix were imperfectly formed, and the Septum Lucidum and Commissura Mollis were absent.* By J. Langdon H. Down, M.D.

XVII. *Syphilitic Inoculation, and its Relation to Diagnosis and Treatment.* By Henry Lee, F.R.C.S.—Mr. Lee, after giving an elaborate *résumé* of the facts connected with syphilitic inoculation, a subject of which the gravity is being daily more appreciated by the profession (thanks in no inconsiderable extent to his own labours), proceeds to give a summary of the conclusions to which he has been conducted by his observations on the matter in question.

The inferences to be drawn are as follows:—

1. That there are two forms of syphilitic disease, distinguished in their origin by the adhesive and suppurative kinds of inflammation, of which one is followed by constitutional symptoms, whilst the other is not.

2. That the adhesive form of inflammation may be distinguished from the suppurative by the nature of the secretion which it produces, and by the results of inoculation.

3. That both kinds of action may be communicated to the same individual at the same time, and that then the suppurative action will develope itself, as having the shortest period of incubation, and subsequently the adhesive action will run its regular course.

4. That when these two actions are developed upon the same part, the affection which results has not the characters exclusively of the

adhesive or of the suppurative inflammation, a mixed form of disease presents itself.

5. That the specific adhesive inflammation may be communicated to a person who has not previously had the disease, either directly by means of the discharge from the primary sore, or more indirectly through the secretions of a person affected with secondary symptoms.

6. That this latter mode of communication is not so common as the former, and appears to take place in general only when the part from whence the secretion is derived is in a state of increased or unhealthy action.

7. That under the circumstances last named, any open sore or abraded surface on a syphilitic patient MAY furnish an inoculable secretion.

8. That such open sore, or abraded surface, may be caused by mechanical irritation, by any secondary form of eruption, by a vaccine vesicle, or by a local suppuratory syphilitic sore in a person previously syphilitic.

9. That the blood of a syphilitic patient MAY communicate syphilis to a person previously unaffected with the disease.

10. That the cow-pox and syphilis MAY be inoculated at one and the same time, and that when such a twofold inoculation does take place, the results are in some respects analogous to those which follow the inoculation at the same time of an infecting and a suppurating sore.

11. That the pure vaccine lymph, even from a syphilitic subject, will not communicate syphilis.

XVIII. *Cases illustrating the Causes and Effects of Fibrinous Obstructions in the Arteries, both of the Brain and of other Organs.* By J. W. SIBLEY, F.R.C.S.—In this paper the author has collected the fatal cases bearing upon this subject which have occurred in the Middlesex Hospital for ten years, amounting to twenty-eight in number. He has analysed them with a view of endeavouring "to estimate the amount of evidence both in favour of and against the theory which supposes that these obstructions depend on fibrinous concretions or vegetations washed away from the cavities of the heart." Mr. Sibley observes, that there appears to be little doubt that plugs in the arteries may originate either by being formed in the situation in which they are found after death (and then they are, perhaps in all cases, the result of local arteritis), or by being carried to the artery from a distant point.

"One chief difficulty" (he remarks) "which has presented itself in assigning a distant cause to these plugs, is the fact that certain cases have been met with in which no vegetations or emboli have been discovered in the heart. In several of these apparently exceptional cases an inflamed lung was noted, and the presence of this lesion satisfactorily accounts for the occurrence of loose masses of fibrine in the blood. In this manner all the cases of obstructed arteries which have fallen under my own observation are accounted for, there having been noticed in each instance either vegetations, loose clots, or some obvious cause in the heart, or an inflamed lung."

The author distinguishes between the appearances presented by an artery filled with fibrinous matter from arteritis, and a plug washed from the heart or elsewhere.

XIX. *Case of Irideremia Totalis.* By E. C. Hulme, F .R.S. —In spite of this defect, vision was comparatively perfect.

The volume closes with—

XX. *A Case of Tetanus of nearly Fourteen Months' Duration.* By Daniel Meadows, M.R.C.S.—The patient was a sailor, aged thirty-six, in whom every kind of treatment was tried. Nothing held the disease in check so well as chloroform and hyperdermic injection of morphia.

> "At last, however, more than fifteen grains of morphia and several ounces of chloroform, used daily, only gave short intervals of quiet. . . . . During the week preceding death more than a pint of chloroform disappeared every twelve hours."

After death—

> "Several spots of effused blood were found on the spinal cord, and a flattened clot, not recent, about the size of a sixpence, was found on the front of its cervical portion, opposite the body of the fifth vertebra."

Two months before his symptoms began he had been kicked on the back of the neck during a quarrel.

Review IX.

Monographie Clinique de l'Affection Catarrhale. Par J. Fuster, Professeur de Clinique Médicale à la Faculté de Montpellier; Médecin-en-chef de l'Hospice Civil et Militaire.—*Montpellier*, 1861. pp. 616.

Clinical Monograph on the Catarrhal Affection. By J. Fuster, Professor of Clinical Medicine to the Faculty at Montpellier; Chief Physician to the Civil and Military Hospital.

There are certain classes of disease which, by the nature and variety of their phenomena, have a great attraction for the scientific physician. The causes of their phenomena seem to be but thinly veiled from his perception, and to be only just successful in eluding his researches. In the variety of their phenomena he sees a rich series of natural experiments, which cannot fail sooner or later to betray to him their origin. At the same time, he is sure that the central truth, when once dragged into the light, will serve to explain the relations and to dictate the rational treatment of countless forms of disorder. One great class of these diseases, for instance, may be included under the head of diabetes and its allies; a second, under that of gout and its allies; a third, under the head of fevers, properly so called. It cannot be doubted but that the true interpretation of these forms of disease will before long be discovered. In all probability the present generation will be more or less fully enlightened as to the nature of some of them, and meanwhile we have the certain gratification of knowing that our sober industry is day by day gathering together the materials for those discoveries which we anticipate. The book the title of which stands at the beginning of the present article, is devoted to the consideration of disorders included under our head of fevers. Perhaps the nearest equivalent which can be given for M. Fuster's name of the "catarrhal affection" is the name "influenza." Many other forms of disease are referred by him to that title—many, indeed, which are not and have never been attended with catarrh; but the author would perhaps look upon an acute case of influenza as the type of a class of diseases, each one of which he would consider to be but a modification of this typical form. M. Fuster is already well known to the profession as the author of works on the climate of France, and its connexion with sundry maladies. His present work shows much of that careful observation, neat arrangement, and clever exposition which distinguishes the writing of the first physicians in France. We regret, however, to be obliged to conclude that this work cannot, on the whole, be considered as a great acquisition to the literature of epidemics.

M. Fuster, instead of confining himself to a wide survey of influenzoïd diseases, to a careful definition of their eccentric forms, and reasonable inclusion of these within the original classifications; instead, moreover, of a careful collection of meteorological and like facts apparently related to catarrhal epidemics, a task for which he was epecially fitted; instead of devoting himself wholly to such a work as this, M. Fuster has excogitated a theory of the nature of the catarrhal affection, and has occupied himself in squaring all the facts to an elucidation of this theory. Unfortunately, we have every reason to believe his theory to be utterly erroneous, and his interpretation of symptoms to be purely fanciful. Curiously enough, M. Fuster is very bitter against Sydenham and others of his predecessors for the reason that they have interspersed much vain theorizing amid valuable descriptions of morbid phenomena. No theorizing is becoming in a sober work on medicine, save M. Fuster's own.

We claim a similar right of condemnation. M. Fuster's book is nevertheless, in spite of his theory, a valuable one. He has an acute perception of relationships, and has endeavoured to shepherd, under the general name of the catarrhal affection, an irregular troop of disorders whose real nature is too liable to be forgotten or misunderstood. Luckily, moreover, the humoral theory, which M. Fuster has adopted, being purely fanciful, may be modified to explain all kinds of phenomena; and we are thankful that this easy modifiability of the theory has obviated in general the necessity of distorting the facts. We had rather, however, have been spared the constant reference of them to the agency of acrid humours; to the coction of crude mucosities, or to the expansive efforts of a benignant entity—the fever—which strives to overcome "spasm and concentration," and so forth.

We will endeavour now to give in M. Fuster's own words an idea of the task which he has set before himself, and which, his theories of the nature of the disease apart, he has industriously and well performed. He says—

"The doctrine of the catarrhal affection at the elevation, where we have endeavoured to establish it, thanks to a mass of ancient and contemporary observations, gathers within its principles the vast tribe of diseases differently called inflammatory, serous, watery, lymphatic, catarrhal, rheumatic, mucous; colds, influenza, &c.; it defines their essential characters, marks their relations and their differences, settles their diagnosis and treatment, destroys their false likenesses to the inflammations of mucous membranes, confirms them in a distinct nosological class, raises them up, in a word, from a decadence as long as it is ill deserved, a decadence so profound, in spite of their great frequency both in the sporadic form and as epidemics, that they are not even at this day mentioned in the pathological treatises which are looked upon as classical." (p. 612.)

M. Fuster looks upon common colds, epidemic catarrhs, and their accompanying diseases, upon influenza with all its satellites, and also upon swarms of chronic ailments, as but different manifestations or fragments of one grand and imposing pathological figure. Thousands of sufferers from supposed chronic inflammations have been in reality entertaining the catarrhal affection unawares in some one of its numberless forms. Among the *filles méconnues* of the catarrhal affection, we find enumerated in a single page chorea, hypochondria, apoplexy, epilepsy, mania, hyperæsthesia, paralysis, neuralgias, inflammations of the skin, of the mucous membranes, of the deep muscles, of all the parenchymata; hypertrophies, atrophies, thickenings, hardenings, indolent and degenerative tumours; mucous and serous fluxes cast out by the natural passages, infiltrated into the tissues, or collected in the cavities. Numberless diseases, hitherto only known under the vulgar names of bronchitis, pleurisy, pneumonia, myelitis, peritonitis, gastritis, dysentery, rheumatism, &c., and hazily regarded as the results of inflammation, are often in truth but so many forms of the Protean affection to which M. Fuster has devoted his six hundred pages of theory and description.

The author gives in his first part an able exposition of the general character of the catarrhal affection, with a survey of the medical traditions relating to it. In the second and third parts we find a very carefully arranged and valuable record of the principal catarrhal epidemics and influenzas which are known to have existed.

M. Fuster has of course made use of the often elaborate and always interesting descriptions of these curious epidemics which we have inherited from physicians who lived and practised during their prevalence. In this part of his work M. Fuster has shown much acuteness and breadth of view. In support of his theory of the unity of the catarrhal affections, he has carefully collected the records of abnormal phenomena, and has happily pointed out their relations to the more general ones. We can warmly recommend the perusal of these chapters to any person who wishes to attain to an accurate and vivid conception of the manifold phases and probable nature of this most interesting class of diseases.

The fourth part contains the clinical teaching which is to be deduced from the doctrines of the first part, and from the consideration of the vast range of facts included in the second and third. We shall endeavour to set out briefly the chief argument of these chapters, and in the course of our short analysis the more striking features of the catarrhal affection will become manifest.

M. Fuster's theory, as it pervades the whole, could with difficulty be dissociated from his facts, and we shall therefore present it much as we find it. The author divides the catarrhal affections into three classes—the common catarrh, the rheumatic catarrh, the mucous or pituitous catarrh. In the first the irritation of the solids and the depravation of the humours is general, and does not bear especially on one part of the system rather than on another. In the second or rheumatic class we find nervous irritation, irritation of the fibrous tissues, and, generally speaking, irritation of the solids rather than of the "lymphatic fluids." The mucous affection is the opposite to the rheumatic. The irritation of the lymphatic or muco-serous fluids in these cases exceeds that of the solids. These latter cases are frequently accompanied by worms. These three classes, however, present the same general features. In the acute cases of each class we see four distinct periods or stages, which in the chronic cases are more or less confused and ill-defined. First, the period of threatenings. Certain symptoms generally appear as the forerunners of the disease itself. Such symptoms are wandering pains, repeated shiverings, aching of the limbs, heightened sensibility, a general feeling of fatigue and oppression, almost always coryza and cough. The second period, or that of invasion, sometimes consists only of the preceding symptoms in an exaggerated form; but for the most part we see also alternations of chills and sweating. There is during this period a hyperæmia of the nervous system, which acts irregularly; a reversal of the true direction of the organic movements; and a peculiar acridity of the fluids. The third stage would seem to have no other object than the restoration of the equilibrium of the disordered activity of the system; and the destruction at the same time of the special alteration in the fluids. To this end are directed its highly excited states of circulation and of heat production, which are fit to disseminate the morbid phenomena over the whole body, and to deliver over the mass of the humours to a "profound and salutary elaboration." The last period, one of resolution and of crisis, announces the triumph of these efforts. This is the time of calm after the storm. At this moment we discover the signs of a general relaxation, preceded, accompanied, or followed by critical excretions. The duration of these periods is relatively variable. In a serious attack the spasm will be more obstinate and profound, and the reaction will have more trouble in bursting through it and in sustaining itself. It is evident that in such a case the expansion will be delayed, will be more or less pronounced, or may not come to pass at all, as in fatal cases. On the other hand, a prompt and ready reaction will bring about a rapid relaxation and free crisis. What we see then, for instance in the "mucous catarrh," is something of this sort. Some atmospheric influence attacks the skin, endowed as it is with an exquisite sensibility, and charged besides with a great secretory function. This direct attack upsets by sympathy the action of the nervous system of the mucous membranes, and of the lymphatic apparatus. This ends in spasmodic phenomena and irritation of the mucous membranes, a depravation of the white fluids, and a repulsion of the mass of the humours from the circumference to the centre. Sooner or later the circulatory system takes the alarm, and floats back with more or less activity towards the periphery, the fluids determined towards the internal organs. This produces an oscillating fever of good intent, and only dangerous when excessive or disordered in its action. Its efficacy is seen in the final release, the relaxation of the spasms, the dissipation of the irritations, the resolution of the inward stases, and the freedom of the critical excretions. The indications for treatment are "to temper, soften, and appease the acrid lymphatic juices of the initial periods;" and in the latter stages to repress the turbulence of the solids and of the liquids, and to make a way for them towards a complete and conformable reaction. Thus, in all the numerous forms in which a catarrhal affection may develope itself, we have these three great features by which the true nature of the disease may be recognised: 1. Irritation; 2. Coction; 3. Exhalation. Whether the attack be acute or insidious; whether the poison bear upon the brain, the lungs, the kidneys, or any other organ; if these three stages be present, the affection is a catarrhal affection, however much it may resemble true rheumatism, an inflammation properly so called, phthisis, softening of the brain, or any other disease.

M. Fuster sketches at some length the phenomena of catarrhal meningitis; of catarrhal sore throat, in its various degrees from quinsy to gangrene; of croup, which is "exclusively a catarrhal localization of the malignant and insidious kind." Under the head of croup, he remarks: "Sometimes in the catarrhal affection the play of the respiratory apparatus is suddenly arrested in a very few hours under the deleterious influence of the disease. Sudden death by suffocation is the result." It is also remarked that the same sudden stoppage may in like manner happen to the nervous or circulatory apparatus. Of catarrhal bronchitis, pleurisy, pneumonia, &c., M. Fuster says: "All these chest fluxions seem cast in the same mould." The precursory symptoms of the "mother affection" seldom fail. A fever, manifestly catarrhal, ushers them in or accompanies them; assists in their resolution; impedes them or associates itself with their malignant manifestations.

Enteritis generally, and above all dysentery, are claimed especially as having very commonly a catarrhal origin. The essential symptoms are here again the same, but of course modified by the change of localization of the morbid humours, and by the different functional derangements. Such a dysentery is to be considered, in fact, as a cold in the large intestine. It shows the precursory and all the other symptoms of the typical catarrhal affection. Moreover, this malady may be almost insignificant, or, on the contrary, as fatal as the cholera or the plague. Again, when the catarrh settles down near the surface, in the joints or elsewhere, it mimics true rheumatism. The accompanying fever has here again a like "ensemble." The pains are very wandering, sharp, superficial, and less obstinate than in true rheumatism. The warmth of the bed eases them. Again, all kinds of skin eruption may be the "envelopes" only of real catarrhal disease. We will content ourselves with mentioning, from among those cited by M. Fuster, roseola, erysipelas, scarlatina, rose or petechial spots, herpes, psoriasis, aphthæ.

It is clear, from what we have stated, that in a favourable catarrhal case, the first period, or that of spasmodic irritation at the surface, should soon give way to the reaction. When, however, the alteration of the "lymphatic fluids" is gradual but constant, we shall observe a chronic condition of irritation. In such cases the fever succeeds with difficulty in bringing about the crisis and general relaxation. We have a chronic struggle, in which the superficial spasm and the irritation may be predominant, and during which, therefore, the morbid humours are kept hovering about the internal organs. In and about these organs they stagnate, and gradually eat away (*creusent*) their substance, until some chronic structural disease, such, for instance, as phthisis, becomes manifest. That phthisis is often of catarrhal origin, M Fuster thinks few will be bold enough to deny. He adds that the two diseases are very often at least of the same nature.

Of the other chronic affections which may have a like nature—that is, we suppose, which may be *directly* engendered by the catarrhal humours—are asthma, chronic pneumonia and bronchitis, dropsies, anasarcas, aneurisms, scirrhosis, mental alienation, &c. Lest it should be supposed that any such cases of chronic disease ought to be attributed to inflammation, M. Fuster presents us also with his theory of inflammation, pointing out the great difference between such an attack and the asthenic diseases accompanied by structural lesion due to erosion by stagnating catarrhal humours. He says:

"In inflammation, properly so called, the only real and admissible inflammations, all the pathological results emanate from, and owe their origin to, the over-activity (*jeu forcé*) of the wheels of the machine, due to the stimulation of a too rich and exuberant blood. They arise by virtue of this effervescence, proceed and grow according to its progress, decline and become extinct as it decreases and passes away. Local inflammations obey this law, as well as the inflammatory fevers: just as the localized catarrhs also follow the law of the catarrhal fever. It is by excess of energy or tone that organs become inflamed; they only repair themselves in consequence of a reduction in the abundance or richness of the blood. The inflammatory condition has at the bottom but apparent relations with the catarrhal.

"Here the primary irritation principally affects the nervous system, and betrays itself sometimes by signs of a more or less painful hyperæmia, sometimes by a profound debility or great prostration; generally, however, by a strange mixture of this irritation and this debility. The humoral alteration affects especially the lymphatic or sero-mucous liquids. The fluxion which the irritation excites, is set up chiefly at the expense of this part of the fluids. The blood is not long certainly in taking part in this movement, which gives rise to a fever of more or less vivacity, and adds the red fluids to the matter of the fluxions, and augments the rapidity of the fluxionary currents." (p. 598.)

It is much to be regretted that M. Fuster has adopted such retrograde doctrines as these. In spite of this mere theorizing, however, we have derived much pleasure and much instruction from his book. All attempts at classification of disease must meet with sympathy from the scientific pathologist; and M. Fuster has done good service in making a careful survey of a class of diseases of the highest interest, and in describing with vivid truthfulness many long series of concurrent phenomena. An extract of those parts of the work which are devoted to the description of influenza in its numberless forms would occupy far too much of our space. We are, however, obliged to confess that, in his anxiety for generalization, M. Fuster has overlooked many a grave difficulty.

Much as a severe "cold" resembles an attack of influenza, it must nevertheless be borne in mind that while the former is referred by universal consent to the action of certain atmospheric conditions, the latter, on the other hand, appears under all conceivable meteoric conditions, and cannot be referred with more than a show of plausibility to any known atmospheric cause whatever.

Again, we think M. Fuster has not shown sufficient caution in his estimate of the feverish symptoms which accompany many forms of disease. The concurrence of a like symptomatic fever with any two or three different morbid conditions of bodily organs, does not prove that these morbid conditions are therefore the same in kind. The alleged processes of coction, or of irritation of the humours, prove still less, being purely imaginary and evolved out of M. Fuster's own consciousness. Thirdly, we conceive that M. Fuster has often mistaken indirect for direct causation. He has not drawn the distinction with sufficient clearness between the eccentric forms which a disease may put on and the accidental complications which may coexist in independence upon it, or may be only indirectly referable to it. Any other malady may accidentally attack an influenza patient without giving occasion to any theorizing as to a common nature for them both; and also epidemic catarrhs may lose themselves in other independent diseases, which existed before the appearance of the catarrhal epidemic, or which may arise during its prevalence or during its subsidence. We do not mean that M. Fuster has ignored these possibilities, but that he occasionally forgets to give them due weight. He mostly errs, indeed, in his constant reference of secondary disorders to catarrh as the natural offspring of the latter, when in truth the catarrhal disease has only acted as a predisposing cause. Nothing is more simply conceivable than that the intense prostration which attends catarrhs should encourage the defection of organs which had long been in an uncertain state. We should look upon the supervention of phthisis, for instance, or of diabetes, as an event which must without doubt be regarded in this light. M. Fuster has, nevertheless, done good service in again drawing attention to the fact of the existence of symptoms referable rather to derangement of the nervous than of the blood system; but as these symptoms may be summed up in the single expression, "want of power," it must be remembered that such symptoms may readily arise from exhaustion in any disease of a serious kind, and may so mask all the rest as to lead the observer to look wrongly upon them as essential or primary. In a word, M. Fuster's work is another instance of the marvellous power inherent in French authors, of setting out the haziest ideas in the clearest and most systematic form, and which makes their writings so imposing when studied by unwary readers. M. Fuster knows, perhaps, not more than many other physicians of the real nature of influenza, and its allies; but he has the knack of putting what he does know in a plausible form, and the talent of embellishing his writing with the discoveries of an accurate and ever wakeful observation. Let us hope that his example may induce others to investigate this important class of diseases with equal industry, equal power of observation, equal clearness of thought, and, we will add, with a higher scientific knowledge.

REVIEW X.

Undersögelser om Barnehovedet i obstetricisk Henseende. Ved Dr. A. STADFELDT. (Særskilt Aftryk af 'Bibliothek for Læger,' V.R., III. B., 2 H.)—*Kjöbenhavn.* 8vo, pp. 84.

Researches on the Head of the Child in an Obstetric Point of View. By Dr. A. STADFELDT. (Reprint from the 'Bibliothek for Læger,' Fifth Series, Vol. III., No. 2.)—*Copenhagen,* 1861.

THE two principal factors in the birth of the child are the *dynamic* and the *mechanical*—or, in other words, the expelling power and the mechanical relation between the passages of the mother and the fœtus. The author selects as the subject of the present essay the reciprocal condition of the pelvis of the mother and the child's largest and least yielding part, the head.

"Contrary to the usual custom, I have made the head of the child in its normal condition the starting point of my investigations respecting mechanical abnormities during birth. In studying and describing the changes of form of the head under normal and abnormal circumstances, I have next endeavoured to determine the conditions on which these changes depend, their prognostic and diagnostic importance, so far as they have come under my observation." (p. 4.)

The author's investigations are based upon observations made in the Lying-in Institution in Copenhagen, and on the preparations preserved in the Museum, kindly placed at his command by Professor Levy. The first subject to which he directs his attention is the *form, size, and power of accommodation of the head of the child.*

The skull of the fœtus in general presents an oval form, with its longest diameter from the forehead to the occiput. Still it is not a completely symmetrical oval, for as in adults it is well known that the cranium exhibits a constant and tolerably uniform *asymmetry* in the two lateral portions, precisely the same kind of obliquity is found in the head of the child. When the latter is viewed from above, and held so that the axis of vision divides the lines drawn from the superior point of the occipital bone, or a little beneath this point, to the orifice of the meatus auditorius on either side, we shall always, the author believes, find the part bounded by the lines to be more prominent on the left than on the right side. This obliquity gives rise to a corresponding divergence in the situation of the tubera parietalia, the frontal bones, &c. The author proceeds to show, that while this physiological want of symmetry may become more marked after difficult labours, it is not wholly attributable to the pressure exercised on the head during birth. If this view is correct, it may be a question, how far the difference alluded to depends upon a greater development of the left hemisphere of the brain, connected with the ordinarily greater strength of the right side of the body, which would, in that case, not always be the result of the more frequent exercise of the muscles on that side.

Besides the oval, we meet with the round form of the head, where the occipito-frontal diameter is proportionally less; and the elliptical, where it is greater than in the first-named variety. Smellie, and also Saxtorph,* have assumed that these varieties depend partly upon the different situation and course of the head in passing through the pelvis. The correctness of this view is not to be denied, at the same time it is indubitable that these three forms may also exist as individual, and perhaps as national, peculiarities, independently of pressure during birth. To elucidate this question, the author undertook the measurement of the heads of one hundred children taken at random, between the eighteenth and forty-eighth hours after a natural and not excessively difficult labour. The results he has represented in an elaborate table, the measurements being given in Danish inches, each of which is equal to 11·6 Parisian lines. From the materials thus obtained, he has investigated the size of the child's head in cases of premature birth in children born at the full period of married and unmarried women, in the children of primiparæ and multiparæ, and in boys and girls. We can only very briefly allude to a few of the principal facts thus ascertained.

Of 75 children born at the full period, 22 presented a predominantly oval form, 35 were referrible to the elliptical, while 18 exhibited the round variety of the skull. Adopting the division of the late Professor Retzius, these numbers would give us 57 *dolichocephalic* to 18 *brachycephalic* skulls, quite bearing out, so far as it goes, the classification from the pen of that distinguished physiologist, to be found in the twenty-fifth volume of this Review (p 503), in his admirable essay on the 'Present State of Ethnology, with reference to the Form of the Skull,' where the "Danes" appear among the "European Dolichocephali." Dr. Stadfeldt suggests that these forms of the head may modify the mechanism of parturition, but at the same time states that it is a point to which he, like preceding writers, has directed but little attention.

It has frequently been stated in obstetric manuals that the heads of boys are larger than those of girls, and that the greater mortality known to exist during birth and in the earliest period of life among male than among female children depends upon the greater disproportion in the case of the former, between the head and the passages concerned in labour. It has also been stated that delivery is attended

* Gesammelte Schriften, pp. 77–85. Kopenhagen, 1804.

with worse consequences, for mothers who have given birth to boys, than for those who have brought forth girls, and by reason of the same mechanical disproportion. Having reviewed the statements of Clarke, Collins, and Simpson, the author gives the results of his own measurements of the heads of 38 boys and 37 girls, and also those of Thulstrup, of Christiania, in the cases of 65 boys and 66 girls, which agree more nearly with his own than those of Clarke.

All these measurements fully establish the fact, in opposition to the statement of Spöndli, who designates the assumption that the heads of male children are larger than those of female, as "a popular prejudice, which has been at least tacitly adopted by many well-known writers." Nevertheless, the author expresses a strong doubt of the correctness of Dr. Simpson's opinion, when he considers the greater mechanical disproportion, due to the relatively larger head, to be the *sole* cause of the worse prognosis for mother and child in the case of male deliveries. The difference is so slight, he observes, that it is really incomprehensible how it could have so enormous an influence upon prognosis, that during birth, where so many forces are set in motion, the trifling difference which exists in the circumference of the head should turn the scale so much against the boys. He believes that one or more other causes have *also* an important bearing on the established worse prognosis for boys than girls during labour or in the first days of life. Among these causal elements is the greater readiness with which, under unfavourable circumstances, the nutrition of the former suffers than that of the latter does, and the greater need of nutriment exhibited by male than female children. He shows, from Dr. Collins' tables, that the proportion of deaths among boys compared with girls is less unfavourable in the case of primiparæ, where it is as 148 : 100, than in the case of births in general, where it is as 151 : 100; while, if the cause of the greater mortality among boys than among girls were purely mechanical, it should have greater effect among primiparæ, in whom the mechanical disadvantages are greatest. Further, the author shows, from a table of his own, that the greater mortality of boys than girls is much more marked in Copenhagen than in the market towns and country districts of Denmark; nevertheless, it is against all probability that the difference between the heads of boys and girls should be greater in the former than in the latter localities.

The author gives a table of the measurements of the heads of 25 children of married women, compared with those of the children of 50 unmarried. As was to be expected, the difference is slight and perhaps accidental. Without attaching weight to the result, he observes that the head in the children of the married parents presented a more elliptical form, while among those of the unmarried it was more oval or roundish.

A comparison of the heads of the children of 32 multiparæ with those of 43 primiparæ affords much higher mean measurements for the former than for the latter. Corresponding to this is the mean weight of the children—$6\frac{7}{8}$ lbs. in the case of the former, against $6\frac{5}{8}$ lbs. in that of the latter. This difference is probably owing to the generally admitted fact, that labour usually supervenes in first pregnancies somewhat earlier than it does on subsequent occasions.

The author next proceeds to consider the power of *accommodation of the head of the child in its passage through the pelvis and soft parts,* derived from the yielding nature of the bones. The numerous deviations which the head thus undergoes in form and size have, he observes, been very well described by Smellie. Dr. Stadfeldt in his first table, in fifty-seven successive measurements, exhibits the changes thus produced by measuring immediately after birth, and again thirty-six hours later, noting each second measurement with *plus* or *minus,* accordingly as it presents an increase or diminution on the first. One head alone, which was very small, exhibited no difference between the two measurements. He concludes that the compression generally takes place in the occipito-frontal diameter, where it can be divided between two sutures, the coronal and lambdoidal, then in the sub-occipito-bregmatic circumference and the posterior transverse diameter, while the compensation usually takes place in the occipito-mental diameter. His opinion thus differs from those hitherto advanced, which refer the predominant compression to the postero-transverse diameter. He agrees with Kiwisch in fixing the limits to the compressibility of a single diameter in the head of a living child at from two to four lines, a limit which, he adds, cannot in general be exceeded without producing fracture, rupture of the membranes at the suture, or other injuries. In a medico-legal point of view, the question is of interest, whether fracture of the bones of the head can occur during a labour which terminates naturally, even where suspicion of disproportion is but slight. The author relates a case of fracture which occurred under his own observation, and which was the more remarkable, as the labour was comparatively rapid, and the measurement did not show any decided obliquity of the pelvis.

The total gain in capacity attainable by the changes above alluded to is difficult to determine, but the author believes that the younger Stein is correct in setting it down at about three-quarters of an inch.

Dr. Stadfeldt concludes with some observations, illustrated with cases, on the *diagnostic signification of the results of accommodation.* His carefully-written essay, and more particularly the elaborate table of measurements it contains, will be found to constitute a valuable contribution to practical midwifery.

REVIEW XI.

1. *On the Health of Merchant Seamen.* By J. O. M'WILLIAM, M.D., &c., Medical Inspector of Her Majesty's Customs. pp. 10.
2. *The Health of the Royal Navy considered, in a Letter addressed to the Right Hon. Sir John S. Pakington, Bart., G.C.B., M.P.* By GAVIN MILROY, M.D., F.R.C.P., &c. pp. 69.
3. *Reports of the United States Sanitary Commission.*

ONE of the most marked features of the present time, and more than anything else, in our opinion, denoting a true advance in civilization, and in that humanity which ought to characterize an advancing civilization, is the attention paid to the removal of the causes of disease. This is most strongly displayed where the effort is most likely to have success—that is, in the instances of particular classes, such as the army and the navy and the mercantile marine. The papers which form the heading of this article are good examples of the kind; and we can strongly recommend them to the careful perusal of all (and who should be excepted?) who take an interest in the welfare of their fellow-men. This advantage belongs to inquiries of the kind, and enhances their value—viz., that the results derived from the experience, man being the subject of it, are applicable, more or less, to society generally.

Each of the papers we have just referred to we shall notice briefly in order, and, owing to our limits, more briefly than we could wish.

The first, that of Dr. M'William, 'On the Health of Merchant Seamen,' is mainly statistical; and the praise is due to its author, that it is the first attempt that has been made to bring the subject in a statistical form before the public.

His introductory remarks, showing how, from an early period, attention has been given by the government of our country to a body of men to whom its prosperity is so much owing, and, as the nursery of the Royal Navy, so much of our national strength and glory, will be read with much satisfaction. Yet regrets cannot but be associated with them, inasmuch as we learn that the rules enacted for the preservation of the health of the men afloat have been too often neglected, owing to a short-sighted policy and to that vicious propensity to gambling adventure which risks everything to make money with as little expenditure as possible. In this unprincipled way, too many of our trading ships are sent to sea in an unseaworthy condition, whence the many and excessive number of shipwrecks; too many leave our ports unprovided with the required medical comforts and medicines, whence scurvy and other diseases amongst their crews; and, not a less evil, the accommodations in too many instances, especially the sleeping berths, are of an unwholesome kind, either from the want of due ventilation, or from being crowded, enfeebling the men, and thereby predisposing them to diseases which a vigorous unimpaired constitution might resist. These are all evils which in time, we hope, will be corrected; and we express the feeling not in despair, inasmuch as the shipowners themselves would find it on the whole most for their profit to correct them. The active interference of Government, by means of inspection, seems hardly to be practicable; our commerce is so vast, hundreds of vessels often leaving the port of London in one tide.

The statistical results which Dr. M'William has obtained are presented in the form of tables, four in number. We shall give only the averages. Those specially interested in the subject will do well to make themselves acquainted with them in detail; indeed, they should possess themselves of the pamphlet, the brevity of which is its least recommendation.

Table I.—Deaths from all causes on board of merchant ships employed in the home and foreign voyages, from 1852 to 1860, both years included:

Tonnage	4,009,765
Force	170,586
Deaths	3,249
Ratio per 1000	18·98

Table II.—Main causes of death during the same period:

Fever	734	......	rate per 1000		4·30
Dysentery	272·9	......	,,	,,	1·62
Scurvy	37·2	......	,,	,,	·22
Drowned & other accidents	1,335	......	,,	,,	7·72
Causes not ascertained	868	......	,,	,,	4·88
Total	3,249	......	,,	,,	18·62

Table III.—Deaths on foreign and home voyages during 1857, 1858, and 1859:

On foreign voyages—strength, 132,339; deaths, 2,416; ratio per 1000, 18·20.

On home voyages—strength, 43,259; deaths, 1,043; ratio per 1000, 24·20.

Deaths in the Royal Navy on the home station from all causes from 10 to 11 per 1000 during a series of years.

Table IV., showing the number of seamen left abroad from foreign-going British ships, and the number engaged abroad and brought home in lieu thereof, in 1857, 1858, and 1859:

Ships	9,270
Tonnage	3,392,762
Men	132,339
Deaths abroad	2,410
Deserted	11,957
Discharged	22,618
Total left abroad	33,319
,, engaged abroad	30,613
,, difference	6,373

These tabulated results we have given, with the hope that through our medical readers the attention of shipowners may be directed to them, and with profit in the way of improvement. Such, we are sure, would be the wish of their compiler—now, alas! no more, struck down in his vigour by death. If this paper be his last contribution, and we believe it is, to the great cause of sanitary science to which he so devoted himself, it is worthy of him—of him who steered the steamer down an African river to the sea amidst dangers more formidable than any which beset the mythic Argo. This heroic exploit marked well the individual, his indomitable zeal and energy, and should bear down his name to posterity in securing approval of a higher kind than that bestowed on merely animal courage in any feat of war, however gallant.

Dr. Milroy's pamphlet, 'On the Health of the Royal Navy,' is of ampler scope than the one we have just considered. It is more than statistical; it treats of the prevailing diseases to which seamen are subject, and in a very instructive way as to their causes. Brief as an epitome, it is not as barren; on the contrary, it is well illustrated by striking examples. The materials from which it is composed are mainly derived from three sources: 'The Statistical Reports of the Health of the Navy,' prepared by Dr. Bryson; the 'Papers relating to the Laws and Practice of Quarantine;' and the author's own large experience.

It is encouraging to see how, with improved sanitary regulations in the navy, sickness of the crews and their mortality have abated, and their efficiency has increased, and this in a very high ratio. There was a time, not more than eighty years ago, when ships of war "were often like the then jails and prisons, with their holds so foul and noisome, that the air used to become so contaminated, as in innumerable instances to produce instantaneous and irremediable suffocation." Then the mortality in the navy was probably two-thirds greater than that experienced during the last three years of the great French war, when the average death-rate of our fleets, manned by 138,000 of all ranks and ratings, was about 33 per 1000 of the entire strength; now estimated at from 3·6 to 37·7 per 1000, varying according to the degrees of salubrity and insalubrity of the several stations, and the nature of the service on which the fleets are employed; or, from all causes, including accidents, from 8·7 to 47·6, the lowest mortality being on the Pacific and Australian stations, the highest on the East Indian and China stations, where dysentery and fever are most prevalent, especially the former.

The diseases which produce the greatest amount of deaths in the service are fever, diseases of the bowels, and diseases of the lungs; the same, with venereal disease and rheumatism, interfering most with efficiency and most productive of invaliding. To these, Dr. Milroy's comments are chiefly confined. "Out of the 2125 deaths from disease alone, during the three years, 1856, 1857, and 1858 (the total loss from all causes amounted to 2735), 559 were due to fevers, chiefly of the continued type; 715 were due to diseases of the bowels, chiefly alvine flux and Asiatic cholera; 440 were due to diseases of the respiratory organs.

The instances he adduces of one or other of these diseases prevailing in certain ships, whilst other ships on the same station remained healthy, are very instructive, as showing almost to demonstration the cause, if not of the malady, at least of its fatality, such as bad air connected with over-crowding and defective ventilation; such as impurities in the hold from accumulation of filth and the decay of the timbers, the latter connected with the growth of fungi. The examples given, so well authenticated, are invaluable; one of them is the notorious *Eclair*, of yellow-fever notoriety; another is the *Britannia*, of cholera note, in which ship, out of a crew of 920, no fewer than 229 were attacked (besides nearly 400 with diarrhœa), of whom 139 died; the pestilence, it is remarkable, breaking out in its full fury in the course of one night, when, owing to boisterous weather, the lower deck ports were closed.

The same circumstances which intensify one of these diseases, such as cholera, or yellow fever, are shown by the author to conduce almost as much to others, to dysentery and consumption, and this, no doubt, chiefly by their tendency to deteriorate and weaken the constitution.

The fact, that consumption is the most uniform and persistent cause of the large destruction of life in our navy, may surprise many of our readers, considering that a sea voyage is so often recommended to those labouring under or apprehensive of this disease, and can hardly otherwise be accounted for than by the deteriorating influence referred to.

Another fact may excite some surprise—that of the deaths from disease exceeding in number, even during war, those from wounds and accidents. Thus, in China, during the protracted hostilities from 1840 to 1843, "the total deaths from wounds received in action amounted to 24, or in the proportion of about 1 of 1000 of mean strength; while the total loss from febrile and dysenteric complaints alone, amounted to 547, or in the ratio of nearly 30 to the same amount of force." Even in the campaign in the Crimea, whilst the annual rate of mortality was only 3 per cent. from wounds, it was 20 per cent. from disease.

What cannot be too much insisted on is, that all the diseases which have been the scourge of the navy, as well as of our army, are in a great measure of a preventible kind; and, as ample experience has proved, can be, and have been prevented by proper attention to a ship and her crew, according to the rules of sanitary science. Captain Cook returning, after circumnavigating the globe, with a perfectly healthy crew, and with the loss of only one man, and that from disease not contracted at sea, is a memorable instance of the kind; as is also that of the ship *Glattan*, referred to by Dr. Milroy, which made a similar voyage with the same success—a ship that was fitted up especially under the direction of Count Rumford and Sir Gilbert Blane.

We are tempted in parting from our author to make an extract from his very able pamphlet, showing well the spirit in which it is written and the great truths which it inculcates—

"It would be a fine thing, certainly, for science and humanity, if ten per cent. say, could be struck off from the present ratios of sickness and mortality throughout the navy. The saving of the loss of service, not to speak of the expense, would be of no slight consequence in a national point of view. Just in proportion, too, to every diminution of disease and death, so, we may be assured, will the robust tone of health and the vigorous alacrity of the well-man be enhanced; for it should never be forgotten that there may be, and certainly is, a great deal of lowered health in all communities, short of the occurrence of actual illness. The problem of State hygiene is not only how to prevent sickness in the public service, but how to maintain the highest possible effectiveness of the men. For the working out of this problem in the navy, the more that the medical officers of our ships are regarded as the preservers of health, and not merely the healers of disease, the better. The more, too, that the bearings of sanitary science are studied by the executive officer, the more they will, I am sure, appreciate their importance, and the more efficient aid they will ever be ready to give to all the recommendations of the medical officers. It is only necessary for them to peruse from year to year the very instructive statistical reports of the service, to fully understand how much the health of a crew depends upon the condition and sanitary arrangements of a ship." (p. 61.)

The Reports of the American, we do not venture to call them of the United States Sanitary Commission, are a peculiar feature in the fratricidal war now raging between the Northern and Southern States, the Commission in question having been undertaken by individuals in their private capacity, and conducted, it would appear, with the sanction indeed, but without the aid of Government, having to depend for pecuniary support on private liberality. Viewed in this light, the undertaking cannot but be considered highly creditable to those engaged in it, denoting a high-minded patriotism and a most humane feeling, and reflecting the same on the community at large.

Referring to their duties, it is stated on the part of the Commission—

"That vested with no legal authority, and with no power beyond that of inquiry and advice in respect of sanitary interests of the United States forces, it was directed especially to inquire into the principles and practices connected with the inspection of recruits and enlisted men; the sanitary condition of the volunteers; to the means of preserving and restoring the health, and of securing the general comfort and efficiency of the troops; to the proper provision of cooks, nurses, and hospitals; and to other subjects of like nature."

These very useful and important interests have been made the express subjects of a succession of reports, of which only a small number have reached us, indeed only six. Judging from those few before us, they are, as might be expected, of a very miscellaneous kind. One of the smallest—so small that it might be put in a pocket-book—is entitled, "Rules for Preserving the Health of the Soldier," with the promise, and we think not exaggerated—

"That if followed with the intelligence and honesty of purpose which characterize the American soldier, they will save the lives of thousands of brave men who would otherwise be lost to the service of their country; and which are addressed alike to officers and privates, inasmuch as the latter are liable to promotion, and upon their officers devolves the responsibility of securing their health, safety, and comfort."

Another, No. 19*a*, bears the title of "Camp Inspection Return." The questions to be answered, as many as one hundred and eighty in number, meet every generally important point connected with the sanitary condition of the army. It is expected that the replies will constitute "a body of military statistics more complete and searching and trustworthy than any now in existence." Another is "On the subject of Pneumonia," a brief treatise on the disease and its treatment. A third is "On the Use of Quinine as a Prophylactic against Malarious Diseases," which is marked "Sanitary Commission, No. 31." A fourth is "On the Condition of the Troops and the Operations of the Sanitary Commission in the Valley of the Mississippi for Three Months, ending November 30th, 1861, where at the time was assembled a force of

250,000 Men of all Arms." The fifth is "A Report to the Secretary of War on the Operations of the Sanitary Commission, and upon the Condition of the Volunteer Army, its Medical Staff, Hospitals, and Hospital Supplies."

This last-mentioned Report is particularly deserving of being read; it is interesting and valuable in many respects. It is admirably adapted to show the principles and elements of military organization; and to us it appears to exhibit in a striking manner the character of the great people who are engaged in this terrible war—illustrating them alike in their strength and their weakness, in their virtues and their failings, the former, we are happy to think, preponderating.

All the reports we have perused are very creditable to their authors; they are written with so much good feeling, so much good sense, and so much accurate knowledge of what is required. Reading them, we were vividly reminded how war calls out some of the best as well as some of the worst feelings of our race, and tasks the invention for the means of the preservation as well as for the destruction of human life. Our prescribed limits will only allow us to notice briefly some of the particulars which have cheered us most in the reading of these reports.

First, of the Commission. It is formed of eighteen members, and of four hundred "associate members." To co-operate with them, Branch Commissions have in several instances been instituted, and "Soldiers' Aid Societies."*

The Commission has distributed 150,000 copies of their publications; "69,000 articles of bedding, clothing, &c., have been collected and prepared, and over 40,000 have been distributed to meet the real and pressing wants of the soldiers."

Secondly, of the Army of Volunteers. It is very satisfactory to be assured that about two-thirds of this immense force, amounting nearly to half a million of men, "are American born," and that about "nine-tenths are citizens educated under the laws of the Union and in the English tongue." "About one-fifth of the regiments possess libraries." "Religious organizations already exist in about half the regiments, and are rapidly increasing in number. The American Tract Society of Boston alone has distributed among them more than 20,000,000 pages (equal to 60,000 12mo volumes)." It is added:

"The number of letters written by the volunteers is remarkable, and a delightful indication of a fact which should remove all fear of a permanent military despotism in the country. In some regiments of 1000 men, it has averaged for weeks about six hundred a-day. For all the regiments, it must have been through the summer not far from three hundred.

"In some regiments there is not a man unable thus to communicate with his friends at home.

"It is believed that two-thirds of the men send home at least half their pay."

* "Composed of patriotic women who have devoted themselves with astonishing success to the preparation of hospital stores, bedding, clothing, and edibles for the volunteers of the army of the West." Through their agency, it is added, "a vast amount of good has been accomplished, suffering relieved, and undoubtedly life preserved."

Another peculiarity of this educated citizen army is that the captains of companies in many instances mess with the men. It is stated, "that never probably was so large an army so well supplied at a similar period of a great war."

We must content ourselves with these few and brief notices of these most interesting reports. They deserve an article apart, and we hope, if we can procure the whole series, to supply it in a future number of this Review.

REVIEW XII.

Bijdrage over het eigenaardig Maaksel van de Lever bij den Olifant in Verband tot het Gemis eener Galblaas. Door J. L. C. SCHROEDER VAN DER KOLK. Overgedrukt uit Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeeling *Natuurkunde*, Deel xii.—*Amsterdam*, 1861. 8vo, pp. 16.

Contributions respecting the Peculiar Structure of the Liver in the Elephant in connexion with the Absence of a Gall-Bladder. By J. L. C. SCHROEDER VAN DER KOLK. Reprinted from the Reports and Communications of the Royal Academy of Sciences, Section *Natural History*, Part xii.

COMPARATIVE anatomy shows that the absence of a gall-bladder occurs among different animals in such a singular and irregular manner, that no physiological reason or explanation whatever can be given for the non-existence of that organ. Now we find a gall-bladder, and again it is wanting in species so nearly allied, agreeing so closely in structure and in mode of life, that it would appear as if nature had acted quite capriciously with respect to its presence or its absence.

Tiedemann thought there was a physiological object or utility in this singular difference with respect to the presence or absence of a gall-bladder in animals. In his opinion the gall-bladder was to be considered as a reservoir of bile, capable of pouring forth an accumulation of that fluid, while the chyme was passing through the duodenum, and that where the organ was absent, the secretion would flow more constantly into the intestinal tube.

According to this view the gall-bladder should occur in animals which have a more periodically interrupted digestion, and which do not find their food everywhere, but must seek it; in such animals the bile would be preserved in the gall-bladder until food had been obtained. Hence we should expect not to meet with this organ in most herbivorous animals, who can always procure their food, and in whom digestion lasts a long time, or rather proceeds almost uninterruptedly. Such is, however, not the case. The sheep, for example, among the ruminantia, and the hare and the rabbit among the rodentia, where the stomach is never empty, and where accordingly digestion would appear to be uninterrupted, nevertheless possess a gall-bladder, and certainly digestion can scarcely be more periodical or interrupted in the ox, which possesses a gall-bladder, than in the hart where it is wanting. Professor Owen found no gall-bladder in one giraffe, while in another he met with two.

From all this it would appear that the presence or absence of a gall-bladder is a matter of very subordinate importance, having little influence on digestion. Such is, however, not the case. The real question is, whether the want of this organ is compensated in any other way? The author believes that it is.

It is well known that no gall-bladder is met with in the elephant, but that previously to the entrance of the ductus choledochus into the intestine, this vessel undergoes a sac-like dilatation, divided into cells, which has been looked upon as a substitute for the gall-bladder, though it is not equal in capacity to that organ as it occurs in other animals.

The author having had, through the kindness of Professor Vrolik, an opportunity of investigating this structure in a very young elephant, succeeded perfectly in injecting the vena portæ with blue, and the bile-ducts with yellow; but the injection of the arteries accidentally failed. On examining the liver, he was astonished at the colossal size and capacity of the bile-ducts through the whole gland, which were such as, so far as he was aware, were found in no other animal. Thus these ducts readily admitted the thumb, and without remarkably diminishing in circumference, ramified through the entire organ, terminating as blind tubes not more than one and a half or two centimètres from the surface. In all other animals in which the author has examined the liver, the bile-ducts accompanying the branches of the vena portæ become slighter, and terminate as capillary tubes.

"These bile-ducts are surrounded by a very thick coat of connective tissue, the so-called capsule of Glisson, which everywhere includes an accompanying branch of the vena portæ, and nevertheless scarcely occupies more than a fourth or a fifth of the section of the bile duct: a circumstance which in the livers of other animals is exactly reversed.

"Both from the walls of the bile-ducts and from the blind extremity arise a number of slighter minute branches, the mouths whereof exhibit themselves as numerous little openings in the mucous membrane of the bile-duct, and which first run for a certain distance in the capsule, to divide into various smaller branches, surrounded by a fine network of veins springing from the adjoining branches of the venæ portarum. This capsule, which is tolerably thick, consists chiefly of connective tissue, though I have thought that I could also discover non-striated muscular fibres, or at least very long fusiform cells.

"The mucous membrane of these bile-ducts is covered internally with a conical epithelium, which, seen from above, exhibits itself in many places arranged in rows, and in others is irregular. Between these epithelial cells occur, in great number, the mouths of very small mucous glands, apparently filled with mucus, around which the epithelium is arranged as a broken ring. The cells are everywhere highly granular, and are furnished with nuclei. This layer of epithelial cells is tolerably thick, the deeper cells being more oval or rounder. Under the epithelial membrane of this mucous structure is a very fine network of capillary vessels, which in the preparations of the elephant exhibited itself very beautifully as a blue reticulation of minute injected veins. In the bile-ducts of a horse, where the arteries also were filled with red, I found the same network coloured red. This network would therefore be the intermediate capillary network between the veins and arteries, as a network of venous capillary vessels in the walls of the larger bile-ducts could not serve for nutrition or for the secretion of mucus. The branches arising from the wider bile-duct and sub-dividing into smaller branches, then repair to the hepatic tissue." (p. 8.)

The author thus succeeded in tracing the direct transition of the bile-ducts into the hepatic tissue, and their mutual connexion; he adds:

"Beale was the first who, in his excellent essay on the ultimate course of the bile-ducts, has satisfactorily shown their direct connexion with the hepatic cells, which had before been suspected, but had not with certainty been seen. According to Beale, the bile-ducts on entering the substance of the liver are first contracted, and are then lost in the wider proper capillary reticulation of the hepatic cells. In the elephant such an arrangement seems not to take place. In this animal a number of lateral branches proceed from the minutest bile-ducts, passing immediately without constriction into the hepatic tissue, or they assist to form the latter."

It is evident that the wide bile-ducts scattered through the large liver of the elephant must be capable of containing infinitely more bile than a simple though capacious gall-bladder; and where, as in many animals, the gall-bladder is wholly surrounded by the substance of the liver, the analogy is still greater. It was therefore important to ascertain whether in other animals also the want of a gall-bladder was thus counterbalanced. Comparing the liver of a horse with that of a calf, the author came to the conclusion, that in this instance also the wider bile-ducts in the former compensated the absence of a gall-bladder. In the pigeon, too, which has no gall-bladder, the bile-ducts were found relatively very wide after they were injected with yellow matter, which in several places penetrated into the hepatic network.

But even if Tiedemann's idea of the mechanical use of the gall-bladder be admitted, there can be no doubt that his opinion, that the secretion of the bile in the liver is always uniform, was erroneous. The investigations of Bidder and Schmidt, and of different later writers, have satisfactorily shown that the activity of the liver is greatly increased after meals. Consequently, even in animals not possessed of a gall-bladder, the bile flows at this time more abundantly into the intestine. For all these reasons, the author believes that the principal use of the gall-bladder is not to act as a reservoir, but to secrete a large quantity of mucus, which, as is well known, mixes with the bile, rendering the latter more viscid and more slimy.

"Mucus is, as has already many years ago been pointed out by Eberle, together with other constituents of the bile, precipitated by admixture with acid chyme; having thus become thicker and more clammy, it unites with the undissolved and insoluble fibres and remnants of food, binds them together, and thus separates the more fluid portion of the chyme from the insoluble part, and in this manner gives rise to the first fæcal formation. But the mucus is not secreted by the proper hepatic tissue, but by the walls both of the gall-bladder and of the bile-ducts; hence I found numerous, though very small, mucous crypts in the wide bile-ducts of the elephant." (p. 13.)

Such are the views of the very distinguished Utrecht Professor of Anatomy and Physiology. Any opinion emanating from so high an authority is worthy of most particular attention; we have therefore quoted very fully from the essay before us.

In conclusion, Professor Schroeder van der Kolk remarks that it is difficult to decide from anatomical observations whether more mucus is added to the bile in animals which have a gall-bladder, or in those who do not possess that organ, inasmuch as the relative capacity of the bile-ducts cannot be very accurately ascertained.

"It may suffice," he adds, "that I believe I have shown that it is probable, that where the gall-bladder is wanting, an equivalent is found in the greater width of the bile-ducts, and in the augmented secretion of mucus occurring in the latter; which, besides the use above pointed out, appears also very much to promote the metamorphosis of matters in the intestinal canal."

The essay is illustrated with five well-executed lithographic figures.

Review XIII.

1. *The Human Foot and the Human Hand.* By G. M. Humphry, M.D., F.R.S., Lecturer on Anatomy and Physiology in the University of Cambridge.—*Cambridge and London,* 1861.
2. *Why the Shoe Pinches: a Contribution to Applied Anatomy.* By Hermann Meyer, M.D., Professor of Anatomy in the University of Zurich. Translated from the German by J. S. Craig.—*Edinburgh,* 1860.
3. *The Foot and its Covering; comprising a full Translation of Dr. Camper's Work on 'The Best Form of the Shoe.'* By James Dowie.—*London,* 1861.

Of all subjects to which the efforts of social and sanitary reformers are directed, those acts or customs which produce serious physical ills without any compensating advantage or pleasure, would seem at first sight to offer the fairest field and easiest victory; though experience has unfortunately shown the difficulty of overcoming prejudice and folly, when sanctioned by prevailing fashion or long-continued use. One of the objects of the works named at the head of this article is to attack an evil which undoubtedly comes under this category, and even if the subject be deemed of little importance compared with the gigantic schemes of regeneration to which our attention is so frequently called in this age of progress, it is one which has the great merit of possessing a simple and effectual remedy, requiring no sacrifice, and within reach of all who will take the smallest possible trouble to avail themselves of it.

But is it really of that trivial and unimportant nature that many are disposed to consider it? Is the proper conservation of the natural form and functions of so valuable a portion of the human frame as the foot not a worthy subject of consideration for the philanthropist and medical man? Is it not a strange reflection, so far are the feet of all persons in our own country and time deformed by the voluntary and unnecessary adoption of an improper form of covering, that in Europe "from the antique alone any knowledge of what is beautiful in the adult human foot can be derived"?*

"We bestow reasonable compassion upon the fate of the Chinese women, who dislocate their feet in obedience to the dictates of a barbarous custom, and yet we ourselves have submitted complacently for ages to tortures no less cruel."

So the Dutch anatomist wrote a century ago, and it is said by one of our present authors that "the boots and shoes of the present day are even, if possible, more objectionable than those of the time when Camper wrote upon the subject." (Dowie, p. 141.)

And are these statements exaggerated? We think not. Indeed, from personal observations upon a large number of feet of persons of all ages, and of all classes of society, we do not hesitate to say that there are few, if any, to be met with that do not in some degree bear evidence of having been subjected to a compressing influence more or less injurious. Let any one take the trouble to inquire into what a foot ought to be. For external form, look at that of any of our approved Grecian models, the nude Hercules Farnese, or the sandalled Apollo Belvidere—watch the beautiful freedom of motion in the wide-spreading toes of an infant—consider the wonderful mechanical contrivances for combining strength with mobility, firmness with flexibility, the numerous bones, articulations, ligaments, the great toe with seven special muscles to give it that versatility of motion which it was intended that it should possess,—and then see what a miserable, stiffened, distorted thing is this same foot when it has been submitted for a number of years to the "improving" process to which our civilization condemns it. The toes all squeezed and flattened against each other; the great toe no longer in its normal position, but turned outwards, pressing so upon the others that one or more of them frequently has to find room for itself either above or under its fellows; the joints all rigid, the muscles atrophied and powerless; the finely-formed arch broken down; everything which is beautiful and excellent in the human foot destroyed; and this without saying a word about the more serious evils which so generally follow—corns, bunions, ingrowing nails, and all their attendant miseries.

Now the cause of all this will be perfectly obvious to any one who compares the form of the natural foot with that of the last upon which the shoemaker makes the covering for that foot. In the words of Mr. Dowie, the fashionable shoe—

"is made upon a last shaped in front like a wedge, the thick part or instep of the last rising in a ridge from the centre or middle toe instead of the great toe as in the foot, the last thus slanting off to both sides from the middle, terminating at each side and in front like a wedge; that for the inside or great toe being similar to that for the outside or little toe, as if the human foot had the great toe in the middle, and a little toe at each side like the foot of a goose!"

The great error in all boots and shoes made upon the system now

* The Proportions of the Human Figure, p. 19. By J. Bonomi. London, 1857.

in vogue in all parts of the civilized world, is that they are constructed upon a principle of bilateral symmetry (fig. 3). A straight line drawn along the sole from the middle of the toe to the heel will divide it into two equal and similar parts, a small allowance only being made at the middle part or "waist" for the difference between right and left foot. Whether the toe is made broad or narrow it is always equally inclined at the sides towards the middle line, whereas in the foot there is no such symmetry, the first or inner toe is much larger than either of the others, and its direction perfectly parallel with the long axis of the foot; the second toe may be a little longer than the first, as generally represented in Grecian art, but it is very frequently shorter; the other toes rapidly diminish in size (fig. 1). The consequent modification in the form of the foot and direction of the toes is shown at fig. 2. Often it will happen that the deformity has not advanced to so great an extent, but unfortunately there are very many worse cases than the one here represented.

FIG.1.

FIG.2.

Fig. 1. Outline of natural sole.'
Fig. 2. The same modified by wearing shoes of the ordinary construction.

It seems perfectly marvellous that any one who had ever looked at a healthy pair of human feet could have thought of the possibility of wearing a stiff, unyielding shoe of identical form for both right and left, and yet the very trifling difference which is at present allowed is a comparatively modern innovation, and is even now too frequently disregarded, especially where most needed: in the case of children. The changes that the foot has to undergo in order to adapt itself to the ordinary shape of a shoe could probably not be effected unless commenced at an early period, when it is growing, and capable of being gradually moulded into the required form.

"At this helpless period of life, the delicately feeble outspreading toes are wedged into a narrow-toed stocking, often so short as to double in the toes, diminishing the length of the rapidly-growing foot! It is next, perhaps, tightly laced into a boot of less interior dimensions than itself; when the poor little creature is left to sprawl about with a limping, stumping gait, thus learning to walk as it best can, under circumstances the most cruel and torturing imaginable." (Dowie, p. 107.)

The loss of elasticity and motion in the joints of the foot, as well as the wrong direction acquired by the great toe, are not mere theoretical evils, but are seriously detrimental to free and easy progression, and can only be compensated for in walking by a great expenditure of muscular power in other parts of the body, applied in a disadvantageous manner, and consequently productive of general weariness. The labouring men of this country, who from their childhood wear heavy, stiff, and badly-shaped boots, and in whom, consequently, the play of the ankle, feet, and toes is lost, have generally small and shapeless legs and wasted calves, and walk as if on stilts, by a swinging motion from the hips. Our infantry soldiers also suffer much in the same manner, the regulation boots at present in use in the service being exceedingly ill-adapted for the development of the foot. Much injury to the general health, the necessary consequence of any impediment to freedom of bodily exercise, must also be attributed to this cause. Since some of the leading shoemakers have ventured to deviate a little from the conventional shape, those persons who can afford to be specially fitted are better off as a general rule than the large majority of poorer people, who, although caring less for appearance, and being more dependent for their livelihood upon the physical welfare of their bodies, are obliged to wear ready-made shoes of the form that an inexorable custom has prescribed.

Before considering the remedy for this universal evil, we may pause to reflect upon the motives which have induced men to inflict it upon themselves, and the following remarks of Professor Meyer upon the subject are certainly deserving of consideration:

"A shoe, then, has to afford protection against unequal and rough ground, as well as against cold and wet. This is the object of a covering for the feet.

"A covering for the foot has, however, to fulfil this object in a manner that will give rise to no disadvantage, the existence of which would essentially diminish the benefits of protection. The remedy would in this case be worse than the evil. Here, however, Fashion, so unfortunately mixed up in all our clothing relations, steps in, and must even have her say on the shape of the shoe. So long as the influence of fashion is confined to the cut and amplitude of the coat, the form and colour of the hat, and the like, the only harm that accrues is the probable production of a somewhat ludicrous effect. It signified little, so far as health is concerned, whether a man wears a grey coat or a brown one, but it is of some importance whether the shoes he wears be broad or narrow, rounded or pointed, long or short. The shape of the shoe has too much influence on health and comfort to be left to the dictates of fashion.

"It is quite clear that the foot must get inside the shoe, and if the shoe differ in shape from the foot, it is no less plain that the foot, being the more pliable, must of necessity adapt itself to the shape of the shoe. If, then, fashion prescribes an arbitrary form of shoe, she goes far beyond her province, and in reality arrogates to herself the right of determining the shape of the foot.

"But the foot is a part of the body, and must not be changed by fashion, for our body is a gift, and its several parts are beautifully adapted for the purposes for which they were intended. If, therefore, we in any way change its normal form, not only do we not improve, but we actually disfigure it."

Few sensible persons can really suppose that there is anything in itself ugly, or even unsightly, in the form of a perfect human foot; and yet all attempts to construct shoes upon its model are constantly met with the objection that something extremely inelegant must be the result. It will perhaps be a form to which the eye is not quite accustomed; but we all know how extremely arbitrary is fashion in all her dealings with our outward appearance, and how anything which has received her sanction is for the time considered elegant and tasteful, while a few years later it may come to be looked upon as positively ridiculous. That our eye would soon get used to admire a different shape, may be easily proved by any one who will for a short time wear shoes constructed upon a more correct principle, when the prevailing pointed shoes, suggestive of cramped and atrophied toes, become as positively painful to look at as to most people of the present time are the once-admired proportions of an unnaturally-constricted waist.

Having so far touched upon the errors of the present system of shoemaking, we now take up the means by which it is suggested that these may be remedied. It is not enough to say that the shoe should resemble in form the part it is intended to cover and protect, for unfortunately, few shoemakers know much either of the structure or uses of the different portions of the foot in its natural condition.

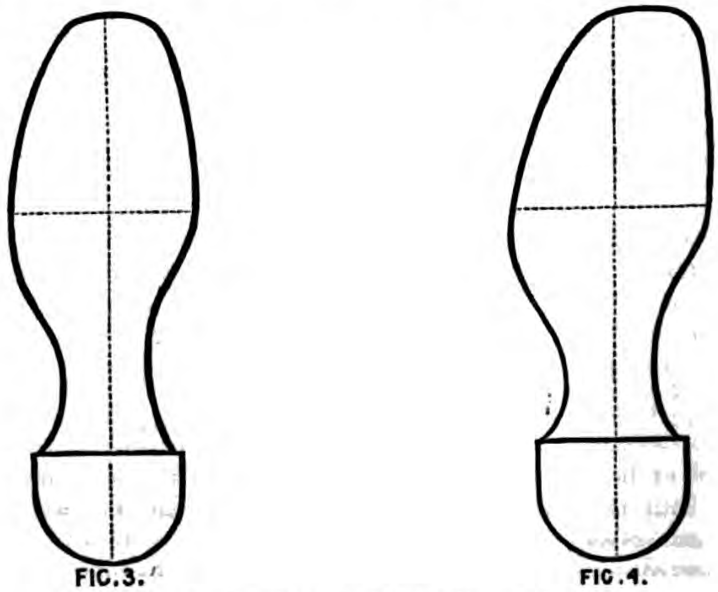

Fig. 3. Outline of sole of ordinary shoe.
Fig. 4. Proper form of sole.

The sole is the foundation upon which the shoe is built, and unless this part is made of a proper shape, the superstructure cannot be perfect. Hence the first thing to understand is the correct principle upon which the outline of the sole ought to be designed. This is very clearly laid down by Professor Meyer, although we think he is not quite right in making the line corresponding to the axis of the great toe, when prolonged backwards, pass through the *centre* of the heel, as it throws the latter part too much to the inside, and produces a twist on the under surface of the foot, which does not exist in nature. The cardinal point to be attended to is, that the inner edge of the sole be perfectly parallel to the long axis of the foot—i.e., a line drawn through the centre of the heel and bisecting a transverse line drawn across the roots of the toes. (Fig. 4.) Of course ample length and breadth are essential, and if the measurement is taken when the foot is off the ground, an allowance must be made for the expansion in both directions which takes place when it is bearing the weight of the body. As in a pair of natural and healthy feet, placed side by side, with the heels together, the great toes are perfectly parallel, and touch each other almost to their ends; so in a pair of shoes made upon this principle, placed in the same position, will the inner margins of the front part of the sole be parallel and in contact. When shoes made upon the ordinary plan are placed with their inner sides together, it will be seen that the toes diverge from each other, and in proportion as this divergence is increased, so is their construction faulty, and the foot that can be placed inside them diverted from its normal configuration. We recommend this easy experiment as a test by which any one can ascertain in a moment how far his feet have been made to depart from the form which, being that given by nature, must alone be considered beautiful.

The correct construction of the sole is by no means the only important point to be attended to in making a shoe, for if the last is not made to correspond with it in all points, a perfect fit cannot be obtained. We have already alluded to the errors in the shape of the lasts now in use—errors which might easily be avoided if the last-maker would only take a cast of a really well-formed foot, and place it beside him while at work. High and narrow heels are of course much reprobated by all the writers before us, but for various minor suggestions for improving our feet coverings we must refer the reader to the actual works.

Although the stocking is more pliable than the shoe, and therefore less capable of producing a permanent impress upon the form of the foot, its shape and size should by no means be neglected, and the very pointed ends generally made to this article of dress, undoubtedly exercise an injurious effect upon the development of the toes, especially in the delicate growing feet of children.

One great obstacle to the introduction of a reformation in this department, is the idea that with the present generation it is too late for any improvement, that our feet have already been reduced to the artificial pattern prescribed by fashion, and could never wear shoes of the form adapted for a Grecian statue. This is true only to a limited extent. If a foot with the great toe much diverted from its normal direction be at once put into a shoe of the shape recommended by Professor Meyer, it will not feel at all at ease; but this is no reason why the change should not be made by degrees. It is surprising to find how readily the toes will recover their liberty of action and natural form if encouraged to do so, the only necessary precaution being that the shoe is made long enough, for as the great toe gradually assumes the straight position, of course the length of the foot is increased. Experience has already shown that, even in middle-aged persons, the restoration of freedom to the toes is attended by superior power of action in the foot and leg, and increased facility of progression.

The ordinary method by which the shoemaker relieves the pain caused by the pressure of the shoe upon the projection of the metatarso-phalangeal articulation, produced by the habitual forcing of the point of the great-toe towards the middle line of the foot, is to give more room in the upper-leather at this spot. This expedient may afford a temporary palliative, but eventually only adds to the misery of the patient, by aiding to increase the obliquity of the great-toe. The sole real and permanent cure for bunions and corns, as well pointed out many years ago by Sir B. Brodie, is to restore the normal position of the toes. This may be a work of time, but with patience and care it may generally be accomplished.

But if we ourselves have suffered from the ignorance and blind adherence to custom of those who had the control over our feet in our infancy, there is no reason why we should transmit the same fate to a future generation. The members of our own profession, to whom the world is so much indebted for the dissemination of correct information upon many questions connected with the sanitary management of infants, should, by their advice and encouragement, and especially by example in their own families, give an extended impetus to the movement inaugurated in these works. We are persuaded from actual experience that if such a movement became general, a vast increase in the health and happiness of our children would be the necessary consequence. The drawing of an outline of the sole of the foot of a grown-up person, relied upon by many shoemakers as the most exact method of obtaining the required form for the shoe, in many cases does more harm than good, as it assists to perpetuate, instead of correct, the already existing deformities; but with a child this plan will give a perfect representation of what is required. A comparison of the outline of such a drawing, and of that of the sole of the shoe usually sold for the same foot, will furnish a serious matter for reflection.

We quite agree with Dr. Humphry in his protest against the common notion of supporting and strengthening the ankles by tight-laced boots, which, as he says—

"Has its parallel in the idea of strengthening the waist by stays. The notion is, in both instances, fortified by the fact that those persons who have been accustomed to the pressure, either upon the ankle or the waist, feel a want of it when it is removed, and are uncomfortable without it. They forget, or are unconscious, that the feeling of the want has been engendered by the appliance, and that had they never resorted to the latter, they would never have experienced the former."

There can be no surer way of producing permanently weak ankles, than by lacing them up tightly during childhood, and so preventing the natural development of their ligaments.

A few words may be useful, in conclusion, upon the character and contents of the works above referred to.

'Why the Shoe Pinches,' is the English title of a well-illustrated little pamphlet, by Professor Meyer, of Zurich, who is already known for his researches into the structure and mechanical adaptations of the foot and lower limbs. We have had occasion to mention one point in which we are inclined to differ from the author, and we think that if more had been said about the form of the last, the work would have been rendered more complete, but on the whole it illustrates so forcibly the errors of the present system of foot-covering, and indicates so clearly their remedy, that it deserves to be in the hands of every maker, and we might almost say, every wearer of shoes. The translator's part is performed by Mr. Craig in an unexceptionable manner.

We are much indebted to Mr. Dowie for bringing before the English public the philosophical essay of Camper, which contains many valuable suggestions, some, however—as those relating to the position of the buckle, and to enormously high heels—are more applicable to the period at which it was written than to the present day. In his own part of the work, Mr. Dowie unsparingly denounces the evils inflicted upon mankind by his fellow-craftsmen, among whom he has long been known as an ardent reformer. The chief peculiarity of the book consists in the advocacy of flexible, instead of rigid soles, an object which is to be gained by the introduction of a piece of a material called 'elasticated leather, at the waist, or middle part of the shoe.

Dr. Humphry's work is an amplification of two lectures delivered at Cambridge. Addressed to unscientific readers, it is written in language the most popular, but bears ample evidence of the extensive knowledge and careful consideration of the subject which might be expected from so accomplished an anatomist. It is divided into two sections, the first devoted to the foot, the second to the hand. Although it might be supposed that the well-known Bridgewater Treatise of Sir Charles Bell had exhausted the latter subject, Dr. Humphry has contrived to bring together much that is novel as well as interesting in relation to the uses of this most important member, and has enlivened the dry details of structure by numerous references to comparative anatomy and to social customs, as those of shaking hands, wearing the ring on the fourth finger, the preferential use of the right to the left hand. The last question has very often been asked, but never satisfactorily solved. Dr. Humphry, after dismissing as untenable any supposed anatomical reason for the disparity of the two limbs, arrives at the conclusion that "the superiority [of the right hand over the left] is acquired, the tendency to acquire the superiority is natural." This specializing of one of the hands for particular kinds of work is an extreme example of that principle of division of labour, which, as carried out in the animal system, is the criterion of elevation of organization. The subject of the proper form of shoes is treated of, though rather briefly, but we are glad to find that the author fully sanctions the views of Professor Meyer, and gives his little work a cordial recommendation.

Review XIV.

The Pathology and Therapeutics of Retention of the Placenta. By Dr. Alfred Hegar, of Darmstadt.—*Berlin*, 1862.

The retention of the placenta opens a wide field for investigation and discussion. It is a subject worthy of an elaborate monograph, and Dr. Alfred Hegar has done well to select it for the exercise of his pen. There is nothing more instructive than thus to take up a symptom or condition as a text, and to study it in all its relations of etiology, pathology, and treatment. When we have in this manner traced back all the varied relations of the given symptoms, we encounter it at the bedside with ready confidence, and, no longer obliged to treat it empirically, we are prepared to adapt our measures to the precise peculiarities of the case.

Dr. Hegar sets out with an anatomical survey of the connexion of the placenta with the uterus during pregnancy. This section is carefully executed; but his descriptions are almost exclusively drawn from German authors, notwithstanding the many important contributions to this subject made by English and French anatomists. He next describes the mode in which the placenta is loosened and partially detached during labour, the detachment and expulsion of the placenta in the stage after delivery of the child, and the involution of the site of placental insertion.

Dr. Hegar defines retention by assuming that we have to deal with this as a pathological condition, if the placenta is not expelled, or at least detached, in the upper part of the vagina within an hour or an hour and a half.

In discussing the etiology of the condition, the author adopts the ordinary and natural division into retention from functional disturbance of uterine action, and retention with abnormal conditions of the placenta, especially of its connexions with the uterine wall. A chief point of interest lies in the conditions and causes of abnormal adhesion of the placenta. Upon this subject the author is full of instruction, and he has collected from authentic sources a mass of most valuable information. This is arranged under the heads of adhesion in hydatiginous degeneration of the placenta, in abortions, in exudation of plastic matter between uterus and placenta, in disease of the uterus. In some cases an exudation of a sero-albuminous and sanguineous fluid has taken place from the inner surface of the uterus during pregnancy. In this manner the connecting decidual layer grows thicker, firmer, adhering on the one side to the uterus, and on the other to the placenta. This condition is not always spread over the entire placental surface, but may be limited to a portion—a cotyledon, for example. It may even be observed affecting the membranes only at points remote from the placenta. He has thus accumulated numerous illustrations of diseases of the placenta, but he has not succeeded in producing anything like a complete history of this important subject.

Upon diagnosis there is little to dwell upon. One trustworthy sign alone we possess, and that is the actual determination of adhesion by the hand *in utero.*

The consequences and results of retention of the placenta form a subject of the highest practical importance. Dr. Hegar discusses it under the following heads: I. Expulsion of the placenta sooner or later after the birth of the child—*first,* without decomposition; *secondly,* with decomposition; which latter, again, may or may not be attended with symptoms of septic fever. II. The cases of supposed resorption of the placenta are discussed. III. There are observations on the total retention of the placenta.

The consequences of not removing the placenta have often been keenly discussed in reference to questions of malpraxis and post-puerperal diseases. Of late years no serious attempt has, we believe, been made to revive the old pseudo-physiological doctrine, that the expulsion of the placenta may be, and should be, left to Nature. Cases are indeed known in which the placenta has been expelled months after delivery without the occurrence of bad symptoms. But a single mishap traceable to this practice will outweigh any number of cases of negative good fortune.

The author shows that one of the evils of retention of the placenta arises from the hindrance to the due involution of the uterus.

The cases of septic poisoning are no doubt often confounded in that term of wide and indefinite comprehension, puerperal fever. Most cases run an acute course. But not all, for Dr. Hegar relates instances in which the course was chronic, lasting many weeks, some ending in death, others in recovery.

The chapter on Resorption of the Placenta contains little beyond the information collected by Naegele. Hegar, however, makes a useful practical distinction between the cases in which an offensive discharge occurred, and those in which there was nothing remarkable in this respect. In the first class it may be conjectured that the placenta was really broken up and came away in the discharges. What became of the other placentas is a greater mystery. Probably the clearest light may be drawn from the observation of what becomes of the placenta in cases of extra-uterine gestation. In these the placenta has been found shrivelled up, or converted into calcareous matter. It may fairly be doubted whether the placenta be really absorbed in those instances where there is no evidence of its disintegration or liquefaction. Accordingly, in some cases when the placenta did not pass, there have been found, on post-mortem examination at lengthened periods of the labour, various metamorphoses of the placenta.

The author devotes a long chapter to Treatment. He lays down, as contra-indications to the artificial detachment of the placenta, such a degree of constriction of the whole cervix, that the introduction of the hand is impossible without injurious force; a marked degree of inflammation of the uterus or surrounding organs, as revealed by excessive pain of the abdomen and fever; a general tetanic condition of the uterus, as described by Stoltz. Manual extraction is indicated by the appearance of dangerous symptoms, occasioned by the retention —especially hæmorrhage; and in the absence of threatening symptoms when it is apparent that the cervix uteri has begun to close.

In the last place, Dr. Hegar considers the general method of dealing with the placenta in ordinary labour. The mode in which this subject is discussed offers a sufficient explanation of the frequency of retention of the placenta amongst our German friends, and explains the necessity of producing so elaborate a monograph upon the subject. He devotes, for example, considerable space to the *new method* of removing the placenta lately described with much emphasis by Dr. Credé, which consists in following up the contracting uterus with the hand upon the fundus! English readers will smile at this discovery, and cease to wonder that retentions are frequent where this practice is new. Not in this matter alone, but in many others, would German obstetricians do well to attempt to diminish the frightful child-bed mortality prevalent amongst them by taking a lesson from English midwifery.

Review XV.

Influence of Tropical Climates in producing the Acute Endemic Diseases of Europeans: including Practical Observations on the Nature and Treatment of their Chronic Sequelæ under the influence of the Climate of Europe. By Sir James Ranald Martin, C.B., F.R.S. Second Edition.—*London*, 1861. pp. 778.

The comparatively short period (we might almost say unusually short for a work whose size and subject combine to put it rather beyond the range of ordinary professional reading) which has elapsed since the publication of the previous edition of Sir Ranald Martin's work on 'Tropical Diseases,' indicates that it has met a real want in medical literature, and may be taken as a proof that its own merits are its best commendation. Although this circumstance would have been a sufficient apology on the part of its author for the absence of any marked alterations or improvements in the present edition, he has not been satisfied with making it a simple reprint of its predecessor. Not only have the contents been re-arranged so as to make the sequence of the various subjects more philosophical than it was before, but considerable additions have been made, especially to the more practical portion of the volume. Hence, whilst in the former edition the weight of the writer's authority may be supposed to have rested rather upon that part of it which dealt with the nature and treatment of acute diseases in the tropics, the present one is enriched by the riper experience which his subsequent practice amongst Europeans who have returned from the East has enabled him to acquire. The results of this experience may be epitomised as consisting in the increased conviction, that whilst the attacks of disease in hot countries must be met by measures as summary and energetic as their course is destructive and rapid, the lesions which they leave behind them in those who return to temperate climes will yield only to remedies of the mildest and least irritating description. It will be seen, therefore, that notwithstanding the vigorous criticism which has been directed against depletory treatment in general during the interval that has occurred between the editions, Sir Ranald Martin still adheres to the high opinion of bloodletting in the treatment of Indian febrile and inflammatory affections which he previously expressed, and which the experience of most Indian surgeons has so emphatically confirms.

"That our fevers," he says, "in Bengal alter their types under different seasons—nay, that instances both endemic and epidemic occur in which bloodletting is not only unnecessary but injurious, the pages of this work sufficiently attest, and death even from its ill-timed use has more than once come under the author's observation; yet the fact remains untouched, that bloodletting practised with the care and thoughtfulness which should mark every step we take in this disease—at the proper time, and apportioned to circumstances of constitution, age, sex, season, and length of residence in India—forms the principal mode of cure in our severer endemic fevers, and of preventing those organic lesions which so frequently follow them."

The support thus given to the theory of the change of type in fevers, so hotly contested in this country and the Continent of late, is further strengthened by the belief which is asserted to prevail in the East, that since the outbreak of cholera in 1817, not only has that disease undergone changes there implying a progressively greater depression of the nervous functions, but also that the endemic diseases of the country have become less and less sthenic in their nature, and their complications less inflammatory and more congestive.

The great Sepoy rebellion, which has occured since the publication of the former edition of this work, has, by the modifications in the composition of our Indian forces which it has entailed, given overwhelming importance to the consideration whether it is possible to maintain a large European army in India without a sacrifice of life disproportionate to the advantages which our tenure of that country holds out. It is gratifying to learn that the opinion of Sir Ranald Martin on this point coincides with that of Sir A. Tulloch, as expressed in his evidence before the Royal Commission on the Organization of the Indian Army, to the effect that in all tropical climates, whether in the East or West, localities may be found where the mortality to which Europeans troops are exposed does not very much exceed that which occurs in their native country. It is to the disregard for all hygienic precautions shown by commanding officers in the selection of camping grounds, no less than to that shown by higher officials in the general arrangements made for the disembarkation and maintenance of English soldiers in India, that the terrible mortality which has so often decimated their ranks is due. The amount of official imbecility which has been shown in building barracks and hospitals in positions of ascertained insalubrity, and in establishing military stations in low and pestiferous localities when higher and healthier ones were available, would be incredible had we not our recent experience in the Crimea to corroborate it. Those who wish to form an idea of the enormous pecuniary sacrifice, and, worse still, the sacrifice of human life, which this variety of official mismanagement has caused, may consult Sir Ranald Martin's pages with profit. The uncompromising firmness with which he exposes the long series of blunders which the management of our medico-military affairs exhibits, and the vigorous terms in which he brands the nepotism that has rendered those blunders possible, are deserving of the warmest commendation. Few have laboured more earnestly than he has to improve the sanitary condition of the soldier when in tropical climates; and though much of the outline which he has sketched in his various suggestions for the improvement of military hygiene has yet to receive the official recognition of Government, we may hope that the time is not far distant when the organization and distribution of our troops will be conducted upon the rational principles which he has laid down. The service which Sir Ranald Martin has rendered by his uncompromising testimony in these matters, will be better appreciated when we remember the evidence of Dr. William Maclean, of the Madras Army, who, in speaking of India generally in 1854, says: "Our cantonments for the most part are to this day in a most disgraceful condition, well-known causes of disease being rife and abounding on every hand, and all for want of some person, or body of persons, who can speak out on the subject so as to compel a respectful attention."

It would be easy for us to fill our pages with extracts from Sir Ranald Martin's valuable work, which is full of interest no less to the historian and political philosopher than to the medical man. Such a course is, however, unnecessary, from the lengthened analysis of it which we gave on its first appearance. We prefer recommending our readers to peruse it for themselves. As an epitome of tropical medicine and hygiene it is indispensable to all whose duties, whether professional or otherwise, may involve a residence in the Eastern hemisphere.

REVIEW XVI.

On Food and its Digestion: being an Introduction to Dietetics. By WILLIAM BRINTON, M.D., F.R.C.P., Physician to St. Thomas's Hospital, and Lecturer on Physiology in that Institution.—*London*, 1861.

THE scope of this work is wide. It is at once a treatise on diet, on the chemistry and physiology of the digestive process, and on the anatomy of the digestive organs. Its varied material, however, is successfully welded into an homogeneous mass. The reader is never allowed to lose sight of the real connexion which subsists between the most commonplace dietetics and the highest physiological generalizations, and details of structure are everywhere interblended with, and made subservient to, the discussion of function. The author states in his preface that his aim has been to illustrate the relation which subsists between digestion and nutrition, by referring the details of demand for food to those of that process of bodily waste by which the demand is dictated, and to harmonize the study of food with that of digestion. We think that he has succeeded well in the task which he has set himself; but whatever be the merits or demerits of the book, as judged from a purely scientific point of view, it has one characteristic which alone should redeem it from neglect:—it is a book characterized by original thought. We will add that, in our opinion, the author has for the most part proved himself as well versed in the chemistry as in the anatomy of his subject, and no less a physiologist than a practical physician.

Having stated thus much, we proceed—not to give a complete analysis of the work, for this our necessary limits and the variety of topics which it comprises render impossible, but to draw attention to a few of the more important, and in some instances debateable, points on which Dr. Brinton has treated, and which he has not infrequently, we think, succeeded in elucidating. The subject matter of the first chapter forms the basis of all scientific reasoning, both on diet and nutrition. It is the inquiry into what are the precise materials which food must supply, as indicated by the phenomena of bodily waste; and as a subordinate question, what is the origin of the water, carbonic acid, urea and its allied substances, inorganic salts, and organized or even organic substances which the expenditure of the body comprises. The questions of how far the urea excreted represents the decomposition of the nitrogenized tissues solely—in other words, how far the quantity of nitrogenized tissue metamorphosed is measurable by the amount of nitrogen in the urine, or whether any part of the urea is derived from the direct oxidation of the surplus albuminous principles received into the blood during digestion, cannot, at least as regards the human subject, be held to be completely solved. Dr. Brinton adopts the "*luxus-consumption*" view, which has found advocates in Lehmann, Frerichs, Bidder and Schmidt. He argues that—

"Inasmuch as half the urea excreted in the human subject nourished by an average diet disappears during starvation, or during the exclusive use of non-azotized food, it may be inferred that this moiety is ordinarily derived from the azotized elements of the food. And a similar fact may be found in the contrast between the proportionate quantities of this compound habitually eliminated by Carnivora and Herbivora, the urea of the former being twice as copious as that of the latter, quite apart from the circumstances of exercise, bodily mass, &c. And just as an analysis of a meat diet, or of the peptone and other substances which are taken into the blood as its elaborated products, would fail to show either urea, or secondary and kindred compounds out of which urea might be directly produced, in sufficient quantity to account for the increased excretion of this substance, so the suddenness with which a meal containing much flesh raises this excretion in exact coincidence with an increase of the other *egesta* (carbonic acid, &c.), confirms the view that no intermediate conversion of such food into the muscular substance itself need necessarily take place. This view has the further advantages, that while, on the one hand, it obviates all necessity for drawing that paradoxical contrast between the nutrition of the carnivorous and herbivorous animal (and even Man) which must otherwise be assumed; on the other hand, it gives to the waste of muscle in both classes that comparative uniformity, as well as that import to nutrition, which all the phenomena of this tissue seem to imply." (pp. 19, 20.)

Whilst acknowledging the cogency of the above argument, we must nevertheless observe that some of the facts on which it rests admit of a different interpretation. Thus, although the diminution of urea which follows a non-nitrogenized diet no doubt depends to a great extent, directly or indirectly, upon the diminished reception of nitrogen, it may also be partly accounted for on the supposition that the non-azotized food appropriates the oxygen necessary for the metamorphosis of the nitrogenous tissues. The phenomena of fasting appear to vary in different individuals, or at least somewhat different results have been arrived at by different observers. According to Moos and Schneller's experiments, the quantity of urea is at once reduced. Beigel, however, found that in six men kept on very insufficient diet for some days, no fall in the urea at first took place, although eventually it rapidly diminished. V. Franque also found that the urea was not so immediately lessened. The argument based on the different proportions excreted by the carnivorous and herbivorous animal is undoubtedly a good one, and Dr. Brinton further enforces it by comparing the amount of muscular waste which takes place in the organism of the timid herbivore, whose existence is almost one continuous act of locomotion to escape pursuit, with the amount we may suppose to be represented by the more slothful habits of the flesh-feeder. We are, however, to remember that the possibility of the formation of urea from albuminous matter in any other way than by the decomposition of nitrogenous tissues, is denied by such physiological chemists as Liebig, Berzelius, and Müller, and that the recent experiments of Bischoff and Voit are held by many to settle the controversy. As these experiments are fresh in the memories of many of our readers,* we shall not again quote them, but merely repeat one or two important inferences to which they point—viz., when a surplus of nitrogenous

* British and Foreign Medico-Chirurgical Review, July, 1861, p. 125.

food is ingested, that instead of being decomposed into urea in the blood, it produces an increase of the nitrogenous organs, so that more nitrogen under these circumstances is ingested than excreted; and that, on the other hand, in the animal fed on an insufficient supply of azotized food, the urea not only corresponds to, but exceeds the quantity which the supply represents, whilst in the animal deprived of food the amount of urea excreted diminishes or increases with the lesser or greater size of the nitrogenous organs. Although, therefore, we do not wholly abandon the theory of a formation of urea from ingesta supplementing its production from tissue-metamorphosis, neither may we fully accept it. It has a certain amount of *primâ facie* probability, but cannot yet be said to rest on a surer scientific basis.

The author adopts the view that the uric acid, kreatin, and kreatinine, and other azotized alkaloids discovered by Liebig, represent intermediate stages in a serial metamorphosis of the tissues which terminates in the production of urea. The facility with which these substances are eliminated from the blood by the kidney he considers no valid objection to the doctrine, as it is quite conceivable that, as regards a variable but small proportion of the entire products of metamorphosis, an arrest may take place at a penultimate stage of chemical change. The varying amount of these substances in the urine of different animals, and in the same animal under different circumstances of diet and exercise, together with the facility with which they are converted into urea and its allied substances by the chemist, are facts in favour of the belief. The inquiry whether the ammonia given off by the animal body is produced in some stage of chemical change anterior to the production of urea, or whether it is formed at a still later epoch of conversion of the nitrogenous tissues, is one which neither chemistry nor physiology is at present able to solve. In lactic acid, inosite, oxalic acid, and other non-azotized substances, Dr. Brinton recognises the steps of a like serial metamorphosis, by which the non-azotized portions of muscle are conducted towards the terminal products of the oxidation of hydrogen and carbon, water and carbonic acid.

The carbonic acid of the egesta is derived from two sources—the oxidation of the fatty constituents of the body, and the oxidation of the surplus carbon which remains after the metamorphosis of albumen into urea. The water excreted may be referred, first, to water of imbibition derived from the blood and tissues; secondly, to water of composition—water combined in the tissues in a manner analogous to that in which water is united with substances under the form of hydrates; thirdly, water derived from the combustion of fat; and, fourthly, water produced by the oxidation of the surplus hydrogen remaining from the metamorphosis of albumen. If we allow that the production of urea is supplemented by the direct conversion of albuminous ingesta in the blood, we may more readily admit that in the same way water and carbonic acid are produced by the oxidation of a proportion of the fatty and saccharine constituents of the food, and that in this manner the constant waste of the adipose tissues is economized. The author draws an argument for the direct combustion and elimination of food, from the conversion of many of the organic salts of the alkalies into carbonates in so short a space of time as that which separates their ingestion and reappearance in the urine. "It would be difficult," he observes, "to specify any tissue which tartaric or citric acid helps to build up; just as, even assuming such a metamorphosis, it would be impossible to imagine processes of histological formation and removal as instantaneous as those which the chemistry of excretion shows this combustion to imply." He draws the same conclusion from the well-known fact of the rapid elimination of gelatine as urea.

In all our reasonings, however, as to the seat of that process which produces animal heat, we must remember that whilst we talk of the blood and the tissues as the two theoretical arenas of combustion, not only are they in microscopically close proximity, but that intervening between them are fluids which must be considered intermediate in composition and office to both. The muscular juice becomes, as it approaches the capillary, a dilute liquor sanguinis, differing probably from the plasma within the vessel only in its smaller proportions of albumen and fibrin. Dr. Brinton is doubtless right in the generalization, that although we may regard the blood and the tissues as the opposite poles of the heat-generating process, that the nutritional fluid is the true site of the decomposition in which that process consists, and that a similar office must be assigned, although in a minor degree, to the serum of the areolar tissues.

We pass over the author's *résumé* of the phenomena of hybernation and starvation, merely observing that he throws a shade of doubt on the often-asserted conclusion that the immediate cause of death in starvation is to be found in the demands at last made on the nervous centres to supply the materials of combustion, and the consequent paralysis of cardiac and respiratory movements. The waste of the adipose tissues is steadily maintained up to the last day of life in a proportion which gives little countenance to the idea that their total failure precedes and causes the attack on the nervous structures. Besides, a more extensive demand on the fatty tissues of the hybernating animal does not result in a demand on its nervous system. The much larger amount of waste which the blood undergoes in the starving than in the hybernant animal, affords, perhaps, the broadest contrast between the two conditions.

The general nature of food, its sources and constituents, are reviewed in the second chapter of the work. The third and three following are devoted to the subject of digestion. After describing the anatomy of the salivary glands, the author discusses the nature of the saliva and the subject of insalivation. The alkaline reaction of the saliva varies much in different individuals and in different states of the individual. It is increased by the stimulus of food and the act of mastication. It is also affected by the conditions of health and disease. The author has found that when saliva has been kept a few hours at a temperature of 100° Fah., a kind of lactic fermentation has resulted—a fact which perhaps may throw light on the alleged presence of a large proportion of lactic acid in the gastric juice, and even on its supposed replacement of the hydrochloric acid in that secretion. The importance of the minute proportion of sulphocyanide of potassium in the saliva is at least very doubtful. The chief ingredient is the ptyalin, a substance which the author suspects to be a mixture (like albumen itself) of several ingredients in variable proportions, rather than a single and invariable compound. Besides the ordinary mechanical office of diluting and moistening the food, the saliva fulfils another purpose, which, although also mechanical, is of high physiological importance, both as regards digestion and the entire organism.

"The saliva constitutes part of a vast stream of liquid which, poured into the upper part of the alimentary canal day by day, to undergo a reabsorption as regards the greater part of its bulk in the succeeding segments, after more or less of change during its transit, constitutes a kind of offshoot of the general circulation, such as must in itself materially aid and modify the general current of blood toward the capillaries, from whence it is poured out. Nor can it be doubted that the large salivary contribution to this stream must especially modify those secretions with which it is first mixed in the stomach and intestine, as well as influence the liver and the portal blood sent to this organ." (p. 83.)

The specific action of the saliva in the conversion of starch into grape sugar differs from the effect of other animal fluids and solids, which induce a like change at a certain temperature, in the circumstances that the action of the saliva is rapid, intense, and unaccompanied by putrefaction. Its properties in this respect are only rivalled by the secretions of the pancreas and the glands of Brunner.

There are few British observers who have paid greater attention to the anatomy of the stomach in disease and health than Dr. Brinton. All that he advances on this subject is therefore worthy of careful examination. He describes two species of glands in the mucous membrane of the stomach—stomach-tubes and lenticular glands. The stomach-tubes (peptic glands of authors) he refers for the most part to the category of simple cylinders. He does not mention the compound tubular gastric glands described by Kölliker as forming a narrow zone upon the cardia. In fact, from the author's account, it is pretty clear that he does not admit their existence. He describes the form of the stomach-tubes as frequently so far deviating from that of a simple cylinder as to present slight constrictions or undulations, and even occasionally a kind of cæcal pouch springing from the lower extremity of the tube. But with these exceptions (which he is inclined to attribute to the effects of mechanical violence), he finds that the gastric tubes form simple straight cylinders, which only widen at their openings on the surface of the mucous membrane. He also does not refer to the mucous glands of the stomach described by Kölliker as compound tubular glands, occurring at the pale pyloric zone, which differ from the compound gastric glands in containing no so-called "peptic cells," being lined throughout with cylindrical epithelium. The upper fourth or fifth of the stomach-tube is lined by a single layer of columnar epithelium, each particle of which contains a nucleus situated near its attached extremity, so near as to appear to be separated from the basement membrane by little more than the thickness of the cell-wall. The remainder of the tube is occupied by large oval or angular cells (the peptic cells of authors) containing a nucleus, which Dr. Brinton finds to be generally in contact with that side of the parietes of the cell which is attached to the basement membrane of the tube. The nucleus frequently exhibits a nucleolus. The cells contain, besides numerous pale flat and extremely delicate cytoblasts, a finely granular material, and here and here refractile dots, having the appearance of oil globules. The author describes this layer of cells as being lined by a series of small angular cells, which surround and enclose the central narrow calibre or cavity of the tube. Above, this last-named layer merges into the columnar epithelium. The interstices of the oval cells are occupied by granules and minute cytoblasts. Such is the description Dr. Brinton gives of the contents of the tubes; it differs in several respects from that given in some standard works on the subject. The fine series of small angular cells lining the layer of large oval cells is undescribed by Kölliker, who says that the latter sometimes have the form of a simple epithelium surrounding the narrow cavity, and sometimes completely fill the tube of the *membrana propria*, and at the bottom of the tube have invariably less distinct cell-walls than in the upper part. Besides the gastric tubes, lenticular glands, closely resembling the solitary glands of the intestine, are found in varying numbers in the stomach.

In reference to the office of the stomach-tubes, and the part played during digestion by their glandular cells, Dr. Brinton opposes the notion that the secretion of the gastric juice is effected by any expulsion of the glandular contents of the tubes, on the grounds that such a theory is inconsistent with their elaborate and dimorphous structure—that in adopting it we must suppose that during digestion the entire cell-growth of the human stomach is reconstructed from sixty to one hundred times within an hour—that during digestion the tubes and their contents present precisely the same appearance as during the fasting state, and that pure gastric juice is perfectly structureless. He believes that the acid ingredient of the gastric juice is furnished by the capillary network beneath the ridges and the papillæ of the mucous surface, whilst he thinks it probable that the organic digestive principle is secreted by the tubes themselves and their contained glandular cells, exuding from them as a solution of unknown quantity and density. That the production of pepsin has its seat in the glandular cells of the gastric tubes is held by most physiologists, and has been confirmed by the observations of Kölliker, Donders, and Goll. But Kölliker admits the discharge of the cells situated in the uppermost parts of the glands as at least an occasional part of the digestive process. He says:

"The cells situated in the uppermost parts of the glands, which, as I find in man, are much smaller than the deeper ones, as if they had arisen from the latter by division, are frequently discharged externally, and are then directly concerned in the process of digestion; at other times all, or almost all, remain in the glands during digestion, and then the fluid part of the gastric juice, which passes outwards, extracts the active material from them."*

The discrepancy between the two views may perhaps be reconciled by supposing that Kölliker's discharge of the uppermost smaller cells is, in fact, a mere bursting or shedding of the epithelium cylinders, and this is the more likely as it is improbable that the process of secretion should in this particular be subject to the variation which he suggests. It is acknowledged by most observers that the mucus of the stomach is derived from the cylindrical epithelium which covers its internal surface, and which, according to many, forms the sole contents of some of its glands. The observations of H. Goll and Kölliker would lead us to suppose that the peptic glands were more concerned in the production of the acid ingredient of the solvent fluid than the author suggests, for these observers found that the stomach of the pig presents a distinctly acid reaction only at the places where the glands containing peptic corpuscles are situated. According to Berlin, the acid and organic ingredients of the gastric juice are in the bird's stomach furnished by two special kinds of glands.

What is the nature of the change by which so-called proteinous bodies are converted into peptones, or, in other words, to what category of actions is that of the gastric juice to be referred? In answer to these questions, Dr. Brinton suggests an hypothesis which may perhaps be an approach to truth. He rejects the belief that the change is the result of a simple process of solution by a dilute acid, or of mere contactive influence (like that of spongy platinum in the acetification of alcohol), or that it is a mere fermentation like that excited by yeast in a solution of sugar, or the combination of a complex acid with proteinous compounds which constitute bases. He believes the change from protein (using the term in its generic sense, as including the group of proteinous substances) into peptone, involving as it does no change in composition, but only in form and reactions, is paralleled by those lower degrees of chemical action "where solution and combination, adhesion and affinity, may be supposed to meet and merge into each other," of which an example is afforded by the union of water with many substances in the form of hydrates. He adds, that it is not impossible that the chief purpose of the secretion may be to enable water so to combine with the various members of the albuminous groups of alimentary substances as to permit their absorption into the blood. As respects the influence exerted by pepsine on protein, he offers no conjecture. He confines himself to the statement, that its action is an assimilation in the strict sense of the word, and in no way comparable to the effect of diastase on starch or of emulsine on amygdaline. With regard to the use of the acid constituent—which he holds to be essentially hydrochloric acid, the presence of lactic acid being secondary or adventitious—he thinks its office may be to commence the process of solution. It undoubtedly also checks putrefaction in the other constituents of the fluid, if it does not absolutely arrest all metamorphosis. To the solvent action of the gastric juice he ascribes

* Kölliker's Manual of Human Microscopic Anatomy, p. 322.

solely the conversion of the albuminous substances into peptone. He does not admit that the change can take place through the agency of any of the intestinal secretions, but asserts that although such metamorphosis is continued in the intestine, it is carried on by the action of the gastric secretion, which, having passed the pylorus, still retains its digestive energy, probably concentrated by a partial re-absorption of its watery element. It will be seen that in this opinion the author rejects the conclusions of Bidder and Schmidt, and Zander, and accepts those of Lehmann and Frerichs. The experiments of Bidder and Schmidt certainly do not provide against the admixture of gastric juice; they only exclude the biliary and pancreatic secretions. To this objection, Lehmann's experiment on intestinal juice obtained from a fistulous opening consecutive to a hernia in the human subject, where all communication with the stomach, pancreas, and liver was obviated by the presence of another fistula higher up, through which the contents of the intestine passed, is not liable. And his observation is confirmed by the fact that an infusion of the intestinal tubes (follicles of Lieberkühn) has no such solvent power on protein as is exerted by an infusion of the gastric glands.

The intestinal juice is alike efficacious with the pancreatic and salivary secretions in converting starch into sugar. But besides this, its principal effect depends upon its alkalinity, which necessarily neutralizes the acidity of the nutrient matter received from the stomach. In fact, as Dr. Brinton observes, the alkalinity of the intestinal secretion may be well supposed to have a definite and complementary relation to the quantity of acid withdrawn from the blood in the stomach. To the secretion from Brunner's glands is also to be assigned the power of changing amylaceous into saccharine matter—a function which accords with the anatomical analogies existing between their structure and that of the salivary glands and pancreas. The multiplicity of hypotheses set on foot with regard to the function of the agminate follicles may be held sufficiently to prove our utter ignorance on the subject. The author first starts with the proposition that these glandular bodies have a function and structure akin to that of the vascular glands—in other words, that they separate from the blood certain ingredients which are subjected in their substance to peculiar changes, and are again returned to the circulating current. He then, however, allows that, in man at least, the materials for conversion may be as readily supposed to be derived from the contents of the intestine as from the blood. It is true that in some other animals a membrane of sufficient thickness to render this absorption improbable exists between the follicle and the intestinal cavity, but no such bar is found in the human subject. The ground is then shifted, and the theory of the supposed analogy between the Peyerian follicles and the lymphatic glands is mentioned and partially adopted. This theory rests upon the fact that a larger number of lacteal vessels occupy the patches of follicles than are proportionate to the number of villi which are implanted on them, and upon the close proximity in which the follicular contents lie to these vessels. This theory derives additional support from the resemblance which subsists between the structure of the Peyerian patch and the lymphatic gland, inasmuch as in both structures vessels and a cell-growth are enclosed within a boundary membrane. Highly suggestive as are these facts, and markedly as the co-existence of pathological changes in the mesenteric glands and the agminate follicles in phthisis and typhoïd points to some functional relationship, it must be borne in mind that the observations of Brücke as to a direct communication between the follicles and the lacteal vessels have not been confirmed, that the follicles have never been found to contain chyle like the alveoli of the glands, and that communication between the follicles is at least rare, whilst the alveoli of the glands communicate directly with each other. If repeated observation showed that the contents of the lacteals coming from the patches were richer in chyle-cells than the chyle from other parts of the intestine, as was once noticed by Kölliker, we might then be justified in ascribing to the agminate patches the same formative function as the lymphatic glands. It does not appear that any new light is thrown by the author on the structure of the intestinal villus or on the exact mechanism by which fatty matter is absorbed by the villi. He follows Kölliker and other authorities in rejecting the statement of a ramification of chyle vessels within the villi. No opinion is given as to the manner in which the fat molecules penetrate the membrane of the epithelium cell, or as to the peculiar striæ in the cell-walls described by the last-named observer, and which he supposes to constitute canals. The alleged action of the muscular layer of the villus discovered by Brücke in propelling the chyle is discussed, and the necessity of an alternate contraction and relaxation in order to such alleged propulsion is insisted on. Relaxation, the author conjectures from analogy, would be excited by an irritation of the sympathetic, whilst the stimulus of distension or stretching might induce contraction. The post-mortem shortening of the villus dependent on the contraction of its muscular layer, and the partial separation of its epithelial investment, are described and figured.

We can trace the fatty matter from the epithelial cell, where it becomes minutely subdivided into separate molecules, into the granular substance of the villus, and thence into the central lacteal vessel. That the steps of this process are entirely due to the osmosis which is proved to take place between a dilute alkaline solution and a faintly alkaline fatty emulsion when separated by an animal membrane, is negatived by the circumstance that the fatty globules of the chyle are not saponified, as is the case in the diffusion of two such fluids through membrane out of the body. And in contravention of this view Dr. Brinton adduces the fact that a larger quantity of fat is taken up by the lacteals than by the capillaries, although the blood is often more alkaline than the chyle, and the amount and rapidity of the stream through the capillaries would certainly favour the diffusive process. Violent inflammation or interference with the bloodvessels prevents the formation of chyle, and there is a limit to its absorption, the villi refusing to take up more than a certain amount—circumstances which are not to be accounted for on a mere mechanical theory of their office.

To the pancreatic fluid we are to look for the chief agency which reduces the fat into an emulsion that can be absorbed by the villi. Its power of separating fats into their organic acids and bases, which has been proved by experiment out of the body, is in the canal counteracted by the admixture of the gastric juice. In proving the emulsifying action of the secretion of the pancreas, physiology has confirmed a position which was long ago indicated by pathological observation, the presence of free fat in the intestinal egesta having been found to accompany and depend upon disease of the pancreas. To this, the chief action of the pancreatic fluid, must be added the power of converting starch into sugar; and, it may be, some at present unknown influence exerted by it on the albuminous compounds. The fact which gives colour to the latter hypothesis is the rapid solution of albumen effected by both the infusion and secretion of the gland when the latter are progressing towards decomposition out of the body—a power which is not acquired by the secretions of the salivary glands under the same circumstances. Yet contrasting the "putrid and unstable solution" resulting from the agency of decomposing pancreas with the peptone of the gastric juice, and remembering the conditions under which alone this action of the pancreatic secretion seems to be called into play, conditions in themselves widely removed from anything we can suppose to take place in the healthy body during life, we must refuse to allow to that fluid any solvent power over the proteinous compounds until it be shown that the power so acquired can be independently called into action under the influence of other agencies during the process of healthy digestion. In describing the minute anatomy of the liver, Dr. Brinton adopts as most probable Kölliker's opinion that the minute interlobular canals forming the apparent commencement of the hepatic ducts merely abut on the outermost of the cells of the hepatic islet. It will be remembered that another opinion is held by Dr. Lionel Beale, who in his recent lectures at the College of Physicians has repeated his decision that the hepatic cells are not free and directly applied to the capillary network, as maintained by Kölliker, but that they are enclosed in tubes which are continuous with, though much larger than, the ducts. Kölliker, however, has admitted that Dr. Beale has demonstrated the existence of such tubes of basement membrane containing cells in the outer part of the lobule, but denies that these extend throughout the structure.

The uses of the bile, its influence on digestion and on life, are as yet unsolved problems in physiology. That it is not essential to one or the other, Dr. Brinton considers proved by the well known experiments of Bidder and Schmidt on animals deprived of bile by diverting the duct, and producing a fistulous communication between it and the exterior of the body. With a largely increased quantity of food, such an animal will live an indefinite time. The extra feeding is made necessary, not only by the constant removal of hydrocarbons in the bile itself, but by the diminished absorption of fat, which is the result of the absence of bile. Wistinghausen's observations have shown that out of the body oil passes through membranes moistened with bile far more readily than through membranes moistened with water, and that oil rises to a much greater height in a capillary tube moistened with bile than in a similar tube moistened with water or a simple saline solution.

We are therefore justified in the conclusion that *one* use of the bile is mechanically to promote the absorption of fat by lubricating the surfaces of the villi; and a confirmation of this view is afforded by the large proportion of fatty element in the fæces in animals deprived of bile. The hydrocarbons thus lost are compensated for by an increased ingestion and assimilation of albuminous compounds.

The other uses ascribed to the bile by various authors, such as the removal of carbonaceous matter from the blood not separated by the lungs, the neutralization in the intestines of the acid chyme, and a solvent action on the chyme, are passed over by Dr. Brinton; and we think rightly, for they are at the best hypothetical. Its use as an antiseptic is noticed under the head of defæcation, but we do not observe that he anywhere refers to the stimulant action which the bile seems to exercise on the intestinal walls, and by virtue of which it acts as a natural purgative. Apart from the secretion of bile, however, there can be no doubt that the liver has an important office in assimilation. It stands between the alimentary canal and the general circulation; through it pass all the alimentary substances which are absorbed into the portal system; and although we know little of the changes they undergo in its substance, we cannot doubt their vast importance. An exact comparative analysis of the blood in the portal vein, and of that in the hepatic vein, would afford us, as the author observes, the exact complement of the bile. An attempt at such a comparison has been made by Lehmann, who finds that the blood, after it has passed through the hepatic laboratory, contains less water and more extractive, in which latter term may be included its saccharine constituent. It has lost half its fat, especially has it been deprived of a large proportion of its elain. One half of its albumen has also disappeared, and the quantity of fibrin is greatly diminished. An increase in the proportion of its pale corpuscles, and a proportionate diminution of the red-blood discs, are also amongst the observed phenomena. Schmidt has observed that the composition of the fat lost from the portal blood would, with slight change beyond a small increase of oxygen, represent the cholic acid and glycogen—the two substances which are the main known results of the hepatic function, the one the chief feature of the biliary secretion, the other of the elaboration of the portal blood. If to these facts we add the results of Bernard's experiments (also quoted by Dr. Brinton) of injecting peptone into the general circulation and into the portal vein—the former operation being followed by the appearance of albumen in the urine, the latter by no such result—experiments which point to, but do not prove, the assimilation of peptone in the liver, we have exhausted the light which science at present throws upon its function.

The metamorphoses which the alimentary substances undergo in the large intestine may be partly due to spontaneous decomposition, but are chiefly produced by the agency of its secretions. The absorption of fat is here effected by the bloodvessels; but the process of absorption in this segment of the canal appears to be chiefly limited to the extraction of matters dissolved in the watery constituent of the mass. The secretions may be referred to the two heads of a structureless alkaline fluid yielded by the secretory tubes, and a scaly epithelium which desquamates from the mucous membrane of the rectum, together with columnar cells, which in smaller proportions are furnished by that of the cæcum and colon. The counteraction of putrefactive changes by the agency of the secreted fluids, and the precipitation of some of the ingredients of the secretions from the higher segments of the intestinal tube, are processes which may be supposed to take place in its lower division. The old view that the food undergoes a second digestion in the cæcum, which is due to an acid secretion from its mucous membrane, is now abandoned. The secretory tubes in the cæcum furnish an alkaline juice of the same character as that given out by the other portions of the intestinal surface, and which is equally incapable of dissolving proteinous matter. It is true that the contents of the cæcum are frequently acid, but the acidity depends on the butyric and lactic fermentations which fatty and amylaceous substances undergo when subjected to heat and moisture, and the acidity is consequently most marked in that portion of the mass which is furthest removed from the neutralizing action of the alkaline secretion. Dr. Brinton conjectures that the time during which the food of a healthy person sojourns in the large intestine is from twenty-four to thirty-six hours; whilst he supposes the passage through the small intestines occupies about twelve hours. The importance of the part played by the former in the absorption of alimentary matters is proved by the fact, that persons in whom a fistulous opening in the small intestine permits the escape of the food, generally fall victims to starvation in a few weeks. The fæces consist of alimentary residuum, a comparatively small fraction of the biliary secretion (by far the greater part of the bile being reabsorbed), and the intestinal excretion, the latter being that ingredient the dismissal of which from the canal is of greatest importance to the organism. It again, however, is but a small proportion of the great mass of fluid poured into the canal, the bulk of which is subjected to reabsorption. In the fœtus and the hybernating animal the intestinal constituent, consisting chiefly of mucus and epithelium, forms from 90 to 95 per cent. of the whole excrement.

The aëriform contents of the alimentary canal are derived from three sources—from the introduction of the external air in the act of swallowing, from the extrication of gases during the chemical changes which the food undergoes, from the evolution of gases by the decomposition of fluids derived from the organism. This last source is probably limited to diseased conditions: the extrication of gas as a secretion or transpiration from the blood is inadmissible for the reasons that in animals deprived of food gases are often entirely absent from the greater part of the alimentary canal; some of the gases present in the canal, such as hydrogen, carburetted hydrogen, and sulphuretted hydrogen, have never been found in the blood in such quantities as to account for their evolution from it; and they are absent in the expired air. Their transpiration from the blood would moreover necessitate us to suppose a deoxidation of water, a supposition opposed by all we know of the chemistry of the organism, and by the fact that hydrogen to the amount of about a pound of water a day is oxidated in the healthy man.

We quote the concluding paragraphs of the chapters on digestion as affording at the same time a good illustration of the author's style, and also of the practical drift and bearing of his reflections:

"Lastly, the subject of digestion would be very incompletely noticed without some allusion to another relation borne by this function to nutrition in general. The admirable researches of Bidder and Schmidt conclusively show that the various secretions which effect the elaboration of the food are habitually poured out in very large quantities. Of these quantities, again, so small a proportion leaves the alimentary canal with the fæces, that the bulk of every such secretion may be regarded as poured into one part of the tube, to leave it by resorption into its bloodvessels, in another and lower segment. What between bile, saliva, and the gastric, pancreatic, and intestinal juices, from twenty to twenty-five pounds of liquid, with solid contents which average about three per cent., are daily undergoing a slow continuous cycle of movement, as a kind of intestinal offshoot of the general current of the blood. The changes or elaborations of these secretions themselves remain in the great part unknown. But even presuming them to be far less important than all analogy would indicate, their influence in merely furthering the general changes of nutrition must be very considerable, aiding, as they would necessarily do, that general exchange of substance which applies the ingredients already rendered effete and useless by one part of the body to the nutrition and function of another. Or—to adopt the readiest illustration—just as a liberal supply of water, the universal solvent and carrier of the nutritive process, defers and protracts the process of starvation, so the stimulation of the digestive organs by the ingestion of food may aid nutrition, quite apart from the nourishment it more slowly prepares for assimilation to the system. Proofs of this action are, indeed, familiar incidents of the records of hunger and starvation, in which substances themselves (quantitatively or qualitatively) little or not at all nutritious, have often been found to produce invigorating effects, which, however fleeting, have been far too rapid and energetic to be otherwise explained.

"One final reflection must also be added—namely, that the magnitude and exactness of the whole digestive process well suggest the wear and tear it implies to the system at large; and the fatigue—if we may use this word in so metaphorical a sense—to the organism in general, which the excessive ingestion of food, whether relative or absolute, must necessarily amount to. The practising physician sometimes sees patients whose constitutions are thus worn out by the mere exertion of good living, uncomplicated by any other variety of mental or bodily toil; and even in those states of debility which demand careful support, it is often a matter of great nicety for him to decide when that generous diet which is called for by the symptoms would, if pushed any further, begin to oppress and detract from the strength it is intended only to support." (pp. 243–245.)

Before leaving the subject of digestion, we may notice the following omission on the part of the author. He nowhere describes the chemical and physical properties of the chyle, or accurately defines the part played respectively by the lacteals and veins in absorption. The assertion that the peptones are for the most part absorbed by the lacteals, has lately been made in a work by a well-known writer. The more general opinion we believe to be, that the fatty matters are principally, but not exclusively, taken up by the lacteals under the form of chyle, whilst the albuminous and saccharine matters pass by absorption into the portal vein. This, we gather from the text, is the conclusion of the author; but we should have been glad to have seen the functions of the two sets of vessels, as far as the present state of knowledge permits, contrasted and defined.

Of the subjects discussed in the chapter on animal food, we may select the nutritional value of gelatin as one which is still open to controversy, although we think that our readers will most of them be inclined to agree with Dr. Brinton in his estimate. Gelatin was long held empirically to possess nutritional qualities both as an adjuvant and preservative of the more valuable elements of animal food. Its reputation as a food arrived at its highest pitch after the time when Papin obtained it in large quantities by the long boiling of bones under a high pressure in his "digester." Subsequently the French and Amsterdam commissions deposed it from its high position as a nutritional element, and arrived at the conclusion that it is entirely destitute of nourishing properties. That this conclusion is not only contrary to the universal experience of mankind, but is open to question and deduction on scientific grounds, is, we think, completely established by the author. The three kinds of gelatin yielded by connective tissue, cartilage, and elastic or yellow fibre, contrast with the protein compounds chiefly in their smaller proportions of carbon and hydrogen. The tissues which yield them are at all events subject to waste by attrition, and constant wear and tear necessitates replacement, however slow their nutrition may be, as indicated by their scanty vascular supply and dilute nutritional fluid. In the herbivore the nourishment of the gelatinous tissues can only be supposed to be effected by a metamorphosis, either progressive (by an accumulation of oxygen and nitrogen), or more probably regressive, of the albuminous tissues. In the carnivorous and omnivorous animals gelatin is always taken in in large quantities, in the form of connective tissue, which lies between and binds together the components of muscle. It is only reasonable to suppose that the gelatin so ingested should tell primarily and principally upon the tissues most akin to its own composition—the collagenic structures, which were long ago estimated by Haller to form one half of the whole body. The experiments which have deposed gelatin as a nutrient are liable to objections. Animals deprived of food at first eat gelatin eagerly, and for a time it defers starvation. In some instances it appears to have a more decided nutritive value, and in such inquiries positive experience is of more value than negative. Satisfactory experiments are only those in which a part of the proteinous food found necessary for the support of the animal has been replaced by the substitution of gelatin, and the results of the commissions render it probable that a portion of the albuminous food of a mixed diet might be so replaced. The author further observes, that by a parity of reasoning both the white and the yolk of egg might be proved to be innutritious, although it is certain that they are nourishing to man, and constitute the sole food of the embryo bird. The belief that a certain portion of albuminous nourishment may be replaced by gelatin, is entirely borne out by the observations of Bischoff and Voit, who find it indisputable that gelatin may replace albumen; and they even think it not unreasonable to suppose that it might play its part altogether, were it possible that sufficiently large quantities could be ingested. They found that the proportion required would be four times as much gelatin as dried flesh, or just as much dry gelatin as undried flesh—a quantity which would defy the digestive powers of any animal. That gelatin injected into the veins reappears in the urine, is no proof of its innutritious character. The fact rests upon slender foundation, for Frerichs failed in confirming it. But according to Bernard's experiment before quoted, the same argument would prove albumen innutritious. The increase of urea after the ingestion of a large quantity of gelatin is equally paralleled by its increase after the assimilation of a large quantity of proteinous matter. But gelatin is not a fair representative of the collagenic tissues—it is only a weak, bulky hydrate of them; under the agency of gastric juice, these tissues, according to Dr. Brinton, yield a fluid undistinguishable from peptone. It is true that Mulder and Meissner deny that a solution of pure gelatin in gastric juice contains either peptone or parapeptone. But on the other hand, it may be observed that Busch found when he administered calf's-foot jelly to a patient having a fistulous opening, that the fluid discharged from the opening contained only one-third of the gelatin that had been administered, and this had lost its coagulating property. Dr. Brinton concludes that not only are the collagenic tissues of great nutritive value, but that gelatin itself ranks as a nourishment in the twofold sense of the term—that it not only undergoes true assimilation, but also a combustion which developes heat. We think, however, that gelatin plays a very small part in the respiratory process. Gelatin is already combined with a large per-centage of oxygen; its further oxidation, therefore, can produce but a small amount of heat. Gelatin subserves also other uses in nutrition. Its mechanical properties stimulate the organs of digestion; its admixture facilitates the digestion of other aliments by increasing their surface. It may also be said to exert a solvent agency on some of the other materials of flesh, and thus to inaugurate the metamorphosis of digestion. Hence it is that gelatin, apart from its intrinsic nutritional properties, is an indispensable adjuvant in the preparation of food.

We pass over many pages treating of the other constituents and the varieties of animal food, and on the general characters, value, and variations of vegetable food, until our attention is arrested by some remarks on the use and preparation of the "staff of life" in this country, which we think are of no less physiological than political and economical interest. We English are proud of our white loaf as we are of most other things insular and peculiar to ourselves. The real advantages, however, obtained by entirely rejecting the outer layers of the grain may well be questioned. Pure white bread may yield a trifle more starch, perhaps even more sugar, but in a part of the husk which we throw aside a larger proportion of protein is stored than is contained in the starchy interior. It has been suggested by Dr. Carpenter that "the present epoch may almost be distinguished, dietetically, as one which favours a rheumatic diathesis in the masses of our population, by an over-starchy and insufficiently proteinous food." Dr. Brinton traces to the same cause many forms of dyspepsia rife amongst our labouring populations. The prices, he also urges, at which the outer coverings of the grain are sold, are less than those at which science and experience would appraise their value as human food. He believes that the present bread supplied to the masses is far inferior to that formerly made in this country, and also far below that still used by the peasantry of France and Germany. The richer classes, amongst whom a large supply of animal food supplements the impoverished bread, do not suffer in the same manner from the deterioration, although they would frequently be benefited by the stimulation which the insoluble constituents of the husk afford to the muscular parietes of the digestive tube. But with the masses, to whom bread is the "munition of life," any such errors as that pointed out must result not only in individual but in race deterioration.

Another retrogression in dietetics indicated by the author is a neglect of the seeds of leguminous plants, which is yearly becoming more general amongst the population. Pease-porridge, pease-pudding, and other dishes of the same kind, are scarcely now known as articles of diet in many districts, whilst formerly they were so generally eaten as to be hawked about the streets of the larger towns. The present population of England can ill afford to neglect such a valuable source of histogenetic material. The history of those European populations who have been fed principally on the potato affords a widely different picture of national development to that of the Anglo-Saxon race in this country, who for centuries were almost entirely fed on pease, beans, and coarse bread.

Before concluding this notice, we turn to the chapters which discuss the dietetic and physiological value of tea, coffee, and alcohol—subjects which at the present time are attracting a more than ordinary amount of attention. Full credit is given to the two former beverages as having directly brought about a great diminution of the gross intemperance which prevailed before their introduction. To their use the author is inclined even to ascribe higher benefits. To their influence on body and mind, and to their office, not merely as substitutes but as antidotes for alcohol, he considers may at least partially be referred "that diffusion of mental industry, and of mental acquirements, which constitute the most striking psychological phenomenon of the condition of the European races of mankind in the present day." He remarks that the discovery of two plants of such widely different botanical affinities, in districts so remote from each other, yielding from entirely different parts of their structure products which, prepared in a dissimilar manner, afford two aqueous decoctions or infusions of similar dietetic use—products which, in their chemical composition, in their three main ingredients, the alkaloïd, the astringent, and the volatile aroma, exhibit an almost identical constitution—is either a most extraordinary coincidence, or "a remarkable illustration of the degree in which the experience and observation of man ultimately lead him to the satisfaction of his most delicate and complex requirements, with all the precision originally conferred by instinct on the lower animals." The advantages derived from the use of tea and coffee may be assigned to two classes of effects: one, a stimulant influence on the nervous energy, evidenced by mental cheerfulness and activity, and the sleeplessness which follows a full dose; the other, an economizing and replacement of food, permitting hunger to be more easily appeased, and diminishing the constitutional as well as digestive requirements of the system, by allowing nutrition to be maintained under a more scanty aliment. How far these two varieties of action, how far the cerebral and nutritional effects of tea and coffee depend on each other, we know not. Dr. Brinton believes that few persons derive from their use both kinds of advantage. They may be used by the labourer as substitutes for food, by the student as spurs to mental activity; but it is probable that the latter mode of action destroys the former, by exhausting that very nervous excitation whose intervention is the source of their dietetic utility to the labourer. Dr. Brinton, however, is not one who accepts unchallenged the conclusions of Boecker and Lehmann as to the action of tea and coffee in checking tissue metamorphosis. If we understand him aright, he thinks such a theory of their action incompatible with the acknowledged effects of these agents in enhancing and stimulating mental and bodily activity. He holds that to check metamorphosis is to lower and deteriorate the vital functions; for life, as far as we understand it, is flux, change, metamorphosis. He therefore criticises the experiments of these observers, on the grounds that they allow nothing for the effects of habit and idiosyncrasy, that the amount of carbonic acid expired during the night is not estimated, and that the total constituents of the pulmonary and cutaneous exhalations are calculated as a mere subtrahend of the other *egesta*, from the bodily weight—in other words, are merely guessed at. This last objection applies not only to the amount of water and carbonic acid, but also holds good in detracting from the value of the observed diminution of urea. For that ammonia is exhaled from the lungs and skin is certain, and the possibility that under certain circumstances urea should be excreted by the skin, appears proved by the experiments of Drs. Goolden and Bernays on the cutaneous secretions given off by a healthy man under the influence of the Turkish bath. We may add, that the same fact has been observed by Dr. Thudichum. We need scarcely say that Dr. Brinton's misgivings as to Boecker's researches have been confirmed by the publication of Dr. E. Smith's experiments, which appeared whilst Dr. Brinton's book was passing through the press. Dr. Smith's observations tend to prove that tea added to a diet otherwise unvarying, increases the discharge of urea, whilst tea, coffee, and cocoa all increase the expiration of carbonic acid.

The author's opinion as to the exact action of alcoholic drinks on the organism in general are thus summed up:—

"1. It is evident that alcohol acts, first and most, on the nervous system; through which it chiefly influences the tissues in general. 2. Not only is there no proof whatever of its 'assimilation' in the strict sense of the word,—as implying a likening or conversion of alcohol or its constituents into the substance of the body,—but even that combustion which it has often been supposed capable of ministering to, seems to be contradicted (certainly remains unconfirmed) by accurate inquiry; and, so far as regards those slender facts on which it may be regarded as resting, is certainly susceptible of other explanations, themselves more compatible with the predominant or exclusive influence of the drug on the nervous tissues. 3. Though the rough and unaided evidence of the senses as to the elimination of alcohol by the skin, lungs, and kidneys, has not yet been followed into its exact details of duration and quantity by the incontrovertible evidence of exact chemical analyses, yet it may be fairly presumed that a process of removal of the drug, without much change as regards some of its ingredients, really obtains. A partial elimination of this kind must indeed be regarded as quite established by the evidence already at our disposal. And it is evident that a large (perhaps even a total) elimination remains not quite impossible; a proposition which is, in some sense, a corollary of that which denies its assimilation." (p. 387.)

We fear it will be long ere the question of the absolute value of alcohol is finally set at rest. We most readily admit that it is given off by the skin, kidneys, and lungs; we allow the cogency of Dr. Brinton's argument, that a diminution of tissue metamorphosis can neither be held to improve nor economize life; that the experiments which tend to prove a decrease in the urea and carbonic acid excreted under its influence are open to the objections he urges; but on the other hand, there are positive facts which seem to indicate that alcohol at least indirectly aids the nutrition of the body. In all Dr. Hammond's experiments, whether under the circumstances of a sufficiency of food or a diminished or increased diet, increase of weight always followed its ingestion. And we believe there are not a few clinical observations which support the same conclusion.

Alcohol diminishes the power of resisting changes of temperature; it also lessens the capability of muscular exertion and blunts mental and sensational acuteness. Yet, Dr. Brinton, as we believe, rightly, advocates its use under the circumstances of civilized life. To use his own apt simile, "It may disturb a balance exquisitely adjusted; and yet, in the main, counterpoise a scale heavily laden with disadvantages." The objection that alcoholic drinks bribe, as it were, the individual to undue and hurtful exertion, and their advantages are dearly purchased at the price of the reaction or depression which follows, is set aside by the experience of the man who finds that his sensations of comfort and cheerfulness are restored after a hard day's work by a glass or two of good wine. As the author asserts, from a moderate quantity of good wine there is no reaction whatever. Neither do observations show that life is shortened by the temperate use of alcoholic stimulants. Hard and sour ale permitted old Parr to attain the age of one hundred and fifty-two, and the Highland shepherd takes his whisky, and the German peasant his "Schnaps," without any demonstrable abridgment of their life period.

How far, on the other hand, teetotalism is compatible with the preservation of the health of a crowded civilized community, has yet to be proved. Dr. Brinton observes that few healthy teetotallers are to be found amongst the inhabitants of our great cities. He says that with a view to determining the question he has sedulously examined not less than from 50,000 to 70,000 persons, including many persons in perfect health. With the wish and expectation of finding it otherwise, he is bound to confess that he has met with but very few healthy middle-aged persons successfully pursuing any arduous metropolitan calling under teetotal habits. On the contrary, he has observed that the constitutions of teetotallers give way with frightful rapidity under the attacks of disease, and numerous have been the instances in which a return to the moderate use of alcohol has been necessitated by "reasons no less valid and imperious than those which, 1800 years ago, induced an inspired saint to prescribe it for a teetotal bishop!"

In commencing our review we disclaimed the intention of making a complete analysis of Dr. Brinton's work. We hope, however that our readers will peruse it for themselves. Setting aside some casual defects, such as an occasionally involved sentence, and now and then an expression or word to which we might take exception, its manner is as good as its matter. Its author has proved himself at once an intelligent observer and an able reasoner.

REVIEW XVII.

1. *Traité Clinique et Pratique des Fractures chez les Enfans.* Par le Dr. A. COULON, &c. &c.—*Paris,* 1861.

Clinical and Practical Treatise on Fractures in Children.

2. *Handbuch der Lehre von den Knochenbrüchen.* Von Dr. E. GURLT. Erster oder allgemeiner Theil, zweite und dritte Lieferung.—*Berlin,* 1862.

Manual on Fractures of Bones.

THE author of the former of these two works was house-surgeon at the Hôpital Ste. Eugénie, under M. Marjolin, and the volume embodies his experience, and to a certain extent that of his instructor, on the subject of fractures in childhood, of which he says 140 cases were admitted during his year of office. We do not observe any definition of the age at which "childhood" terminates in M. Coulon's classification, but in looking over the cases which he quotes, the most advanced age appears to be fourteen. M. Coulon has purposely omitted fractures of the skull from his treatise, for the singular reason that "their history belongs to that of injuries of the head." It is for this very reason that we should have thought it most desirable to study the peculiarities of such cases in early life. He has thus deprived himself of the opportunity of giving cases which might have illustrated the interesting questions of the treatment of depressed fractures in early life, of the possibility of a simple indentation in the young skull without actual fracture, and of the comparative frequency of fractures of the vertex and the base in early and in adult life. Nor do we observe any reference to fractures of the spine, which, though rare, are not unknown in childhood.

The work is divided into two parts, of which the first treats of the anatomy, causes, and treatment of children's fractures in general; and the second comprises a short chapter on each particular fracture, with illustrative cases. We cannot avoid saying that both parts might be materially reduced in volume with no loss to the value of the work. In fact, as the quantity of original observations in these 261 pages is very small, it may be doubted whether it would not have been better for M. Coulon to have published the results of his experience in the form of a contribution to some journal of good reputation, or some medical society. The immense multiplication of books of all sorts and sizes in these days has so reduced the circulation of the great majority of them, that the author gets more credit and the public more information from a good "article," which necessarily comes before the eyes of thousands of readers, than from a second-rate book, which counts its circulation by the dozen. In the present work it was surely unnecessary to set out at full length the various kinds into which fractures might be classified, and the various methods of their diagnosis, since these particulars are identical for all ages. Nor was it essential to go through all the plans of treatment which are in daily use. In all these matters it would have been abundantly sufficient to assume a knowledge in the reader of what is to be found in every text-book, and to have pointed out what is peculiar to childhood. So in the second part, fractures are described which, according to the author himself, have never been found in children (e.g., that of the coronoïd process of the ulna); and the cases which are adduced might all have been shortened, and a very large number of them omitted. But while we think the volume diffuse, we must allow that it shows a good amount of honest work, and that the author has succeeded in illustrating many points of interest, and we hope will succeed in directing the attention of surgeons more forcibly to a subject which is not much studied, though it would well repay study. We will conclude our notice of M. Coulon's book by citing two or three of the observations which seem to us of most interest. On page 8 he notices the thickening of the periosteum by which rickets is accompanied, as a cause of the frequency with which fractures without laceration of that membrane are met with, and cites a very good example from his own practice. In speaking, however (p. 46), of fractures in rickety children, he lays it down as a rule that such fractures do not consolidate till the period of softening in the bones has passed: an observation which our own experience would lead us to believe totally unfounded. On p. 15 we find that the united experience of MM. Marjolin and Coulon has not enabled them to produce a single example of true separation of the epiphyses, notwithstanding the frequent occurrence of fracture near the large joints — a striking example of the rarity of the accident. We may add also, that after examining a good number of specimens, we have found in most of those in which the accident was recent, that the fracture, if it traversed the epiphysal line at one part of the bone, generally left it at another; so that we have been led to conclude that the bone is at least as strong at the epiphysal junction as at any other part. As to non-union of fracture, we find, as might have been anticipated, that this unfortunate event is extremely rare in childhood, so that M. Coulon is obliged to recur to the practice of M. Guersant for the only case he can bring forward (p. 49). As to the treatment of fractures by immovable apparatus, M. Coulon judiciously dissuades the application of any splint which does not admit of easy removal for examination of the limb, until the consolidation is so far advanced that no considerable displacement of the fragments can take place. Especially in children is it necessary to be very cautious in the use of such apparatus, as too many unfortunate cases have proved; but on what data M. Coulon is speaking when he says that vicious union is more frequent now-a-days than formerly, we know not (p. 64). We are surprised not to observe among the aids to reduction of fracture (p. 56) any mention of anæsthetics. In the treatment of complicated fractures (p. 67), M. Coulon lays down rules what to do when a large artery is wounded, but he does not say whether such a complication has ever been noticed in early life—a complication the more improbable, seeing that, as he correctly observes (p. 68), splintered fractures are hardly ever met with in young bones. Still, if such an event should occur, probably the course recommended by M. Coulon, of putting digital pressure upon the trunk of the vessel above the seat of fracture, would be the best that could be adopted immediately after the injury. We cannot say the same for the direction on the next page (p. 69), as to the proper treatment of a severe complicated fracture, in which there has been a question of primary amputation. "If," says M. Coulon, "immediate amputation has not been performed, and gangrene should come on, the line of demarcation must be waited for, before performing secondary amputation." It is an elementary doctrine, at least in English schools, that there is no such necessity, but that amputation above the seat of injury may be performed whenever the patient's strength will permit.

We could cite many other points connected with fracture, on which M. Coulon's experience has enabled him to make interesting observations, but space fails us. The last we shall bring forward is that alluded to by M. Coulon (on p. 76), but not illustrated by his own practice, as to the possibility of elongation after the consolidation of fracture. It seems that MM. Baizeau and Herpin, of Geneva, have asserted that limbs left shorter than natural after fracture have recovered their natural length, and M. Baizeau believes that he has proved the possibility of this fact by experiments on young animals. But, as M. Coulon observes, the facts must be much more fully and precisely known before any opinion can be founded on them; and as to his own practice, he says that he never saw a fracture in a child unite with any appreciable shortening of the limb: so that he has been most fortunately deprived of any opportunity of examining the question.

As Dr. Gurlt's treatise has been already spoken of in this Journal, when we reviewed the first part of the first volume,* we need say less about the present portion of the work, which contains the second and third parts, completing the first volume, and concluding the subject of the general pathology of fracture. Dr. Gurlt's production is in every respect the precise opposite of M. Coulon's; for while the latter is a record of individual experience merely, confined to a special point, and directed almost exclusively to practice, avoiding all literary and all merely pathological details, Dr. Gurlt's aims at being a complete *résumé* of all the facts and opinions bearing on this branch of medical science which are to be found in the whole of literature, although his own experience and observation have furnished him with some original matter. A book of this kind is one of reference exclusively, and, as we pointed out in our previous notice of Dr. Gurlt's book, the ponderosity incidental to such compilations is not relieved by Dr. Gurlt's method of treatment. Hence we cannot profess to have done what we are sure none of our readers will do—viz., read this book through; but we have had occasion, both at this and other times, to refer to Dr. Gurlt's work for information on special points connected with his subject, and have always found such information ample and apparently trustworthy, and accompanied in some cases with intelligent and instructive criticisms. It is not necessary to go in detail through the contents of the book; but, as a specimen of what the reader may find in this laborious compilation, we will select the chapter on Ununited Fracture, Chapter X. This occupies no less than 144 pages, and contains a table exhibiting the nature and results of the treatment of 484 cases, properly classified, and with a distinct reference to the original account of each, as published in German, English, French, American, and Italian books and periodicals, in searching which Dr. Gurlt has shown the industry characteristic of his countrymen. The anatomy of the lesion is first treated of, which he divides into (1) soft union (cartilaginous, as he terms it, though no evidence is adduced of the actual presence of true cartilage in what we should be inclined to regard as fibrous tissue); (2) no union, the broken ends atrophied; (3) false joint. Then follows a collection of illustrative cases, with drawings of some striking specimens from different museums. The causes are next set forth, and divided into *general* conditions: (*a*) syphilis (doubtful), (*b*) pregnancy (ditto), (*c*) cachexia from loss of the nutritive fluids or from general debility, (*d*) extreme old

* Vol. xxvi. p. 72, July, 1860.

age, (*e*) severe acute diseases; and *local* conditions—viz., (*a*) unfavourable position, &c., of the fractured ends, (*b*) interposition of muscles, tendons, or foreign bodies, (*c*) defective innervation (doubtful), (*d*) defective blood-supply from tied or wounded arteries, (*e*) disease in the fractured ends, (*f*) inflammatory affections of the limb, (*g*) defective treatment, either from unskilfulness on the part of the surgeon, or indocility, &c., of the patient; and as an appendix to all this, the spontaneous softening and reabsorption of previously formed callus is mentioned and discussed. The diagnosis and prognosis are then spoken of at no great length; and, finally, we have the treatment, divided into eighteen heads—viz., (1) external application of tincture of iodine, 29 cases, 19 cures; (2) the use of caustics to the skin, 4 cases, 2 cures; (3) electricity and electro-puncture, 8 cases, 3 cures; (4) compression and fixing, 78 cases, 33 cures; (5) permanent extension, 19 cases, 14 cures; (6) friction of the fractured ends together, 99 cases, 40 cures; (7) subcutaneous laceration, 16 cases, 11 cures; (8) subcutaneous scarification, 9 cases, 2 cures; (9) acupuncture, 13 cases, 8 cures; (10) subcutaneous perforation, 25 cases, 17 cures; (11) pegging, 30 cases, 15 cures; (12 *a*) common setons, 140 cases, 66 cures; (12 *b*) setons of wire or other substance, 6 cases, 5 cures; (13) cauterization of the fractured ends, 9 cases, 6 cures; (14) paring or scarification of the ends, 13 cases, 7 cures; (15) resection of the ends, 125 cases, 71 cures; (16) resection combined with ligature or suture, 29 cases, 17 cures; (17) resection combined with screwing the fragments together, 5 cases, 1 cure; and, finally, (18) amputation, 23 cases. The tables also show the bones which were affected; the number of deaths which followed in unsuccessful cases; the number in which benefit, though not cure, is said to have followed; and those in which the event is left entirely doubtful. Finally, from a criticism of the whole collection of cases, the author deduces a few plain and very sensible general rules for the employment (or the avoidance, in some cases) of each of these numerous methods, for which, however, we must refer the reader to the work itself (pp. 726–7).

We may mention that Mr. Jordan's proposal for sub-periosteal resection of the fractured ends finds no favour from Dr. Gurlt. We have selected this chapter for observation as being a favourable specimen of the book in its exhaustiveness and literary completeness, and at the same time as showing the amount of stiff and heavy work which a reader must be prepared to encounter who wishes to qualify himself to follow or oppose Dr. Gurlt in the conclusions to which his more than German industry has led him.

Review XVIII.

A Treatise on Diseases of the Joints. By Richard Barwell, F.R.C.S., Assistant-Surgeon to Charing-cross Hospital, &c.—*London*, 1861. pp. 469.

The fifth and last edition of Sir Benjamin Brodie's well-known and valuable work appeared in 1850, a work which, admirable as it was, was not sufficiently exhaustive to deserve the name of a treatise. Its title, 'Pathological and Surgical Observations on the Diseases of the Joints,' most accurately represented its nature; it was a record of the experience and researches of a very acute, experienced, and cautious observer. On the other hand, it gave scarcely any account of the numerous investigations published by English, and none whatever of those by Foreign authors;* and yet even twelve years ago considerable advance had been made towards a more exact knowledge of the minute anatomy of the healthy tissues; pathological anatomy had been developed by Laennec, Rokitansky, Lebert, &c.; and, lastly, the introduction of subcutaneous tenotomy by Stromeyer had given a great impulse to the study of deformities, of which so many resulted from joint-disease.

From that to the present time there has appeared in our language no work that could be considered as at all a satisfactory exposition of the existing state of our knowledge of this subject, and thus there could be no question that a full and systematic treatise was urgently demanded, one to which both the scientific student and the busy practitioner could have recourse in time of need. But before proceeding to examine how far Mr. Barwell has succeeded in supplying this desideratum, we must not forget to mention that in 1845 Bonnet published his very valuable treatise, and in 1854 there appeared the 'Traité des Tumeurs Blanches des Articulations,' by Dr. J. Crocq, a work to which we have often had occasion to refer, and which we are sorry to find unquoted by Mr. Barwell, for we think his work would have gained in more ways than one had he perused it.

A glance at the table of contents suggests the remark, that as the author gives a chapter on physiological anatomy, he might very well have collected his observations on their pathological anatomy and physiology into another, for by such an arrangement many of the succeeding chapters would have gained in unity and clearness. Nor can we see why his disquisitions on rheumatism, gout, scrofula, should be scattered in different portions; their only value in this work is to show the etiology of joint-diseases, and they should accordingly be placed together in a chapter specially devoted to that subject. Chapter III., on acute rheumatism, is rather oddly separated by two chapters—one on pyarthrosis, the other on strumous synovitis—from the sixth, which is devoted to rheumatic synovitis. This plan is very

* The only work by any continental writer mentioned is Bichat's 'Traité des Membranes.'

different from the methodical distribution of Crocq's treatise, in which the subject is divided into six sections, on the history, pathological anatomy, etiology, pathological physiology, symptomatology diagnosis and prognosis, treatment. These sections are again formed by subsections and chapters; thus the section on treatment contains a first part, on general therapeutics, in fifteen chapters; and a second, on the special treatment of the particular joints, in eight chapters.

Mr. Barwell commences his volume with a chapter on the physiological anatomy of the joints; it contains little to which we need call our reader's attention, and much that might very well have been omitted. One or two points only require notice. The author describes at length the minute structure of the articular lamella, which, he says, is lighter in colour, more transparent than the rest of the bone, does not contain bone-cells or canaliculi, but, "in reality, consists of a series of very minute parallel tubes, which run in a wavy course from the bone to the cartilaginous surface. Among these, but having no special, if any, communication with them, are the bodies mentioned by Kölliker as undeveloped bone-cells." The author considers that these tubes permit the passage of nutrient fluid from the bone to the deep surface of the cartilage.

It has often been questioned whether the articular cartilage is covered by an epithelial layer, a continuation of that covering the synovial membrane. Microscopical examination of sections shows that the appearance of a distinct layer is produced by the cartilage-cells gradually becoming flattened and placed horizontally as they approach the free surface. This gradual change is well represented in the figure at p. 12. This structure of the cartilage is urged as a further proof of the source from which the cells derive their nourishment.

"The position of these cells and their gradual separation from each other, and diminution in size, prove that they derive their nutriment from the attached surface, and this is in consonance with the arrangement in other structures which line cavities, for they are all nourished by the deep surface. Thus the absence or presence of vessels upon the fœtal cartilage is of importance, because if present it (*sic*) would establish the fact of a structure lining a cavity being nourished by its free surface. Besides, as it is clear that, at least during intra-uterine life, there is large provision for nutrition of the cartilage from the deep surface, the presence of such vessels would show that a structure having one free, one attached surface might be nourished from both. Moreover, it would prove that a cellular structure might be nourished from the surface towards which the cells grow. But the two first facts would be isolated, and the last is hardly conceivable; and as others besides myself have failed to discover this arrangement, there must have been in the observation of such vessels some occult source of error."

The only other point to be noticed is Mr. Barwell's account of Weber's theory, and of his own experiments. He commences with the statement that "it has been a theory in physiology, that as the synovial membrane is a closed sac, it prevents any admission of air to the inner structures of joints, and forms therefore a vacuum, producing thus a pressure from the exterior which aids materially in keeping the joint-surfaces in contact." We know not whence the author has drawn this, as it appears to us, very extraordinary statement; we are afraid that it can neither be received as a representation of what is ordinarily believed, nor yet of Weber's ideas as to this matter. All we believe that he endeavoured to show was that the pressure of the atmosphere played an important part in keeping the bones of the hip and other joints together; that, speaking in general terms, the weight of the limb was supported by the atmosphere, and that thus the muscles and ligaments were relieved from much unnecessary labour. As we think Mr. Barwell has misunderstood the theory, it will not be necessary to discuss his experiments at length. No one imagines that it is impossible to separate the femur from the ilium; the real question is whether greater force will be required to do this than would be necessary if the limb were *in vacuo*. The *experimentum crucis* would be to try this with the joint placed in the receiver of an air-pump. Weber, we believe, performed this experiment, and with a result which appeared decisive to him.

The next chapter is on acute synovitis, and on the whole gives a satisfactory account of this disease. A few of the statements require qualification; thus, that the knee is invariably affected in gonorrhœal rheumatism: such is certainly very often but by no means invariably the case. Rollet,* in 28 cases, had only 22 of affection of this joint; Brandes, in 34 cases, 28; and Foucart, 14 in 18 cases. The following passage is worthy of notice:

"An old notion fixed, as the first effect of synovitis, dryness of the membrane; and I see, in a recent book on Joint Diseases, that its author, Mr. Bryant, of Guy's Hospital, still adheres to that idea. The theory, however, has no facts to support it, and was supposed to be set at rest by the experiments of M. Richet of the Hôpital Bons-Secours, of M. Bouley, of M. Rey, and of others, none of whom mention dryness as a result of artificially excited and carefully watched synovitis. The first author indeed expressly denies its existence. He had opened, at different times, the joints of several dogs, and observed hour by hour the alterations taking place in the synovial membrane. He says—'after ten hours the membrane lost its polish, but I never at any time could find that it was drier than in the normal condition—this dryness of the serous membranes in the first stage of inflammation is admitted by all authors, yet nothing is less proved, and it is to explain their crackling (*bruit parcheminé*) that this particular condition has been invented.' The first effect, then, of the inflammatory irritation is congestion of the subsynovial tissue, accompanied by rapid secretion of serous or thin synovial fluid into the joint."

The process of granulation, by means of which the synovial membrane becomes gradually thickened, is described at length; it is, according to the author, "precisely similar to that which occurs in a wound healing by the second intention, or in an old abscess which is filling up."

The researches of Dr. R. Volkmann† on the catarrhal forms of pyarthrosis, will necessitate a considerable change in our views as to the production of pus in joints, should they eventually be confirmed.

* J. Rollet: Recherches sur la Syphilis, &c., p. 412. Paris, 1861.

† Langenbeck's Archiv für klinische Chirurgie, Band i. S. 408. Berlin, 1861.

It has been almost universally admitted, as is of course well known, that pus was not produced in those less severe cases of synovitis which were followed by recovery; secondly, that the pus occasionally found in rheumatic cases, &c., was the result of unusually violent inflammation; and lastly, that pus once formed in a joint was rarely absorbed, that it was almost always discharged externally. All these assertions will require modification, for he proves that pus may occur in the slighter forms of synovitis, rheumatic, gonorrhœal, &c., and that it is in many cases readily absorbed. In short, he endeavours to show that there are two kinds of suppuration in joints—a superficial, less violent form, somewhat resembling the catarrhal inflammation of a mucous surface; and a parenchymatous variety, affecting the deeper layers of connective-tissue, accompanied or followed by ulceration, destruction of the cartilages, formation of abscess and of granulations, &c. After describing the pathology of suppurative synovitis, Mr. Barwell proceeds to consider the symptoms of synovitis in general, and then specially the position and swelling in each joint. As to the diagnosis of inflamed bursæ about the hip, they may generally be recognised by the absence of any alteration in the apparent length of the limb, by the absence of pain on striking the trochanter or the foot, and by the swelling being limited to the region of the bursa. The assertion, that when in synovitis of the knee the secretion is rather large in quantity it causes fulness of the popliteal space, seems to us very doubtful. At the back of the joint the ligaments are the strongest, and there is such a thickness of soft parts that any perceptible swelling does not appear probable. In the treatment we find mention of subcutaneous incision of the synovial membrane, but not of puncture of the joint with a trocar, a method which has been employed and strongly recommended for particular cases by Volkmann in the paper just quoted. Case 7, incised wound of the knee, would have been better treated by ice, for, as the author says in the next page, " locally, ice, or at least very cold water constantly renewed, is the very best application." Some excellent cases of wounds of the knee were published in the 'British Medical Journal' two or three years ago by Mr. Humphreys, of Cambridge; and it is difficult to understand the stupid adherence to a routine system of dressing accidents which prevents so many surgeons from appreciating the extreme importance of this treatment with ice. The publication by the New Sydenham Society of Dr. Montgomery's translation of Esmarch's essay will, it is to be hoped, induce some to abandon the use of poultices, &c., in recent injuries.

The third chapter is on acute rheumatism, and is almost entirely devoted to showing that the rheumatic affection of joints is a genuine inflammatory disease; the greater part of the fourteen pages thus employed might have been well omitted.

The succeeding chapter commences with a long dissertation on pyæmia, its symptoms, pathology, &c., which is certainly quite out of place in a treatise on diseases of the joints, and which unfortunately only ends in rather negative conclusions. There then comes a short sketch of the local symptoms of pyarthrosis, in which we find it maintained that in gonorrhœal rheumatism the case often does not advance to the actual formation of pus, a statement which will require alteration, for, on the one hand, Volkmann* has shown that pus-cells in small quantity are produced; whilst, on the other hand, it might almost be asserted that abscess never occurs, Velpeau, Foucart, Bonnet, Brandes, and Rollet all agreeing in this opinion. The joints in this disease are occasionally red, and not always extremely white, as stated by Mr. Barwell.

As to the abscesses occurring in pyæmia, the author recommends—

"Not to open them while the patient's health continues in a very low depressed state, unless absolutely necessary. I have seen the bad symptoms greatly increase after such an operation. The pus in them is often in a very peculiar state, and the least admission of air causes rapid putrefaction. If an abscess must, however, be opened, the wound should be closed as soon as the pus has ceased flowing, and pressure by means of a bandage is to be applied. When, however, the pus is in a joint cavity, the earliest possible opportunity which the condition of health permits should be taken to evacuate the matter by a wide incision in the most depending part. Chlorinated soda lotion upon lint may be inserted into the wound, or indeed, the cavity may be rinsed every day with that fluid by means of a gum-elastic tube and syringe."

Towards the end of this chapter there are a few further remarks on gonorrhœal rheumatism, which is considered by the author to be really "a slower form than ordinary of purulent infection produced by inflammation of the prostatic veins," a view which is, perhaps, worthy of consideration, but which, we are inclined to think, will not prove correct; the connexion of this form of synovitis with a peculiar form of iritis,† is alone sufficient to cause us to feel some doubt, and we cannot say that we are prejudiced in its favour by the author's further statement, that he has "no cases of dissection to prove this position; persons do not die under gonorrhœal rheumatism."

Strumous synovitis is the disease next considered. Under this denomination we find that form of synovitis described in which there is a peculiar thickening of the synovial membrane by a gelatinous, soft, pulpy material, of a light brown or rose colour, proceeding from both sides of that membrane, and sometimes developed to such an extent as almost entirely to fill the cavity of the joint: pus also is not unfrequently produced in more or less abundance. The newly formed granulations would, in a healthy individual, soon pass on to a further development, that into fibrous or imperfect areolar tissue; whilst in a typical case of strumous joint disease they will continue in the same state without forming fibres, and never advancing beyond their first crude condition. The cartilages and bones ultimately become involved, and the case may then serve as a typical example of the "white

* Loc. cit.

† Rollet has devoted an interesting chapter to iritis considered as one of the manifestations of gonorrhœal rheumatism, loc. cit. p. 393; and Mr. Wordsworth has recently published a paper on the subject in the Ophthalmic Hospital Reports, vol. iii. p. 301.

swelling" in its worst form. All these various processes are described satisfactorily by the author, without the addition of any new information of importance; the structure of the tissue which was formerly called fungous or lardaceous, was clearly explained many years ago by Lebert, in his treatise 'On Scrofulous and Tuberculous Diseases.' Crocq also has entered fully into this question; even Sir Benjamin Brodie, relying apparently on clinical observation, ultimately arrived at a correct conclusion.

The two forms of scrofula described by Mr. Barwell, would, perhaps, be better entirely separated, as indeed appears now to be the tendency; for the sake of comparison, we append the characters given by Dr. Jenner:*—

Tuberculosis (as signifying the condition of the system in which there is a tendency to the deposit or formation of tubercle).—Nervous system highly developed; mind and body active; figure slim; adipose tissue small in quantity; organization generally delicate; skin thin; complexion clear; superficial veins distinct; blush ready; eyes bright, pupils long, eyelashes long; hair silken; face oval, good-looking; ends of long bones small, shafts thin and rigid; limbs straight. Children the subjects of tuberculosis usually cut their teeth, run alone, and talk early.

Leading pathological tendencies.—Fatty degeneration of liver and kidneys; deposits or formations of tubercle, and their consequences; inflammation of the serous membranes.

Scrofulosis.—Temperament phlegmatic; mind and body lethargic; figure heavy; skin thick and opaque; complexion dull, pasty-looking; upper lip and alæ of nose thick; nostrils expanded; face plain; lymphatic glands perceptible to touch; abdomen full; ends of the long bones rather large; shafts thick.

Leading pathological tendencies.—Inflammation of the mucous membranes of a peculiar kind; so-called strumous ophthalmia; inflammation of the tarsi; catarrhal inflammations of the mucous membrane of the nose, pharynx, bronchi, stomach, and intestines; inflammation and suppuration of the lymphatic glands on trifling irritation; obstinate diseases of the skin; caries of bone.

It is curious to observe how the term scrofula has gradually changed its meaning; it was originally employed to designate the various enlargements of the lymphatic glands, which now, according to Lebert, should almost always be considered as tubercular; it was not till the latter part of the last century that it was used in a more extended signification to include a number of affections of the bones, skin, eyes, &c., which were supposed to be closely allied to the glandular enlargements; since that period it has been applied by different writers in very different senses, and generally very loosely, either as denoting a peculiar constitution, or as a generic name for a number of diseases which had certain appearances in common, or as quite synonymous with tuberculosis; the adjective scrofulous has even had a wider range,

* Medical Times and Gazette, 1860, vol. i. p. 259.

having been commonly used in a most vague and indefinite manner to denote any chronic inflammation of a joint, of the eyes, &c., with little or no tendency to recovery. More recently the writings of such men as Milcent,* Lebert,† Jenner, have diffused some knowledge of the fact that there are many and important differences between scrofulous and tubercular disease, and that a more definite signification may be given to scrofula. Although Mr. Barwell has clearly perceived that these two forms are distinct, and has especially noticed the difference in the treatment, as we shall soon see, he has failed to recognise how utterly fallacious are many of the characters which he has given. Lebert‡ says, in reference to a very similar description of scrofula by Hufeland :—"Les hommes sérieux qui se sont occupés de ces maladies, ne peuvent voir dans cette description pittoresque que de la poésie et du roman, et ces lignes ne témoignent guère d'une expérience médicale de cinquante années de pratique." The same author proceeds thus—

"This picture is, first of all, very much overdrawn; there is especially a singular medley of purely accidental features and of characteristics possessing a greater value, such as the thickening of the upper lip and of the nose, the bloating of the face, the enlargement of the submaxillary region, &c.; but this external aspect is the exception in scrofulous cases, and not the rule; we find it at least in as great a proportion amongst persons affected with tuberculisation of the external glands, without any scrofulous complication. We have also many times observed the same appearances in cretins, who were neither scrofulous nor tubercular; and if we analyze our own statistics of the scrofulous, strictly so-called, we find from our notes that in 537 cases only 81 presented the appearances designated under the title of the scrofulous habitus, 36 of these were males, 45 females, and we have included in the number of 81, 18 persons who had at the same time glandular tubercles. It follows from this analysis, that only in rather more than 1 in 7 of the cases do we observe this peculiar external appearance, which therefore loses all pathognomonic value."

And, again, the same author says—

"We have quite as often had undeniable proofs of scrofula and of tubercles in dark-complexioned as in fair persons, in those with finely chiselled features as in those with the face broad and flat, in the quick and lively as well as in the dull and lethargic. The complexion was tolerably often brown, sometimes it was very white and very delicate, but generally there was nothing very striking in its appearance. As to the colour of the hair and eyes, we found an almost equal number amongst the dark, the light, and those with medium shades. Nothing, therefore, in the present state of science, proves that the temperament has any influence as to the predisposition to scrofula and tubercle."

Mr. Barwell recommends in "that form of scrofulous disease which is marked by thick unwieldy connective tissues," genuine scrofula, a purge of calomel and jalap, or of calomel with rhubarb; such a purgative may require repetition once or twice. Small alterative doses of mercury may be given for a day or two with advantage; the iodide of potassium is specially indicated. "Quinine, mineral acids, and bitters,

* De la Scrofule, de ses formes, &c. Paris, 1846.
† Traité pratique des maladies scrofuleuses et tuberculeuses. Paris, 1849.
‡ Loc. cit. p. 65.

are the tonics most beneficial. . . . . Iron is far less valuable, and cod-liver oil very frequently disagrees."

"The form of struma which is distinguished for the fine delicate formation of the connective tissues, is to be managed on a different plan. Purges and mercury in any form must be avoided; the inaction of the intestinal canal is to be combated by mild vegetable aperients; rhubarb given in pill immediately before or with the last meal at night, is an excellent plan, and anything like a violent or irritating evacuant does harm. Iodide of potass in the most typical cases of this sort of struma is not beneficial; the whole class of alteratives are not needed. On the other hand, tonics are extremely valuable. Cod-liver oil is especially indicated, as we desire increase of nutriment; in these cases it very seldom indeed disagrees. Quinine, if the appetite fail, is useful; but iron is to be much more highly prized; in fact, I esteem steel and the oil as the best medicinal agents."

Three stages of the joint-disease are pointed out: 1st, the inflammatory and granulating processes; 2nd, the cartilaginous and osseous inflammation; 3rd, the consolidating and degenerative processes, separately or in combination; and the symptoms then follow. The pain produced by pressing the bony surfaces of the joint together, and which used to be attributed to ulceration of the cartilages, is considered by the author to indicate that the articular lamella has given way, and that some of the cancelli are laid bare; this pain had been already attributed by Sir B. Brodie, and we think by others, to disease of the bone. The actual cautery is strongly recommended when the first symptoms of inflammation have somewhat subsided. When the joint has become quiescent, when there is no longer pain on pressure, passive motion, shampooing, &c., are very useful; they are contra-indicated by "an active condition of the swelling evidenced by pain and tenderness, any considerable amount of degeneration or suppuration, starting pains, and tenderness of the joint surfaces." Ankylosis may be prevented by a judicious use of passive motion; perhaps in only one case should it be intentionally promoted, that case being when the disease has been so extensive, the ligaments so relaxed or destroyed, that the limb would otherwise not support the weight of the body. With Mr. Barwell's concluding sentence we most cordially agree:

"The experience which I have had of the fungous form of strumous disease, and the efficacy of well-adapted treatment, convinces me that extensive suppuration and utter loss of health are the only reasons which should cause us to remove the part. The mere presence of sluggish granulations, evidenced by intractable and long-persistent tumefaction, is no valid cause for such an operation. All such tissue may be made to consolidate or to be absorbed by the adoption of fitting means."

An admirable sentence. Time, good air and diet, careful treatment, will work wonders in the hands of a genuine surgeon.

Chapter VI. is on chronic rheumatic synovitis, which is distinguished from the strumous by the great tendency of the newly-formed granulations to become organized: the synovial membrane is transformed into a tough, firm tissue of a light reddish-brown hue; anchylosis is produced by broad new growths of bone, and thus quite differs from that occurring in scrofulous cases where the bones seem to sink into one another.

In the succeeding chapter we find syphilitic and gouty synovitis described; then follow chapters on hydrarthrosis, on loose cartilages in the joints, and on acute articular osteitis; there comes next an article on strumous articular osteitis, perhaps the best in the book. Mr. Barwell is not aware of any observations of crude tubercle in bone; he will find several such in Lebert's work, from which we have already quoted; the presence of lime in pus from bone has been determined long since.

The symptoms by means of which strumous articular osteitis may be distinguished from strumous synovitis are tabulated. We should gladly have inserted this comparative sketch, had space allowed. After attending to the general rules of hygiene and constitutional treatment, the most important point is to place the limb in a good position; this may be effected gradually by a splint provided with a screw, or forcibly under chloroform; in many cases subcutaneous tenotomy will be very useful. In some rarer cases tenotomy may be called for, with the view of simply relieving the starting pains, even when the limb is quite straight. The author has hitherto had no case in which he has found such a proceeding necessary; such a treatment has been employed by Dr. Bauer, of New York, who has given a most favourable report of its efficacy.

Chronic rheumatic osteitis follows, and is apparently so called because the author believes it commences with osteitis. Prof. C. O. Weber,* however, in an interesting paper, has endeavoured to show that the earliest changes in this disease occur in the cartilage.

On the inflammation and degeneration of cartilages, the author commences with a short account of the researches of Sir B. Brodie, Goodsir, Redfern, &c., but does not refer to Virchow, who first really showed these changes to be inflammatory; for although Goodsir, Bowman, and Redfern, by their investigations into the diseases of non-vascular parts, cornea, cartilage, &c., had pointed out many of the changes, still the ideas of inflammation at that time current were so firmly bound up in vascularity and exudation, that these alterations could not be recognised as truly of an inflammatory nature; Virchow distinguished one form of inflammation in kidney, muscle, and walls of the vessels as parenchymatous, depending on changes in the tissues themselves, and unaccompanied by exudation; and he soon extended this doctrine to the non-vascular parts, such as cartilage. Mr. Barwell says: "I believe myself to have been the first to have pointed out that those diseases of cartilage which accompany the inflammation of other tissues in the joint are in reality inflammation." Our previous remarks show that we believe Virchow really deserves the credit. Prof. C. O. Weber† also published, in January, 1858, a very elaborate paper—one in some respects more full than Mr. Barwell's account.

* Virchow's Archiv, Band xiii. S. 82–87. Berlin, 1858.

† Ueber die Veränderungen der Knorpel in Gelenkkrankheiten: Virchow's Archiv, Band xiii. S. 74.

However, although we cannot allow the claim of priority, we are quite willing to admit that the author's account is in many respects very careful and satisfactory.

The chapter on disease of the hip-joint is rather deficient; the diagnosis from other affections, such as caries of the ilium, disease of the sacro-iliac articulation, &c., should have been carefully explained; even the diseases which but rarely simulate it should have been mentioned.* Much valuable information on these points will be found in the works of Crocq and Lebert, and in a lecture on sacro-iliac disease by Mr. Erichsen, recently published in the 'Lancet.'

The next two chapters, on bursæ, synovial sheaths, and the so-called hysterical joint diseases, contain nothing to which we need call attention: in the following one, "On the Restoration of Mobility and Conformity to Crippled Joints," the assertion that Dieffenbach employed sudden and forcible ruptures without tenotomy is incorrect, as may be seen by a reference to his work on operative surgery. Many instructive observations occur in this chapter; for one only have we room. Mr. Barwell is considering the amount of force permissible to break down ankyloses. "The use of considerable force is justifiable for the reduction of a malposture of the knee-joint, but unjustifiable for the re-establishment of mobility in a joint already in a good position."

The last portion of this work is on the removal of diseased joints, and contains a good deal of excellent advice: the great principle is, not to excise or amputate till nothing else can be done—*ferrum est ultima ratio.* As regards the choice between these two methods of operation, if there should be any doubt, the surgeon can commence with excision and end with amputation, should it prove necessary. The author says of the appearances presented by the section of bone:

"These may deviate from the norm by hyperæmia, extravasation, granulation, suppuration, and by wasting or induration of the cancellar walls. This last appearance is in all instances favourable to excision, in direct proportion to the amount of tissue thus affected, in contrast to the amount which has undergone softening; it is a sign of a constitution capable of a sthenic inflammation; such condition is very rarely spread over the whole section surface, and then only in the rheumatic form of inflammation. In strumous cases, induration of the bone tissue, when present, alternates with softened portions, and the more of the thickened tissue be found upon section, the more favourable is the case. Other appearances, hyperæmia, granulation, suppuration, and wasting of the cancellar walls, will all be present in cases of strumous disease so far advanced as to justify operative interference; none of these should of themselves militate against completing the excision; but if they be diffused over the whole, or nearly the whole section, be hardly at all intermingled with indurated portions, and not confined to one or two spots surrounded by thickened osseous tissue, it will be better to amputate the limb. . . . . A diffuse inflammation, wherever it be situated, and whatever be its products, always marks so low a constitutional state, that we should give the system as little reparative labour as possible; the granulation produced by

* A case is recorded in the 'Medical Times and Gazette,' new series, vol. viii. p. 317, where some enlarged and tender glands in the groin apparently were the cause of pain in the hip, limping, flattening of the buttock, &c.; and another case in the 'London Medical Gazette,' vol. xii. p. 127, where periostitis of the trochanter simulated morbus coxarius: such cases deserve some notice.

such an inflammatory act will hardly form the sound tissue so essential to success in these cases, but will greatly tend to the degenerative processes. The more straw-coloured be this tissue, the less should we trust to its organizing force. Again, if the section surface be throughout of a dirty yellow, from diffuse suppuration, and the cancellar walls be all softened, impressable with the finger, or incisable with the knife, amputation should be at once employed. If such appearances be limited to circumscribed portions of the section surface, there is still a further investigation to be made, namely, the depth to which the softening extends. Unless the superficies of such a portion be very soft, it is hardly likely to extend far; the gouge, or Mr. Marshall's osteotrite, may be used, and the diseased portions removed; but if they run a long way into the bone, more especially if, as sometimes happens, the several spots unite, and form a large space of softened tissue some way from the surface, the case is not suitable for excision."

In short, the inflammatory changes should be limited to the surface, or little more than the surface, of the articular extremity, and some tendency towards reparation should be exhibited. The different methods of performing excision, and pretty full details of the statistics, are given for each special joint. One case of excision of the knee on a female has been often asserted to have died of tetanus; she did not: the operation was performed by Mulder, a professor at Gröningen, and she ultimately perished from long-continued suppuration, hectic, and exhaustion.

In the note on excisions of the knee performed by German surgeons, there are some errors: Textor, sen., had 4 cases, of which 2 died, and 1 was amputated a year later (*operation de complaisance*); Textor, jun., 2 cases, both died; Fricke, 3 cases, 2 deaths, 1 recovery; Demme in Bern, 2 successful cases; Bruns, 3, of which 2 were successful (probably before 1850); Roser, 2, 1 death, 1 amputation; Prof. Günther, 1 in 1845, death; Heyfelder, 1, in 1849, death. As to the statistics of amputation, those of the Devon and Exeter Hospital, published by Mr. James,* give a better result than those of Guy's Hospital, at which Mr. Barwell expresses surprise; at the former hospital, there were from 1816 to 1849, 118 amputations of the thigh for disease, of which 11 died (= 9·322 per cent.); at the same hospital the mortality for all amputations of the thigh = 16·438 per cent., and for injuries alone = 46·428 per cent.

The method of excising the ankle-joint recommended by Mr. S. Buchanan† seems a great improvement in comparison with the older plan described by Mr. Barwell.

To conclude this somewhat fragmentary analysis, we may say that whilst, on the one hand, we consider that the arrangement of this work is on the whole not good, the author's style diffuse, the language not always grammatically correct, and that scarcely sufficient attention has been paid to the literature of the subject; on the other hand, we are glad to admit that the matter is almost always excellent, and that it will form a valuable addition to English medical literature.

* Transactions of the Provincial Medical and Surgical Association, vol. xvii. p. 52.
† Glasgow Medical Journal, vol. ii. p. 1. Glasgow, 1855.

PART SECOND.

Bibliographical Record.

ART. I.—*On the Immediate Treatment of Stricture of the Urethra, by the Employment of the "Stricture Dilator."* By BARNARD HOLT, F.R.C.S.—*London*, 1861. pp. 56.

FOR the treatment of those strictures in which a small instrument can be passed, but which resist the ordinary method of gradual dilatation, there are three principal methods in use. The followers of Mr. Syme recommend division on a grooved staff from the perinæum; those of M. Civiale, the division of the stricture internally by means of a sort of bistouri caché passed through the stricture, and made to divide it as it is withdrawn; and, finally, Mr. Holt, in the little treatise before us, gives us the result of his large experience of the third method—that of tearing or splitting the stricture by driving a large rod through it. For the method of performing this simple operation, we must refer our readers to the work itself; our only concern here is with the advisability of the proceeding. Mr. Holt tells us that he has operated on more than 100 cases without any bad consequences, and we can ourselves testify to having seen cases so treated at the Westminster Hospital with great success; so that we may perhaps assume that the risk connected with the operation is not very great. Still, we do not see any evidence in the work before us that the cure is at all more permanent than that effected by gradual dilatation, and some risk must be incurred by this rough and violent proceeding, from which the dexterous use of dilatation is free; so that we must conclude that Mr. Holt does not mean to recommend its adoption except as a very occasional expedient, and that in performing it in so great a number of cases he was rather intent upon testing the method than intending to set an example for other surgeons to follow. If so, we are quite prepared to admit that, as far as present experience goes, his method seems to involve less danger than that of Civiale, and, *à fortiori*, than that of Syme; but we consider that only a very small per-centage of cases are in want of the application of any extraordinary method, if only the ordinary ones are used with sufficient tact and patience. In all other respects, Mr. Holt speaks with much moderation and good sense of the treatment which he advocates. His little book is well worth reading, and its suggestions are worthy of a trial in suitable cases.

ART. II.—*A Manual of the Practice of Medicine.* By GEORGE HILARO BARLOW, M.A. & M.D., Senior Physician to Guy's Hospital, &c. Second Edition.—*London*, 1861. pp. 738.

THE results of a long-continued experience within the walls of a busy metropolitan hospital are attractively presented in this little handbook, and we can cordially recommend it to any who may at times feel the want of practical hints on medical questions in the various walks of our profession. This second edition materially differs of necessity from the earlier one, inasmuch as it embodies a consideration of several important cardinal points, as well of theory as of practice, which have demanded readjustment since the appearance of the first edition. Such an one is the "Treatment of Inflammation." Dr. Barlow has apparently discussed this and other subjects of equal importance in an impartial and judicious manner.

We perceive that he still adheres to the views which he formerly set forth respecting the treatment of cholera.

ART. III.—*The Functions and Disorders of the Reproductive Organs in Childhood, Youth, Adult Age, and Advanced Life, considered in their Physiological, Social, and Moral Relations.* By W. ACTON, M.R.C.S., &c. &c. Third Edition.—*London*, 1862. pp. 218.

IN July, 1857, we had the opportunity of thanking Mr. Acton for the endeavours which, in an earlier edition of this work, he made, usefully and becomingly, to treat the intricate problems which form the subject of this book. Nevertheless, we yet find reason, as we did on the former occasion, to take exception to sundry passages which, in our humble opinion, might well have been omitted. We are not insensible of the great difficulty of avoiding such in the adequate handling of the various points in question, nor, remembering that *puris omnia pura*, are we so over-fastidious, so falsely delicate, as not to call a spade a spade; but we must object to certain illustrations, certain quotations, and self-accusations on the part of clients unnecessarily introduced and enlarged upon, which, given so unreservedly, lend to the book a prurient colouring that ought, we think, to render our readers cautious as to the hands in which they would place it. If asked to indicate any part of the work more specially practical and appropriate, we should perhaps name the second chapter of Part II., one in which are pointed out the multifarious ramifications of a gigantic and loathsome but ill-understood evil (solitary indulgence), and judicious methods of prevention and cure set forth. Mr. Acton more than once, and very appositely, quotes the words of that good bishop of our own Church, Jeremy Taylor, on the subjects of his work, which are indeed common both to the physician and the theologian. He might well have done so in reference to the particular subject above alluded to; for whilst we agree with him in the absolute necessity of warning children against and helping them to withstand a baneful evil which of necessity will so often cross their path of life, we have weighty reasons for suggesting that, with regard to this particular form of sensuality, there is no mode of prevention, no means of cure to be offered to the young so efficacious (a thousand times more so than mere "scientific information") as the regular habit of watching, under proper direction, and checking the tendencies of their own minds in this respect. For as that Bishop remarks, "The Church of God in all ages hath commended, and in most ages hath enjoined, that we confess our sins, and discover the state and condition of our souls, to such a person whom we or our superiors judge fit to help us in such need. . . . . . And the shame of opening such ulcers may restrain your forwardness to contract them." Mr. Acton evidently alludes to this mode of grappling with the roots of the evil, but does not seem so openly to enforce it as he might do, both with propriety and effect.

ART. IV.—*A System of Surgery, Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL GROSS, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c. Second Edition. In Two Volumes.—*Philadelphia*, 1862.

IT is but little more than a year since we had to welcome the first edition of this voluminous and useful work; and in the present edition the author has given us no cause for withdrawing any part of the favour with which we received the earlier issue.

We find that a considerable amount of new matter has been introduced, and a smaller type used. We also find that a large number of illustrations (some of them, by the way, a little too rough and coarse) have been added.

Especial consideration has been devoted to the subject of gun-shot wounds.

We are glad to learn that a translation of this work into the Dutch language is now in progress at Neuwediep, in Holland; and we trust it may be as well appreciated in that country as it has been in our own.

ART. V.—1. *Sulle Malattie da Fermento Morbifico e sul loro Trattamento.* Memoria del Dottor GIOVANNI POLLI, Membre del Reale Instituto Lombardo di Scienze, &c.

On the Diseases produced by Morbid Ferments, and on their Treatment. Memoir by Dr. G. POLLI.

2. *Saggio Farmacologico sui Solfiti e gli Iposulfiti Medicinali.* Del Dottor GIOVANNI POLLI.

Pharmacological Treatise on the Medicinal Sulphites and Hyposulphites. By Dr. G. POLLI.

M. POLLI being desirous of applying to the treatment of disease the properties which sulphurous acid possesses of arresting, or as he expresses it, paralysing, fermentation, has found that the sulphites and hyposulphites of alkaline and earthy bases fortunately enjoy qualities in that respect identical with those of the constituent acid, besides having the advantage of being largely tolerated by the economy. The products, indeed, of putrescent decay are not affected by these substances as they are by those disinfectants which destroy them by supply of oxygen; the sulphites and hyposulphites, in their passage through the system, on the contrary, become converted into sulphates, absorbing oxygen. Their therapeutic use in phlegmonous disorders might very well have suggested itself, if we may consider, with Mülder, that the principal phenomena of these disorders, pyrexia, increased elaboration of fibrin, plastic effusion, depend mainly on a hyperoxidation of proteinous bodies in the blood. But it is chiefly with regard to a group of diseases in which a morbific ferment may be supposed to be present, that the author expects benefit from their action, this being, with regard to fermentation, more universal than that of other known substances, by whatever law that action may be explained.

Under the head of *catalytic* diseases, M. Polli enumerates the exanthemata and dartrous affections, rheumatic, intermittent, typhoïd, puerperal, and other fevers, pyæmia, &c. From this preliminary he has entered on a very extended series of experiments, seventy in number, performed on dogs, which consist of injections of more or less putrid blood and pus, as well as glandered mucus, into their veins, while subject to the administration of the salts. Undertaken in a very philosophic spirit, these experiments would appear in their results to favour his expectations and to encourage a more extended inquiry. Our space only allows us to mention, that the sulphite of magnesia, sulphite of soda, and hyposulphite of soda seem best adapted for pharmaceutical purposes; they may be given in doses of from one to twelve or more grammes daily, and their administration in a prophylactic sense seems to promise most advantage. We hope to return to the subject in a future number.

ART. VI.—*On the Relative Influence of Nature and Art in the Cure of Syphilis.* By THOMAS WEEDEN COOKE, Surgeon to the Royal Free Hospital, &c.—*London*, 1861. pp. 64.

THIS pamphlet is an attempt to demonstrate that the use of mercury in syphilis is altogether to be reprobated, and that the disease ought to be left to the curative powers of nature, assisted in the elimination of the poison principally by hygienic and dietetic means; to which, however, Mr. Cooke adds a course of medicine, but rather, as it seems to us, as an excuse for keeping the patient under medical supervision than as an essential part of the treatment. The only medical treatment, in fact, which Mr. Cooke speaks of, is the administration of fifteen grains of chlorate of potash with twenty minims of dilute hydrochloric acid three times a day. Mercury he would banish altogether, as being always useless and generally noxious. Of the use of iodide of potassium he speaks in the most slighting manner. Local applications he thinks of no importance, provided the sore be kept clean. Cauterization he says nothing about. We are not inclined, however, to complain of the inadequacy of Mr. Cooke's directions on the subject of treatment (although we regard the method which he has prescribed as utterly inactive), since it is evident that the main object of his work is not to teach the surgeon to treat syphilis, but to induce him to let it pursue its course. If Mr. Cooke's theory is a true one, we are bold to say that the interference of the surgeon in a case of syphilis is quite unnecessary, for the "hygienic and dietetic" measures he advocates are nothing but what most men of sense would prescribe for themselves, and which no medical advice could persuade a man devoid of sense or of self-control to follow; so that it would result from such doctrines, that the ravages of venereal diseases would be best met by leaving the whole thing alone. To prove so startling a proposition, or even to render it probable, a very different style of reasoning, and a very different series of facts to those which Mr. Cooke has here given us, would be required. His reasoning, if it deserves to be called so, is hazy, crude, often contradictory, and not infrequently a mere tissue of assertions; while the facts on which it rests (such of them as are given in the book, and we may assume that they are at any rate not below the average of the author's unpublished cases) are anything but perfect. The precise indications for the use of mercury or iodide of potassium in the various forms of syphilis, and the true meaning and natural progress of the different syphilitic lesions, both primary and secondary, are very difficult and very interesting questions; but we have seen too many instances of apparent good results from both the so-called "specifics" to acquiesce in the sweeping condemnation which Mr. Cooke pronounces against them, while we have seen also too much of the ravages of syphilis in cases which have received no specific treatment whatever, to place much confidence in the resources of what Mr. Cooke calls "our alma mater, Nature." At any rate, if all our previous impressions and theories on this subject are to be overthrown, let it be by some logical reasoning, not by an author who disbelieves the power of mercury to cure syphilis because its *modus operandi* is unknown, yet believes that chlorate of potash can do it, for the following satisfactory reason—

"I am resolved to believe, until it can be proved to the contrary, that it carries a large quantity of oxygen, as well as chlorine, into the blood, and that these gases, combining with the constituents of that fluid, depurate it; and so we get, as it were, at the back of the disease, and push it out of the body by gradually substituting a healthy for a vitiated fluid." (p. 51.)

We need hardly say, that Mr. Cooke is in error in representing that those who advocate the use of mercury in Hunterian chancre believe that every such chancre is followed by secondary symptoms if left to itself; so that to adduce cases in which such sores are said to have existed without secondary symptoms is beside the question, especially when unaccompanied by any details proving the real character of the sore.

ART. VII.—*The Ambulance Surgeon; or, Practical Observations on Gun-shot Wounds.* By P. L. APPIA, M.D., &c. &c. Edited by T. W. NUNN, Assistant-Surgeon to the Middlesex Hospital, and A. M. EDWARDS, F.R.S.E., Lecturer on Surgery in the Edinburgh Medical School.—*Edinburgh*, 1862. pp. 266.

WE have read Dr. Appia's work with great pleasure and profit; and we have to thank the translators for bringing it under the notice of the English professional public. At the same time, we think that had they performed their task with somewhat more care, the result would have been more to the comfort and profit of their reader. As it is, blunders have been allowed to slip into the translation, especially of the first part of the book, which are unpardonable. Will it be credited that two surgeons, uniting their knowledge of the French language for the translation of a medical work, could have written the following (pp. 99, 100): "I have observed a young girl who sunk in a few days after a fright which placed her in a condition between tetanus and passion (*rage*)." As the translators, by supplying the French word, evidently showed that they did not understand their own translation (and no wonder), they should not have been above resorting to the Dictionary, where they would have discovered that "*la rage*" is the common French term for hydrophobia. Take another example, p. 77: "Hæmorrhage may produce its effects (*éclater*) very tardily—e.g., a young man with a transverse gun-shot wound of the neck died in a few hours of bleeding, which occurred during a paroxysm of mirth when the wound was almost healed." Surely the word "*éclater*" is not so unfamiliar, even had not the context shown that the author's meaning is that bleeding may *break out* at a very late period. Other examples of mistranslation might be cited, but we will not dwell longer on what are, after all, probably only errors of haste.

Notwithstanding these blemishes, the book is of considerable value, although it is not, and does not aim at being, a complete system of military surgery. It is rather a summary of the results of Dr. Appia's own experience; and the interest of the work lies principally in a comparison between the old and modern doctrines, showing how far the opinions of French surgeons have been modified, on the one hand, by the introduction of the more rational and more supporting plans of diet, and the invention of new operations intended to be conservative; and on the other, by the introduction of new engines of destruction, and the awful scale to which the carnage of modern warfare has risen in consequence of the greater range and precision of fire-arms. It will be seen by perusing Dr. Appia's pages that the balance has been preserved pretty steady by these opposing agencies. The disastrous effects of the conical ball in shattering bones and extending fractures far beyond the point of impact, have much restricted the application of operations for excision and resection in modern military surgery, and have also led to disappointment in estimating the internal mischief from the external wound, so that attempts at conservation frequently turn out disastrous; while the vast mass of wounded thrown upon the hands of the medical officers in such battles as that of Solferino, where in twenty-four hours, 11,500 French, 5,300 Sardinians, and 21,000 Austrians were laid *hors de combat*, would, even in cases otherwise appropriate, prevent any treatment requiring deliberation in operating and minute care in the subsequent attendance, and oblige the surgeon to terminate the case by the sweep of the amputating knife. "The art of destroying life," says Dr. Appia, "seems destined to advance with a speed which the science intended for its preservation is never able to attain." In the matter of excisions, however, we think our author might have been a little more copious with advantage. Excision of the knee he does not even mention, though it has been tried in modern warfare, and might perhaps attain occasional success in such circumstances as those of the battles in the streets of Paris, where the wounded can be immediately transported to fixed hospitals provided with every necessary appliance. "Resection of the upper extremity of the femur" he only mentions, without giving any directions as to its applicability; and says that "it does not appear to have been tried in the Crimea," which may be true of the French army, but it was practised with success by Dr. O'Leary, who removed the head, neck, and trochanter of the femur on the same day that the wound was received.* Excisions of portions of the foot are unnoticed; the only mention of excision of the shoulder is comprised in the notes of one fatal case; and from the account of excision of the elbow, the reader would infer that a stiff arm was the greatest success of which the operation is capable. Hence we may conclude that French surgeons are not much in the habit of practising excisions for gun-shot injury. It is on the treatment of gun-shot fractures of the femur that Dr. Appia is most diffuse; in fact, it is on this subject only that his treatise appears to aim at completeness, since the injuries of most of the other regions of the body are passed over somewhat cursorily. In such fractures Dr. Appia is strongly in favour of primary amputation in all but the simplest cases, except when the injury is so near the upper end of the bone that it would be necessary to amputate at the hip-joint. We will quote Dr. Appia's own words on this important point:

"The authentic statistics we have given, and the numerous failures we have observed in 1848, owing to the attempts at preservation of limbs, have convinced us, that if the first duty of the surgeon is to save the life of his patient, he ought not to be led away by the delusive hope of saving him a deformity. His duty, especially in war, is to amputate immediately, every time there is a comminuted fracture of the femur, if the wounded man is not reduced by hæmorrhage, or any other wound, to a perfectly hopeless state." (p. 132.)

He goes on to except injuries which would require primary exarticulation at the hip, since that operation has proved fatal in thirty consecutive cases.

We cannot avoid regretting that a surgeon who seems to have seen so much of gun-shot wounds, should not have thought it worth while to

* We speak on the authority of Mr. Longmore, in Holmes's 'System of Surgery,' vol. ii. p. 82; but no doubt the case is also recorded in the 'Medical and Surgical History of the War in the Crimea,' published by authority.

spend a little more space upon the interesting questions of penetrating wounds of the chest and abdomen, so as to estimate the diagnostic value of each of the symptoms in determining the existence and extent of visceral complications.

We have thus dwelt principally upon the second part of Dr. Appia's work, which treats of the special injuries. The first part, which is devoted to the general surgery of gun-shot wounds, is judicious and practical, but does not contain much which calls for remark. One thing which strikes us as remarkable is, that he saw no case of tetanus among about a thousand wounded in the Parisian hospitals in the street-fights of 1848. And here we may notice another and somewhat important inaccuracy into which the translators have allowed themselves to fall, and which, in another edition, should be corrected. They make their author say (p. 84): "Seeing the scarcity of cases of tetanus in 1848, I am obliged to have recourse in this serious complication to my observations made out on the fields of battle." He then goes on to quote five cases, all from *civil* practice: the bite of a dog, a fall on the knee, a punctured wound, a fractured thigh and contusions, and extraction of teeth. It is evident that Dr. Appia's words must mean, not "out on the fields," but "away from" scenes of warfare.

After the section on the special injuries, a few plain practical directions on the transport of wounded men and the first dressing of fractures will be found. This is followed by a chapter on disinfectants, by Mr. Nunn, which is well written and very useful, though it might have been longer with advantage; and the whole ends with a chapter on surgical apparatus, which we conclude is written by one of the translators, since the instruments recommended and the authorities referred to are all English, but the authorship is not distinctly stated. In this chapter, besides what its title would lead us to expect, the subjects of dressing wounds, operations on arteries and the common kinds of fractures, are included. A useful novelty in the part on ligature of arteries is a series of plans of sections of the limbs at various levels, by which the positions and relative depth of the principal vessels can be judged of at a glance.

In conclusion, we wish success to this little book, and we hope that the demand for a second edition may soon give the translators an opportunity of making it as creditable to their French scholarship as in its present state it certainly is to their ability as teachers of practical surgery.

ART. VIII.—*On the Diseases and Injuries of the Hyoïd, or Tongue-Bone.* By GEORGE D. GIBB, M.D., &c.—*London*, 1862. Pamphlet.

IN this "opusculum" attention is drawn to a corner of the pathological domain which, as the author rightly observes, has been too much neglected. Dr. Gibb has brought under contribution most of our London pathological collections, and by way of illustrations, has added several woodcuts of interesting specimens, of which he has given us a description. We wish, however, on this latter point, that he had had his "Special

Artist" with him, as the engravings might have been made very much more useful and descriptive, and certainly more artistic. We hope that the attention given by the author to that little appendage of our most "unruly member," the hyoïd bone, will have the effect of directing observers to the closer consideration of its pathological conditions.

ART. IX.—*Transactions of the Obstetrical Society of London.* Vol. III. For the Year 1861.

THE present volume indicates well-doing and progress on the part of the Obstetrical Society, and fully justifies the early hopes of its founders. The council have acted wisely, we think, in adding to the details of some of the papers communicated, a condensed account of the discussions which took place upon them. Such addition, in particular instances, cannot fail, if carefully and correctly made, to enhance the value of the individual cases, and to constitute a species of "reports" on the part of the collective Society.

As unfortunately, owing to a press of matter, we have only a definite space at our disposal, we are unable to criticise the various communications in the volume, or even to notice all of them. Our readers must be content to gather an estimate of the entire 'Transactions' from the mention only of the following contributions:—"On the Treatment of Sickness in Uterine Inflammation and Diseases of Menstruation," by Dr. Tilt. "Ovariotomy: with cases, and remarks on the different steps of the operation, and the causes of its mortality," by Dr. Tyler Smith; evidently a highly important paper. "On Uterine Hæmatocele," by Dr. Madge. "On the Indications and Operations for the Induction of Premature Labour and for the Acceleration of Labour," a lengthy and very interesting paper by Dr. Barnes. "On the Treatment of Cases of Abortion in which the Placenta and Membranes are retained," by Dr. Priestley. "A Case of Hydatid Mole expelled from the Uterus immediately after a Living Fœtus and its Placenta, at about six months' gestation; the hydatid growth being the degenerated ovum of a twin conception," by Dr. Hall Davis. "On Unusual Elongation of the Fœtal Head as a cause of difficulty in the application of the ordinary Obstetric Forceps," &c., by Dr. Graily Hewitt. A very elaborate paper by Dr. Uvedale West, illustrated by lengthy tables, in answer to the question, "Whether the ergot of rye, when administered to the mother during labour, is dangerous or not to the life of the child?" "On the Influence of Abnormal Parturition, Difficult Labours, Premature Birth, and Asphyxia Neonatorum on the Mental and Physical Condition of the Child, especially in relation to Deformities," by Dr. Little: a valuable communication, also of great length, and accompanied by careful tables and two interesting engravings, well executed by Mr. W. West. A paper "On Vaginismus," by Dr. T. Marion Sims, of New York. A paper "On Puerperal Fever," by Dr. Tilbury Fox. And, lastly, we will point out a very interesting description, by Mr. Robert Gleig, of an instrument for the application of electric heat as a means of cauterization in the treatment of certain uterine diseases.

We cannot conclude without drawing attention to the address by Dr. Tyler Smith, wherein he pays a well-timed tribute to the memory of Dr. Rigby, his predecessor in the president's chair, from whose history he draws an excellent lesson upon the value of recreation to the members of our hard-working profession. We are glad to learn that the Society has published a form of 'Register of Cases,' which, if adopted, will lead to a convenient uniformity in the drawing up of cases, &c.

ART. X.—*Hæmorrhoids and Prolapsus of the Rectum; their Pathology and Treatment: with especial Reference to the Application of Nitric Acid; with a Chapter on the Painful Ulcer of the Rectum.* By HENRY SMITH, F.R.C.S., Assistant-Surgeon to King's College Hospital. Third Edition. 1862. pp. 141.

THE short chapter on "Painful Ulcer of the Rectum" did not appear in the previous edition. The statements throughout the greater part of this little work are (without containing much that is not well-known) simply and intelligibly presented; and to those not conversant with the subject, afford a good practical account of the origin of the diseases alluded to, and of the mode of treatment best adapted for their relief. We venture, however, to suggest that some at least of the space occupied by the details of cases of piles, &c., might well have been devoted to a few words of diagnosis, often obscure, between some of the rectal diseases noticed and other graver affections; and we think that the few observations, well introduced as they are, respecting the confusion of symptoms from rectal diseases with those of the womb, might advantageously have been extended. As regards local treatment, we think that the use of belladonna or atropine, or of the chloroform and oxide of zinc ointment, as suggested by Mr. Curling, might with benefit have been considered; and we should have been glad to have known what experience the author has had of the india-rubber or metal "plug," which we have found in one or two very intractable and painful cases of internal piles (one in the person of a well-known London physician) to answer remarkably well.

At page 13 the author takes occasion to "show up" the serious evils arising from physicians performing operations. Now, though we decidedly think that if a physician is not in the habit of performing an operation, he ought to delegate the task to a surgeon who is, yet those who live in glass houses should not throw stones, and we cannot resist the opportunity of stating, that some physicians, at least, are equally as able to point to perhaps still greater evils which have resulted from surgeons taking charge of medical cases.

PART THIRD.

Original Communications.

Art. I.

On Irregularities of the Pulmonary Artery, Arch of the Aorta, and the Primary Branches of the Arch, with an attempt to illustrate their Mode of Origin by a reference to Development. By Wm. Turner, M.B. Lond., F.R.C.S E., F.R.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

Numerous cases of irregularities of the pulmonary artery, arch of the aorta and its primary branches, have been recorded in the periodical scientific literature of this and other countries. Many of these have been collected together in the pathological works of Meckel, Otto, and others, and in some of the treatises on descriptive anatomy, more particularly those of Tiedemann and Quain. The description of these irregularities has been almost exclusively confined to an account of their position, course, and relations, with, in occasional instances, some practical and physiological conclusions which might be deduced from them. A classification also has to some extent been adopted, founded upon peculiarities in their anatomical arrangement. Here and there isolated attempts have been made to explain the deviations of the vessels from their normal course by a reference to their mode of development, but as yet no systematic account has been offered of these irregularities from the developmental point of view. Before a satisfactory exposition of this aspect of the subject can be afforded, it is necessary that the development of the vascular arches in the early embryo should be carefully studied, for it is only by the examination of the arrangement of the vessels in their initiatory stages that the clue to the irregular development can be obtained. Until within a comparatively recent period, vascular embryology has been in a somewhat confused and imperfect state, so that difficulties have arisen in the way of reconciling the descriptions of embryologists with many well-ascertained facts in the anatomy of the vessels. By the early laborious researches of Von Baer, Burdach, Allen Thomson, and Bischoff, we have been made acquainted with the general plan of development of the great vessels proceeding from the base of the heart. It is, however, to the more recent investigations of Rathke (who has supplemented certain points in the descriptions of those who preceded him, respecting which doubts were entertained) that we are indebted for the views now most generally accepted upon this important subject.

It is proposed, in the first place, to give a sketch of the present state of our knowledge of the mode of development of the arch and the chief vessels connected with it. For this purpose I shall take as my guide the facts and opinions laid down by Rathke, first in Müller's 'Archivs' (1843, p. 276 *et seq.*), and subsequently expanded in his recent memoir, entitled, 'Untersuchungen über die Aortenwurzeln der Saurier,' Wien, 1857.

In the four divisions of the vertebrate kingdom, at an early period of embryonic life the vascular system consists of the following parts: a short trunk, proceeding from the heart (truncus communis arteriosus); of two longer trunks situated in the visceral chamber, under the vertebral column, which run parallel to each other, and subsequently converge, so as to coalesce and form a single vessel (trunk of the aorta). Passing between the truncus communis and the trunk of the aorta, on each side are a series of arch-shaped vessels (vascular arches), which are situated in the wall of the visceral chamber. These arches consist of five pairs, though it is very doubtful whether they all exist at the same time. It is by the junction of these different arches with each other, under the vertebral column, that the parallel vessels which subsequently unite to form the trunk of the aorta arise. The trunk of the aorta is thus to be regarded as a secondary vessel, for which the parallel trunks serve as two especial roots (primitive aortic roots).

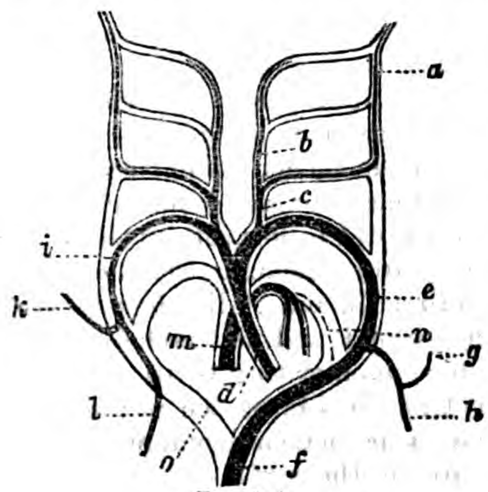

Fig. 1.*

The mode of development from this simple typical arrangement into the condition met with in the fully formed animal is due to metamorphoses taking place in these vessels in certain given directions. Some of the changes are progressive, and produce an increase in the size and importance of certain of the vascular arches, and the trunks to and from which they proceed. Others, again, are retrogressive; the development and growth of the vessels either not being commensurate with the development and growth of the individual, or atrophy to a greater or less extent takes places, so that portions of them disappear.

* Scheme of the normal mode of development of the arch and great vessels in the mammal (after Rathke):—*a*, internal carotid; *b*, external carotid; *c*, common carotid; *d*, ascending aorta; *e*, arch of the aorta; *f*, descending aorta; *g* and *h*, left vertebral and subclavian arteries; *i* and *l*, right subclavian; *k*, right vertebral; *m*, pulmonary artery; *n*, ductus arteriosus; *o*, right aortic root. The non-shaded parts indicate the vessels which in the course of development disappear.

In the vertebrata above the batrachia the truncus communis splits up longitudinally either into two or three canals. In mammals and birds the first-named arrangement prevails. One of these canals is continued into the two vascular arches of the fourth pair. In the course of development one or other of these arches—in the bird the right, in the mammal the left—together with the canal with which it is continuous, becomes developed into the arch and ascending part of the aorta, whilst the corresponding arch on the opposite side to a great extent disappears, that portion of it which remains becoming modified into a branch of the aorta. The other canal into which the truncus communis divides, is continued into the fifth pair of vascular arches. In the bird, each of these arches sends a branch to the lung of its own side, which branches, together with the canal from which they proceed, become developed into the pulmonary arteries. In the mammal only the fifth left vascular arch gives off a branch, which, dividing dichotomously, supplies the pulmonary vessel for each lung. The fifth right arch, and the remaining part of the fifth left arch, which does not go to form the pulmonary artery, constitute the Botallian or arterial ducts, which form the anastomoses between the fourth and fifth arches. These in the course of time either altogether disappear, or persist merely as fibrous cords. In the reptile the truncus communis divides into three canals. One of these becomes continuous with the fifth pair of vascular arches—i.e., with the pulmonary arches. The other two are prolonged into the fourth pair of vascular arches, one on each side, which increase greatly in size, and form the ascending parts and arches of the aorta. They ultimately unite to form the descending aorta.

The carotid arteries in the higher vertebrata follow in their development a definite plan. The common carotid on each side divides into an external and internal carotid branch. Each common carotid is a development of the ventral communicating vessel between the fourth and third arches. The external carotid is formed by the ventral communicating vessels between the third, second, and first arches. The internal carotid consists of the third arch, and of the dorsal communicating vessels between it and the second and first arches. The first and second arches, and the dorsal communicating vessel between the third and fourth, disappear, except in the lizards, where the last-named vessel persists, so as to give a permanent communication between the internal carotid and the arch of the aorta.

The subclavian arteries present, not only in the different divisions of the higher vertebrata, but also on opposite sides of the same animal, greater diversities in their development than are seen in the carotid arteries. The right subclavian in the chick (and it is probable that this observation may be extended to birds generally) grows out laterally from the side of the anastomosis between the fourth and fifth right vascular arches, which anastomosis later on represents a portion of the arch of the aorta. That part of the aortic arch which exists between the place of origin of this branch and the common carotid of the same side, increases no more in length, but, on the contrary, shortens itself, until finally the two arteries come together at their origin, so that a common trunk, an innominate artery, is formed. The left subclavian most probably arises as a lateral twig from the anastomosis between the fourth and fifth left vascular arches, which twig, gradually widening out, becomes blended at its base with the left common carotid, so as to form the left innominate artery. In the mammal, the left subclavian artery corresponds in its development to the right artery in the bird—i.e., it arises as a lateral twig, whilst the right subclavian proceeds from, and to some extent forms a part of, the fourth vascular arch itself. It is probable that those portions of the right subclavian which in descriptive works are termed its first and second parts, are really portions of this arch; whilst the third part, with the axillary, is the lateral twig. In man, and those mammals in which a right innominate artery exists, this vessel is formed by the commencement of the fourth right vascular arch, which persists, and from which the right carotid and subclavian arteries appear to proceed as branches.

By the aid of the above necessarily condensed description of the normal mode of development of the great arteries, I pass, in the next place, to the consideration of the various irregularities to which these vessels are liable. As the variations from the normal arrangement present so many diverse forms, it will be necessary to classify them under different heads. I propose, then, to divide them into three great groups. It must be understood, however, that it does not necessarily follow that any given case which we may examine is exclusively confined to one of these groups; for, as we shall hereafter see, it is not at all unusual for the same case to illustrate irregularities in more than one.

The division I propose is as follows—

1st. Irregularities in the development of the truncus communis arteriosus, generally accompanied by greater or less imperfection of the auricular and ventricular septa.

2nd. Irregularities in the development of the fourth and fifth pairs of vascular arches, and of the right and left aortic roots.

3rd. Irregularities in the development of the branches of the fourth pair of vascular arches.

First Group.—This group constitutes a very important series of vascular irregularities, inasmuch as the cases composing it are generally accompanied by deficiencies in the structure of the heart itself. It exhibits in a striking manner the effects that may be produced by a diminution in the normal formative power, through an obstruction to the proper progressive metamorphosis. The cases presenting it consequently possess at the time of birth an arrangement of parts such as in a normally-developed fœtus exists only at a comparatively early period of intra-uterine life.

This group is capable of being subdivided as follows:

A. Single heart. Aorta and pulmonary artery arising from a common trunk.

B. Absence or imperfection of ventricular septum. Auricular septum entire or perforated. Aorta and pulmonary artery more or less communicating with each other.

C. Transposition of the aorta and pulmonary artery.

Subdivision A.—The cases which fall under this subdivision present the simplest form of heart and arteries that has been met with in the human fœtus at the time of birth. So simple is the heart, that it represents in man a condition almost identical with the perfect structure in the fish, and a distribution of vessels similar to that met with in the Batrachians. Nay more, as Meckel* has pointed out, in certain of these cases the position of the heart corresponded to that seen in many reptiles, for, owing to a partial defect of the diaphragm, it was situated in a depression on the liver. Illustrative cases have been recorded by Wilson,† Farre,‡ Forster,§ Ch. Bernard,‖ and probably Martin St. Ange.¶ In the cases described by the three first-named observers, the venæ cavæ opened into the single auricle, but the termination of the pulmonary veins was not so constant, for in Forster's case they opened into the auricle, in Farre's into the appendix, in Wilson's into the superior vena cava. In all the cases, but one auricle, one ventricle, one auriculo-ventricular opening existed. The single ventricle gave origin at its base to a common trunk. In Wilson's case this trunk, after a short course, divided into two, pulmonary artery and aorta; the former almost immediately separating into two branches, one for each lung; the latter forming an arch in the usual manner, from which vessels proceeded to the head and upper limbs. In Farre's and Forster's cases, the common trunk did not primarily divide into an aorta and pulmonary artery, but sent off directly from its posterior part two branches, one for each lung, and then continued onwards, so as to form the arch and descending aorta. Two cases, agreeing with the above in possessing but a single auricle and ventricle, though differing from them in the condition of the two arterial trunks, which in them were distinct vessels, have been recorded by Breschet.** They will be more particularly described in Group II., Subdivisions C. and H.

In the above cases, cessation of the normal development of the parts must have occurred very early in fœtal life. Embryologists have shown that the division of the common arterial trunk into the aorta and pulmonary artery takes place in man between the fifth and eighth week. In Wilson's case, this division had to some extent been effected, so that it presented a stage slightly more advanced than the cases either of Farre or Forster. Breschet's cases in this respect were even more complete, for the aorta and pulmonary arteries were distinct

* Handbuch der Path. Anat., vol. i. p. 417.

† Philosophical Transactions. 1798.

‡ Pathological Researches, p. 2, fig. 1.

§ Transactions of the Pathological Society, vol. i. p. 48.

‖ L'Union Médicale, March, 1860; British and Foreign Medico-Chirurgical Review, July, 1860.

¶ Bulletin de la Société Anatomique, 1826. This case I have not been able to refer to.

** Mémoire sur l'Ectopie du Cœur: Répert. Général., tom. ii. p. 7.

vessels. The commencement of the division of the ventricular part of the heart into two cavities by the formation of a septum is also of very early occurrence. Ecker* has seen, in an embryo about six weeks old, the septum presenting the appearance of a crescentic fold; and Kölliker† figures in a heart from an embryo about four weeks old, a rudimentary septum. This structure is completed at the end of the seventh week. The formation of the auricular septum begins, after the completion of the ventricular and arterial septa, in the eighth week of embryonic life, and it is not completed until after birth by the closure of the foramen ovale.

Subdivision B.—The cases which fall under this head are much more numerous than those which have been described under the former one. This is what might be expected when we bear in mind that they represent a higher form of development, and consequently, one more nearly approaching to the normal condition. They possess many analogies in structure, especially in the imperfect division of the ventricle into two cavities, to the completed development of that part of the heart in the higher reptiles—tortoises, lizards, and serpents. In the various scientific and medical journals, and more especially in the 'Transactions' of the Pathological Society of London, numerous illustrative examples may be found. The most simple form which has been met with is the case described by Standert,‡ where the ventricle was single, not a trace of the septum existing, and where the normal division of the auricles into two chambers was simply indicated by the presence of two appendages, and by a muscular band in the position of the septum. A single artery arose from the ventricle. Cases have also been recorded by Tiedemann,§ Lawrence,‖ Clark and Owen,¶ Crisp,** Hale,†† and Vernon,‡‡ in which a simple undivided ventricle existed. In other respects, however, these cases presented considerable diversities. In those of Lawrence and Crisp, the auricular division was very imperfect, and but a single auriculo-ventricular opening was found. In Clark's and Owen's case, and in that of Dr. Vernon, whilst but a single auriculo-ventricular orifice existed, the division into two auricles was more strongly marked. In both, the right auricle was the one which communicated directly with the single ventricle, the left opening into the right through the foramen ovale. In Hale's case, there were evidently two distinct auriculo-ventricular orifices, for both a mitral and tricuspid valve are described. From the account given by Tiedemann of his case, a boy, aged eleven years, one would be led to suppose that the auricular septum was entire.

The condition of the great arteries also varied in these cases. In those of Clark and Owen, Crisp and Vernon, the division of the truncus

* Icones Phys., Taf. 30, figs. 22, 23.
† Kölliker's Entwicklungs-geschichte, p. 403. Leipzig, 1861.
‡ Philosophical Transactions, 1805, p. 228.
§ Zoologie, Band i. S. 177.
‖ Farre's Pathological Researches, p. 30.
¶ Lancet, 1848, vol. ii. p. 664.
** Transactions of the Pathological Society, vol. i. p. 49. †† Ibid., vol. iv.
‡‡ Transactions of the Medico-Chirurgical Society, vol. xxxix. 1856.

communis had not taken place, so that the pulmonary arteries arose from it as separate branches. On the other hand, in those of Tiedemann, Lawrence, and Hale, the division of the common trunk was complete, the aorta and pulmonary artery arising as distinct vessels from the base of the single ventricle.

Imperfection of the auricular septum, through patency of the foramen ovale, unconjoined with any other irregularity of the heart or great vessels, is the commonest malformation to which this organ is liable.

Imperfection of the ventricular septum is also, comparatively speaking, tolerably frequent, and so many cases exhibiting it have been recorded, that it is almost needless to particularize individual examples.

The imperfection usually consists in a defective formation of the base of the septum, a roundish, oval, or crescentic opening existing at that spot, through which the two ventricles communicate with each other. It is generally found that, along with this defect, the orifice of the aorta is situated partly over the left and partly over the right ventricle, so that both chambers open into it. This is simply one of the results of imperfect development of the basal portion of the septum. A reference to Ecker's 'Icones Physiologicæ' will show us that at the sixth week, after the commencement of the formation of the ventricular septum, the truncus communis springs from the right ventricle only. As the septum becomes finished, it gradually shuts off that portion of the common trunk which constitutes the aorta from the right chamber. In these imperfectly developed hearts, such an occlusion has not taken place.

As a rule, the aorta and pulmonary artery are quite distinct vessels. A very curious case has, however, been described and figured by Meckel,* in which a very slight imperfection of the ventricular septum was conjoined with partial persistence of the common arterial trunk. A single vessel arose from the base of the ventricles, more from the right than the left; it ascended to the right side, and almost immediately gave off a very short trunk, the pulmonary artery, which divided into two branches, one for each lung. The ventricular septum was deficient merely at its base. A second case, also described and figured by Meckel,† is remarkable in having the aperture in the septum ventriculorum not in the usual place. It was nearer the apex than the base. Although the heart was from a fœtus, yet it was observed that the foramen ovale was closed. There was no communication between the trunk of the pulmonary artery and the ascending aorta. A case presenting the opposite arrangement to the second of Meckel's cases has been described by Dr. Wilks.‡ In it, whilst the ventricular septum was entire, a communication existed between the trunk of the pulmonary artery and the ascending aorta through a deficiency in the arterial septum.

From a consideration of the cases which I have placed under this

* Tab. Anat. Path., Tab. I. Fig. 1; and Handbuch, Band i. S. 430 et seq.

† Ibid., fig. 2; and Ibid., p. 439, et seq.

‡ Transactions of the Pathological Society, vol. xi.

subdivision, we may lay down the two following very important propositions.

1st. When a heart exhibits want of progressive metamorphosis in one direction, it by no means follows that it should also exhibit it in another.

2nd. When an obstruction takes place in the development of a part of the heart very early laid down, it by no means follows that structures subsequently mapped out in it should not be partially or even completely developed.

As illustrations of the first of these propositions, the cases of Tiedemann, Lawrence, and Hale may be cited, in which, whilst there is a complete absence of the ventricular septum, that of the arteries, which in a normally-formed heart dates its rise from the same period as the septum ventriculorum, is complete. These cases are also important, as showing that the arterial septum, although originating at the same period as the ventricular, is developed quite independently, and is not, as has been supposed by some, a mere continuation of it. At the same time, the first case of Meckel, and those of Clark and Owen, Crisp and Vernon, teach us that a deficiency of both the ventricular and arterial septa may co-exist.

The second proposition is illustrated by the whole of the cases quoted. In every instance except that recorded by Wilks, whilst the ventricular septum was altogether absent, or but imperfectly developed, the division of the auricles into two cavities by a septum more or less perfect had been begun. In the second case of Meckel, and probably also in that of Tiedemann, the auricular septum was completely formed. It has already been pointed out that in the normal progressive development of the heart, the formation of the auricular septum commences after the completion of those of the ventricles and common arterial trunk.

Subdivision C.—The cases included under this head may very conveniently be subdivided as follows:

1st. Those in which transposition of the aorta and pulmonary artery is accompanied by a partial or general transposition of the viscera, both thoracic and abdominal.

2nd. Those in which transposition of the two great arteries is accompanied merely by a transposition of the ventricular portion of the heart.

3rd. Those in which the transposition is confined to the two great arteries.

The consideration of the first set of cases may be satisfactorily postponed to the second division of this paper.

Examples of the second set of cases have been recorded by Farre* and Walshe.† In the description given by the latter physician, the apex of the heart is stated to be formed by the right ventricle, from which the aorta sprang, anterior to and overlapping the pulmonary

* Pathological Researches, p. 29, fig. 14.

† Transactions of the Medico-Chirurgical Society, vol. xxv. p. 1.

artery, which arose from the left ventricle. The right ventricle had much thicker walls than the left. The right auriculo-ventricular valve was bicuspid; the left tricuspid; ventricular septum not perforated; foramen ovale in auricular septum open; venæ cavæ opened into right auricle; pulmonary veins into left. In both cases the ductus arteriosus was pervious, though its calibre was small. Walshe describes it as arising from the posterior border of the pulmonary artery about an inch and a half above the origin of that vessel, and opening into the aorta exactly opposite the origin of the left subclavian. In this case, then, the aorta arose from its own ventricle—i.e., from the one with the thickest walls and with a bicuspid valve; but this ventricle, instead of being situated on the left, formed the right side of the heart. The ventricles, therefore, were transposed along with the arteries. The auricles, however, occupied their proper position, for the venæ cavæ opened into the right, the pulmonary veins into the left.*

As examples of the third set of cases may be quoted those recorded by Baillie,† Langstaff,‡ and Peacock.§ In them the transposition appeared to have affected the great arteries merely. In Baillie's case, a child, aged two months, the ductus arteriosus joined the aorta a little beyond the subclavian. It was sufficiently open to admit a crow's quill. The foramen ovale was a little more closed than in a new-born child. The heart was of the common size for a child two months old, and except for the circumstances which have been stated, had nothing remarkable in its structure.

We must look for an explanation of the causes which produce this very curious transposition of the heart and great vessels, to changes taking place early in embryonic life. It is to the researches of Von Baer‖ that we are especially indebted for an account of the process which tends to produce it. That acute embryologist has shown that, in the first stage of development, the embryo lies with its ventral aspect on the upper surface of the membranes of the yolk. At the commencement of the second stage—i.e., when the head and thorax are differentiated from the posterior part of the embryo, a rotation of the embryo takes place, so that it lies with its left side in relation to the yolk, from which side the vessels pass to the vitelline membranes. It is this rotation of the embryo to the left which determines the normal inclination of the heart to the left side. During the examination of several hundreds of chicks, Von Baer has only seen one in which the right side of the embryo was completely turned to the vitellus, and in this the heart was transposed to the right side. Allen Thomson has also seen¶ on one occasion an embryo lying on its right side, and in

* H. Meyer (Virchow's Archiv, 1857, vol. xii. p. 364) has recorded a case of a corresponding nature. In his paper may be found references to many other cases of transposition.

† Engravings of Morbid Anatomy, pl. vi. fasc. 1.

‡ London Medical Review, vol. iv.

§ Transactions of the Pathological Society, vol. vi.

‖ Entwicklungs-geschichte. Erste Abtheilung.

¶ Glasgow Medical Journal. July, 1853.

this case the position and form of the heart and vessels connected with it were quite inverted. It would appear, therefore, that the rotation of the embryo to the right instead of to the left determines the transposition of the heart to the right side.

Second Group.—The original symmetrical development of the vascular arches on the two sides of the body during the normal progressive changes becomes greatly modified, and ultimately constitutes the unsymmetrical arrangement with which we are familiar in the fully-developed man. Cases, however, occasionally arise, in which, through some cause, inflammatory or otherwise, operating very early in embryonic life, an atrophy, or retrogressive metamorphosis, occurs in the normal direction; as a consequence of which, a progressive metamorphosis takes place in the opposite. Thus arches, or parts of arches, and aortic roots, which as a rule altogether disappear, may, through the arrested development of their fellows on the opposite side, persist, take their place, and perform their functions. Some few cases, indeed, have been recorded in which certain of the arches on the two sides persisted throughout life, and preserved their original symmetrical arrangement.

So various are the forms which changes in the normal development of the fourth and fifth pairs of vascular arches, and the right and left aortic roots, admit of, that it will be necessary, in order properly to consider them all, to separate this group into several subdivisions. I propose the following:

A. Persistence of the fourth pair of vascular arches, and of both aortic roots.

B. Persistence of both aortic roots, and of the fourth right vascular arch. Atrophy of the fourth left arch.

C. Persistence of fourth right vascular arch and right aortic root. Atrophy of left aortic root, and partially of fourth left arch.

D. Atrophy of fourth left vascular arch between the origins of the common carotid and subclavian arteries. Persistence of the fifth left arch and left aortic root.

E. Atrophy of fourth left vascular arch beyond the origin of the left subclavian. Persistence of fifth left arch and left aortic root.

F. Tran-ference of the left subclavian artery at its origin from the fourth to the fifth left vascular arch.

G. Atrophy of fourth right vascular arch. Persistence of fourth left arch and right and left aortic roots.

H. Persistence of the fifth pair of vascular arches.

I. Atrophy of the fifth pair of vascular arches.

K. Persistence of the primitive aortic septum.

Subdivision A.—In the two highest classes of the vertebrate kingdom—birds and mammals—the whole of one aortic root (right or left, as the case may be), and a part of one fourth vascular arch, disappear in the progress of development. In the class Reptilia, on the other hand, the fourth vascular arches on the two sides, as well as both the aortic roots, persist throughout life. Three or four cases have now been recorded in the human subject in which the reptilian arrangement was to a certain extent preserved. The best known of these cases are those which were observed by Hommel* and Malacarne,† and which have since been described and figured by Meckel,‡ Tiedemann,§ and Quain.|| In both these cases the aorta arose from the base of the heart by a single trunk, then divided into two branches, which after a short course reunited to form the descending aorta. Through the space situated between the bifurcation and reunion of these branches the trachea and œsophagus passed. Quain¶ also refers to another case recorded by Zagorsky, in which it is probable that a similar arrangement existed. I have met with a case of a like description alluded to by Bertin in his work on the 'Maladies of the Heart.** He states that J. E. Bertin reports, in his manuscripts upon angiology, that he had found the cross of the aorta double in a child ten or twelve years old. He describes the division of the aorta into two branches, and its reunion to form the descending aorta, as almost similar to the two arms of a river, which flow together after having formed an island.

In these cases the anterior of the two divisions of the aorta is the true left aorta, for it is situated in front of the trachea and œsophagus, and from it the left carotid and subclavian arteries arise. In Hommel's case, the aorta did not divide until it had extended two inches from the base of the heart. In Malacarne's, the place of division was only 3⅓ lines from the heart. These cases correspond more closely in their arrangement with the Chelonian than with any other family of reptiles. In the tortoises the two aortæ are intimately connected together for a short distance after they arise from the heart, before they diverge to their respective sides. These cases of double aorta are especially interesting, because they illustrate the mode of formation of the descending aorta. They show that it is a secondary vessel, formed by the junction of the two aortic roots. They represent in the fully formed subject a very early stage of vascular development. It is interesting also to notice the position of the trachea and œsophagus in these cases. In Hommel's and Malacarne's, in which this is carefully recorded, they pass, the former in front of the latter, through the interspace between the two arches. There can be little doubt that they had the same disposition in Bertin's case. This is exactly the position which these tubes occupy in the early period of development, a position which, owing to the persistence of both fourth arches and both aortic roots, remained permanent throughout the lives of these individuals.

Fig. 2.††

* Commercium literarium, Hebdom. 21. Norimbergæ, 1737.

† Delle osservazioni in chirurgia, Parte 2, 119.

‡ Tab. Anat. Path.; Handbuch.

§ Plates.

|| Arteries, p. 22, plate 5, figs. 8, 9, 10.

¶ Idem. note to p. 161.

** Maladies des Cœur, p. 433.

†† Scheme of the mode of formation of a double arch of the aorta through persistence of both fourth vascular arches.

Bearing a close relation to the above cases, is one which was met with by Prof. Allen Thomson, a note of which, with permission to record it, has been kindly granted by him:

"The College, Glasgow, 22nd Jan., 1862.

"MY DEAR SIR,—The case of right aortic arch, of which you lately saw the preparation in my collection, occurred in the body of an adult which was examined in my dissecting-room in 1857. I look upon it as possessing peculiar interest, because of its constituting a link which seemed to be wanting among the cases already recorded with sufficient accuracy, to complete the series of varieties in the form and position of the aortic arch and larger vessels arising from it, which are related to the condition of this part of the aorta in early fœtal life. I intend to publish an account of this dissection; but as you are at present writing upon the subject of the varieties of the aortic arch, I am happy to comply with your request, and communicate to you the chief facts which I have ascertained in this case.

"The principal abnormal condition consists in the aortic arch being situated to the right side of the trachea and gullet, and in the existence at the same time of an imperfect structure which, by its union with a left innominate artery, occupies the place of the left or normal arch. There is no transposition of the heart or other viscera. The aortic arch thus passing on the right of the trachea and gullet, after ascending to the level of the second dorsal vertebra, suddenly bends backwards on itself with a sharp angle, and descends in the thorax in front of the vertebral column, placed somewhat more to the right than usual, and presenting a long and slight convexity towards the right side till it reaches the eighth or ninth dorsal vertebra, when it regains its usual position.

"Viewed from the front, the appearance of the arch differs from that which is normal mainly in its transposition to the right side, and in the change of the order in which the three large vessels take their origin from it. The first of these vessels, which springs from the arch nearer the heart than usual, is an innominate or brachiocephalic trunk dividing into *left* carotid and *left* subclavian arteries; the second is the *right* carotid, and the third is the right subclavian artery. From the left side of the preparation, however, it is seen that opposite to the third and fourth dorsal vertebræ the upper part of the descending aorta, or back part of the arch, forms a considerable bulging or dilatation towards the left; and that this dilated portion, passing behind the trachea and gullet to the left, comes forward in a more pointed form till it meets the extremity of the impervious cord of the ductus arteriosus, with which it is united. Upon the upper side of this bulging part of the aorta there is also a flat fibrous band of nearly half an inch in length, equally impervious, by which the dilatation of the aorta and the ductus arteriosus is closely and firmly united to the left subclavian artery, at a short distance from its origin from the brachio-cephalic trunk.

"The left pneumogastric nerve, descending to the left of the aortic arch, gives off its recurrent branch on arriving at the bulging part of the aorta; and this latter nerve winds round the ductus arteriosus as usual, and in this case passes also behind the band uniting the aorta and subclavian artery, to gain its usual place in the groove between the trachea and gullet, whence it ascends to the larynx. On the right side, the pneumogastric nerve, passing over the aortic arch, gives off its recurrent branch below it; and this nerve then passing behind the arch, ascends to the larynx in the groove between the trachea and gullet, as on the other side.

"I may farther mention as a peculiarity in this case, the fact, that the thoracic duct, passing up from the lower part of the thorax as usual between the vena azygos and the right side of the aorta, does not in the upper dorsal region cross to the left side behind the aorta, but ascends vertically behind the summit of the aortic arch till it reaches the space between the right subclavian and carotid arteries, whence it rises into the neck on the inside of the vertebral vessels, and then, turning down with a full arch in front of these vessels, it joins the angle of union of the right jugular and subclavian veins.

"I need not here mention farther particulars as to the other vessels and nerves, the most of which are nearly normal.

"From what has now been stated, it appears that in this case the gullet and trachea are closely embraced, in their passage through the upper part of the chest, by a narrow cincture formed of the following parts—viz., the summit of the aortic arch on the right; the bulging part of the back of the aortic arch behind and towards the left; and in front the first part of the left brachio-cephalic trunk, together with the cord of the ductus arteriosus and the band which unites them to the bulging part of the aorta.

"In the majority of the cases of right aortic arch which have been recorded, and in which there was no left arch, the left carotid artery is generally the first vessel rising from the aorta, and the left subclavian artery has been found to proceed from the back part of the arch (or from the left root of the descending aorta of the fœtus), just as happens in those well-known cases in which the right subclavian artery has been observed to proceed behind the gullet and trachea from the back part of the usual left aortic arch.

"The interest of my preparation consists, as you will see, in its forming a transition between the more common cases of right aortic arch in which the left carotid artery generally forms the first branch, or in which more rarely there is a left brachio-cephalic trunk, but in both instances without union to the left part of the aorta; and such cases as that of Hommel, represented in Tiedemann's fourth table and in Quain's fifth plate, fig. 8, in which two aortic arches of nearly equal size closely encircled the trachea and gullet. In the preparation I now describe, the brachio-cephalic trunk and the band uniting it to the bulging part of the aorta represent the left or usual aortic arch reduced to the impervious condition in a part of its course.

"The interesting relation which this bears to the history of the formation of the aorta from the fourth fœtal arch on one or other side, the confirmation it affords of Rathke's view as to the origin of the subclavian arteries from the back part of the arches or aortic roots of the fœtus, and the connexion of all this with the varieties in the position of the recurrent nerve according to the persistence of the aortic arch on the right or left side, and the aberrant origin of the subclavian artery in either case, will no doubt occur to you in studying this subject; as will also the rectifications to be made in the description of such cases as those represented by Quain in Plate VII. figs. 1 & 1*b*, and in Plate VI. fig. 12, and by Tiedemann in Table III. figs. 5 & 6 (from Walther, one being the same as the last quoted from Quain), in which the fact of the aortic arch being on the right side, which I conceive must have been the case, appears not to have been recognised. I refrain, therefore, from extending these remarks to a greater length in this letter.

"I am, dear Sir, very truly yours,

"ALLEN THOMSON.

"To William Turner, Esq., Demonstrator of Anatomy."

Subdivision B.—I have brought together under this head a series of cases which possess, in common, a right instead of a left aorta, and in which the left subclavian artery apparently arises from this right aorta, below the origin of the right subclavian artery. In its course to the left arm it passes between the œsophagus and the spine. Although agreeing in these primary particulars, yet these cases present variations in the condition of the fifth left vascular arch. It will therefore be advisable to consider them under distinct heads, which will be distinguished from each other by different states of the ductus arteriosus.

1st. Cases in which the ductus arteriosus is quite patent.

2nd. Cases in which the ductus arteriosus is atrophied to a mere fibrous cord.

3rd. Cases in which the ductus arteriosus is altogether absent.

Two cases, which illustrate the first of these heads, have now been recorded. In one described by Klinkosch,* the aorta and pulmonary artery had their ordinary position at their origin. The former arched over the right bronchus, and was situated at first to the right of the trachea and œsophagus; the latter passed to the left of those tubes. It is then stated that the canalis arteriosus extended transversely behind the trachea and œsophagus to join the aorta, and that the left subclavian arose from it at some distance from the aorta. Meckel† has recorded the second case, which has been already adverted to in Group 1, Subdivision B, as possessing an aperture in a peculiar position in the septum ventriculorum. The aorta and pulmonary artery had the same arrangement as in the case of Klinkosch, and it is stated that the left subclavian arose, not from the aorta, but from the arterial duct. The preparation in each instance was from a fœtus, so that the patent condition of the ductus arteriosus merely represents the normal state of that tube in the fœtal heart. A case has been recently recorded by Schwegel‡ of a new-born child, in which there was evidently the same arrangement of vessels as in the above cases, but in which the arterial duct is described as already stopped up with a clot of blood. This case must be regarded as presenting the state of transition, from this the first, to the second set of cases included under this subdivision.

Two cases also have been recorded which may be classed under the second head; one by Fiorati and Sandifort,§ the other by Mr. Ewen.‖ In both instances the arrangement of the vessels is noted, but their relation to several of the adjacent structures is not very precisely stated. Two years ago I met with a corresponding case in the dissecting-rooms of the University of Edinburgh; and as I am thus enabled to give from personal observation an account of the very peculiar arrangement of parts in this rare form of irregularity, I purpose in the next place describing it.

Adult male subject.—Aorta arose from base of left ventricle, ascended to right, passed obliquely behind sternum as far as level of second right costal cartilage; it then passed almost directly backwards to the right side of the third dorsal vertebra; it then descended gradually,

* Quain, Arteries, p. 160, pl. 20, fig. 10, 11.

† Handbuch, Band i. S. 435, Band ii. S. 105; Tab. Anat. Path. Fasc. 1, fig. 2.

‡ Ueber einige gefäss-varietaten des menschlichen Körpers: Prag. Vierteljahrsschrift, 1859, S. 121.

§ Museum Anatomicum, vol. i. p. 273, Taf. 107, figs. 1, 2. Quain, pl. 7, fig. 1, 1b.

‖ Guy's Hospital Reports, 1840, p. 233.

inclining inwards, so that when it entered the diaphragmatic opening it was situated on the anterior aspect of the bodies of the vertebræ. Thus the aorta arched over the root of the right lung. Its branches arose in the following order: Left common carotid, right common carotid, right subclavian, left subclavian. The three first-named branches passed, without any great departure from their normal course, to their respective distributions. The mode of origin of the left subclavian requires to be more minutely inquired into.

Proceeding from the left side of the descending part of the arch was a short, thick, pouch-like trunk, having a diameter almost equal to that of the common iliac artery. It was about half an inch in length, and it passed transversely to the left side, on a level with the anterior aspect of the disc between the third and fourth dorsal vertebræ, and immediately behind the trachea and œsophagus, the last-named tube being in contact with its anterior surface. When it had reached the left margin of the œsophagus, it joined the obliterated ductus arteriosus. At the point of junction, the left subclavian artery arose, and then ascended to the inner margin of the left scalenus anticus. From the same pouch-like trunk a small bronchial artery for the left lung proceeded.

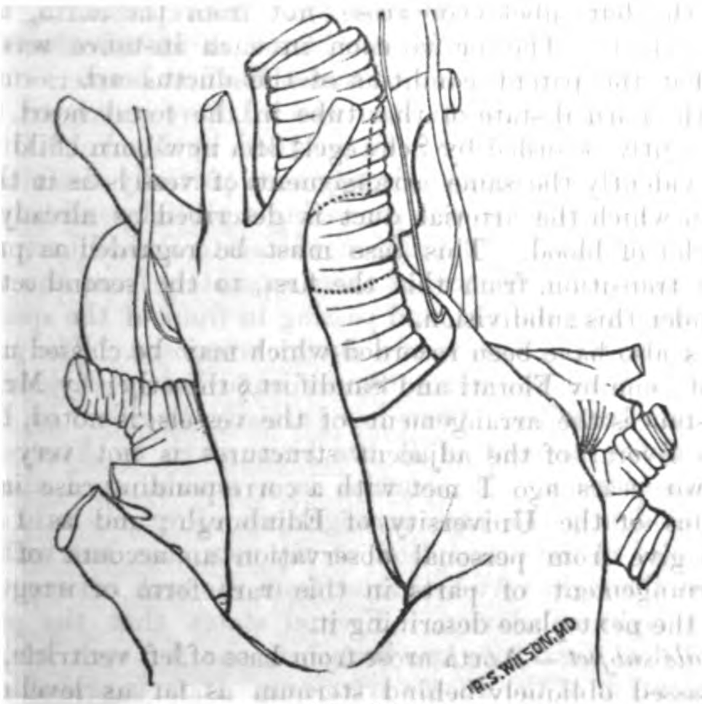

Fig. 3.*

The pulmonary artery occupied its usual position on the left of the ascending aorta, and then divided into its right and left branches. The ductus arteriosus was thick and strong, but quite impervious. It

* Drawing of case in which the right aorta gave off the left subclavian from a pouch-like trunk situated behind the trachea and œsophagus. This short trunk is indicated by the dotted outline.

ascended for about an inch, and joined on the left of the œsophagus the short pouch-like trunk already described. The venæ cavæ, vena azygos, and venæ innominatæ normal.

The left vagus descended in front of the left subclavian, and then passed downwards behind the root of the left lung. When on a line with the attachment of the ductus arteriosus to the short trunk, it gave off its recurrent branch, which hooking round the extremity of the obliterated duct, ascended between the trachea and œsophagus to the larynx. The right vagus crossed over the arch of the aorta, and then gave off its recurrent branch, which passed behind that vessel to reach its normal position between the trachea and œsophagus. It may be interesting also to observe that the spinal column had its convexity in the upper part of the thorax, turned to the right, a fact which disproves the assertion made by some anatomists, that the lateral curve in this region is due to the force exercised by the pulsations of the aorta. This direction of the curve to the right had already been observed by Otto in a case of right aorta.

The fibrous, cord-like state of the ductus arteriosus met with not only in my case, but in those of Ewen and Fiorati and Sandifort, must simply be regarded as a result of the changes which take place in this vessel during extra-uterine life. As representing an adult condition, it is therefore to be considered as an advance upon the patent state of the vessel described under the first head.

Belonging to the third head, into which I have separated this subdivision, are cases which have been recorded by Meckel,* Otto,† and Peacock.‡ Meckel's case has already been partially described under Group 1, Subdivision B, as one possessing a partial persistence of the common arterial trunk. In addition, the aorta arched over the right bronchus. The four large trunks arose separately from the arch, the left subclavian being the last, and passing in front of the spine to the left side. There was no trace of a ductus arteriosus. The pulmonary artery derived from the aorta almost immediately divided into its right and left branches. Otto has partially described his case himself, but a fuller account of it has been given by Frenzel§ in his inaugural dissertation. It occurred in a female, aged about forty. There was evidently the same arrangement of vessels in it as I have already detailed in the description of my own case, but with this exception, that the right vertebral arose from the arch between the right carotid and right subclavian. Frenzel states that the pulmonary artery divided as usual into two branches, one for each lung, but he makes no mention of the ductus arteriosus. If, as is probable, judging from the accompanying figure, the duct was absent, we are justified in placing this case along with Meckel's and Peacock's. If, on the other hand, the duct were present, the case should then be classed along with Fiorati and Sandifort's, Ewen's and my own. In Dr. Peacock's case the aorta arched over the right bronchus, and gave off

* Handbuch, Band i. S. 435. † Seltene Beobacht. ii. S. 61.
‡ Transactions of the Pathological Society, vol. xi.
§ De ramis ex arcu aortæ insolite prodeuntibus. Wratislaviæ, 1835.

the four trunks in the order already described. The pulmonary artery passed upwards and to the left, and divided into its two branches. It is especially stated that there was no ductus arteriosus.

The absence of the ductus arteriosus in these cases is a very singular and important fact. It is hardly to be placed in the same category as the shrivelling up of the duct into a fibrous cord, which takes place after birth, and which is apparently due simply to disuse of the vessel. It is rather to be regarded as a congenital malformation, and as produced at a comparatively early period of intra-uterine life. Meckel's case affords a powerful argument in favour of such an opinion, for the heart was that of a fœtus, in which the duct ought to have been present as an open canal. Peacock's case also was only eleven months and a half old; Otto's case, in which the absence of the duct is not distinctly stated, being the only one in which the adult period of life had been reached. There is no evidence that in any of the cases falling under this subdivision, were the viscera transposed.

The points of greatest interest in these cases are:

1st. The arching of the aorta over the right bronchus.

2nd. The absence of an innominate artery.

3rd. The peculiar origin of the left subclavian; and finally, in the first and second set of cases, the existence of a vascular ring around the trachea and œsophagus.

(To be concluded in our next.)

ART. II.

Experiments and Observations on the Structure and Function of the Stomach in the Vertebral Class. By WILLIAM BRINTON, M.D., Lecturer on Physiology in St. Thomas's Hospital.

THE share taken by the stomach in the function of Digestion has attracted so much attention during many years past, that, in publishing* some of those conclusions on this subject to which I have been led by long, though interrupted, research on the Anatomy and Physiology of the alimentary canal, I am compelled to forego any summary of the current opinions which my conclusions would especially modify; the more so, that these opinions are themselves far too numerous and contradictory to sustain the necessary abridgment.

I may premise that, some years ago, Dr. Kölliker† showed that the tubes occupying a considerable portion of the stomach in the Dog were essentially dimorphous in structure, containing, within an external layer of oval "glandular" cells which fills their lower end, a delicate (and necessarily much narrower) layer of flattish, small, polygonal cells, forming a minutely tesselated single layer. Shortly after,‡ I found that this description was also applicable to the stomach of the Horse, Dog,

* A brief abstract of this paper has appeared in the Proceedings of the Royal Society.

† Mikroscopische Anatomie, Band ii. S. 141.

‡ Compare Cyclopædia of Anatomy, Supplement, article *Stomach*, p. 324.

Cat, Rabbit, Guinea-pig, and Cow, and lastly, to the human stomach; and that, in all, the axial epithelium (as it may be called) gradually merges, as it ascends the tube, into those columnar cells which occupy the summit of the tube on the free surface of the stomach.

This dimorphous structure of the stomach, I am now in a position to affirm, extends without exception throughout the Vertebrate Class. At least of the various Birds, Mammals, Reptiles, and Fishes, accessible to ordinary efforts in this country, I have examined very* numerous *genera* and *species*, without failing to discover it in some part of the stomach.

The chief varieties of this structure in different animals refer almost exclusively to its diffusion in the organ. The gastric organization exemplified by the Ruminants, as well as by some Pachyderms, complicate the true stomach by the addition of parts which, though appended to it, or even continuous with it, are recognised at a glance as utterly devoid of gastric properties. The hard, many-layered, scaly epithelium by which they are lined, and the massy unstriped muscular substance, which many years ago I discovered to be the chief material of their variously produced or folded wall (the *honeycomb*, *manyplies*, *paunch*, &c.), concur with the absence of any specific secretion, and with the complete powerlessness of their acidulated infusion on protein-compounds, to show that all these receptacles are homologous with the œsophagus, of the lower end of which they must be considered as developments. Nor does the circumstance, that they sometimes affect the shape, as well as situation, of the cardiac end of the true stomach—with which (as in the Pig) an œsophageal cardia is sometimes widely continuous—at all imply their possessing any gastric structure or function, or affect the above general statement respecting their physiological import.

* Among which I may enumerate the following:—

MAMMALS.

Equus caballus.
Equus asinus.
Bos taurus.
Cervus elaphus.
Cervus canadensis (Wapiti).
Lepus timidus.
Lepus cuniculus.
Cavia cobaya.
Canis familiaris.
Felis catus.
Mus rattus.
Mus domesticus.
Phocæna communis.
Ovis ammon.
Delphinus delphis.
Phoca vitulina.
Sus scrofa.
Ovis ammon.

BIRDS.

Phasianus gallus.
Phasianus colchicus.
Tetrao tetrix.
Perdix cinerea.
Anser ——.

REPTILES.

Testudo mydas.
Testudo Europœa (?).
Python (boa constrictor).
Coluber natrix.
Vipera berus.
Rana bufo.
Rana esculenta.
Triton cristatus.

FISHES.

Gadus morrhua.
Gadus lota.
Gadus merlangus.
Gadus æglefinus.
Pleuronectes flesus.
Pleuronectes limanda.
Pleuronectes maximus.
Pleuronectes rhombus.
Pleuronectes hippoglossus.
Pleuronectes solea.
Pleuronectes platessa.
Salmo eperlamus.
Salmo salar.
Salmo fario.
Zeus faber.
Raia clavata.
Raia maculata.
Raia oxyrhynchus.
Anguilla vulgaris.
Mugil cephalus.
Scomber vulgaris.
Petromyzon marinus.
Petromyzon fluviatile.
Cyprinus tinca.
Cyprinus carpio.
Cyprinus gobio.
Cyprinus leuciscus.
Mullus barbatus.

With deviations of this kind, however, the various arrangements of the dimorphous structure have no essential connexion. These variations are more frequent and important, and relate to details of gastric structure and function of great interest, both as respects the secretions in general, and the secretion of the stomach in particular; which latter, indeed, invites special attention by its striking chemical properties, its strongly acid reaction, and the wide and energetic metamorphosis it effects in the proteinous ingredients of the food.

In spite of the exceptions already alluded to, the cardiac end of the organ is generally *the* stomach *par excellence* in the sense of its possessing, chiefly or exclusively, tubes occupied by this dimorphous epithelium; which, lining their lower three or four-fifths or thereabouts, is exchanged, above this level, for the ordinary columnar epithelium everywhere found on the general cavity of the organ.

Towards the pyloric end of the organ this arrangement is usually modified in one of two ways.

In many animals—chiefly of the Mammalian class—the columnar cell-growth dips into a continually increasing depth of the gastric tubes, which at the same time are often simplified by the fusion into a common cavity of a variable distance of their upper or free ends. In this manner those of the gastric tubes which immediately adjoin the pylorus often lose all their dimorphous cell-growth, becoming columnar even to their blind ends on the submucous areolar tissue, the total length of the tubes being but little decreased. Of such a condition, the stomach of the Dog affords a well-known illustration.

In other animals—including most of the Fishes and Reptiles—the modification of structure, traceable from the cardia to the pylorus, is more rapid and simple. The decreasing length claimed by the dimorphous cell-growth corresponds with a rapidly diminished length of the whole tube, which, though sometimes also showing, in its large increase of diameter, the fusion of a group of tubes, oftener remains as a series of short cylinders, lined exclusively by columnar epithelium. In short, it is as though the mucous membrane had been gradually thinned by the cutting away of a continually longer segment from those attached or blind ends of the tubes which contain the dimorphous cell-growth, leaving at last nothing more than their open ends, covered by the ordinary columnar cells.

The arrangement in the human subject is curiously contrasted with both of these, the two chief varieties of gastric structure in the Vertebrate series. It is not less singular, too, from the facility with which it is overlooked.

The pyloric end of the human stomach is generally affirmed by anatomists to have a mucous membrane greatly thicker than has the cardiac; and its digestive function, according to many physiologists, repeats this structural superiority. Both statements should be reversed.

It is the extreme difficulty of examining the normal human stomachs which has led to these errors. As seen in ordinary necropsies, after the interval which reverence for human life very properly interposes between death and examination, the stomach, either itself diseased, or at any rate generally sharing in that morbid state of the system which has caused death, and peculiarly liable to all the casualties of those physical processes which precede and inaugurate decomposition in the blood and the tissues, is yet more spoilt for all observation and experiment capable of supplying physiological conclusions as to its state during life and health, by the solution which the gastric juice has itself effected on the tissues which secreted it, and on a variable extent of the adjacent structures. And it is only the rare opportunities afforded by the execution of criminals, or the scarcely more frequent casualties of large Hospital practice, which afford any chance of examining a healthy human stomach before it has undergone any material change after death.

In one or two cases of the kind last hinted at, in which the sudden death, during exceedingly cold weather, of healthy young adults, from such maladies as delirium tremens and epilepsy, has placed at my disposal a stomach perfectly free from disease, the immediate removal of the corpse into a cold room, and the anticipation of the usual period of a necropsy, has enabled me to verify the details on which I base the following statements—

The cardiac and pyloric sacs of the organ are lined by a mucous membrane, the thickness of which, in these two regions respectively, has a proportion nearly as five to two. In other words, the mucous membrane of the cardiac sac is more than twice as thick as that of the pyloric end of the organ. The dimorphous cell-growth, however, extends throughout the whole of the stomach. But inasmuch as the tubes, the length of which precisely corresponds to the thickness of the mucous membrane, are lined in their upper part by columnar epithelium (as indeed are the tubes containing this cell-growth throughout the animal kingdom), the statement, that the absolute length of this columnar lining varies little in any part of the stomach, itself implies the assertion, that the relative length of this lining to the dimorphous cell-structures beneath it is more than twice as great at the pyloric extremity.

In all animals, the dimorphous cell-growth seems the immediate source, and is certainly the exponent, of the specific power of the stomach in digesting proteinous substances. In experiments* on artificial digestion, the solvent power of the gastric mucous membrane, or of any part of it, always varies, other things being equal, with the quantity of the cell-growth casually or specifically present. Hence, in the vast majority of animals, the gastric mucous membrane immediately

* The details of such experiments on artificial gastric digestion are so simple as to require but a brief description. A given weight of the mucous membrane, carefully minced to a pulp, is pounded in a mortar with a given proportion of distilled water. Filtered, and acidulated with hydrochloric acid, its powers are tested by minute equal cubes of hard-boiled white of egg; where desirable, in a brood-machine, which maintains an even and suitable temperature, averaging 102°. Of course, in the comparative experiments, it has been necessary to secure the sameness of every one of these elements of the process. For this reason, the proportions selected have always been one part of mucous membrane, four of water, and fifteen per cent. of a dilute hydrochloric acid, equivalent to three per cent. of the strong acid of the London Pharmacopœia.

adjoining the pylorus is almost devoid of such digestive powers, exhibiting none but a feeble action, plausibly ascribable to a physical transfer or imbibition from the more active neighbouring parts. Hence also, in the human stomach, the difference between the energy of this membrane near the cardia and the pylorus closely corresponds with their relative amounts of this cell-growth, as above noticed, the proportion being more than two to one. Hence, again, that specific process of solution which engages every stomach after death, attacks with disproportionate energy the cardiac end of the organ; which end is usually so thinned and dissolved by this process (partly solvent, partly putrefactive), long before the ordinary period of a necropsy, that it is no wonder the comparative thickness of the mucous membrane in different regions of the organ should have been often misjudged.

The various series of experiments summed up in the above paragraph render it obvious that, of the two constituents—acid and pepsine—long known to be requisite for artificial gastric digestion, one, the pepsinous, is absent from all the dimorphous cell-growth. In other words, while these experiments abundantly indicate that this cell-growth contains pepsine, the well-known fact, that the neutral or faintly acid liquid formed by infusing the stomach in distilled water is impotent for digestive purposes, until supplemented by an acid, proves even more conclusively that it does *not* contain a quantity of acid correlative with its pepsine.

The source of the acid thus seems to be referred to some other texture than the dimorphous cell-growth; and, considering what has been observed by Claude Bernard, and verified by myself,—the acidity of the free surface of the gastric mucous membrane (which is occupied by columnar epithelium), as contrasted with a much less acid, or even neutral, reaction of its deeper parts (corresponding to the dimorphous cell-growth),—it might be questioned whether the two elements of the gastric secretion are not so divided between the two corresponding gastric cell-structures, as that the columnar cells occupying the tops of the tubes near the general cavity of the organ secrete the acid, while the dimorphous cell-growth in the lower ends of the tubes secretes the organic or pepsinous constituent.

Any direct comparison between the degrees of acidity possessed by equal weights of the superficial or columnar cell-growth from the cardiac and pyloric regions respectively of the human stomach, has not hitherto succeeded in my hands. For, as already mentioned, the opportunities for making satisfactory examinations of this kind are very rare and fleeting. While, in ordinary specimens, the greater acidity of the pyloric surface seems sufficiently explained by the circumstance, that the cardia has always lost, by solution and decomposition, some of that superficial layer with which its acid reaction seems to be chiefly, if not exclusively, connected.

In animals, also, research of this kind gives a somewhat variable and equivocal answer. As a rule, the pyloric is decidedly the more acid region of the two. But even in the same species, age, and circumstances of digestion, there is sometimes little difference in this respect.* Argument from analogy affords no more definite results. The rapid conversion of starch into sugar by the salivary glands and the pancreas, is effected by means of a cell-growth, singularly like that tesselated epithelium which lines the axis of the proper gastric tubes, and which thus lies within a kind of hollow cylinder formed by the larger oval cells occupying the lower segments of these tubes. And during their decomposition, both the secretion and watery infusion of the pancreas convert protein-compounds into an unstable and putrefactive substance, which exhibits reactions closely akin to those of peptone.† Hence we may surmise that, as a matter of morphology, it is not unlikely the organic agent of the gastric metamorphosis may be exclusively found in those minute axial cells, which offer so close an anatomical resemblance to the cell-growth which mediates analogous changes in the case of these salivary organs; and that the large oval cells, which are quite unlike any other cell-growth in the animal body, have a function no less specific than their structure—namely, the unloosing of hydrochloric acid‡ from the neutral chlorides of the arterial blood sent to the stomach, and especially from the copious chloride of sodium. Again, remembering the close similarity between all the arrangements of the columnar epithelium found in the stomach, and those of the analogous cell-growth so widely diffused over the whole free surface of the mucous membrane of the bowels; and the curious assumption (so to speak) of duodenal structure, which we find in the pyloric region of the Dog and various other Mammals; we may surmise that the office of this cell-growth is mainly one of *absorption;* which, specially located, for obvious reasons, on the gastric surface, fulfils, to Digestion in the stomach, a relation analogous to that subserved by the same process throughout the intestine. While, inasmuch as there are no indications whatever of any severance of the naturally conjoint offices of the two elements—acid and pepsinous—of the secretion of the stomach, even to the extent of giving these two gastric ingredients independent variations of quantity or intensity in different animals, we are justified in according a provisional preference to any view which would refer their origin to associated, if not identical, structures. In other words, it is not to any structure common to stomach and bowel, like a columnar epithelium, that we must refer the acid of the gastric juice, its most remarkable and essential ingredient. On the contrary, there is an *à priori* presumption that the unique function belongs to the unique structure—namely, the cell-growth of large oval or "glandular" cells. And there is a concurrent (but independent, and therefore multiplied) presumption,

* The contingencies of a mere transfer of gastric juice from the cardiac to the pyloric end of the stomach, and of a subsequent concentration of this secretion by reabsorption of its water, must be remembered in connexion with such differences.

† See the remarks on Pancreatic Digestion at the close of this paper.

‡ As respects the import of the lactic acid sometimes found in the gastric juice, the author would refer to his essay, *Stomach*, in the 'Cyclopædia of Anatomy,' Supplement, p. 324, et passim.

that its secretion is, in some way or other, associated with that of pepsine; in the sense of being furnished by structures which are both co-ordinate and co-extensive. This presumption again refers us to the same element of the dimorphous cell-growth; the other constituent of which, the axial epithelium, closely resembles the essential secretory structure of organs, such as, in the case of the salivary and pancreatic glands, furnish the material agents of analogous, if not identical, metamorphoses.* I cannot think that this view is rendered quite untenable by the greater acidity of the surface of the gastric mucous membrane, as contrasted with its deeper portions; and of the pyloric, as contrasted with the cardiac end of the organ.

Dr. Beaumont's observations† on the visible details of the secretory process in the stomach are, *mutatis mutandis*, exactly what would generally be seen on looking at the surface of the skin with a lens during the commencement of a violent sweat. And though we now know how erroneous, as a matter of anatomy, was the impression he arrived at (that the ducts of the gastric mucous membrane opened on the ridges and points by which its free surface is studded), it need hardly be pointed out that the mere structure of the tubes is a sufficient indication that the bulk of the secretion comes from them; and that, on physical grounds, the gastric juice might well tend to accumulate in drops on the free extremities of the projections of the membrane before spreading as a layer of liquid over the general cavitary surface of the stomach.

On the other hand, the greater acidity of the columnar cell-growth lining the open ends of the tubes, as well as of the secretion found here, by no means implies that this acidity really originates at this exact level of the tubes. The supposition, that the gastric juice consists of the dehiscent cell-growth of the mucous membrane, is one easily refuted on merely quantitative grounds, which would show that even a reconstruction of these cells every minute would scarcely correspond with the amounts of gastric juice sometimes observed to be poured out during active secretion. While, to say nothing of the comparatively large calibre of the tubes near their open ends, there are good reasons for adopting, with respect to the secretory process in the stomach, a view which is certainly suggested by a full consideration of the cir-

* See the remarks respecting the function of the pancreas.

† Dr. Beaumont made use of magnifying glasses, by the aid of which he could distinguish the spheroïdal glandular follicles, and the papillæ situated in their interstices. These papillæ, or *villi*, he found to be scarcely visible until food was applied to the mucous membrane; when they underwent a kind of erection, and protruded from its surface in the shape of small sharp processes. From these, according to this faithful observer, the gastric juice appears to exsude. Its secretion begins by the gradual appearance of innumerable lucid specks, which are smaller than the mucous follicles. These specks or points rise through the transparent mucous coat, and seeming to burst, discharge themselves upon the very points of these vascular papillæ, as a thin, transparent, colourless, limpid, acid fluid, which collects in small drops, trickles down their sides, and spreads over the whole gastric surface. So thoroughly persuaded was Dr. Beaumont (op. cit.) that the fluid exsuded from the papillæ alone, that he had not the least doubt the excretory ducts of the follicles were enclosed in these villi, and terminated in the lucid specks just alluded to, although he admits that he could not see any apertures here.

cumstances of some other secretory organs. That, whatever be its exact details, the preparation of the gastric juice, begun in the blind ends of the tubes, is only completed at or near their opposite or open extremities; that, at any rate, as regards the acid ingredient, the process is far more likely to be the subtraction, from a liquid allied to blood-liquor, of the materials whose removal would leave the secretion, than any mere construction or addition of this ingredient; and that in assuming such a selective or preferential absorption, we are only extending to these structures, with a specific form and intensity, a capacity which we are forced to suppose operating in the case of the urinary secretion,* and even in a lower degree through the whole surface of the alimentary canal;—such are the physiological opinions which a careful study of the anatomy of all these parts would seem to suggest.

Another view, which regards the columnar cell-growth as shielding the stomach by the alkaline mucus which it forms, is even more unwarranted by facts. This cell-growth is invariably acid. And the layer of alkaline mucus, by which during fasting it is covered, can always be distinctly identified as derived from other sources, and, indeed, from other organs: to wit, from the duodenum, the liver, the œsophagus, and especially from the salivary organs opening into the mouth.

That the living stomach, which decomposes the blood sent to it during digestion into acid and alkali, retains this power for a very short time after systemic death, has been shown by the experiments of Bernard. But while I can confirm this statement, I may add, that it is blood only which evokes and undergoes this severance of its salts into acid and base; and that these salts themselves, even though they doubtless yield the hydrochloric acid that streams from the mucous membrane during digestion, are alone quite incapable of bringing about the change. Solutions of chloride of sodium injected into the gastric arteries of a dog in the course of a few seconds after its being pithed, give rise to no secretion of acid, and return unchanged from the veins. Indeed, the salt exerts a specific influence unfavourable to secretion; such a stomach yielding a mucous membrane which, even after careful washing, remains almost impotent for the purposes of artificial digestion.

Another point of great interest is the alleged exhaustion of the pepsinous constituent of the gastric mucous membrane during the process of gastric digestion. I have found that, substantially, no such exhaustion occurs. So far as I can judge from the most careful examination of animals of the same species, age, and size, a given weight of mucous membrane from the same part of the stomach can effect the solution of the same amounts of albumen; whether it be taken

* I cannot forego the mention of this analogy, although the errors of the view commonly adopted as regards the mechanism of the urinary secretion, and the advantages —I would almost say the necessity—of this absorptive and subtractive form of secretion, could only be deduced from a review of details, both physiological and pathological, quite beyond the limits of this paper.

from a stomach at the end of the digestion of a full meal, or from the stomach of an animal which has been excited to keen hunger by long fasting. In one or two instances only have I been able to substantiate any perceptible difference, and even then, in a degree not exceeding that which the other circumstances of the observation would themselves go far to suggest or explain.

The precise effect of age upon the human stomach I have not yet been able to decide. That the gastric mucous membrane sometimes retains a high degree of potency at the very term of human life (at seventy or eighty years of age, and even beyond), I am quite able to affirm. But the *maxima* of albumen dissolved by a given weight of this membrane have all been observed in persons between the ages of twenty-five and thirty-five, and in the male sex.

As regards the various animals examined, it may be laid down as a rule, that differences of any importance almost always apply to different *genera*. It is very rarely that different species of the same genus exhibit any noticeable contrast, either in the anatomy of the stomach, on the one hand, or in the potency of its mucous membrane for the purpose of artificial digestion, on the other.

The contrast of the cardia and pylorus prevalent throughout the normal series, has already been sufficiently insisted on. The greater acidity of the pyloric region, impotent as it is for digestive purposes, is, perhaps, greater in various fish (as in the Salmon tribe) than in most other animals.

The texture of the mucous membrane, and indeed of the stomach in general, offers, in various animals, contrasts which are more or less analogous to those seen in other parts of the body. The clean, dense, highly organized tissues of the intestines in the *Pleuronectidæ*, as contrasted with those of the *Gadidæ*; their rich reddish colour in the *Salmonidæ*, &c. &c., all illustrate this rule: which appears to indicate nothing more than that correlation of organs and structures, traceable throughout the whole of both kingdoms of organized nature.

The potency of the gastric mucous membrane does not seem, as a rule, to increase proportionately to the mere complexity of animal organization, or to what might be vaguely (but intelligibly) termed the height of the animal in the developmental scale. On the contrary, its solvent energy is more closely related to the habits of the animal, carnivorous or otherwise. Of course it by no means follows that the total effective energy of the stomach, as an organ, is exactly proportional to that of its mucous membrane: the mere accumulation of pepsine in which tissue might well be replaced, among higher animals, by a greater activity of the circulation and secretion of the stomach. But it is singular to notice, that the numerical *maximum* of gastric power which I have hitherto obtained has been in the voracious codfish, in whom an *unit* of mucous membrane mediates the solution of about five times its weight of moist albumen, a proportion at least twice as great as any I have hitherto noticed in the human subject.

Another still more unexpected and recondite law is deducible from my observations. It has long been known that, in the human subject, temperature exercises an important influence on the digestive process: that the solvent power of an artificial gastric juice is diminished by cold, increased by heat; falling to what is almost a *zero* at 32° Fahr., and rising to a *maximum* at a little over 100°. It has also been verified, that the injury inflicted by cold is merely temporary, being at once removed by an increase of temperature. Finally, it has been found that, at or near the temperature which coagulates liquid albumen, the energy of the natural and artificial gastric fluids is at once annihilated; while even their less active properties are so changed as to allow of their rapid decomposition or putrefaction.

Experiment soon convinced me that the latter destruction of energy took place at a temperature considerably below the degree generally requisite to coagulate albumen. Of course the details of this process of coagulation—and amongst these, its rapidity and completeness—vary to some extent with the variety of albumen made use of, and especially with the density of the solution itself. But a careful comparison shows that an interval of from 5° to 10° separates that point of the thermometric scale of Fahrenheit at which a solution of albumen, like the serum of the blood, begins to coagulate, and that at which the natural or artificial gastric juice of the same animal (e.g., the dog) loses all its digestive power. For example, the complete destruction of pepsinous energy occurs at 115° to 120°; while, with a heat very slowly increased, the complete precipitation of albumen is deferred to 130° or even 140°.

Again, it is equally certain that cold protracts, rather than checks, the solvent process: so that, although an artificial gastric juice prepared from the human stomach accomplishes the solution of a given weight of albumen with infinitely greater rapidity under the influence of a temperature of 102° to 105°, yet the final result is little altered by leaving a similar specimen at 40° to 60°. The colder liquid requires many days to effect what the hot one brings about in four or five hours. But each ends by dissolving exactly the same proportions. In both, too, the bounds of saturation are approached with disproportionate slowness; the small proportions of albumen last taken up being dissolved with a great (and continually increasing) delay.

On extending my observations to the lower Vertebrata—Fishes and Reptiles—I was struck by the curious contrast often discernible between a stomach evincing the most vigorous solvent powers (as shown by the energetic solution of the creature's prey occupying this organ), and the preparation, from the mucous membrane of the very same stomach, of an artificial juice which was capable of little or no action on albumen. A scanty, slow solution during the experiments at 102°, and a complete arrest of all subsequent digestion, was all that could be observed in experiments made with the same stomachs in which a fish or a frog had been found lying more than half dissolved by the gastric juice of its devourer when first laid open. It really seemed at first sight as though, in these rapacious tyrants of the sea and land, the gastric mucous membrane had scarcely any power at all.

The details, however, just mentioned respecting the Mammalian stomach, soon led me to some inquiries on the influence of temperature, which completely cleared up this discrepancy, and at the same time established a law of great novelty and significance. In this, the most material of functions, whose processes, so to speak, the chemist can imitate with close exactness out of the body, by an artificial digestion scarcely distinguishable from the natural solution; we are thrown back, as it were, upon the mystery of life, and indeed remitted to a definite study of organic chemistry, by the discovery that the very same temperature which is indirectly necessary for the chemical operancy of the pepsine of one animal, is destructive, and indeed utterly annihilative, of that of another.

For example, taking the artificial gastric juices of several animals —the Dog, the Skate, the Cod—as prepared by a perfectly uniform process, we should find that in each of them there was a temperature, the application of which for only five minutes destroyed all digestive power. This temperature, which may be estimated as averaging 125°, 110°, and 90° in these three animals respectively, evidently, in the case of the first of them, approaches very nearly to that heat which coagulates albumen. But, as already pointed out, even here there is a perceptible interval; while, in the two other cases, the temperatures which bring about the destruction of gastric energy, and the coagulation of albumen, are completely independent of each other.

A few degrees—on an average about ten—below this "destructive" temperature, we find a point in which rapidity and amount of solution are, as it were, opposed to each other. The albumen submitted to the artificial gastric juice at this temperature is more rapidly attacked; but a smaller proportion is finally taken up than if the heat had been more moderate.

The conjoint maximum of rapidity and energy seems about ten degrees lower still. Even at this effective temperature, however, the cold-blooded digests less quickly than does the warm-blooded animal; although, at the same absolute temperature, the gastric juice of the latter would act with a disproportionate slowness.

And as a curious illustration of these facts, it may be further noticed, that in most of these animals the very same temperature which exalts the power of the artificial digestive juice upon albumen, injures the power of the same juice if no albumen be present. In one or two instances I have found this injury carried to the degree of a destruction of almost all solvent power, although the application of the same temperature was furthering the digestion of albumen in another portion of the same artificial gastric fluid, to which it was imparting an unusual (if not *maximum*) rapidity of action.

The circumstance that heat damages the future operancy, while it favours the immediate operation of the artificial digestive fluid, has already been noticed in reference to the contrast between the increased rapidity of solution, and the diminished proportion of albumen taken up, under the influence of a temperature a few degrees below that which destroys the power of the fluid. The above effect of heat on the non-digesting solution does but repeat and exaggerate this contrast. And an analogous relation is traceable in the effect of time; which, in periods that vary for the artificial digestive fluids of different animals (and, as a rule, are longer in those of lower organization, so that, among the Vertebrata, the artificial gastric fluid of the Fish best resists this change), completely destroys all its future digestive efficacy, without any change in the appearance, reactions, or analysis of the fluid, even though, during the greater part of the same period, the solution of albumen by other specimens proceeds without interruption.

A careful consideration of these curious facts goes far to suggest that the action of the gastric juice, whatever its exact details, is, in some sense, a transference to the proteinous ingredients of the food of a chemical process already going on in the pepsinous fluid. It is true that we are unable to verify any change detectible by an analysis. But inasmuch as this proposition may be repeated of the metamorphosis of albumen into peptone, it obviously forms no argument against—on the contrary, is almost a presumption in favour of—the identity of those changes which are proved to occur, under circumstances so analogous, in the destruction of the digestive energy of an artificial gastric juice, on the one hand, and in the formation of peptone by such a liquid, on the other. Both changes are protracted by cold, recovered by warmth, rendered quicker but less efficacious by a higher degree of warmth; and, lastly, destroyed for ever by a still higher temperature—a temperature which, again, has a close, though indirect, relation to the coagulation of albumen itself.

What this process is, I have endeavoured to surmise elsewhere,* with arguments to which I would only add here whatever seems deducible from those details which my subsequent observations supply. To the view I have there suggested—that peptone is a hydrate of albumen—I would now add, that pepsine is itself undergoing a similar process to that which forms peptone: in other words, is itself completing a similar hydration.

That water is really added, and in a state which may be regarded as midway between solution, on the one side, and combination, on the other, is suggested by a variety of considerations.

For example, the violent attraction for water evinced by pepsine when dried at a low temperature, is paralleled and confirmed by the attraction shown by the gastric mucous membrane, even when moist. When minced and pounded to a pulp, and rubbed down with distilled water, as in the ordinary course of these experiments, the tenacity with which much of this water is retained—in other words, the extreme slowness and scantiness of the filtration by which the pepsinous solution is collected—is not only remarkable, but quite *sui generis.* In circumstances otherwise identical, the mucous membrane of the cardiac end of the organ will be found to yield to filtration a much smaller quantity of such a solution—or, conversely, to retain much more of the water added to it—than any other part of the

* Cyclopædia of Anatomy, article *Stomach*, Supplement, p. 337.

animal body; the decomposing pancreas being perhaps the only exception.

Again, it may be noticed that pepsine and peptone agree in the circumstance that, when dry, they bear with impunity, as regards their subsequent properties, a heat which far exceeds (140°–150°), what would suffice to destroy their specific characters, supposing them to remain moist. This fact, which has indeed been pointed out by various other observers, specifically indicates that it is at any rate not the water present in the solutions of these substances, as the agent by which they are dissolved, which gifts them with the digestive energy possessed by the one, or the extreme solubility (in the sense of being incapable of precipitation by almost all re-agents) which characterizes the other. Furthermore, inasmuch as the addition of water to a solution of peptone enables it to take up additional quantities of proteinous substances, it may almost be said to impart a degree of the digestive energy of pepsine. While we may certainly assert that all the properties of peptone are so specifically present in pepsine, that the latter substance offers no difference save in the single circumstance of its digestive energy; which, as we have just seen, is rather an enormous difference of degree, than a specific contrast or difference of kind.

As I am only anxious to contribute facts, arranged so as to suggest their own conclusions, I must pass by with a hasty allusion, the light which seems to be thrown by the philosophy of chemistry on such details as those just mentioned. The influence of heat in these observations might be supposed analogous to its effect on chemical affinity generally; or to that somewhat hazardous illustration of this law which is recognizable in the well-known influence of high temperatures in favouring decomposition.* But the early reversal—or rather arrest—of all effect, as we advance up the thermometric scale, sufficiently indicates that, even assuming this law to obtain here, its limits are dictated by other and more specific circumstances; themselves perhaps as recondite as those which reverse the ordinary rule, and impair the influence of heat on the solvent powers of water, in the case of the hydrate of lime.

At any rate, there is much in the action of pepsine to remind us of that condition of water respecting which physiology is beginning to give us something more than the conjectures hitherto at our disposal; and which, for want of a better phrase, I may term *water of organization.* Certainly in pepsine and peptone, as well as, *mutatis mutandis,* in all the structures of the animal body, the phenomena which are the necessary preliminaries of combustion seem to indicate that water is specifically combined with the other ingredients of the mass; and that in a way which, however it may modify chemical reactions, really seems mechanical rather than chemical. Furthermore, I would suggest that it is by virtue of the large and rapid changes worked by comparatively slight differences of heat in the mechanical properties of water—perhaps by modifications in the mechanical (rather than atomic) arrangement, or in the degrees of adhesion between the water and the substances

* Compare the remarks on the Pancreas.

to which it is attached, that those singular effects to which I have called attention are due. On such a view, even the extraordinary influence of various grades of organization in modifying the temperature destructive to pepsine, might receive an explanation in some sense chemical. And certainly the transference of a force, or rather tendency, from pepsine to albumen, would be capable of illustration from those familiar, but recondite phenomena, which, in the case of crystallization and evaporation, often cause one substance to determine for another, which is chemically almost indifferent to itself, all the details of its assumption of the solid or gaseous condition respectively.

Two circumstances may be finally noticed as favourable to this view of the action of pepsine. One is, that there are appearances of such a combination of water with proteinous substances as precedes, as well as survives, downright solution. The solution of albumen by an artificial digestive fluid is significantly contrasted with the solution of salt or sugar by distilled water, in the circumstance that instead of merely dissolving, as do these two substances, the mass of albumen swells and becomes transparent throughout its whole bulk, before undergoing any loss of substance, or any solution of continuity. Nor can it be said that this state is the result of a mere physical imbibition of the cubes of albumen with water. For it is easy to show that even a prolonged soaking of albumen in water quite fails to parallel (or even imitate) this change. Another is that, even when the whole albumen present is completely dissolved to a degree which does not allow the eye to recognise the slightest imperfectness or want of transparency in the solution, yet a small proportion may be recovered or precipitated by the ordinary tests for this substance; even though—as in a gastric solution unsaturated with albumen—a period of a few minutes suffices to remove all amenability whatever to these reactions.

Hence the hydration of albumen would seem to have two stages: one, in which the water attaches itself to the organic substance; the other, in which it assumes relations which are capable of subverting all the reactions ordinarily pertaining to its associated mass of albumen. The former of these two stages not only seems to be the necessary prelude of the latter, but also to be that specific element of the process which is mediated by pepsine; and which, on the grounds above surmised, is destroyed by heat, with the result of destroying all possibility of the subsequent transfer of this molecular change to any proteinous substances with which it may be afterwards brought into contact.*

I may perhaps be allowed to conclude by citing some results, de-

* It is singular to notice the contrast offered by the decomposing infusion (see *infra*) of pancreas to the artificial gastric juice, in the solution of albumen. Under the influence of the pancreas, the cubes of albumen used in such experiments undergo no swelling, and acquire no transparency, but fairly crumble away into the turbid liquid which dissolves them. I scarcely like to attempt to square this fact with the above surmise, or I would ask whether it is not possible that this putrefactive hydration may be a destructive rather than constructive process—a removal of some ingredient of the decomposing albumen leaving a hydrate formed at the expense of its other ingredients, rather than a *quasi* "primary" combination of albumen with the water around it?

duced from researches collateral to those of which I have summed up the chief conclusions, and interesting from their bearing on the function of the stomach, as well as on digestion generally.

1. That capacity of dissolving proteinous substances which has lately been attributed to the pancreas, and which is especially deduced from the artificial digestion of albumen and allied substances by the secretion and infusion of this gland, may be utterly denied. At any rate, as regards the latter liquid, it may be unhesitatingly asserted, that the power is one which, though apparently specific to the pancreas, and capable of forming, from albumen, a substance with reactions akin to those of peptone, is yet, in all its variations and circumstances, an incident of putrefaction, of which it forms an early and fleeting stage. Under proper precautions as to time after death, and temperature prior to the experiment, the watery infusion* of the Mammalian pancreas, at all stages of digestion, remains for some time utterly powerless, at the temperature (103° F.) natural to it *in sitû*. At the same time it deserves notice that the peptonic liquid which it gradually forms from albumen during its own decomposition, cannot be made by the similar infusion of even the salivary glands, which are most allied to the pancreas by their structure and office; and that, while its rapid progress towards an intensely putrid character is strangely contrasted with the stability, for months and years, of a solution of true peptone, there is a curious approximation in the quantity of albumen dissolved by a given weight of pancreas, and a given weight of gastric mucous membrane taken from the middle (for example) of a Dog's stomach, or the pyloric region of a Man's.

As regards the influence of the fresh pancreatic juice obtained from animals in whom the pancreatic duct has been rendered fistulous by the process of Claude Bernard, I have no observations to offer. But it is, I think, significant to point out that the statements of this admirable experimentalist as to the extraordinary and unprecedented liability of this secretion to decomposition exactly tally with what I have carefully verified in the case of the pancreatic infusion; and thus reduce its alleged power in effecting the solution of albumen to the same law. Admitting that such a law no way explains this strange and specific chemical change, I must say that it seems entirely to negative its occurrence in the healthy living animal.

2. Analogous† experiments on the intestinal mucous membrane deny to the secretion of this tissue two powers hitherto ascribed to it—namely, the conversion of albumen into peptone, and of starch into sugar. Of these two metamorphoses, the former is seen in such experiments not to occur at all; and the latter only in that very limited degree, and at that slow rate, which warmth and decomposition together enable almost all the tissues of the animal body to effect. And since—to recal here the physiological rule which I have propounded

* The addition of acids and alkalies has no effect, save such as is explained by their retarding and hastening putrefaction respectively.

† Here, also, acids and alkalies seem devoid of all direct influence on the power of the infusion of the tissue.

elsewhere—since we are entitled to expect from the solutions of all these tissues of the digestive organs an action which (as in the case of the stomach and other organs) shall repeat, with due allowance for inevitable dilution and impurity, that of the fresh secretion; while, on the other hand, the vivisections to which we are sometimes asked to look as the sole basis for our physiological conclusions rarely or never exclude the certainty of admixture of secretions, and even the contingency of their concentration by an absorption of their watery ingredients—I venture to think that the digestive function of the intestinal mucous membrane, and especially of its tubes,* must be regarded as undetermined, save as respects the aid which they may incidentally afford to Digestion by their absorptive and excretory office.

Art. III.

Observations on the Structure and Growth of the Elementary Parts (Cells) of Living Beings. By Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Professor of Physiology, and of General and Morbid Anatomy, in King's College, London, Physician to the Hospital, &c. &c.

In this paper I propose to give a short account of the results of some recent observations on the anatomy of tissues generally, and I shall endeavour to state as briefly as possible some of the conclusions to which I have been led with reference to the mode of their formation and growth. I have examined the textures of the simplest and most complicated organisms, prepared in the same way, in the hope of being able to define more exactly than has hitherto been done the precise seat of the production of new tissue and the removal of the old, and with the view of tracing the changes through which matter passes as pabulum, living structure, and products of decay or disintegration. I shall endeavour to demonstrate generally which is the oldest and which is the youngest part of any given elementary structure, which portion has already passed through various stages of its existence, and which has but just commenced its life.

The discussion will be confined as much as possible to the changes which are going on in each elementary part of a tissue, or, as it might be otherwise expressed, to the action of the "cell." In order to avoid the necessity of entering into the cell theory, and thereby occupying much time in giving definitions, taking exceptions to the terms generally employed, or attempting to alter their meaning, I shall ask permission to describe merely what may be observed in my own microscopical specimens, and draw conclusions from these facts alone. By pursuing any other course, I should necessarily be compelled to consider at length all the views now entertained on this complicated subject, which would terribly tax the patience of the reader.

I propose to employ the term *elementary part* where the word "cell" would ordinarily be used. Thus a particle of epithelium, a spore of

* Commonly, but inexactly, called the follicles of Lieberkühn.

mildew, a cell of a leaf, are elementary parts. In a structure like cartilage, consisting of what are termed *cells* or *nuclei* and intercellular substance, the cell or nucleus, with a certain proportion of the intercellular substance around it, constitutes an elementary part. In perfectly-formed epithelium the elementary parts are easily separated from each other, but in cartilage of all ages, and in epithelium at an early period of its formation, the elementary parts are not separable from each other. In other words, the material of which the so-called "cell-wall" is composed is continuous throughout the tissue. I shall endeavour to show that this "intercellular substance," or matrix of cartilage, tendon, and other tissues, really corresponds to the cell-wall of an epithelial cell, the investing membrane of a spore of mildew, &c. It must be borne in mind that the physical characters of hardness, elasticity, extensibility, &c., of a tissue, depend entirely upon this substance, which is known as cell-wall, matrix, or intercellular substance, and are not in any way due to the inner part of the cell or nucleus.

I shall be able to prove that wherever such structure exists, there is or has been a certain quantity of granular matter forming circular, oval, or stellate masses, which is always soft, and from this substance the matrix, cell-wall, or intercellular substance is formed. This soft granular substance corresponds generally to what is termed the nucleus, or to the nucleus and the soft material immediately surrounding it.

It is scarcely necessary to state that great difficulty is experienced in attempting to apply the theories now generally taught to numerous individual cases, and that a view which seems to account satisfactorily for the changes occurring during the life of one tissue, fails as an explanation of the phenomena observed during the formation of an adjacent and perhaps closely allied texture.

An inquiry like the present could not be prosecuted unless it were possible in the first place to determine the portions in several elementary parts from different tissues, which correspond.

The Corresponding Structure in different Elementary Parts.—By soaking various tissues very soon after death in carmine or in certain other colouring matters and glycerine, with certain precautions, the following points are observed with reference to each elementary part (cell):

1. That the outer portion corresponding to the cell-wall, or to this and the intercellular substance, is not stained at all.

2. That the material termed "cell-contents" is sometimes faintly coloured, sometimes coloured only in part, and sometimes not coloured at all.

3. That the nucleus is always coloured of a very dark red colour.

In certain fibrous structures, the outer perfectly-formed fibrous tissue remains perfectly colourless. Within this is a layer faintly coloured, and within this again is a layer of a darker colour, while the nucleus in the central part is the darkest of all. If, therefore, the elementary part be regarded as a fibre-cell, the *wall* is not coloured, the *cell-*

contents and *nucleus* are coloured, but in this case there is no possibility of defining exactly where the cell-contents cease and the cell-wall begins. One structure gradually shades into the other.

In the ovary of certain insects where ova of various ages exist, the oldest ovum is faintly coloured, and the tint increases up to the most recently-formed ovum, which is of an intense red colour. It can be shown that this result does not depend solely upon the capsules of the youngest ova being thinner and therefore more permeable to the carmine solution than those of the older ones.

Hence it may be stated generally *that the granular matter in the central part of certain elementary parts, though separated by a considerable thickness of firm tissue from the carmine solution, is darkly coloured, while the tissue in actual contact with the red solution is not coloured at all.* In some cases the dark colour gradually shades off towards the outer part, which is quite colourless. The part coloured most intensely is never outside, and the colourless portion is never inside. Of structures differing from each other in age, the youngest is always coloured most deeply.

Starch granules, fat globules, globules of earthy salts, crystals and other substances deposited in elementary parts, are not coloured. By the use of carmine we are enabled to separate every living structure into *matter which becomes coloured* and matter which *does not become coloured*, and we have to account for different gradations of colour which are constant in different parts of certain elementary parts.

This action of carmine is not transient or temporary, but the characters above described are retained by specimens which have been soaked in colourless glycerine for upwards of three years.

In the fat-cell and in the starch-cell at an early stage of development we have an external portion colourless (cell-wall), then granular matter slightly coloured, with oil globules deposited in its substance not coloured at all, and amongst the granular matter at one side, the nucleus tinged of a very dark red. As development advances, the fatty matter increases in quantity, and at last the coloured matter is found between the outer colourless cell-wall and the fat or starch which has accumulated. *Fat, starch, &c., therefore consist of matter formed from germinal matter, and, like the cell wall, do not possess active powers.*

Several very interesting and highly important facts have been ascertained by examining ordinary mildew, in different stages of growth, after it has been prepared with the carmine solution. These have been described in a paper published in vol. ii. of the 'Archives,' p. 179. The following are the main facts demonstrated:—In each spore, as well as in the developed plant, an outer membrane is observed perfectly free from colour. Within this is a quantity of granular matter, in which numerous spherical particles can be discerned well coloured with the carmine solution. Many small and perfectly spherical spores may be seen in different parts of the fluid in which the mildew is examined, and among them several which have much increased in size, some of which are of an oval form, some of them exhibiting a constriction in or near the middle, as if the mass were about to divide into two. It will be observed that these spores differ from each other very much in the thickness of the outer membrane. In some cases this is cracked and ragged on the external surface, and several distinct layers can be made out, one within the other. Perhaps its thickness equals about one-third of the diameter of the whole. In others the envelope is comparatively thin, and in those cases in which a thallus has formed, the external membrane is very thin indeed.

Now it is easily proved by attentive examination, that where the structure has grown rapidly the internal granular coloured matter is abundant and covered by a thin membrane; while, in cases in which the partially developed spore has been exposed to conditions adverse to its rapid growth, the external colourless portion is always very thick, and the outer surface sometimes uneven or ragged.

In the growth of the mildew, then, how is the new matter formed? Is it deposited upon the external surface of the investing membrane, or is the new matter produced by the granular matter in the interior? To put the question still more simply, Is the capsule, the so-called cell-wall, formed by deposition of matter from the medium which surrounds it, or is it formed from within? or which is the oldest part of the capsule—its external or internal surface? If the new matter were deposited upon the external surface, we should expect to find that the membrane would become thicker and thicker as the growth of the organism advanced, while the central portion would remain unaltered. This, however is not the case; on the contrary, we find that, in the case of the mildew, as growth proceeds, the wall in certain parts becomes considerably thinned. It is clear, therefore, that the increase in size cannot be due to deposition from without. The matter deposited upon the inner surface of the capsule is always softer than its general substance, and the external surface of old capsules is cracked and ragged.

In many of the algæ this external surface serves as a nidus for the development and growth of smaller algæ—a fact which clearly shows it has ceased to be active, and is incapable of resisting the action of external conditions. This is the oldest part of the capsule which is going into decay, and the small algæ are living in part upon the products thus produced.

I have examined several structures in the various classes of animals and vegetables, and always with the same general result. *If a series of structures derived from the lower and higher plants and animals, at different periods of development, and diseased growths, be examined, each is seen to be composed of elementary parts, and each elementary part may be separated into a coloured and colourless portion.* The coloured portion is always surrounded with a thin layer of the colourless matter, which may be hard, soft, or diffluent.

The fact being determined, that in every elementary part of every living structure a part is coloured by carmine and some other colouring matters, and in certain cases, in one elementary part different portions are coloured with different degrees of intensity, it is interesting to inquire if the coloured and colourless portions are dissimilar structures growing independently of each other, and each possessing inherent powers of growth, or if they are only different stages in the development of tissue of which the outer and uncoloured portion is composed.

The facts brought forward with reference to the mildew prove, I think, that in this case at least it is the *youngest* part of the capsule that is in contact with the granular matter within, and the *oldest* on the outer surface in contact with the medium which surrounds it. The new material is added upon the inner surface of the capsule, layer after layer; and where there are several layers, the innermost is the youngest, and the outermost the oldest portion of the structure. It follows, therefore, that the inanimate material for the nourishment of the structure passes through the outer membrane, and is taken up by the granular matter within, which communicates to it the same properties and powers this living matter itself possesses, and which it has inherited from pre-existing living matter. In the growth of epithelium, the masses which are coloured by carmine are very close together on the deep surface; but as they recede from the vascular surface and advance in age, they gradually become separated from each other by the growth of the material which is not coloured by carmine.

In a structure like cartilage or tendon, where there is a considerable quantity of the so-called intercellular substance, the masses coloured by carmine almost touch each other at an early period of development, being only separated by a very thin layer of soft substance, which is not coloured by carmine. As the tissue advances in development, the distance between each coloured mass increases. In fœtal tendon and cartilage this fact is very striking, so that in equal bulks of the tissue at different ages, the proportion of coloured and colourless material varies very greatly. In the fœtal texture there may be twice or three times as much coloured as colourless matter; while, when the tissue is developed, the quantity of colourless matter will be ten or twenty times as great as the coloured.

The capsule of the mildew, the wall of the epithelial or other cell, the intercellular substance of cartilage, and some other tissues, all increase in quantity as growth proceeds. These structures exactly correspond to each other. They are not coloured by carmine, and are all produced on the surface of masses of granular matter which is coloured. In all, the ratio of the external colourless matter to the internal coloured matter increases as age advances. It remains to be proved that the new matter of epithelium, cartilage, and other tissues, like that of the mildew, is added upon the inner surface of the colourless matter; but this must be deferred till the conclusion of my paper.

Increase in Number of Elementary Parts.—It has been shown how elementary parts increase in size, and it is now necessary to consider how they increase in number. In a tissue like cartilage it is believed that, as the masses of coloured matter increase and divide, the colourless material also *grows in*, and forms a partition, between the two resulting masses.

I have never been able to obtain a single example of the division of a mass of colourless material only, and where this increases in thickness the coloured matter is always present. On the other hand, there is not, I believe, a single living structure in nature in which the coloured matter may not be shown to increase independently of the colourless material. At an early period of the existence of a living structure, or during the early stages of development, there is no difficulty in demonstrating this point in specimens prepared with carmine. The younger the structure, the greater the proportion of the matter which is coloured. It is probable that in all cases a very thin layer of matter, which is not coloured, surrounds the masses of coloured matter, but it is frequently so very thin and soft as not to be demonstrable.

Besides multiplying by division, the coloured material may increase in one or more directions by forming offshoots, and these may remain connected with the parent mass for some time, but if detached each offshoot is capable of carrying on an independent existence. In the mildew the formation of a bud seems to take place thus: A small collection of granules, covered by a thin layer of membrane, passes through a pore in the capsule, or the latter becomes very thin at one point and is pushed outwards by the growing matter. This, being separated from the pabulum only by a very thin membrane, grows rapidly, and soon an offshoot of a considerable length is produced, and from this, under favourable circumstances, new offshoots soon proceed.

Now, in this process, so far from the external envelope or colourless matter taking part, it evidently acts as an obstruction, for an outgrowth never takes place from one of the thick-walled masses without the wall becoming very thin at the point at which the offshoot occurs, or the formation of an actual opening through which a certain amount of the granular matter within escapes. This latter, brought into close relation with its pabulum, absorbs nutrient material, and soon increases in amount, but for some time it remains continuous with the mass from which it proceeded. The covering of the offshoot is at first very thin, but it gradually becomes thicker as its age advances. If pabulum be abundant, the coloured matter increases very rapidly, and the colourless matter exists in the form of the thinnest membrane; but if the pabulum be scarce, the former substance does not increase rapidly, offshoots are not produced, and the external matter becomes much thicker.

The facts to which I have briefly alluded, and corresponding series of facts observed in other structures, compel me to conclude that the inner material which is so readily coloured by carmine is the active growing part of every living structure, that it, and it alone, is essential to *increase*, to *reproduction*, and indeed to the *existence of a living tissue*. The external material which is not coloured by carmine does not possess the power of growth or reproduction. It is formed from the inner soft granular matter, and all the properties it manifests are dependent upon the powers of the matter which produced it.

Of Germinal Matter and Formed Material.—The active part of the germ of every living being, of every tissue at every period of life, of every structure developed in disease, the active part of everything that has life, consists of a material which appears granular under the microscope, and which possesses the property of being coloured by carmine and certain colouring matters. It is this substance, and this alone, which possesses all those wonderful powers characteristic of living beings. I have proposed to call it *germinal matter.* So far as its microscopical characters are concerned, and the property of being coloured by carmine, this germinal matter exhibits precisely the same characters in every living structure. The germinal matter of an embryo resembles that of the tissues of an adult, and the germinal matter from an elementary part of a most inveterate morbid growth could not be distinguished from that of a healthy tissue; neither have I been able to detect any difference between the smallest particles of the germinal matter of the lowest fungi and that of the tissues of the higher plants and animals.

It is however very different with regard to the outer matter which is not coloured with carmine. As this was once in the state of germinal matter, and was formed from it, I propose to call it *formed material.* The formed material of elementary parts of different animals, and that of the tissues of the same animal, and that entering into the composition of a tissue at different periods of life, possesses the most different, and perhaps opposite, characters and properties. Upon this difference the character of every tissue with which we are acquainted depends. The difference results not from any peculiar properties of the formed material itself, but it depends upon the powers of the germinal matter which produced it—*powers which this last derived from pre-existing germinal matter.*

The whole of the germinal matter of an elementary part is not generally converted into formed material, but a small portion which is much more intensely coloured by carmine than the rest generally remains comparatively quiescent while the remainder is undergoing active change. This is the nucleus. The nucleus is a new centre of growth, and within it new centres may arise. The nucleus has the power of resisting the action of conditions which would destroy the remainder of the germinal matter, so that the nucleus may retain its vitality under certain circumstances which would certainly cause the destruction of the elementary part, and this nucleus may at a future time grow and produce an infinite number of new elementary parts. The elementary parts resulting from the successive development of new nuclei may differ widely from each other in power, and the formed material produced by them may differ much in its character.

Of the Structure of Germinal Matter.—I have brought forward evidence which seems to indicate that the elementary particles of germinal matter are invariably spherical, although the masses compounded of them vary very much in form. There is reason to believe that the spherical particles are themselves composed of spherical particles, and so on to a minuteness far beyond that which it is possible to realize.

I think that of each spherule the circumference is the oldest portion, and I believe that the inanimate matter about to become living passes into the central part of the spherical particles. In these living spherules, which are free to move in fluid, there is a continual movement of particles from centre to circumference, and this movement persists as long as life lasts. This movement from within outwards gives rise to the currents in the opposite direction, by which the inanimate pabulum is brought to the centre, where it becomes living, and takes the position of particles which were animated a little before it commenced its existence. It would appear that the relation of the elements which enter into the composition of the pabulum to each other becomes altered, their ordinary affinities or repulsions being suspended. It is probable that the elements are gradually so placed in the spherules, that when the latter have arrived at a certain period of existence, the ordinary forces of their component elements again become active, and the chemical compounds are formed which we meet with in the tissues and in secretions.

In the *nutrition* of an elementary part, the nutrient pabulum first of all passes through the formed material into the germinal matter. Thus constant currents, which ensure the passage of fresh particles of fluid, are maintained through the formed material, and there can be no doubt that thus its normal characters are preserved. An alteration in the character of the fluid, or in the rapidity of its circulation, will produce important alterations in the formed material. The pabulum, or certain constituents of it, assumes the form of germinal matter, and the particles pass through certain definite stages of existence, and at last become formed material. In the case of a secreting elementary part, this formed material is soon resolved into the products of the secretion, which perhaps are altered as soon as formed, and gradually resolved into simpler and more stable compounds. The formed material of a tissue may be slowly disintegrated and gradually removed, or it may accumulate to be thrown off in considerable masses, as in the case of the cuticle, hair, nails, &c. If it is removed imperceptibly, germinal matter is the active agent concerned in its removal. The formed material of bone and many other tissues is thus slowly absorbed. Masses of germinal matter grow and multiply at the expense of the tissue which has reached the termination of its existence. A soft spongy structure, consisting principally of germinal matter, takes the place of the bony tissue; at length the rapid multiplication of this germinal matter ceases. It undergoes conversion into formed material more or less fibrous in its character, and this in its turn again gives place to the development of new germs, in the formed material of which calcareous matter is deposited, and so new bone is produced to be again removed after the lapse of a certain period of time.

Let me in conclusion describe very briefly the alterations which occur during the formation of a structure like epithelium, and the important changes which result from the ordinary conditions under which growth takes place being modified.

If a vertical section be made through the epithelium and corium of the tongue, prepared in the same manner as the other specimens which I have described, the following points will be noticed. The epithelium forms several layers which differ from each other in age. At the deep surface, close to the corium, a number of small and nearly spherical massès of germinal matter, separated from each other by a very thin layer of soft formed material, will be observed. Many of these are undergoing division, and thus they increase in number. If the tissue be torn or broken by pressure, the fracture does not always take place through the formed material midway between two elementary parts, which invariably happens when growth is more advanced, but it not unfrequently passes into the cavities in which the masses of germinal matter are imbedded, as occurs in the case of cartilage, and some of the masses of germinal matter are set free. As we pass a little farther outwards, the masses of germinal matter become larger, and are separated from each other by a greater distance, and this is caused by an increase in the proportion of the formed material. Division of the masses no longer takes place. Still farther outwards, the same changes are much more marked, and for several layers, the size of each individual mass of germinal matter and the proportion of formed material increase,—but now a new point is noticed. Each elementary part consisting of germinal matter, surrounded by a certain thickness of formed material can be separated from its neighbours without difficulty. As we proceed still nearer the surface, the elementary parts become flattened, and the formed material is harder, and although the formed material has increased in proportion, the mass of germinal matter has diminished in size, and from this point the germinal matter becomes less and less, until in the outermost elementary parts, it is very small, and constitutes what is known as the nucleus of the epithelial cell, so that at first the germinal matter grows and divides, next germinal matter and formed material both increase, and then the germinal matter begins to decrease, while the proportion of formed material still continues to increase.

The explanation of these facts seems to me to be very simple. The masses of germinal matter on the deep surface are nearest to the nutrient fluid. They absorb nutrient matter, increase, reach a certain size, and divide. Gradually, as they are caused to recede from the vascular surface by the formation of new masses beneath them, they become less freely supplied with nutrition, and although they cease to divide, they continue to grow, but more slowly. The oldest particles of the germinal matter of each mass undergo conversion into formed material, and this process is more than counterbalanced by the production of new germinal matter from the pabulum which is absorbed, and therefore the mass of germinal matter still increases in size. At length the distance from the nutrient surface becoming very considerable, and the formed material having increased greatly in thickness, the amount of nutrient matter transmitted to the germinal matter within becomes very small. The conversion of the latter into formed material continues, and as the supply of nutrient matter is insufficient to compensate for the quantity of germinal matter converted into formed material, the size of the mass of germinal matter diminishes.

I have endeavoured to give a simple account of the changes which occur during the formation of epithelium, where it forms several layers, and these observations will also apply to cuticle in a normal state. I will now briefly advert to certain modifications occurring in the nutrient processes when the conditions under which growth takes place are altered.

I have stated that there are reasons for believing that germinal matter possesses the *power* of infinite increase, but that in the normal state this power is not manifested, owing to the conditions under which growth occurs. If the germinal matter, however, be set free, either by a rupture of the wall of formed material which surrounds it, or by softening of the formed material itself, the germinal matter being supplied with an increased quantity of pabulum, does increase, and divide, and subdivide freely, tending to produce a greatly increased number of elementary parts resembling those from which it was derived; and this increase is so rapid in certain cases, and occurs to such an extent, that we are quite justified in concluding that the power of multiplication is unlimited, and will continue to be manifested so long as the conditions favourable to its growth persist.

Suppose a blister is applied to the skin. In a period varying from six to twelve hours the superficial layers of the cuticle separate from the deeper ones and a quantity of fluid collects in the interval between them. If the contiguous moist surfaces be examined, several elementary parts are seen invested with a moderately thick layer of formed material, while others are surrounded with a very thin layer indeed. Multiplication of the masses is proceeding very fast, and the more rapidly this increase takes place, the thinner is the layer of formed material. In fact there is not time for the production of formed material. The nutrient matter is rapidly converted into germinal matter, and this divides; at the same time, new centres of growth appear. Growth and subdivision continue as long as any nutrient matter is present. The more rapid the multiplication the less do the particles resemble those of cuticle. At last, the process of multiplication continuing to take place very rapidly, and in a fluid medium, the elementary parts produced assume the spherical form. This is one way in which pus is produced from epithelium. The pus corpuscles must be regarded as the direct descendants of the germinal matter of the cuticle, and it may be stated generally that when germinal matter multiplies rapidly, the structures which result closely resemble pus in whatever tissue the change may occur. The germinal matter of any tissue in the body may produce spherical masses of germinal matter which would be called pus. It must be borne in mind that at first there is always a *tendency to the formation* of elementary parts resembling those of the tissue. The characters of elementary parts are therefore influenced by the conditions under which they grow, and the proportion of the formed material to the germinal matter depends in great measure upon the supply of nutrient material. It is most important to study the changes which are brought about in elementary parts by the altered conditions under which they are produced.

I cannot help concluding therefore that all the changes characteristic of living beings are directly dependent upon powers resident in the particles of germinal matter. Of the nature of these powers or forces resident in the germinal matter, we know little more than that they were inherited and were resident in pre-existing germinal matter of the same kind. The structure formed from this germinal matter, which I have called *formed material*, but which is ordinarily termed cell-wall, and in some cases intercellular substance, is destitute of any formative power whatever. I believe that the phenomena which alone can be considered peculiar to living beings, take place in the germinal matter, and in this alone; and hence investigations into the nature of the changes and the character of the forces which are concerned in the production of the tissues of living beings must be restricted to this portion of their structure.

The following are some of the principal points I have endeavoured to prove in this communication:—

1. That all tissues consist of elementary parts, and that each elementary part (cell) is composed of matter in two states—*germinal matter* and *formed material*.

2. That the only part of the matter of which living structures are composed which possesses the power of selecting pabulum, and of transforming this into various substances, of growing, multiplying, and forming tissue, is what I have termed *germinal matter*.

3. That the powers of growth of this germinal matter are infinite, but for the manifestation of the powers even in a limited degree, certain conditions must be present. Growth always occurs under certain restrictions.

4. Germinal matter is composed of spherical particles, and each of these of smaller spherules. New centres of growth originate in the spherical masses. Nuclei therefore are not formed first and other structures built up around them, but nuclei are new centres originating in pre-existing centres.

5. That all tissue (cell-wall, intercellular substance, &c.) was once in the state of germinal matter and resulted from changes occurring in the oldest particles of the masses of germinal matter.

6. That "intercellular substance" corresponds with the "cell-wall" of a single "cell," and that there is no more reason for believing that this structure results from any inherent power to form matrix, or that the intercellular substance is simply deposited from the nutrient fluid, than for believing that the capsule of mildew can grow independently of the matter it encloses, or be formed by being precipitated from the medium which surrounds it. There is a period in the existence of cartilage and allied structures in which there is no true "intercellular substance."

7. In nutrition, the inanimate matter permeates the formed material, and passes into the germinal matter, where it undergoes conversion into this substance. The older particles of germinal matter become converted into formed material. Growth therefore always takes place *from centre to circumference.*

8. That the relative proportion of germinal matter and formed material varies greatly in different elementary parts, in the same elementary part at different periods of its growth, and in the same tissue under different circumstances. The more rapidly growth proceeds, the larger the amount of germinal matter produced in proportion to the formed material.

9. That in all living beings the matter upon which existence depends is the germinal matter, and in all living structures, the germinal matter possesses the same general characters although its powers and the results of its life are so very different.

ART. IV.

An important Case of Paralysis and Muscular Atrophy, with Disease of the Nervous Centres. By C. B. RADCLIFFE, M.D., Fellow of the Royal College of Physicians of London, Physician to the Westminster Hospital, &c.; and J. LOCKHART CLARKE, F.R.S.

PART. I.

Clinical and Dead-house Observations. By Dr. RADCLIFFE.

THE chief interest of the case which I am about to relate is in the light which is thrown upon it by Mr. Clarke. What I have to do is indeed little more than to transcribe, almost *verbatim*, the clinical and dead-house notes which were taken at the time, and to append a few extracts from some notes which Dr. Gull has been good enough to furnish me with. I am very sorry that the case remained so short a time under observation. I am also sorry that certain points, as the condition of the paralyzed muscles, were not looked to in the examination after death; but I was chiefly careful that the cerebro-spinal axis should be sent to Mr. Clarke without injury, and I and all must be glad that this end was fully secured. Looking at the clinical facts, it was obvious that there was no material injury in the seat of intelligence, and it was probable that there was some grave injury to the parts which rule the movements of the tongue and pharynx, and the respiratory movements generally. Without this latter injury, indeed, it was difficult to account for the palsied and wasted state of the tongue, for the difficult deglutition, for the occasional trouble of breathing, for the mode of dying. These points were dwelt upon at the time in a short clinical lecture, but I do not recur to them now, for I am only wishful to give place to Mr. Clarke with as little delay as possible.

CASE.—Mr. Frederick P——, aged forty, a native of the United States of America, and formerly a surgeon in the U.S. army, married, residing recently at 25, Surrey-street, Strand, was admitted into the Westminster Hospital (Burdett Ward), under the care of Dr. Radcliffe, on the 17th April, 1861.

Present state.—Mr. P—— is in bed, propped up in a semi-recumbent position by pillows. His countenance is bright and intelligent, his complexion remarkably pale and transparent, his body and limbs greatly emaciated, especially the arms, which are literally little more than skin and bones.

Asking him whether he was able to change his position, he gave utterance to some low, unintelligible sounds, and moved his legs about, but not his arms. On further inquiry, the arms were found to be wasted to the last degree, stretched out towards the pubes, and somewhat pronated and flexed, the relics of the muscles being tense and rigid, and altogether disobedient to the will. The legs could be moved about in any direction without much difficulty, but somewhat slowly, and all power of standing or walking was absent. In the arms, no sign of contraction could be produced by percussion or by the shocks of an induction coil, beyond certain slight flickerings in parts of the deltoïd and great pectoral muscles; in the legs, no reflex movements could be produced by tickling the soles or calves. Examined carefully by the points of a pair of compasses as well as by pinching, there was no appreciable change in common sensation anywhere, not even in the hands and arms. Very expressive changes in the countenance, accompanied by shakes of the head, and certain faint inarticulate sounds, showed very clearly that pinching and tickling were by no means agreeable to the patient—showed, in fact, that the blight which had abolished speech and palsied the body to a great extent, had not extended to the mind. And Mrs. P——, who was standing by the bedside at the time, removed all doubt upon this point by saying that her husband was "too intelligent, if anything," and that he was never tired of hearing read books requiring attention and thought. "I only wish he would sleep more," she said, adding as her opinion, that sleeplessness and occasional attacks of difficulty of breathing were much more prominent and distressing symptoms than the palsy.

On asking to see the tongue, this organ was found to be wasted, flaccid, and curiously slow in its movements. It did not appear to be more than half the usual size, and it certainly required many efforts before it could be got beyond the teeth. There was no deflection or protrusion. Deglutition was slow, and only accomplished with much difficulty. The appetite was pretty fair; the bowels somewhat constipated, but perfectly under the influence of the will. Nor was there anything wrong in relation to urination.

All the time of the examination, the breathing was disturbed and hurried, more so at the beginning than afterwards. At first the walls of the chest were almost motionless. Now and then, after every twelfth breath or so, there was a pause, followed by a deep-drawn sigh. The pulse was about ninety, and not particularly wanting in power.

In other respects, there was nothing positive requiring notice. There was no pain anywhere, and no tenderness in the cervical or any other region of the spine. Nor was there anything faulty in the action of the special senses.

Previous history.—Mrs. P——'s account is this: Mr. P—— now and then giving assent by nods. Two years ago, while serving in California, he had a sun-stroke, which made him insensible for half an hour, and left him weak and shaken for several days, but not to such a degree as to prevent him from resuming his duties within a week. A month later, finding his hands and arms becoming very weak, he was obliged to give up his appointment. Soon after this he was stunned by some ruffians, and robbed of all he possessed. Later still, after an interval of a couple of months probably, he got a place as surgeon on board a small vessel bound for England, and in this way he reached this country thirteen months ago. He had been incessantly sick all the way, and when he arrived, he was emaciated to the last degree, and scarcely able to move hand or foot. A week or two later, he was admitted into Guy's Hospital, and there he remained until a few days ago. He ascribes his exceeding paleness to a course of mercury, pushed to salivation, and continued over several weeks after his arrival in this country. Prior to the sunstroke in California, he had never, so he says, had a day's illness, but he allows that he had led a very dissipated and intemperate life up to that time.

Treatment.—The treatment ordered was the continuous current from six simple galvanic cells along the palsied upper extremities, the poles applied one to each hand for twenty minutes at a time, twice a day; and three times a day two drachms of cod-liver oil, with five minims of the phosphorated oil of the Prussian Pharmacopœia. The diet ordered consisted of eggs (he preferred them to meat), beef-tea, an extra allowance of bread, and half-a-pint of port wine, half the wine to be taken with a little hot water and sugar at bed-time.

April 18th.—Mrs. P—— says her husband has had a much better night than usual, and less difficulty of breathing upon awakening, and he assents by nods. He is breathing more freely than he did yesterday, there being now evident movement in the sides of the chest, though by no means as much as there ought to be. Upon carefully examining the chest by percussion and auscultation, there was found to be nothing in the state of the lungs or pleuræ to account for the hampered state of the breathing. At the time of this visit, he was trying to swallow a morsel of bread sopped in beef-tea, and he must have been at least five minutes before he succeeded. He likes the galvanism, and thinks it does him good.

April 20th.—In very good spirits, thinking himself decidedly better.

April 22nd.—*Dead.*

Death happened suddenly in the night. At 4·30 A.M., the night-nurse left him awake, and as well as usual; a few minutes afterwards she was summoned from one of the adjacent wards by one of the patients, and before she could reach him, all was over. The patient who summoned the nurse, and who slept in the next bed, said that he was awakened by the sounds of struggling and difficult breathing, and that he at once jumped out of bed and ran to find the nurse. On the previous day he had expressed himself in some way intelligible to his wife as certain of recovery, and at bed-time he was quite quiet and comfortable.

Post-mortem examination twelve hours after death.—Body exceedingly emaciated, especially in the arms, which, as before said, are literally little more than skin and bones. Rigor mortis universal and perfect. No trace of injuries about the head or elsewhere. The brain and its membranes perfectly healthy.* The membranes of the spinal cord healthy, but the cord itself without any evident brachial enlargement. Looking at the cord, as it lay in its canal, it seemed to be of the same diameter throughout; it seemed, too, as if the nerves proceeding from the brachial enlargement were smaller than they ought to be. No sections were made, as the whole cerebro-spinal axis was reserved for special examination hereafter. The thoracic organs presented no sign of disease beyond these, that the heart was a little paler and smaller than usual, and that the lungs were somewhat gorged with blood. The abdominal viscera were all healthy.

The account furnished by the kindness of Dr. Gull confirms many of the foregoing particulars, and supplies others which illustrate some points in the progress of the case. The account itself is in two parts: the first is from a clinical account of the case prior to its coming under Dr. Gull's own care; the second is from a report of the case subsequent to this period.

Mr. P——, according to the first part of this account, was admitted into Guy's Hospital on the 26th October, 1860. "He states that he was in good health until thirteen months ago. He was then in California. One day, after a long ride in the hot sun, he was seized with vertigo and pain in the head, and on dismounting he fell down insensible. This insensibility lasted from five to ten minutes. After a week's interval he quite recovered, with the exception of a slight impairment of vision. A month later he began to find a weakness in his left hand, but at that time no other symptom troubled him. Gradually the left hand became more weak—so much so, that after eight months from the onset of his symptoms, he could scarcely use it. The right hand subsequently became affected in the same gradual manner. On admission into the hospital, he was much emaciated generally, but the arms were so in an extreme degree. He had very slight power over the left fore-arm, but he could not raise the arm from the bed, nor use the fingers to grasp anything. The right arm was not quite so powerless; he could move it to a slight extent about the bed, but not support it. The legs could be drawn up and moved in any direction feebly, but he could not stand. There was no anæsthesia. There was a sense of formication over the shoulders, and slight occasional twitching of the muscles of the lower extremities."

Mr. P——, according to the second part of the account under consideration, came under Dr. Gull's care in February, 1861. At this time "the muscles of the upper extremities were thin and wasted to

* This is evidently a mistake in the notes, for, on examining the brain, Mr. Clarke found the membranes firmly adherent along part of the marginal convolution of the longitudinal fissure, and the cortical grey substance corresponding thereto in a softened state.

the last degree. There were still traces of contractility of the muscles under galvanism, most marked in the right arm. The sensibility was diminished, but not lost. The muscles of the lower extremities were thin and feeble, but not paralyzed. He could flex the legs, and the sensation was not greatly lessened. He retained power over the sphincter. The urine was acid and normal in colour. The articulation was much affected, so that he could hardly make himself understood. The tongue could be put out, but only to a slight extent, and with a tremulous quivering motion. The eye was intelligent, and the features not inexpressive. Deglutition was impaired."

There was no important change in the symptoms for the several months during which he remained under Dr. Gull's care, and it is distinctly said that "he always exhibited much intelligence." The treatment during the latter half of Mr. P——'s stay in Guy's Hospital consisted of no more than good nourishment, ammonio-citrate of iron, and laxatives.

PART II.

Morbid Anatomy of the Nervous Centres. By MR. LOCKHART CLARKE.

All the most prominent symptoms of disease—the extensive paralysis and muscular atrophy described in the history of the patient, are so clearly and satisfactorily explained by the lesions of structure discovered on examination of the nervous centres, that the case now before us must be considered one of the most remarkable and interesting on record. As in another recently published,* the ordinary and inefficient method of examining the nervous centres must have failed to detect the minute structural changes upon which the wasting palsy was actually dependent, and would perhaps have resulted in ranking the case as one of simple muscular atrophy. The method I employed may be thus briefly described: after examining under the microscope small portions of the suspected parts in a perfectly fresh state, in order to ascertain, if possible, the *nature* of any morbid product that might exist, the cord and medulla oblongata were divided transversely, by clean incisions, into several pieces, which were then immersed in a weak solution of *bichromate of potash* (in the proportion of one part of the crystallized salt to about 400 of water), for two days, in a cool place. They were next removed to a solution of *chromic acid* (one part of acid to 200 parts of water), and at the end of about a month were fit for making sections by means of a sharp razor, wet with spirits of wine. The sections were then moistened with dilute glycerine, covered with thin glass, and examined under different powers of the microscope. But whenever this plan was insufficient, and more transparency as well as a finer definition of structure were required, corresponding sections were subjected to the method of preparation which is known more particularly as my own.†

In the lumbar region, the diameter of the cord was not appreciably

* Beale's Archives of Medicine, No. 9, 1861.

† For a description of this method see Philosophical Transactions, 1859, Part I.

below the average in a state of health. But in transverse sections it was evident that through the *middle* of the enlargement, the anterior cornu of grey substance was rather smaller than natural. When carefully examined, the nerve-cells composing the large groups were found to be much reduced in number, but to a variable extent in different parts. In sections treated with glycerine only, not a vestige of the missing cells could be discerned; but in corresponding sections prepared according to my own method, they could be very distinctly seen, even under a low power, lying between the others, but much altered in shape, and reduced to a singular state of atrophy. None of them were larger than the *nuclei* of the surrounding healthy cells; the majority were much smaller, without any traces of nuclei or distinct granular contents, and consisted of irregularly-stellate and apparently membranous bodies, which, in some instances, seemed like their shrivelled sheaths, or like radiating portions of the connective tissue. See Fig. 1,

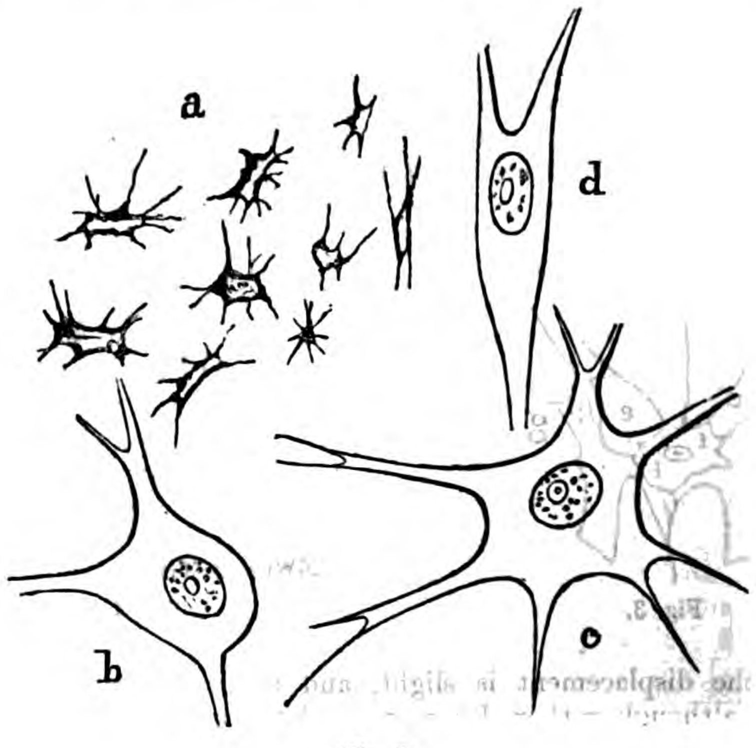

Fig. 1.

a, which represents similarly atrophied cells from the cervical enlargement, magnified 420 diameters; *b c d* are healthy cells from the same quarter, and magnified to the same extent.

The central canal was about its normal size, but the epithelium around it was increased or hypertrophied. This hypertrophy consisted in an unusually large accumulation of round and oval epithelial nuclei, intersected with fibres proceeding from them.

In the upper third of the lumbar enlargement, the anterior cornua were but little affected, but there was the same hypertrophy of epithelium around the canal; and beyond this, chiefly in the substance of the posterior commissure, there were in some sections small irregular or oval patches of a clear or finely granular material, which would seem to have been originally semi-fluid. Scarcely any traces of corpora amylacea were observed; but both here and in the anterior grey substance, the bloodvessels appeared larger than natural. The posterior cornua were in every respect healthy.

In the dorsal region of the cord, the grey substance presented considerable morbid appearances, as well as curious displacements of some of its parts. In most cases the posterior vesicular columns (*e e*, Fig. 2) were normal in size and structure, containing their usual number of large nerve-cells; but in some sections, as shown in Figs. 4 and 5, the left column was the smaller; and in many instances both columns were displaced unsymmetrically with regard to each other, as seen particularly in Figs. 3, 4, and 5. As effects of apparently the same cause, the rest of the posterior grey substance, as well as the parts surrounding the canal, were thrown into abnormal shapes and positions. In Fig. 2, which represents a section from the lower part of the dorsal

Fig. 2.

Fig. 3.

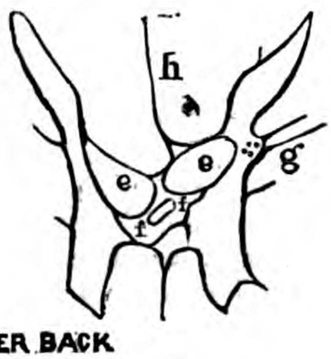

Fig. 4.

region, the displacement is slight, and the two posterior vesicular columns, although rather different in shape, are nearly equal in size; but the symmetry of the entire lateral halves of the grey substance is evidently destroyed. In Fig. 4, neither of the posterior vesicular columns is below the average size; but that on the right side is tilted, as it were, backward and inward, while the canal and its surrounding tissue (*f f*) is drawn round in the same direction; and the posterior median fissure (*h*), instead of descending to the posterior commissure in the median line, turns aside to the right, and terminates on the middle of the vesicular column. The *tractus intermedio-lateralis* (*g*), or the angular tract which projects into the lateral column, together with the rest of the posterior grey substance, is also more prominent on the same side. In Fig. 3, the displacement and want of symmetry are slight, but the posterior vesicular column is considerably larger on the right side.

In all these sections, the pale tissue (*f f*) surrounding the canal was increased beyond its natural quantity. A large portion of it, particu-

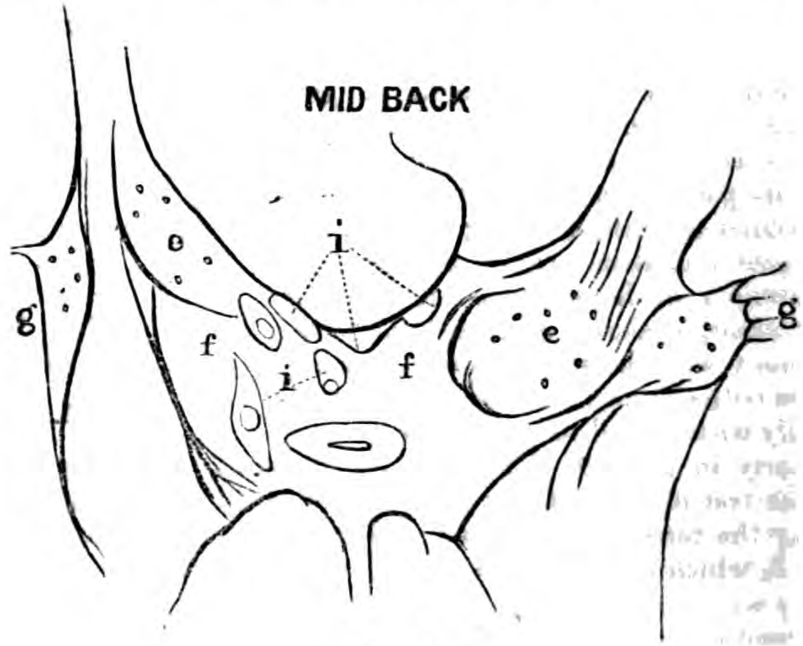

Fig. 5.

larly at the sides, was occupied by the hypertrophied epithelium or aggregation of granular nuclei. In its posterior division, forming the transverse commissure between the vesicular columns (*e e*), there were evident marks of disease. In almost all the sections some of the nerve-fibres of this commissure were more or less injured, and in many instances, while some of them were entirely lost, others were divided, as if eroded, into small bits which were mingled with granules and granular nuclei, like the smallest of those belonging to the epithelial layer. There were also found in this commissure, and in other parts around the canal, some small round or oval spaces filled either with a perfectly pellucid, or more or less granular material. These were more numerous towards the *middle* of the dorsal region, as shown at *i i*, Fig. 5. They were very frequent around bloodvessels, which sometimes appeared to be more or less diseased or degenerate, and from which they would seem to have been effused.* In the same region, the posterior vesicular columns (*e e*) were frequently unequal in size as well as different in shape; and, like the rest of the posterior substance, on the opposite sides, were unsymmetrical in regard to position.

* These morbid spots were similar to those which I found in corresponding parts of the cord belonging to the case already mentioned (Beale's Archives, No. 9.). It is probable that the short canal or tube with granular walls which I described and figured in the centre of one of those spots, was the remains of a degenerate blood-vessel.

(See Fig. 5.) In this section the relative position of the *tractus intermedio-lateralis*, *g*, was very like that represented in Fig. 4.

In the upper part of the dorsal region the cord was least affected,—indeed, there was scarcely any morbid appearance, save a trifling hypertrophy of the epithelium and connective tissue around the canal. Throughout the whole of the cervical region, however, the case was very different. That portion which gives off the brachial nerves, and which in the healthy cord is known as the brachial or cervical enlargement, was not larger in diameter than any other part of the same region; so that in reality there was no cervical enlargement at all. In transverse sections through this part, it was not difficult to perceive that the anterior cornua of the grey substance were unnaturally small, and somewhat altered in shape. This was more particularly the case through the whole of the lower and middle portion of the brachial region. When very thin sections, treated simply with dilute glycerine, and covered with thin glass, were subjected to a magnifying power of even 500 diameters, I was surprised to find that scarcely a vestige could be seen of the large groups of cells which are found in corresponding parts of the healthy cord; and that only one or two solitary cells could be detected in their place. But when sections from the same part, instead of being treated with glycerine, were subjected to my own method of preparation, I was much gratified on finding, in company with Dr. Radcliffe, that in consequence of the superior definition and sharpness of outline imparted by this method, the entire *groups* of cells could be very easily distinguished even under very low powers; but the *cells* were wonderfully altered from their natural appearance. Under a one-inch power, they looked like aggregated granules; but under a one-eighth-inch object glass, with a power of 420 diameters, they had the appearance represented at *a* in Fig. 1. They were all more or less atrophied and shrivelled, and in form and dimensions presented a striking contrast to the healthy cells; *b*, Fig. 1, is an exact representation of one of the few healthy cells found in the group, magnified to the same extent; and *c d* are two cells from a corresponding part of a perfectly healthy cord, under the same magnifying power.* In the upper part of the brachial region the number of healthy cells was greater than in the middle and lower part, but their proportion to the atrophied cells was still small. Through the remaining cervical portion of the cord, there was a similar state of atrophy of the grey substance. At the level of the fourth cervical nerve, the anterior cornua were narrow, remarkably pointed, and directed obliquely inward to an unusual extent. The posterior cornua, also, were unnaturally slender, and the posterior vesicular columns were below their average size in a state of health. In the *tractus intermedio-lateralis*, which is very large and prominent in the region of the third cervical nerves, the nerve-cells were very much wasted and mis-

* Sections showing the nerve-cells both in the atrophied and normal condition, as above described and represented, will be deposited in the Museum of the Royal College of Surgeons. I believe this is the first time that atrophy of the nerve-cells has been detected and described.

shapen. This *tract*, as I have elsewhere shown, is connected with some of the roots of the spinal accessory nerve; while other roots of the same nerve reach the anterior cornu, in which, as already stated, many of the nerve-cells were atrophied.*

All the white columns of the cord in every region, but particularly in the cervical region, had suffered more or less from atrophy or degeneration. This degenerate condition was rendered very striking by comparing the sections side by side with others from a healthy subject. In the nerve-fibres, the change was observed chiefly in the axis-cylinders, which were frequently reduced to less than one-half their normal diameters. Between the fibres, however, the connective tissue was unnaturally abundant; and the hypertrophy of this tissue, with atrophy of the white nerve-substance, appeared to be the chief causes of the curious displacements of parts already described. In the cervical region, the anterior roots of the nerves were decidedly below their average size.

Of the medulla oblongata, no portion could be considered perfectly healthy. At its lower part, the only morbid appearances were occasional and trifling atrophy of the nerve-cells in the anterior grey substance, with some hypertrophy of the connective tissue of the *white* substance. But from about the lower end of the olivary bodies to the commencement of the fourth ventricle, the morbid changes were much greater and more extensive. This space includes the principal part of the grey tracts or vesicular centres from which the hypoglossal or lingual nerves take their origin. Above the calamus scriptorius, these tracts or cylindrical columns of nerve-cells form part of the floor of the fourth ventricle along the sides of the median line; and below the calamus, they lie in front of the central canal, at the sides of the median raphe.* They consist of large multipolar nerve-cells, like those of the anterior cornua of the cord, of which they are the analogues. Now these grey columns were, in some parts of their course, reduced to about two-thirds, and in other parts, to one-half their natural diameters; while their cells, like those of the anterior cornua of the cord, were more or less shrunk or atrophied. The hypoglossal or lingual nerve-roots in their course through the medulla, were also in some places not more than half their natural size; and in other places could scarcely be discerned.

The central nucleus of nerve-cells which gives origin to the *upper* roots of the spinal accessory nerve,† was not appreciably affected.

Both of the olivary bodies, with their transverse decussating commissure, were healthy; none of their numerous cells appeared to have suffered any degree of atrophy; but behind them, in the central parts of the medulla, some of the nerve-cells scattered amongst the transverse arciform plexus, as well as the axis-cylinders of longitudinal fibres, were affected in this way.

* See Researches on the Grey Substance of Spinal Cord: Philosophical Transactions, 1859, Fig. 12, plate 25.

† See my Researches on the Intimate Structure of the Brain: Philosophical Transactions, 1858, Plate 16, Figs. 28-32.

The fourth ventricle, pons Varolii, and indeed every other part of the encephalon, except one of the superficial convolutions, were in a healthy state. This was the marginal convolution of the longitudinal fissure (*Première convolution de deuxième ordre*, of Foville). For about an inch of its length, near the middle of its course, the membranes were very closely adherent to its grey surface, which was softer than natural, and could not be separated from them without laceration.

Such was the morbid anatomy of the nervous centres in this remarkable case. In a physiological point of view it also presents some facts of importance, which, however, may be more safely or advantageously considered after a few more cases of a similar nature have been examined with the same care.

ART. V.

A Note on the Mortality after Excision of the Knee, as practised hitherto in London, and on the Statistics of Metropolitan Hospitals. By T. HOLMES, Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

A HIGHLY meritorious and carefully prepared essay has lately been published in America, entitled, 'On the Excision of Joints,' by Richard M. Hodges, M.D., a work which contains such an ample and intelligent digest of the experience of modern surgeons on this important subject, that it will, I conclude, be brought prominently under the notice of the readers of this Review. I shall therefore here only quote the results which, according to Dr. Hodges, have attended the operation of excision of the knee as it has been practised hitherto. Dr. Hodges has collected, from all sources, 208 cases of excision for chronic disease of the knee—65 from Mr. Butcher's writings, 35 from those of Heyfelder, and the rest from various journals and other publications, to all of which full references are given. Dr. Hodges seems to have searched with great industry through all the available sources of information in the English, French, and German languages, and this list comprises *all* the cases he has been able to collect. I have not thought it necessary to verify the details by referring to the original sources, but as the references are given, it would be possible for any one within reach of a good medical library to do so. I have no doubt, however, that his conclusions are correct. They are certainly startling, and they contrast very painfully with the statements as to the success of the operation which have been made by the ardent advocates for excision. In round numbers, one-third of the cases died; more than half are known to have failed; and there is no direct evidence of success in more than one-third of the cases, even accepting the statements of those who furnished the notes. The exact numbers are these: 60 were directly fatal without amputation, and 9 others were known to have died after amputation; the whole number of those who underwent amputation was 42; in 14 other cases the limb is stated to have been more or less useless:—thus making the whole number of known failures 116. In 27 other cases there is no information on the subject of the usefulness of the limb, the simple statement of "recovery" being all that is given. In the remaining 65, the accounts furnished represent the patient as having obtained a useful limb.* I cannot see what objection can be made to these statistics, as far as the facts themselves go. The mortality taken at 69 is a minimum, since of the 42 amputated only 35 are accounted for, the result being left uncertain in the other 7 cases. The number of failures, if taken at 116, is also a minimum, since it is all but certain that some of the 27 unaccounted for must have failed; while, in admitting the fact of success in the 65 cases in which it is claimed, we are leaving out of view the great tendency of advocates of a given plan of treatment (and nearly all those who furnished the notes are in this position) to over-estimate the success of their favourite plan, as well as the great frequency of occurrences which afterwards spoil a union that seemed useful at first, such as gradual yielding and flexure backwards or sideways, recurring disease, interrupted growth in childhood, &c. No one would think of comparing such success as this with the results of amputation in metropolitan hospitals; since it has been clearly proved that the rate of death in amputation of the thigh, at its lower part, for chronic disease of the knee, is about one-seventh of the number of cases;† and of those who recover, hardly any are not relieved from local disease, irritable and diseased stumps being rare in this amputation. Several objections, however, may be made to the comparison between Dr. Hodges' statistics and those of amputation in London hospitals, all of which objections may be included in the statement that the two series of cases were not under the same conditions. But no such objections could be raised against a comparison instituted between a complete series of the cases of excision of the knee-joint treated in the metropolitan hospitals, and those of amputation of the thigh in the same institutions, since the patients were of the same class, subjected to the same influences of habits and constitution, under the same surgical treatment, and in the same atmosphere. I had wished, therefore, to procure satisfactory data of all the cases of excision treated at all the metropolitan hospitals, including the following under that term: St Bartholomew's, Guy's, St. Thomas's, King's College, St. George's, University College, the Westminster, the Middlesex, the London, Charing-cross, St. Mary's, the Hospital for Sick Children, the Great Northern, and the Royal Free Hospital.

* I hope I shall not be accused of want of charity or want of courtesy if I say that these accounts, especially in journals (which are necessarily compiled somewhat hastily), must not be accepted without some hesitation. I well remember reading the account in one of the journals of an excision of the knee, where the patient was said to have "recovered with a sound and useful limb." During the very week of the publication of this account, I witnessed the amputation of this "sound and useful" member, which had been riddled with sinuses leading to diseased bone during the ten months that had elapsed after the operation, and had been a constant source of torment to the patient.

† This statement rests on the statistics of 89 cases at Guy's Hospital, published by Mr. Bryant in the 'Medico-Chirurgical Transactions,' vol. xlii.; and 35 at St. George's, published by Mr. Cooper and myself in the 'Medical Times and Gazette,' 1861, vol. i. Both series gave exactly the same mortality in amputation of the thigh for chronic disease—viz., one death in seven amputations.

I much regret, however, that the defective method in which statistics are kept at nearly all our hospitals (I might say, the utter absence of all attempt to keep such statistics in many of them), has disappointed me in this attempt. Still, though baffled in my attempt to obtain complete statistics, I have succeeded in procuring a considerable list of authentic cases—in fact, one which includes the very great majority of the operations which have been performed at the above-mentioned hospitals. This list may be taken as nearly correct for the number of deaths in the cases comprised in it, and it may perhaps, without serious error, be accepted as showing the percentage of deaths which would prevail in the series, if it could be completed. I believe, also, that the number of secondary amputations is correct for the cases included in the list. But the details which would show the usefulness of the limb in those cases which recovered—which I regard as the most important point of all—are, I am sorry to say, not to be obtained in sufficient numbers to allow of any sound deduction being made. Hence, although I have to thank the surgical authorities of all the hospitals for the greatest courtesy and liberality in allowing me to make use of such materials as they possess,* I am compelled to say that these materials are insufficient, and that the following list will give only negative results, i.e., it will show the minimum of ill success which may have attended the operation as practised in London, but will give no reliable data as to the number of useful limbs which were really turned out by the 95 operations of which it is composed.† It may be interesting to add the numbers, as showing the great diversity of opinion which must prevail among hospital surgeons as to the propriety of performing the operation. Of the 95 cases, 32 are from King's College Hospital, 14 from St. Thomas's, 13 from University College, 6 from the Great Northern, 5 from St. George's, 5 from the Westminster, 3 from St. Bartholomew's, 3 from Guy's, 3 from Charing-cross, 3 from the Hospital for Sick Children, 2 from the Middlesex, 2 from St. Mary's, 2 from the London, and 2 from the Royal Free Hospital.‡ Of these 95 patients, 27 are known§ to have died; and in 10 others the operation failed, as shown in 8 cases by amputation, and in the other two by the limb being reported as useless, though it is not known to have been removed. This would make the minimum rate of mortality 28·4, and the minimum rate of failure 38·9 per

* I would particularly express my thanks to Mr. Fergusson and Mr. Henry Smith for the statistics from King's College; to Mr. Allingham for those from St. Thomas's; and to Mr. Erichsen and Mr. H. Thompson, for giving me access to the surgical case-books of University College Hospital.

† This list is composed of cases extracted (1) from hospital registers, (2) from the case-books or private note-books of the surgeons, and (3) from the journals.

‡ I may mention that all the particulars in my possession, as to mortality, failure, &c., of cases at the various hospitals, have been placed at my disposal with free permission to publish them. I have omitted these particulars merely to save space, but I would publish them if it were thought worth while. Meanwhile, the documents on which the calculations are based are in my possession, and I shall be most happy to show them to any person interested in the subject.

§ I use this term advisedly, since I have no doubt that death occurred in some of the other cases, of which only a short notice has been preserved in the journals, and none in the hospital records.

cent. But out of the 58 remaining cases the accounts of 10 are either unsatisfactory, leading to the suspicion that the limb was not useful, or are entirely vague; while of 19 of the others, forming a portion of the King's College series, I have only the most summary account; and although I know that many of these operations succeeded, some, I have no doubt, must have failed. Hence we may suspect that no better success has attended the operation in the hands of the hospital surgeons of London than in those of other operators, and that as many of the cases have failed as have succeeded.

Now, it is by no means my intention, in writing these few lines, to dwell on the failures of a novel method of treatment, still less to decry or to endeavour to banish from practice an operation which, on the contrary, I hold, in properly-selected cases, to be extremely useful. I only wish to show how very fallacious the statements which have been hitherto made as to its relative mortality, when compared with that of amputation, have been; how uncertain we really are what benefits have resulted in the so-called successful cases; and, as a consequence from these facts, which, I presume, will soon be patent and admitted by every one, how urgently needed is some better plan of keeping the records of our great hospitals, if the ample experience which might be collected from them is to be made available to the great body of the profession. The exaggerated and inconsiderate assertions which were made of the incredible success of excision of the knee by its earlier and warmer advocates, have done much to discourage more sober-minded practitioners from an operation which, had it not been unduly extolled, would sooner have come into more extended, though perhaps less indiscriminate use; and these assertions would probably have never been received as true, had authentic hospital statistics been at hand by which to test them. At present, the operation is in danger of being discredited by failures which perhaps are not essential to its performance. We are told, and with great probability, that the recent mortality after the operation shows signs of diminishing, that the selection of cases fitted for the operation is now better understood, that the importance of certain rules for the performance of the operation and its after-treatment, is now recognised and acted upon, and so that fewer patients will die, and those who recover will do so with more useful limbs. I sincerely hope that it may be so, and I am anxious to believe that it will; but we cannot forget that we were told far more confidently, as a matter of fact, a few years ago, that the operation *had* proved more successful in both these particulars than amputation; and if that assertion had any foundation in fact, the present altered condition of things can only have been brought about by the cases of excision having turned out *less* successful recently. At any rate, the want of reliable data from the great schools of surgery in this metropolis is a fact, and one which cannot but be held to be discreditable to them as places of sound surgical instruction. It is not true that there is any difficulty in collecting such statistics, though of course there is some trouble. To preserve authentic and full records of all the cases in a hospital, may or may not be worth while; if it is judged worth what it costs, it is merely a question of expense; but to preserve adequate notes of important surgical operations is within the reach of every surgeon who chooses to spend a few minutes each time he goes his round in directing and superintending the willing exertions of his pupils. Nor have I found that there is very much difficulty in keeping special cases under occasional observation for a sufficiently long time after they leave hospital, to furnish the requisite materials for judging of the usefulness of a limb after excision. At any rate, the fragmentary and incomplete reports too often published of these cases, are worse than useless, since they can only mislead.

The conclusion of the whole matter is this—and I submit that it is one of sufficient importance to attract the serious attention and to call for the united action of the hospital surgeons of London—the operation of excision of the knee, instead of having been less fatal than amputation of the thigh, has turned out, in general practice certainly, and in the practice of the London hospitals most probably, more than twice as fatal as that operation; and in both classes of cases its failures appear to have been more numerous than its successes. No data exist, however, for instituting an accurate comparison between the two operations, nor for estimating the real value of the successes obtained by excision, in consequence of the defective nature of the arrangements for preserving the records of their experience at most of our large hospitals. The same want of exact information, if it is allowed to continue, will prevent us from judging of the real amount of the improvement which the extended experience of surgeons may effect in the statistics of the operation, and thus prevent or retard its general adoption in appropriate cases; and what is of still greater importance, the same want of statistical information prevents us from forming any other than a conjectural judgment on numerous other surgical questions of the greatest interest.

Detached cases, hastily compiled and published without due consideration, so far from remedying these evils in any measure, can only aggravate them; what is wanted is that our great schools of surgery should make arrangements for preserving adequate and full details, not, however, necessarily at any great length, of the whole of some of the most important groups of cases—arrangements which, if they were found to answer, might gradually be extended so as to embrace the whole experience of the hospital. The persons by whom such reports are compiled should be paid officers of the hospital, and paid at such a rate as to secure the services of competent persons. Offices of this kind would form suitable objects for competition between the students of each school after the completion of their course, and would prove a far more useful item of expenditure than the sums annually lavished at some schools upon the unmeaning and often mischievous system of prizes.

I will not, however, attempt to sketch out any plan of my own; it will be sufficient for me, at the present time, if I can succeed in awakening the attention of those among your readers who have the power of remedying it, to this serious deficiency in the arrangements for medical education.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.
Fellow of the Royal College of Physicians, Physician to the German Hospital, London.

I. General Physiology.

F. P. Liharzik: *The Law of Growth, and the Frame of Man.* (Vienna, 1862.)

From a large series of observations, measurements, and calculations, Liharzik, of Vienna, concludes that the structure of the whole body of man is based on seven fundamental dimensions: 1. The length of the head, from its vertex to the point of the chin. 2. The length of the neck, from the chin to the upper margin of the sternum. 3. The length of the sternum, from its upper margin to the end of the xiphoïd cartilage. 4. The distance from the xiphoïd cartilage to the upper margin of the symphysis pubis; the navel dividing this distance into two equal portions. 5. The length of the thigh and leg. 6. The distance from the centre of the internal malleolus to the sole of the foot. 7. The length of the clavicle. By means of these fundamental dimensions, and with the further aid of circles described with a radius equal to the length of several of these dimensions, the frame of man may be delineated. To obtain for any period of life a figure of ideal regularity and beauty, nothing is required but the knowledge of the gradual growth of the seven fundamental dimensions for every age and for both sexes. This growth is regulated by the following laws: 1. The whole growth of all the parts of the body comprises twenty-four epochs, extending over a period of twenty-five years. 2. The first solar month after birth constitutes the first epoch, each following period being a month longer than the epoch immediately preceding it, so that the second epoch extends over two, the third over three, the twenty-fourth over twenty-four solar months. The duration of all the twenty-four epochs is three hundred months, or twenty-five years. 3. The twenty-four epochs are subdivided into three sections, the first section embracing six epochs—viz., from birth to the 21st month of life; the second, the following twelve epochs—viz., from the 22nd to the 171st month; the third, the last six epochs—viz., from the 172nd to the 300th month. These three sections are characterized by the peculiarity, that the epochs embraced by each of them show an equal increase of growth. The growth within each epoch of the first section is greater than that within each epoch of the two other sections; in the second it undergoes a proportional diminution; and in the third, several parts of the body grow again with greater energy. An artist from Vienna (F. Müller) has modelled, according to these laws, 24 statuettes representing the gradual and normal growth of man for both sexes.

It is evident that the knowledge of the normal dimensions of different parts of the body may become important for pathology, as the deviation of certain portions from the normal proportion would indicate a greater liability to certain diseases.

Liharzik maintains that this law of growth is not confined to man, but that all growing organisms are subject to it, that all have 300 epochs of growth, that, therefore, as soon as the duration of the first epoch has been ascertained, the duration of the whole period of growth can be calculated. Thus in the domesticated horned cattle the duration of the first epoch is fixed at four days; the whole period of growth is therefore equal to 300 times $4 = 1200$ days, or about three years and fifteen weeks; in the apricot (from the moment of the falling of the calyx), the first period lasts six hours, so that its full period of growth amounts to $300 \times 6 = 1800$ hours $= 75$ days.

Tables annexed to the work represent in figures the increase of growth of the different portions of the body in every epoch; and photographs taken from the twenty-four statuettes show the appearance of man during the different periods of development.

II. Lymphatic Vessels and Lymph.

1. L. Teichmann: *The Absorbent System, from the Anatomical Point of View.* (Leipzig, 1861. With plates.)
2. W. Weiss: *Experimental Researches on the Lymph Stream.* (Virchow's Archiv f. Pathol. Anatomie, &c., vol. xxi., p. 526, 1861.)
3. H. Nasse: *Preparatory Researches to the Doctrine of the Formation of Lymph.* (Congratulation to Prof. Hensinger's Jubilee, Marburg, 1862.)

1. L. Teichmann, who for many years has been occupied with anatomical researches on the lymphatic system, has embodied these in an elaborate work which well deserves the careful attention of the physiologist. We can communicate here only some of the principal results at which the author arrives: 1. The capillaries of the lymphatic system are provided with distinct walls of their own. 2. They derive their origin from the stellated cells. 3. According to the organs in which they lie, they commence either as plexus or blind ends. 4. They form a system by themselves, and are not connected with the bloodvessels except by occasional larger trunks. 5. Their lumen is, in general, larger than that of the blood capillaries; it depends, however, to a great degree, on the locality. 6. The copiousness of lymph capillaries in different organs depends partly on the structure of the latter, partly on the degree of denseness of the surrounding tissue. 7. They are totally different from the bloodvessels, by their shape and their manner of ramification. 8. In the cutis and mucous membranes the lymph capillaries are more distant from the surface than the blood capillaries. 9. Some organs do not possess any lymph vessels. 10. The lymph capillaries either pass directly into the trunks, or they are previously united into reservoirs, the efferent vessels of which enter the trunks. 11. The lymphatic trunks, in their course, divide sometimes into many branches, and form plexus. With regard to the formation of lymph globules, Teichmann maintains that they are formed within the lymph and chyle vessels themselves, as the walls of the latter are impenetrable for all kinds of solid substances, except their continuity be destroyed.

2. The experiments of Weiss on the lymph stream were made on foals and dogs. The lateral pressure in the truncus trachealis dexter of foals amounted to between ten and twenty millimetres of a solution of soda of specific gravity 1080. The flow of lymph is accelerated not only by the respiratory movements, but also by all muscular action in other parts of the body. The average quantity of lymph passing through the lymphatic vessels of the neck is, for

one kilogramme, about 200 grammes, or one-fifth of the entire weight of the head. Increased pressure in the blood capillaries, through ligatures applied to the jugular veins, causes an increased production of lymph. The experiments on the cervical portion of the thoracic duct show that the lateral pressure is increased during the expiration, diminished during the inspiration, the former effect being caused by the increased propelling forces and the increased pressure in the jugular veins, the latter by the aspiration of the thorax. The quantity of lymph flowing through the thoracic duct seems to be considerably influenced by the quality of food; thus it amounted, within twenty-four hours, in a foal fed on hay, to about one-tenth of the entire weight of body; in another one, fed on milk, to one-fifth of the entire weight. With regard to the origin of the lymph vessels, Weiss inclines to the view that they are continuations of the canaliculi, or ramifications of the connective-tissue corpuscles (Virchow, Leydig). The *vis à tergo* acting on the flow of lymph seems to be due to the pressure of the blood.

3. The difference in the theories on the formation of lymph—i.e., whether it be a mere product of transudation from the blood, or a kind of secretion—has led Nasse to perform a large series of experiments on dogs on the quantity and quality of lymph obtainable from the lymphatic trunk of the neck, under varying influences: 1. The *quantity* of lymph obtained in relation to the weight of body manifests a considerable variation. Muscular and lean dogs yield a much larger amount than fat dogs; young and active animals produce likewise more than old and slow ones; the average flow after flesh-feeding is about thirty-six per cent. greater than after potato-feeding, and fifty-four per cent. greater than during starvation. The comparison between the composition of the blood and the amount of lymph seems to show that a greater proportion of water in the blood coincides with a greater abundance of lymph. The average quantity of lymph obtained in 1000 minutes for 1000 parts of body was 4·643. In those cases in which the operation was performed first on one side of the neck, and then, a few weeks later, on the other side, the quantity of lymph obtained in this second experiment was greater than it had been in the first, a result which, in part at all events, seems due to an alteration in the blood (greater proportion of water), consequent on the wound and the traumatic fever caused by the first experiment. 2. With regard to the *physical qualities* of the lymph, it is at first limpid, becomes during coagulation turbid, but after the removal of the clot, again limpid. There is a great variation in the colour of the lymph of different dogs, which variation does not depend on the nature of the food, but probably on the peculiarity of the different individuals. The period of coagulation varies considerably, some lymph, especially the scantily flowing, coagulating already in the tube through which it is collected; while other specimens, especially those flowing more abundantly, coagulate only after fifteen or twenty minutes, or even later. The specific gravity varied between 1010·4 and 1018·8. 3. The *chemical analysis* was, on account of the limited material, restricted to the most important constituents. The amount of *solids* varies much (from 21·2 to 60·3 per mille); it depends, to some degree, on the composition of the blood, which contains, on an average, five times more solids than the lymph; yet this proportion is by no means constant. The proportion of *water* is subject to much greater variations in the lymph than in the blood, a peculiarity by which lymph differs from simple transudation, more resembling in this point the character of secretions. The lymph of young dogs seems to be, like the blood, more watery than that of old dogs. The *albuminous* substances averaged 29·1 per mille; the *extractive matters and salts*, excluding chloride of sodium, 3·8 per mille; and *chloride of sodium*, 6·7 per mille. Every lymph contains an alkali albuminate, which is precipitated by a small quantity of acetic acid, but is redissolved by an excess of acid. The proportion of *alkali*

varied between 0·74 and 1·24 per mille, and was greater after flesh-feeding, a circumstance which, Nasse suggests, may be due to the increased secretion of hydrochloric acid in the stomach during the digestion of flesh. The proportion of chloride of sodium to the other solids varies according to food and other circumstances, and is smallest after flesh-feeding. Its relation to the water is remarkably constant, ranging only between 1 to 140 and 1 to 144. The amount of *fibrin* can be ascertained only with difficulty, and never quite accurately, as it cannot be altogether deprived of lymph globules; it exhibits a great variation, which, however, may be due to a difference in the quality of the fibrin. The comparison between the fibrin of the lymph and that of the blood showed an average proportion of 1 to 4·4, the quantity of the former being 0·5771 per mille, that of the latter 2·56 per mille There seems to exist also some relation between the fibrin and the solids of the lymph, 1 per mille of the latter corresponding to 0·013 per mille of the former. Nasse, with true modesty, does not yet consider himself entitled to draw from the results of his diligent researches any inferences with regard to the formation of the lymph.

III. Nutrition; Metamorphosis of Tissue; Animal Heat; Liver; Urine.

1. C. Schmidt: *On the Chemical Constitution and the Process of Formation of Lymph and Chyle.* (Bulletin de St. Petersburg, 1861; and Meissner's Bericht de Zeitsch. f. rat. Med., vol. xiii., p. 365, 1861.)
2. Bartsch: *Observations on the Metamorphosis of Tissue in New-born Children.* (Archiv d. Ver. f. wissensch. Heilk., vol. v. p. 123.)
3. C. Voit: *Researches on the Influence of Culinary Salt, Coffee, and Muscular Movements on the Metamorphosis of Tissue. A Contribution to the Doctrine of the Conservation of Force in the Organism.* (München, 1860.)
4. L. Lehmann: *What is, under varying conditions, the Influence of Bodily Exercise carried to Fatigue on the Human Organism, and especially on the Tissue-change?* (Archiv d. Ver. f. Wissensch. Heilk., vol. iv. p. 484, 1860.)
5. C. Speck: *On the Action of Bodily Exertion carried to Fatigue, under varying conditions, on the Tissue-change.* (Archiv d. Ver. f. wiss. Heilk., vol. iv. p. 521, 1860.)
6. M. Traube (of Ratisbon): *On the Relation of Respiration to Muscular Action, and on the Function of Respiration in general.* (Virchow's Archiv, vol. xxi. p. 386, 1861.)
7. F. W. Pavy: *Contributions to the Physiology of the Liver. The Influence of an Acid in producing Saccharine Urine.* (Proceed. Roy. Soc., vol. xi. p. 335, 1861.)
8. F. W. Pavy: *On the Influence of an Acid in producing a Diabetic State of the Urine.* (Guy's Hospital Reports, vol. vii. p. 209, 1861.)
9. F. W. Pavy: *On the Influence of Alkalies in Checking the Production of Artificial Diabetes.* (Guy's Hospital Reports, vol. vii. p. 197, 1861.)
10. J. Bergholz: *On the Quantity of Urine Excreted during Exercise of the Lower End of the Upper Limbs.* (Archiv f. Anat. und Physiol., Jahrgang 1861, p. 131.)
11. H. Bamberger: *Is Ammonia a Normal Constituent of the Urine?* (Würzburger Medic. Zeits., vol. i. p. 146; and Meissner's Bericht, loc. cit., p. 300.)

1. C. Schmidt gives in his researches on lymph and chyle* a calculation of the nutrition and tissue-change in a foal. With an ingestion of 1·79 kilog. hay, and 0·64 kilog. oats, and 3·819 kilog. water for 100 kilog. weight of body, the animal gained every day 0·58 kilog. in weight; the excretions

* Conf. this Journ. No. lvii., p. 224. 1862.

amounting at the same time to 0·317 kilog. urine and 3·401 fæces. On these data, and on further analysis, the author bases the following calculation. Ingestion in grammes (without the water drunk):

	Un-dried.	Exic-cated.	Water.	Carbon.	Hydro-gen.	Nitro-gen.	Oxygen.	Mineral constit.
Hay	1790	1543	247·0	706·8	77·1	23·1	597·1	138·9
Oats	640	543·4	96·6	275·5	34·8	12·0	199·4	21·7
	2430	2086·4	343·6	982·3	111·9	35·1	796·5	160·6

Expenditure through rectum and kidneys:

	Un-dried.	Water.	Exic-cated.	Carbon.	Hydro-gen.	Nitro-gen.	Oxygen.	Mineral constit.
Urine	317	245·1	71·9	25·9	2·7	9·0	8·1	26·2
Fæces	3397	2559·7	837·3	325·6	42·9	18·5	317·2	133·1
	3714	2804·8	909·2	351·5	45·6	27·5	325·3	159·3

Ingestion:

Inclusive of water drunk	6250·3	4162·6	2087·7	982·3	111·9	35·1	796·5	161·9
Assimilated & expired	2536·3	1357·8	1178·5	630·8	66·3	7·6	471·2	2·6
Assimilated	580·	462·7	117·3	78·1	11·4	7·6	17·6	2·6
Expired	1956·9	895·1	1061·2	552·7	54·9	—	453·6	—

Additional oxygen required = 1473·9 grm.

With the air expired are . . 895·1 grm. preformed } water.
494·1 „ generated }

1389·2 „ vapour of water.
2026·6 „ carbonic acid.

The oxidation produces 4,268,940 units of warmth.

2. Bartsch examined, at the suggestion of H. Nasse, the ingesta and egesta, and the alteration of weight of body in infants, from the first to the eighth day of life. The infant receives on the first day scarcely any food, and loses weight, but on the second day there is, in general, already an increase of weight. The quantity of food ingested rises from 20 grammes in the first twenty-four hours to 750 grammes on the eighth day; the mean quantity of food for twenty-four hours was on the first day 20 grammes (four observations), on the third day 162 grammes (four observations), on the sixth day 500 grammes. The increase in weight for 100 parts of milk consumed averaged in the infants two days old, 32 parts, in those three days old, 31, in those four days old, 13, and in those five days old, 11 parts. For 1000 parts of body an infant of one day takes 6, of two days 56, of three days 111, of four days 125, of five days 125, of six days 126, and of eight days 209 parts of milk daily. The mean daily expenditure changes from 118 to 693 grammes, and for 1000 parts weight of body from 41 to 211.

The expenditure through the lungs was calculated from the loss of weight between two weighings, the infant having been, in the mean time, enveloped in gutta percha; it amounted in an infant, five days old, to 4·5 grammes in an hour, in one six days old, to 3·6 grammes (three observations), and in an infant seven days old, to 3·7 grammes (four observations).

3. Voit's researches consist of four separate papers, in addition to an introduction of twenty-seven pages—viz., 1. On the Influence of the Culinary Salt

in the Food on the Processes of Tissue Change; 2. On the Influence of Coffee on the Animal Organism; 3. On the Manifestations of Animal Force in their Relation to the Tissue Change; 4. Appendix, with remarks on the precautions necessary in experiments on tissue change. Voit's work forms a continuation of the researches described in Bischoff and Voit's 'Laws of Nutrition in Flesh-feeders.'* The first and third series of experiments were even performed on the same animal which had been the subject of those experiments. *Culinary salt* is, according to the author, a powerful stimulator of the metamorphosis of tissue; it increases, by means of its physical properties, the capillary circulation of fluids in the organism; it increases the oxidation of albumen, and through this the quantity of urea excreted. Culinary salt is also a true diuretic. In order to excrete the salt from the body, water is required; this water passes always through the kidneys (the only channel for the excretion of culinary salt in the dog), and is, if the supply of water from without is limited, abstracted from the tissues.

Voit's experiments with *coffee* on a dog led to the inference that coffee does not, as is usually assumed, diminish the metamorphosis of nitrogenous tissue, and the excretion of urea, but, on the contrary, rather increases these processes. On the whole, the dog appeared to be more lively after the use of coffee. The author made also experiments with *coffein* on frogs, and found it to cause, at first, increased irritability of the nervous system, a tendency to reflex-movements and to tetanic convulsions; later, however, phenomena of paralysis. The pupil becomes dilated; the capillary vessels are filled with blood; the heart's contractions are at first increased, later reduced in frequency, they are arrested during the tetanic paroxysms. The author attributes the principal effects of coffee to its action on the nervous system, not to its influence on the tissue-change. The nervous system being rendered more susceptible, the same exciting cause produces a greater effect. Coffee thus refreshes, Voit thinks, the fatigued body, renders the lassitude less perceptible, and in this manner enables us to endure prolonged exertion. The experiments on the influence of *bodily exercise* (tread-wheel) on the tissue-change in the well-known dog lead to the unexpected result, that the excretion of urea was not at all, or only very slightly, increased by bodily labour. Voit infers, therefore, that muscular action does not cause increased decomposition of albuminous substances, while it is accompanied with a greater consumption of fat. As the decomposition of albumen is not the source of the production of force, connected with muscular contraction, Voit is inclined to look for it in the development of electricity.

4. L. Lehmann's researches on the influence of bodily exercise carried to fatigue were made on five healthy individuals of different ages. The exercise consisted principally in climbing hills and in dancing. The weight of body exhibited, in general, after such exercise a decided decrease, which amounted in one instance to $\frac{1}{40}$th of the whole weight of body, in spite of a liberal supply of food; but in some instances no decrease of weight was observed. The quantity of urine is in general, but not always, diminished. The gaseous excretions are, in most persons, considerably augmented. No distinct alteration was observed in the fæcal excretions. The quantity of urea was not always increased, and never considerably so. The amount of uric acid was decidedly not diminished, but rather augmented. The quantity of culinary salt in the urine was sometimes above, sometimes below the average; that of sulphuric acid almost always above, that of phosphoric acid not regularly so. The earthy phosphates and the free acidity were not perceptibly affected. The frequency of the pulse is above the average, as also that of the respiratory movements. The temperature is, in the majority of instances, immediately

* Conf. this Journal, vol. xxviii. p. 125. 1861.

after the fatiguing exercise increased, the difference amounting to from $\frac{1}{8}°$ to almost $\frac{3}{4}°$ F. While thirst becomes generally augmented, the sensation of hunger is often diminished.

5. Speck's experiments are made on a single individual, a healthy agricultural labourer, twenty-eight years of age. He communicates five series of observations; the first (nine days) shows the influence of bodily exertion with plentiful food; the second (nine days), that of rest with the same food; the third (four days), that of profuse perspiration, caused by warm covering while in bed, the food remaining the same; the fourth (four days), that of exertion with scanty food; the fifth, that of rest with scanty food. Speck, too, observed considerable decrease of weight of body during exertion, in spite of liberal ingestion of food. The metamorphosis of tissues is increased during the period of exertion, but becomes much diminished during the time of rest following the exertion. During the period of exertion the amount of excretions through urine, lungs, and skin, is larger in the day-time, smaller in the night, while the opposite occurs during the period of rest. The total amount of insensible perspiration is much increased by exertion, that of urine is rather diminished. The total excretion through the urine of urea, uric acid, culinary salt, sulphuric acid, and phosphoric acid, is increased by exertion. It appears a remarkable fact that the body in the same number of hours gained more weight during the time of rest, with profuse perspiration (third series), than during the time of rest without perspiration. The weight of body became diminished while the perspiration lasted, but it increased considerably from the cessation of the perspiration up to the commencement of the following period of perspiration. The total amount of excretions within twenty-four hours is much lowered by a profuse perspiration of several hours' duration; the quantity of urine is below, that of the excretion through lungs and skin is above, the average. The quantity of fæces was largest during the period of profuse perspiration, less during the period of rest, smallest during exertion. Speck found diminution of the average daily temperature through exertion, increase through rest. The respiratory movements are increased in frequency through exertion (more so when combined with plentiful food), decreased through perspiration. The mean frequency of the pulse sinks below the average through exertion, although it is increased while the exertion lasts; it becomes increased through rest and through several hours' perspiration. The loss of weight through exertion is caused not only by the waste of nitrogenous, but also by that of non-nitrogenous, substances. The loss was smaller when food was liberally supplied soon after the cessation of the fatiguing work.

6. M. Traube understands by the term *respiration* the process by which every single organ is provided with that amount of oxygen which is required for its nutrition and for the maintenance of its activity. In another place he defines respiration as "that physiological process by which, through the combustion of the alimentary substances in the animal organism, the latent forms of these substances become fully developed." From the consideration of the experiments of Lavoisier, and of Regnault and Reiset, on the relation of temperature, muscular action, digestion and size of body, to the consumption of oxygen by warm and cold-blooded animals; from the habits of various animals in cold and warm climates; from the so-called "respiration" of muscles and other organs; and from the phenomena observed on plants and eggs (organisms uninfluenced by nerves); from the consideration of all these subjects, Traube concludes that the production of heat is by no means the principal purpose of respiration, but that the two most important functions of respiration consist in the formation of cells, and in the development of muscular activity. With regard to the chemical action of the muscle in the respiratory process, the author's view is, that the oxygen passing from the blood through the capillary walls forms with the muscular fibre a loose chemical combination; that the

fibre in this state is able to give off the oxygen to other substances contained in the muscular fluid, and possessing a greater affinity for the oxygen. The muscular fibre acts, therefore, according to Traube, as a kind of ferment, which undergoes itself during this action no decomposition, but causes such to take place in the surrounding fluid. On this theory the following inferences are based: 1. The muscle is an important factor in the chemistry of respiration, by transferring the oxygen of the blood to combustible substances. The muscular system forms one of the principal foci of combustion and production of heat; 2. The organized part of the muscle is not destroyed by its action; 3. The substances, by the chemical alteration of which the muscular activity is maintained, need not, as Liebig assumes, be albuminous, but may be non-nitrogenous; 4. Urea can, therefore, not be regarded as a measure of the muscular force developed; 5. Albuminous bodies are not decomposed by the muscular activity.

By comparing M. Traube's fifth inference with the result obtained by Voit in his experiments on muscular exertion, it will be seen that the former author arrived by reasoning at the same conclusion to which the latter was led by experiment, however widely their theories otherwise may vary. L. Lehmann's and Speck's observations, too, coincide in so far with those of M. Traube and Voit, as they show that the amount of muscular force developed does by no means correspond to the quantity of urea excreted through the kidneys.

7, 8, and 9. Pavy has examined the influence of the injection of phosphoric acid into the blood on the appearance of sugar in the urine. When he had injected only a small quantity, no sugar was found in the urine; in several experiments the injection of a large quantity (an ounce and more) caused immediate death; but in two instances, "when the injection was pushed to the fullest extent that the animal would safely bear, a saccharine state of the urine was the result." Twelve and eighteen drachms of the pharmacopœia phosphoric acid had been injected in these cases. The author ascribes this result to a "perverted condition of the processes belonging to the liver," the presence of the acid in the blood inducing the change of the amyloïd substance into sugar. The author has also succeeded in producing a diabetic state of the urine by injecting the acid into the duodenum and upper portion of the small intestines, having previously failed to obtain this result by injection into the portal vein, on account of the plugging of the veins caused by the action of the acid on the portal blood.

On Pavy's researches regarding the influence of alkalies in checking artificial diabetes, and in causing the disappearance of hepatine, we have reported in vol. xxviii. p. 243, of this Journal.

10. Bergholz found that he secreted, during exercise of the lower limbs, more urine than he did during the same space of time while remaining quiet, and during an equal number of pulsations. During exercise of the upper limbs only, on the contrary, he secreted less urine than during rest, in the same space of time, and calculated for the same number of pulsations. The author is of opinion that motion of the lower limbs causes increased pressure in the renal arteries. He ascribes also in dropsical affections to the motion of the lower limbs diuretic influence.

11. Bamberger was unable to find ammonia in fresh, healthy urine. He tried as well the test of chloride of platinum, as also that of a solution of hematoxylin, by which he had been able to discover the smallest trace of ammonia, purposely added to the urine.

IV. Nervous System.

1. *The Brain, and its Use.* (Cornhill Magazine, April, 1862, p. 409.)
2. W. H. Flower: *Observations on the Posterior Lobes of the Cerebrum of the Quadrumana, with the description of the Brain of a Galago.* (Proceed. Roy. Soc., vol. xi. p. 376, 1861.)
3. Thos. B. Peacock: *Tables of the Weights of the Brain and of some other Organs of the Human Body.* London, 1861. (Reprinted from the Monthly Journal of Medical Science, vol. vii. (N.S., vol. i.), 1847.)
4. Thos. B. Peacock: *On the Weight and Specific Gravity of the Brain.* (Pathological Transactions, vol. xii. p. 27, 1861.)
5. A. Meyer: *On the Sensorial Functions of the Spinal Marrow.* (Prager Vierteljahrschrift, vol. i. p. 44, 1861.)
6. J. van Deen: *On the Insensibility of the Cerebro-spinal Centres to Electrical Irritation.* (Moleschott's Untersuchungen, vol. vii. p. 380.)
7. A. Waller: *Experimental Researches on the Functions of the Vagus and the Cervical Sympathetic Nerves in Man.* (Proceed. Roy. Soc., vol. xi. p. 302, 1861.)
8. A. Waller: *On the Sensory, Motory, and Vaso-Motory Symptoms resulting from the Refrigeration of the Ulnar Nerve.* (Proceed. of the Roy. Soc., vol. xi. p. 436, 1861.)

1. The author of 'The Brain, and its Use,' gives a philsophical view of several questions connected with the anatomy and physiology of the brain. We must refer our readers to the essay itself, mentioning here only the author's original notion with regard to the connexion of the brain with consciousness: "Since in the instance of the brain it is undeniable" (he says) "that material actions depend on mind, may we not accept this as a rule of all material actions? What is once, surely may be always. The brain then would not differ from other material existences in being connected with feeling and with thought, but would be distinguished merely by being connected with thought and feeling that are *ours*. It reveals to us, so, the law of all matter—to be ruled and moved by mind: but the brain alone is thus moved directly by our mind. Of the mind that rules and uses the rest of nature we are not conscious; it is not 'we,' but it is not therefore non-existent. The brain seems to us, then, so strange an exception in nature, because only at this one point do we rightly perceive it as an instrument of consciousness."

2. W. H. Flower has examined the brain, and especially the "posterior cornu of the lateral ventricle" and the "hippocampus minor," in several animals belonging to the three families of the order *Quadrumana* (Catarrhina, Platyrrhina, and Strepsirhina). The author closes his communication by the remark, that "many links are still wanting in the chain of evidence required to determine the true history and classificatory value of the posterior horn of the lateral ventricle, and the peculiar disposition of cerebral substance constituting the hippocampus minor; but the conditions in which they have been found at so many distant points of the series, appear to lead almost irresistibly to the following conclusions:—1st. That these parts, so far from being (as has been stated by some anatomists) peculiar to the human brain, are common to man and the whole of the quadrumana, including even the lowest forms. 2. That they attain their maximum of development in species which do not belong to either extremity of the series. 3. That in the lower forms their diminution takes place chiefly in the antero-posterior direction, corresponding with the reduced length of the posterior cerebral lobes, the greater part of which is occupied by them. 4. That in the higher forms they are narrower in proportion to their length, and bear a similar ratio to the surrounding mass of cerebral substance. 5. That the extreme of the last condition is met with in

man, where these parts are also characterized by their variability in size and form, want of symmetry on the two sides, and frequent rudimentary condition, or even entire absence.

3 and 4. T. B. Peacock has republished some very valuable tables, based on 356 weights of the brain, from his own observation and that of the late Professor Reid. He now adds further tables on the weight of the healthy encephalon and its several portions, based on the examination of 36 male and 21 female bodies, and of some diseased brains. He, besides, took the specific gravity of the healthy brain in 9 male and 6 female individuals, and also of some diseased brains. While Sankey* and Bucknill† ascertained the specific gravity of the brain by placing pieces of it in solutions of common salt and of Epsom salts, Peacock's observations are made "by first weighing the brain and its several portions in air, and then in distilled water, and calculating the specific gravity by the common formula—viz., as the weight lost by the brain in water is to the weight in air, so is the specific gravity of distilled water (1000) to the weight required." The following table shows the highest, lowest, and mean specific gravity of the encephalon and its several portions, according to the author's observations:—

MALES.

	No. of observations.	Ages.	Highest.	Lowest.	Mean.
Cerebrum	9	21 to 59	1·0381	1·03031	1·03488
Cerebellum	9		1·0448	1·0369	1·04162
Pons varolii and medulla oblongata	9		1·0461	1·0351	1·04006
Encephalon	9		1·0392	1·03211	1·03623

FEMALES.

	No. of observations.	Ages.	Highest.	Lowest.	Mean.
Cerebrum	3	23 to 66	1·5355	1·0349	1·0351
Cerebellum	3		1·0411	1·03807	1·03952
Pons varolii and medulla oblongata	3		1·0445	1·0368	1·0406
Encephalon	4		1·03734	1·0354	1·03616

From Peacock's observations and tables, the earlier and recent considered together, the following inferences may be deduced:—1. The weight of the brain in the adult male averages about forty-nine or fifty ounces avoirdupois, and ranges from about forty-two to nearly sixty ounces; the brain of the adult female is, in the average, about five ounces and a quarter lighter. 2. The brain increases in weight up to adult age, and again declines in advanced life. The view of some older authors (Soemmering, the Wenzels, Sir W. Hamilton), that the brain arrives at perfection at about the seventh year, is evidently incorrect. 3. "The proportion which the whole encephalon bears to the body varies greatly, according to the state of obesity or emaciation of the subject, but it generally decreases with the advance of life." In the recent series, the mean proportion in the adult male, from twenty-one to forty-four years of age, was 1 to 32·73, and the range from 1 to 23·5 to 1 to 37·9; in the adult female, from twenty-four to forty-two years of age, the mean was 1 to 39·2, and the range 1 to 29·3, and 1 to 45·8. In the former series the average proportion in adult males, from twenty-five to fifty-years of age, was 1 to 37·2, the range 1 to 25·2 and 1 to 79·98; in adult females the mean 1 to 33·5, the range 1 to 24·1 and 1 to 44·8. In twenty-three children under six years of age, the proportion ranged between 1 to 5·7, and 1 to 14·3. 4. The cere-

* This Journal, vol. xi. p. 240. 1853. † Ibid., vol. xv. p. 207. 1855.

bellum bears almost the same relation to the whole encephalon throughout the duration of life, at least after very early age. In the adult male the average proportion was 1 to 9·03, the range 1 to 7·7 and 1 to 10·2; in the adult female the average 1 to 8·9, the range 1 to 8·3 and 1 to 9·5.

5. A. Meyer examines the correctness of the views of Pflüger, Schiff, and other physiologists with regard to the sensorial functions of the spinal marrow. The author's experiments, for the description of which we refer to his essay, were performed on frogs and nearly allied animals. He sums up his inferences in the following theses:—1. Decapitated frogs are still capable of perception and of voluntary motion. 2. These functions depend on the medulla oblongata (which by the process of decapitation in frogs is left entirely or partly undestroyed), and not on the medulla spinalis. 3. The medulla spinalis of frogs and allied animals, as also of the higher vertebrata, and especially of man, is not endowed with the power of perception. 4. Further researches are necessary to decide the question whether the spinal marrow of the salamander, the lizard, the eel, and similar animals really possesses the faculty of perceiving.

6. Van Deen having formerly* shown that the spinal marrow is insensible to mechanical and chemical irritation, endeavours in the present essay to prove, that as well the spinal marrow, as also the medulla oblongata and the brain, are unable to propagate the electric stimulus, applied to them, to the motor nerves, provided the place of application to the centres is sufficiently distant from the motor nerves themselves to prevent their being directly influenced by the electric stream. For the accurate description of the experiments, performed principally on frogs, we must again refer to the original. In another series of experiments, the author shows also that the spinal marrow does not propagate the electric stimulus in a centripetal manner—viz., to the medulla oblongata and cerebrum. Van Deen concludes from these researches, and those alluded to, on mechanical and chemical irritation, that the function of the nervous centres is excited only through organic influences, which act on them either from other centres, or through the nerves.

7. While, hitherto, almost all the experiments on the function of the vagus and the cervical sympathetic nerves have been performed on animals, A. Waller describes the effects *on man* of "mechanical irritation by simple pressure on the trunk of the nerve." For this purpose he generally applied pressure of the fingers at the highest point of the neck behind the ramus of the lower jaw. "Generally the first sensation experienced," the author states, "is that of want of breath, which is followed by deep and laboured inspirations: this dyspnœa occurs in healthy persons in about a minute or two." At the same time disturbance of the heart's action may be perceived—viz., at first, increased number of pulsations with decreased power, and later, retardation to about four or five beats per minute below the initial number, with further decreased force of the pulse. Slight uneasiness over the precordial regions often accompanies these motory phenomena. The heart's action may be so much enfeebled as to produce syncope. The stomach symptoms are uneasiness over the stomach, sometimes nausea. In the intestines borborygmi were observed. Tingling and heat, lasting in some instances for half an hour after the removal of the pressure, have been noticed on the corresponding ear. In only one instance the pupil was affected, becoming at first dilated, and, after removal of the pressure, contracted. All the above symptoms quickly disappeared after the removal of the pressure, leaving, however, a degree of lassitude lasting for some hours.

8. In the second communication, Waller mentions that he considers most,

* On the Insensibility of the Spinal Marrow to Foreign Impressions: Moleschott's Untersuchungen z. Naturlehre, vol. vi. p. 297; and this Journal, vol. xxvi. p. 241.

if not all, of these symptoms produced by compression of the vagus as the result of reflex action. The experiments on the ulnar nerve were performed by the application of ice or of water of 0° Cent. (32° Fahr.). The principal results obtained are: 1. The first effect of the application of cold to the ulnar nerve on the sensory function is a state of hyperæsthesia in the course of the nerve. 2. This hyperæsthesia is succeeded by a state of quiescence or freedom from pain, which is followed by anæsthesia. 3. The first effect on the motory functions of the nerve is that of increased excitability. 4. This increase in its turn subsides, until all the parts below the refrigerated portion of the nerve are paralyzed, owing to the interruption of its conductivity. 5. In this paralytic condition the muscles remain contracted to a certain extent, and the inner fingers in a state of flexion. 6. The first thermal effect of cold on the nerve is frequently a fall of temperature of 0·5 Cent. at the inner fingers below that of the outer fingers. 7. As the nerve becomes paralyzed, the temperature of the two inner fingers rises above that of the outer to the extent of 5° or 6° Cent., owing to the paralysis of the vascular nerves and vessels of the parts supplied by the ulnar nerve. In cold weather a much greater difference may be found in the temperature of the two sides. 8. While the temperature of the inner part of the hand is rising, that of the outer decreases on account of the diversion of part of the blood of the radial into the ulnar artery. 9. After the removal of the refrigerating body, the nerve quickly regains all its original powers, except that the normal equilibrium of temperature of the two sides of the hand is but slowly restored. It is probable that this is to be attributed to the slowness of contraction of the organic muscular fibres of the ulnar artery.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
Member of the Royal College of Physicians, Physician to the St. Pancras and Northern Dispensary, London.

I. *On the Iodide and the Oxy-iodide of Antimony, and on the Therapeutical Action of these Substances.* By Dr. Van den Corput, of Brussels. (Bulletin Général de Thérapeutique, Jan. 30th, 1862.)

Dr. van den Corput has employed in his practice for more than a year the different compounds of iodine with antimony, and the oxy-iodide in particular has yielded such remarkable results that he considers it to be one of the most active of the antimonial preparations. The iodide of antimony is obtained by carefully heating in a glass retort one equivalent of powdered metallic antimony with three of iodine. The mixture soon fuses in the form of a thick liquid of a deep brownish-red colour, which is the iodide of antimony; and on cooling it solidifies into a mass which has a metallic fracture, and furnishes a powder of a brick-red colour. If great heat is employed, the iodide is volatilized without decomposition, and is condensed in the form of shining translucent scales. The reaction takes place with the disengagement of heat, and may lead to an explosion if too large a quantity is operated upon at one time, and therefore it is better to add the metallic antimony gradually to the iodine. The iodide of antimony, when in contact with water, is decomposed in the same manner as the chloride, into soluble hydriodic acid and a pulverulent yellow precipitate formed of hydrate of oxide and iodide of antimony, analogous to the powder of Algaroth. Alcohol also decomposes it by removing iodine. The oxy-iodide of antimony is the only chemical form in which the combination of iodine and antimony can be conveniently used for internal administration, since by the contact with the liquids of the digestive canal the iodide of antimony is decomposed, as it is in water, into insoluble oxy-iodide of antimony and hydriodic acid. The oxy-iodide of antimony may consequently be obtained by rubbing up iodide of antimony with water, and thus decomposing it into hydriodic acid and oxy-iodide of antimony of a bright yellow colour; but it is better to prepare this compound by adding an acid solution of chloride of antimony to a solution of iodide of potassium. A precipitate is immediately formed of a beautiful lemon-yellow colour, which after a few minutes changes to an orange yellow. When the decomposition is complete, the precipitate is collected on a filter, washed, and dried. It is decomposed by most of the acids; hydrochloric acid dissolves it, setting the iodine free; and caustic alkalies also change it by combining with the iodine. When exposed to heat, it is resolved into antimonious acid and iodide of antimony, which is volatilized.

The researches of Dr. Van den Corput have convinced him that the iodide of antimony is chiefly adapted for external application as a revulsive. The irritating properties of this salt resemble those of tartarized antimony; while the oxy-iodide, corresponding in its composition to Kermes' mineral, produces internally an action analogous to that of the last-named preparation, although its special effects are much more powerful. The oxy-iodide is, in fact, a drug of great efficacy, being at the same time an expectorant and a powerful alterative. When suspended in a mucilaginous vehicle, in the dose of from 5 to 25 centigrammes (a centigramme is ·1543 of a grain), it frequently excites at first nausea and sometimes vomiting, at other times it causes frequent and copious stools. The effects may be easily moderated by the addition of opiates or some other narcotic agent which is capable of deadening the susceptibility of the stomach. Tolerance appears to be established more readily, as in the case of tartarized antimony, by doses raised from 20 to 50 or even 70 centigrammes in twenty-four hours. In general, when taken in such doses, the drug excites at first a great diaphoresis, soon followed by diminution and considerable depression of the pulse. The number of inspirations is diminished in frequency, and this effect is accompanied by extreme muscular weakness. The oxy-iodide of antimony is particularly serviceable in inflammation of the parenchyma of the lungs, and especially in the second stage of pneumonia; also in the treatment of subacute bronchitis and of œdema of the lungs. Its alterative and diaphoretic properties are also manifest in the treatment of acute rheumatic affections, as well as in certain inflammatory diseases of the heart. As to the iodide of antimony, its employment must be limited to the outside of the body. When applied to the skin, in the form of plaster or ointment, it produces an energetic revulsion, by causing on the surface a pustular eruption similar to that produced by tartarized antimony. But it has this advantage over the latter, that, independently of its local derivative action, it operates besides on the organism in a general manner by giving up a part of its iodine, which is then either directly absorbed, or by being vaporized by the heat of the body, surrounds the patient with an iodized atmosphere.

II. *On the Employment of Perchloride of Iron in Dysentery.* By Dr. BAUDON. (Bulletin Général de Thérapeutique, November 30th, 1862.)

Dr. Baudon writes a brief account of his experience of the use of perchloride of iron in the dysentery which prevailed last summer in some parts of France. He employed the medicine in twelve cases of well-marked dysentery, characterized by tormina and tenesmus, stools partly bloody, partly green, like boiled sorrel, twenty, thirty, and forty evacuations in the day. In general the disease was subdued in from four to eight days after the attack. The treatment was as follows:—Two injections, and sometimes three, were thrown up daily (after the intestine had been previously evacuated by copious enemata of tepid water), each injection containing a wine-glassful of water, with the addition of twelve to twenty-five drops of solution of perchloride of iron, with the addition of laudanum if the pain was very intense. The patients also took a draught every day containing twelve to thirty drops, as a maximum, of the solution of the perchloride; and the diet was of a very scanty kind. Dr. Baudon states that he seldom found the symptoms to continue beyond two days and a half to three days under this treatment.

III. *On the Antagonistic Effects of Opium and Sulphate of Quina.* By Dr. N. NIVISON. (American Journal of the Medical Sciences, July, 1861.)

Dr. Nivison, while admitting that the effects of opium and quina are in great measure antagonistic to each other, thinks that the most favourable therapeutical results are often obtained by their conjoined operation. Among other reasons in favour of the administration of these remedies in conjunction, Dr. Nivison adduces the following. The free use of opium often tends to reduce the biliary and renal secretions to a dangerous extent, but when combined with quina, this tendency is in a great measure counteracted. Opium also frequently reduces the respiratory action to such an extent that the blood may be only imperfectly aërated, but the contemporaneous use of quina counteracts this effect, while the desirable results of the opium are obtained. In many cases of extreme exhaustion, as from protracted hæmorrhages and the like causes, opium is employed as a stimulant, but it often produces unpleasant narcotism, an effect which may be avoided by the joint administration of quina. Again, quina is often given to relieve congestion by its power of causing contraction of the capillaries, but it sometimes only excites the general circulation; now the addition of opium controls this excitement, and ensures the legitimate operation of the quina. Neuralgia is palliated by opiates, and sometimes cured by quina, but cases of this disease will often yield more speedily to the combined influence of opium and quina than to any other known remedies. Dr. Nivison also thinks that idiopathic fevers may be successfully treated by opium and quina in combination.

IV. *On the Therapeutical Uses of Phenic Acid.* (Bulletin Général de Thérapeutique, May 30th, 1861.)

According to the researches made at the Hôpital St. Louis, in Paris, in the clinical wards of M. Bazin, and communicated to the Académie des Sciences by M. J. Lemaire, the following solution would form a very economical mode of treatment in scabies and porrigo: phenic acid, 1 part; acetic acid, 40 parts; water, 100 parts. In porrigo, a compress dipped in this solution is applied once a day. In scabies, a single lotion is sufficient for killing the acarus. The acetic acid is added to the preparation, in order to make it penetrate the epidermis and reach the base of the hair-bulbs. The phenic or carbolic acid is a product obtained from the distillation of coal, and consequently has no great commercial value, and M. Lemaire has, therefore, endeavoured to apply it to hygienic purposes. Its most important use is in the preservation of dead bodies. M. Lemaire states that a man's body may be kept in an excellent state of preservation for less than fifty centimes.

V. *On the Use of Nitrate of Silver in Acute Dysentery.* By Dr. Duclos, of Tours. (Bulletin Général de Thérapeutique, August 15th, 1861.)

Dr. Duclos considers that dysentery always commences at the lowest part of the large intestine, and if such be the case, then he argues that the local medication of this part must arrest the disease and prevent its extension, or at least must modify its virulence even in the worst cases. Local treatment being, therefore, admissible in this disease, the beneficial effects will be in proportion to its employment at the earliest period. Dr. Duclos was induced to employ nitrate of silver as the remedial agent, from observing its well-known efficacy in purulent ophthalmia, aphthous eruptions in the mouth, and other such affections, and also from witnessing the practice of M. Trousseau, who employs this salt with great success, both by the mouth and in injections, in cases of simple enteritis in very young children. M. Duclos treats dysentery in the following manner: a slight laxative is first administered, such as a small dose of calomel or seidlitz powder, in order to unload the intestine of fæcal matter, and then a simple mucilaginous enema is thrown into the rectum. When the laxative and the injection are both evacuated, an enema is administered, consisting of twenty-five centigrammes of nitrate of silver (a centigramme is ·1543 of a Troy grain) in one hundred and fifty grammes of water (a gramme is about fifteen grains), together with some laudanum if necessary, but the latter is only added if it is quite impossible to retain the enema for a few seconds. The enema is kept in the rectum for from four to ten minutes and then allowed to return, and as it has been administered immediately after an evacuation, it is certain that the silver salt has been in direct contact all the time with the diseased surface. An enema is thus given night and morning, and M. Duclos has even given three in twenty-four hours; and this treatment is continued for several days, with the double view of avoiding constipation, which is the habitual attendant of true dysentery, and of giving the enema immediately after a dysenteric evacuation has cleansed the last portion of the intestine. The results of this treatment are, that in general the stools are rapidly modified both in frequency and composition. Most frequently, from the period of the fourth or fifth enema, they become less frequent, and also less bloody and slimy; the tenesmus also diminishes with great rapidity, and the pain produced on pressure of the left side is less acute. No bad results were observed to follow this treatment, but sometimes an excessive irritability of the anus or the rectum rendered it necessary to change the form of administering the injection. The dose of the nitrate of silver was then reduced, or a greater or less quantity of laudanum was added to secure its tolerance by the intestine. Dr. Duclos has employed the nitrate of silver in the manner described in a great number of cases of dysentery, and often with great success, and he records five cases at length. His views have been confirmed by the observations of other practitioners, and they may be summed up as follows: 1. The nitrate of silver, administered at the commencement of acute dysentery, in appropriate doses, constitutes a remedial agent which has produced very excellent results in Dr. Duclos's hands; 2. Its action has been equally efficacious, whatever may have been the age of the patient, and in all the forms of dysentery; and 3. In certain cases, when administered quite at the beginning and always in injection, it has seemed to exert, at the end of a few days, an almost abortive action upon the disease.

VI. *On the Excretion of Mercury during and after Mercurial Treatment.* By Dr. F. C. Schneider. (Medizinische Jahrbücher, 1861, No. 3.)

Dr. Schneider has instituted a number of experiments and investigations in order to determine the question of the excretion of mercury after mercurial treatment. He has found that the most delicate test of the presence of mercury in the liquid state is sulphuretted hydrogen; but when the mercurial preparation is contained in the urine, it is not detected so readily as in pure water. Another very delicate test of the presence of mercury is obtained by the electrolytic precipitation of the metal from its solution. By this method the smallest portion of mercury may be separated from the most dilute solutions and rendered accessible to chemical reagents. The latter are indispensable for the detection of the mercury, because the electrolytic deposit is generally not sufficiently evident to obviate the necessity for further tests. By the application of galvanism the mercury is deposited upon the cathode, when amalgamation takes place, and the presence of quicksilver may be further rendered evident by converting it into vapour and bringing it into contact with the vapour of iodine, which produces the well-known scarlet iodide of mercury.

After showing the possibility of determining the presence of mercury, by analysis, in organic fluids, Dr. Schneider proceeds to record his investigations upon the human subject. He accordingly gives the results obtained by him from the urine, first, of individuals affected with secondary syphilis, but who had never been treated by mercury; and, secondly, of some who were affected with syphilis, and who had either undergone mercurial treatment a long time before the experiments, or who were still undergoing the treatment, or who had been taking iodide of potassium after the mercurial treatment. In some other cases, which terminated fatally, Dr. Schneider was able to examine not only the urine, but also the liver and the brain; and in one case, pieces of bone, the liver, spleen, kidneys, and brain were all subjected to experiment.

In the urine of syphilitic patients who had never undergone any mercurial treatment, no quicksilver was detected by electrolysis; and in cases where the mercury was rubbed into the skin, no traces of the metal were found in the urine. During the internal use of the mercurial preparations, however, the urine constantly contained quicksilver. Within two years Dr. Schneider has examined fourteen cases of this kind, and has always obtained positive results. The excretion of the mercury lasts for some time after the end of the treatment, and Dr. Schneider has constantly found it in the urine a week afterwards; in one case he found it four weeks afterwards, and in another case six weeks afterwards. With regard to the effect of iodide of potassium on the mercurial treatment, his experiments do not favour the generally received opinion that the internal use of the iodide facilitates the excretion of mercury from the system. He has made a number of experiments during the last two years in order to determine this point, and has examined the urine of persons who have taken the iodide of potassium both during and after a mercurial course. When the iodide of potassium is taken immediately after a mercurial course, the urine contains mercury, but the same is the case when the iodide is not employed. In a fatal case in which death occurred during the mercurial treatment, Dr. Schneider subjected the liver and the brain to chemical examination, and found mercury in both, but the liver contained more than the brain. In another case, in which the patient died of pericarditis some weeks after the cessation of the mercurial treatment, various parts of the body were examined chemically, but the kidneys alone gave very slight traces of mercury, and the solution obtained from the liver gave only a doubtful reaction.

VII. *On the Medical Properties of the Wild Thyme, and particularly of its Use in Spasmodic Cough.* (Revue de Thérapeutique, February, 1862.)

M. Joset states, that by the simple administration of an infusion of wild thyme, slightly sweetened and mixed with gum, he has observed the improvement and even the cure, as if by enchantment, of cases of hooping-cough, taken indifferently at all the periods of the disease. The same was the case in stridulous sore-throat, and in convulsive and catarrhal coughs. In the worst cases of hooping-cough the pathognomonic paroxysms, although they did not entirely disappear at the end of a few days, became so much modified in their character, that the disease resolved itself into a case of simple bronchitis, which was easily treated. These remarkable cures, so rapidly effected, and obtained only by the administration of wild thyme, have led M. Joset to look upon this plant as a sovereign remedy, and in some degree a specific one, in the affections of the air-passages. The employment of this plant is not a novelty, for it was formerly recommended very extensively in the treatment of obstinate coughs, and it enters into the formation of some popular powders and syrups. M. Joset advises it to be given in the form of a concentrated infusion, slightly sweetened, to be taken in any quantity which the patient can drink, and until the desired effect is produced. The favourable result has generally ensued at the end of a very few days.

VIII. *New Mode of Treatment of Local Gangrene by Oxygen Baths.* (Bulletin Général de Thérapeutique, May 30th, 1862.)

M. Réveil has analysed the gangrenous parts in some patients, and he has arrived at the conclusion that the essential cause of gangrene is a diminution or absence of the oxygen necessary for the integrity of the life of a tissue. This theoretical opinion has hitherto led to no practical benefits; but lately M. Laugier, at the Hôtel Dieu, has founded upon it a mode of treatment which has been highly successful. A patient having one of his great toes partly mortified, and the skin under the ankle painful, altered in colour, and also threatened with gangrene, had his foot placed in a simple apparatus in which the disengagement of pure oxygen kept it continually in a bath of this gas. The rapid result was the arrest of the gangrene, and the return of the threatened parts to the healthy state. The removal of the eschar in the toe ensued, and the cicatrization was almost completed. Another patient came into the hospital attacked with spontaneous gangrene of the two last toes of the left foot; the adjoining skin, as far as the articulation of the foot with the leg, was red, painful, and threatened with mortification. The same mode of treatment was adopted, and after a few days the gangrene remained limited to the parts first attacked. The adjoining skin remained healthy and presented no more redness, the pain was very much diminished, and at the date of the last report there was reason to hope for a favourable result, although the patient, as in the former case, was seventy-five years of age. Hence M. Laugier concludes that, whether the theory advanced be true or not, oxygen-baths rapidly arrest, at least in certain cases, the progress of spontaneous gangrene of the extremities.

IX. *On the Relations of Secondary Syphilis to Chronic Hydrargyrosis.* By Dr. Overbeck, of Detmold. (Report in American Medical Monthly, April, 1862.)

The chief points of inquiry to which Dr. Overbeck has directed his attention are the absorption of metallic mercury and the effects and diseases produced by mercury. In making experiments by absorption with the ointment, he found that there were finely-divided globules of the metal not only in the skin and areolar tissue, but also in the intercostal muscles, the pleura, the fæces, the kidneys, the liver, the blood, the mucous membrane of the mouth, the fleshy substance of the heart, the brain, and the sediment of the urine, but not in the bones. The experiments were made on cats, dogs, and rabbits, on which animals a part of the thigh, chest, abdomen, or head was shaved, ointment rubbed in, and then a bandage applied, so that the animals could not lick at the part operated upon. The globules found by Dr. Overbeck had generally a diameter of from $\frac{1}{200}$th to $\frac{1}{2000}$th of a line, while in the ointment their diameter was on the average $\frac{1}{50}$th of a line. The absence of the mercury in the bones is a very remarkable fact, which becomes of additional interest by the results obtained in producing the mercurial cachexy in the lower animals. The chief question to be determined between mercurialists and non-mercurialists is whether there is a difference or not between the symptoms of secondary syphilis and of chronic hydrargyrosis. The most prominent constitutional affections following the administration of mercury in the lower animals were inflammation of the skin and of the conjunctiva, salivation, inflammation of the stomach and intestines, hyperæmia of the liver, the kidneys, the urethra, and the salivary glands, and certain changes of the blood. But in the bones no trace of hyperæmia, periostitis, caries, necrosis, or any other morbid alteration, could be discovered, although the whole of the bones of the animals operated upon were carefully examined. There are therefore no diseases of bones caused by mercury in the lower animals, and hence the conclusion may be drawn that there are none in man. Still, it may be objected that pathological phenomena might occur in man which are not observed in the lower animals, and therefore it becomes necessary to argue the question separately in man. The circumstance that in man particles of mercury visible to the naked eye have been observed in the bones, is not questioned by Dr. Overbeck, but he explains it by assuming that they were observed only after the bones had been macerated, while they never exist in the bones of the living body, nor immediately after death. By putrefaction the bloodvessels, the connective tissue, the fat, the blood globules, and all albuminous matters, are destroyed; and thus the mechanical impediments to the confluence of small particles of mercury are removed. The presence of the metal in macerated bones is therefore of no importance whatever. The only bones which may be really affected by mercury are the jaws, in which necrosis undoubtedly takes place after the administration of the drug; but this is evidently due partly to the direct spreading of inflammation from the mucous membrane of the mouth, and partly to the periosteum being laid bare, thus giving rise to periostitis and subsequent necrosis of the jaw-bones. Mercurialism and syphilis, according to Dr. Overbeck, are widely different morbid processes. The essence of chronic mercurialism consists of anæmia, the albumen and the blood-globules being diminished, the coagulability of the blood being increased, while the amount of water may be augmented or diminished. Mercurialism is not accompanied by plastic exudation of any kind, which is the very essence of constitutional syphilis.

X. *On the Therapeutical Employment of Croton Oil.* By Dr. Joret. (Bulletin Général de Thérapeutique, November 30th, 1861.)

Dr. Joret recommends very strongly the use of croton oil both internally and externally, and regards it as one of the most efficacious remedies in the domain of modern medicine. It is employed much more commonly as an external application, and it may be advantageously substituted for antimonial ointment, Burgundy pitch plaster, and the greater part of the external irritants used to produce an eruption on the skin. It is very useful in all cases of rheumatic pains, gouty rheumatism, arthritis, pleurodynia, sciatica, lumbago, and in pleurisy and neuralgia. Dr. Joret has also found it useful when employed by friction in laryngitis and in acute and subacute bronchitis. M.

Nonat has used it with great success in the same manner in acute and subacute enteritis; and M. Huguier has even recommended it to be rubbed on the abdomen in chronic affections of the uterus and its appendages, in order to prevent the inflammatory symptoms which may ensue from operations on that organ. Dr. Joret thinks that the only objection to the use of croton oil is its acrid and disagreeable taste, but this may be obviated by employing it in gelatinous capsules. He states that the oil may be advantageously given internally instead of any of the known evacuants; and that in all kinds of dropsy it should be used, for it always relieves, and sometimes cures, the disease; and that as a derivative it is preferable to antimonial ointment, the effect of which is more painful and much less rapid. That in children and old persons it is undoubtedly useful in the affections of the air-passages—as laryngitis, bronchitis, pleurisy, asthma, and phthisis; also in enteritis and entero-colitis, in all cases of muscular rheumatism, gout, and neuralgia; and, lastly, in certain affections of the uterus and its appendages.

XI. *A Case of Ulceration of the Velum Palati cured by the Arseniate of Soda.* (Bulletin Général de Thérapeutique, January 15th, 1862.)

M. Bouchut has lately recommended the arseniate of soda in cases of scrofula, and among the latter he relates one of a scrofulous perforation of the velum palati, considered to be syphilitic, in which the curative action of the arseniate was demonstrated in a decisive manner. The patient was a child, ten years old, of a scrofulous habit, with enlarged cervical glands; the velum palati was red, and presented on the right side an irregular perforation, with ulcerated and jagged edges; the uvula deviated to the left side, being drawn in this direction by the muscles which remained healthy. The iodide of potassium was administered from the first day of the child's admission into the hospital, and the dose was increased from time to time; but, although it was continued for a month, it produced no effect upon the disease. At last the arseniate of soda was prescribed in the dose of five milligrammes (a milligramme is about 0·0154 troy grain) every day, and it was raised gradually to fifteen milligrammes. This treatment was continued for two months, at the end of which period the ulceration existing around the perforation of the velum palati was completely cicatrized, and five or six weeks afterwards there was no morbid appearance discernible, except the perforation, which remained, and the general health was quite restored. In this case the syphilitic origin of the ulceration was suspected, but not proved. M. Bouchut does not attribute any specific action to the arsenic in the treatment, but he regards it as a tonic, and one of the best of its kind, since it stimulates the appetite and increases the molecular nutrition of the tissues. It thus improves the functions, which are languid in all scrofulous cases, and from this languor arises the chronic and obstinate character of scrofulous diseases.

XII. *On the Treatment of Peritonitis by the Continuous Application of Cold to the Abdomen.* (L'Union Médicale, April 3rd, 1862.)

M. Béhier, in a communication made to the Académie Impériale de Médecine, recommends the application of cold in cases of peritonitis. He at first relates the particulars of several cases of metritis and metro-peritonitis which he has seen rapidly cured solely by the influence of continued irrigations of cold water on the abdomen. He afterwards explains the results he has obtained in the treatment of puerperal diseases by applications of ice to the same part. M. Béhier, for this application, makes use of bladders of caoutchouc, filled with fragments of ice, renewed every two hours. The first effect of this measure is the rapid diminution of the pain, which does not return unless the application of the cold is interrupted. Since the month of October, 1858, 801 women have been confined at the Hôpital Beaujon, and of this number ice was applied to 355 women, of whom 244 presented only at the time of the application a swelling of the appendages of the uterus and slight pain, which rapidly disappeared; in 68 others the symptoms were more threatening, and there was a well-marked feverish excitement. Thirty-nine women died out of the whole number of those who were confined; but even in these cases there was some modification of the symptoms, for the duration of the disease was prolonged beyond that which it presented before the adoption of this mode of treatment. M. Béhier therefore hopes that the plan may prove useful in the peritoneal inflammation of child-bed; and he adds, that he has never observed any bad symptoms to result from this application, which interrupts neither the lochial secretion nor that of the milk.

XIII. *On Febrifuges.* By P. Garnier. (L'Union Médicale, December 21st, 1861.)

Besides quinine and arsenic, both of which are of established efficacy in the treatment of fevers, the stearate of quinine and soda has been recommended for external application. M. Thirault, of St. Etienne, has recommended a formula consisting of stearate of quinine and soda, with animal soap and glycerine; and from comparative observations made in hospital practice, it is found that the iatraleptic use of this ointment gives rise to the appearance of quinine in the urine on the second day of administration; whereas, when a similar dose of sulphate of quinine is used, it does not appear until the third or fourth day. The employment of the stearate of quinine in powder, thrown upon the surface of a blister placed upon the splenic region in a case of intermittent fever, was followed by a complete cure. When given comparatively with the sulphate in eight cases of well-marked and obstinate intermittent fever, it was found that with the sulphate the alkaloïd did not appear in the urine until the fourth day, while with the stearate it appeared on the second. Tannin has been recently recommended as a febrifuge by M. Leriche, of Lyons; and it appears to be of particular service in the paludal cachexia, in which it acts as a tonic and plastifying agent, for its specific action is doubtful. When associated with quinine, it may be substituted in many cases for the sulphate. In one case, which resisted sulphate of quinine, arsenical preparations, and change of climate, Dr. Thomas, of Tours, gave for six days fifteen drops of tincture of aconite daily, and the fever disappeared, but this was probably an accidental case of success. Lastly, M. Camerario mentions that the Urtica urens has cured, at Lecco in Italy, several cases of intermittent fever which resisted the sulphate and iodide of quinine. He has employed the leaves and the seeds in powder, in doses of one to two grammes (a gramme is fifteen grains) given before the paroxysm. The employment of this plant is said to be followed by a feeling of heat in the stomach and the head, and a general itching, as in urticaria.

XIV. *On the Value of the Expectant Treatment in the Pneumonia of Children.* By Br. Barthez. (Bulletin Général de Thérapeutique, April 30th, 1862.)

In consequence of the mild nature of simple pneumonia, MM. Rilliet and Barthez, together with M. Legendre, have been in the habit of leaving the disease in many cases to its natural course. They believed that they would thus cure the patients as well as by active treatment, and the results have justified their expectations. From the month of August, 1854, to the month of June, 1861—that is to say, for about seven years—M. Barthez has treated in the hospital 212 children attacked with simple pneumonia, among whom he had only two deaths, and in these last both lungs were affected. In half of these cases no active treatment was adopted; in many of the others only mild measures were recommended—such as an aperient, an emetic, and a bath; and about a sixth of the cases were subjected to somewhat active treatment. To this rather considerable number of cases, M. Barthez adds several more, which he has treated in private practice during the same period; so that he thinks he can determine the mildness of uncomplicated pneumonia in children so far as the city of Paris is concerned, whatever may be the seasons or the years, or the seat and extent of the disease, and whatever may be the treatment, whether active or insignificant, or none at all. He makes a reservation, however, for double pneumonia, which is the only form he has seen terminate in death in the proportion of 2 to 13. The patients particularly alluded to by M. Barthez were from two to fifteen years of age. Before this latter period simple hepatization is still most frequently cured, even when it is very extensive; but it has also been known to terminate in death. After fifteen years and up to twenty, M. Barthez also believes in the cure of the disease, so far as his recollection extends. On the other hand, the pneumonia described by him does not comprise all the inflammatory diseases of the lung, nor the pneumonia which supervenes during the course of fevers, nor that which accompanies tuberculosis.

The following are some of the general conclusions at which M. Barthez has arrived. Pneumonia, when left to itself, begins to terminate in resolution from the sixth to the eighth day from its commencement, and a slight course of treatment makes no difference in its progress. Bleeding appears to be contraindicated in this disease, and M. Barthez has remarked that several children who had lost blood were pale and emaciated during the whole period of a long convalescence. When resolution has once commenced the disease is very rapidly terminated, and one day is sometimes sufficient for the purpose, but more generally it occupies between two and six days. The extent of the inflammation has a great influence upon the duration of the disease; thus, when it occupies the whole of the organ, its progress is the most slow and its duration is the longest. Also double pneumonia takes more time to become resolved than the simple form. The conclusion which seems to M. Barthez to follow from his cases is, that in a child attacked with simple lobular hepatization, the best course is to adopt good hygienic measures, and to abstain from all active treatment, and especially from the repeated abstraction of blood, the evident effect of which is to weaken the patients unnecessarily, and considerably to protract their convalescence.

XV. *On the Preparation of Oxygenated Water, and its Therapeutical Use.* (Compte Rendu de l'Académie des Sciences, November, 1861.)

Dr. Ozanam gives the name of oxygenated water to water which is distilled and afterwards charged with oxygen under the influence of high pressure. The experiments he has made have led him to establish three modes of operation by this new medicine. 1. It improves the condition of the blood in cases where that fluid is impaired or deficient, as in dyspnœa, asthma, slow asphyxia, cyanosis, diseases of the heart, hæmorrhoïds, and hæmorrhoïdal visceral congestion. 2. It possesses an oxidizing or metamorphic action in cases where the organic products are arrested in their development, as happens in glycosuria, gout, the uric and oxalic gravel, and perhaps in scrofula. 3. It exerts an exciting and regulating action on the brain and the thyroïd gland, and hence its use in goître and cretinism. If, in fact, snow-water taken as drink gradually produces these morbid conditions, it is because it is entirely deprived of vital air. On the other hand, oxygenated water, as well as the inhalation of gaseous oxygen, produces no results in hemicrania, and unfavourable ones in cases of inflammatory disease. Thus, in croup, the oxygen temporarily tranquillizes the dyspnœa, but it increases the fever. In the treatment of ulcerated cancer the oxygenated water revives pretty well the powers of the patient, and the wounds assume a more vivid and rosy colour, but they do not heal; and if the surfaces are bathed with rags steeped in oxygenated water, even when very slightly charged, the ulcer is soon observed to become gangrenous on the surface. Oxygenated water is perfectly limpid and pure, and the gas is disengaged in the form of very fine bubbles. Having little taste, it resembles in this respect water which is deprived of air; and, like the latter, it is a little heavy for the stomach.

XVI. *On the Treatment of Puerperal Fever by the Sulphate of Quinine.* By Dr. Cabanellas. (L'Union Médicale, March 20th, 1862.)

In a paper presented to the Académie Impériale de Médecine, Dr. Cabanellas proposes to treat puerperal fever by the administration of the sulphate of quinine. He states that it has been successful in seven cases in which he employed it, and that he is convinced of its efficacy when it is used in isolated cases and at the commencement of the symptoms, and he has reason to hope that it may prove efficacious in epidemic visitations of the disease. He attributes the efficacy of the treatment in great measure to the mode of administration, to which therefore he directs special attention. All the seven cases which were treated were characterized by intense feverish excitement, tension and pain of the whole abdomen, or merely great sensibility on pressure over one or other ovary, and in two cases nausea and vomiting. In six of these cases he began by giving some ipecacuan, and after having applied emollient poultices over the abdomen, he waited until the temporary relief afforded by the emetic was succeeded by the return of the general and local symptoms. The day, or the second day, after the administration of the emetic he gave the quinine, in the dose of ten or fifteen centigrammes every hour day and night, with the most scrupulous exactness, even recommending that, dnring the first forty-eight hours the sleep should be disturbed, so as not to lose a single dose. At the same time the poultices were continued, emollient enemata were injected, if necessary, and the patients drank acidulated fluids when they were thirsty. At the end of twenty-four hours, the patients generally complained of some singing in the ears, but the quinine was hardly ever vomited, and the pulse began to fall. The improvement was more and more marked every day; the local symptoms were gradually relieved; and in a few cases the patients asked for broth and other kind of nourishment on the third day of the disease. These requests were granted, without interrupting the administration of the quinine every hour, but in proportion as the symptoms became more favourable the sleep was allowed to continue without interruption, though on the express condition that the medicine should be given as soon as the patients awoke. The singing in the ears, the deafness, and a little vomiting do not contra-indicate the continuance of the remedy; and Dr. Cabanellas has never observed these symptoms to be attended with bad consequences, and they moreover disappear as soon as the doses are discontinued or given at more distant intervals. When the absence of the feverish excitement has lasted four or five days, if the local symptoms are almost removed, the doses are given less and less frequently; and if the improvement continues, the medicine is no longer given. On two or three occasions it was necessary to return to the first doses after they had been suspended, and once the quinine was resumed in larger doses than at first. In the cases which were observed, the cure took place after a variable interval of from five to fifteen days. The sulphate of quinine appeared to Dr. Cabanellas to be equally serviceable in arresting the fever and diminishing the local symptoms in a case of phlegmasia dolens which supervened in the lower limbs of a female recently confined.

XVII. *A Pommade to prevent the Fall of the Hair.* By Dr. DAUVERGNE. (Bulletin Général de Thérapeutique, December 15th, 1861.)

The beneficial effects of resinous and balsamic preparations in the diseases of the hair and the hairy scalp have long been known, and recent researches in the clinical wards of the Hôpital St. Louis, in Paris, have confirmed the empirical recommendations of the use of these agents. Dr. Dauvergne has therefore often recommended ladies who were losing their hair to use tar-ointment, by means of which he has seen the hair in many cases resuming its strength after it has been affected with pityriasis, porrigo, favus, &c. But the disagreeable and penetrating smell of the tar was almost an invincible obstacle to its use; and Dr. Dauvergne has therefore endeavoured to conceal its odour by mixing it with perfumes of various kinds. He considers tar preferable in many cases to cantharides, inasmuch as the latter agent is inapplicable to the diseases of the hairy scalp, or of the hair-bulbs, or the hairs themselves. The pommade proposed by Dr. Dauvergne consists of tar, benzoin, musk, essence of patchouli, lard, &c.; and although in large quantity it retains a little smell of tar, yet when used for the hair, the perfume of the patchouli and the musk predominates. The exact proportions are the following:—Lard, 30 grammes (a gramme is 15·434 grains); Norwegian tar, 3 grammes; butter of nutmegs, 2 grammes; benzoin, 2 grammes; Fioraventi balm, 3 grammes; baume de commandeur, 3 grammes; musk, 5 centigrammes (a centigramme is ·1543 of a grain); and essence of patchouli, 50 centigrammes.

XVIII. *On the Therapeutical Use of Malt.* (Moniteur des Sciences Médicales, December, 1861.)

For some years the German physicians have employed malt as a medicine. The solution of malt after macerating an hour in water at 75° Cent., exhibits the peculiarity of containing gluten in a soluble state. To the presence of this albuminoïd body the German physicians attribute the successful results which they have observed in certain catarrhal affections of the bronchial mucous membrane, and in the different symptoms of dyspepsia. M. Frémy, in France, has lately employed this agent with favourable results. The substances employed by him, and which were sent from Baruth, near Berlin, were powdered malt, a beer made of malt, and malt-powder for baths. It should be mentioned that the powdered malt of Baruth is very rich in diastase—a character which does not exist in the powder made in Paris—and the German powder also contains more lupulin. M. Frémy administered malt to sixty-four phthisical patients, who it is admitted were not cured; but the general phenomena which usually accompany pulmonary tubercle—such as sweating and diarrhœa—were almost always relieved; and in the dyspeptic condition often following chronic bronchitis in old persons, malt also appeared to effect a rapid cure. It seems to relieve the bronchitis and restore the digestive powers. Simple dyspepsia also disappears rapidly under the use of malt. M. Frémy concludes that the introduction of malt into therapeutical use is likely to be attended with beneficial effects, and that it forms a very useful restorative medicine.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By John W. Ogle, M.A., M.D. Oxon, F.R.C.P.
Assistant-Physician to, and Lecturer on Medical Pathology at, St. George's Hospital.

I. *On Extra-Cranial Blood-Cysts communicating with the Sinuses of the Dura Mater.* By Dr. Hermann Demme, of Berne. (Virchow's Archiv, 1861, Band xxii, Hefte i. and ii.)

The author distributes the recorded cases of such cysts in three classes. 1. Those which arise from effusions of blood from the external cranial veins which undergo conversion into cysts, and become connected with the sinuses in various ways. 2. Those proceeding from dilatation of the outer cranial veins which communicate with the sinuses; and 3. Those which are caused by an outward bulging of a sinus.

As regards the *first* class, he states that epicranial extravasations of blood which arise in cases of injuries to the skull without external wound, may be converted into cysts, and become connected with a sinus by the intervention of a lacerated vein; or the communication may be produced by resorption of the bone, and the establishment of an opening into the veins of the diploë, by the pressure of the blood sac. Especially may a traumatic extra-cranial extravasation become connected with the sinus, if it coincides with an emissary vein of Santorini.

Again, such communication may be brought about by bony resorption, the result of pressure of the Pacchionian bodies. Of this the author proceeds to give an instance with all details, which was as follows:

Case.—A robust man fell from a height, and died ten hours afterwards. After death, a large and extended extravasation of blood was found, for the most part, under the pericranium, about the posterior part of the sagittal suture. On close examination, a sharp-edged opening, of the size of a cherry-stone was found on the left of the sagittal suture, in which a Pacchionian granulation covered with coagula projected; and on opening the cranium, there was found a second extravasation, the coagulum of which was interwoven with the meshes of the large sinus of the falciform process, and directly communicated with the extra-cranial mass by the opening occasioned by the resorption produced by the Pacchionian bodies. There seemed to be no doubt that the intra-cranial hæmorrhage was the result of rupture of one of the numerous venous vessels penetrating the fibrous meshes of the blood channel.

The majority of extra-cranial cysts communicating with the sinuses appear to belong to the first class. The author then quotes descriptions of and allusions to cases by Hecker,* Dufour,† Stromeyer,‡ Bruns.

Instances of the second class of these extra-cranial cysts are cited from Bruns,§ Francke, Melchiori,‖ Pelletan,¶ Merssmann.**

The author then gives the following as an instance of the third class of these formations, premising that even the sinuses may be enlarged either in consequence of a gradual resorption and penetration of the cranium in an outward direction, or by the intervention of a pre-existing opening at the fontanelles.

Case.—The patient was a child, nine months old, which had a tumour, of the size of a small apple, in the middle of the sagittal suture of the head.

* Erfahrungen und Abhandl. im Gebiete der Chirurgie, p. 151. Erlangen, 1845.
† Gaz. Méd. de Paris, p. 767. 1851. ‡ Deutsche Klinik, S. 160. 1850.
§ Handbuch der Chirurg., Band i. Abth. 1, S. 191.
‖ Gaz. Med. di Milano. 1843.
¶ Clinique Chirurg., tome ii. p. 76. Paris, 1810.
** Observateur Méd. Belge. 1834.

This had existed since birth, and the brother of the child had died with a similar tumour at the back of the skull.

The child was pale and weakly, with a rachitic thorax and large belly. The tumour was rounded and smooth, and its surface covered with very few hairs. Its walls did not appear transparent when examined by aid of a light. The resistance of its walls was unequal, in some parts fluctuation existing, whilst in others a hard mass was to be felt. On gradual pressure the tumour became smaller, but could not be removed, and on pressure being continued, the patient became livid in the face. The tumour also became larger when the child cried, and the walls assumed a darkish blue colour, as also when the jugular veins were compressed. There was also an evident rising and sinking of the tumour, synchronous with expiration and inspiration; and at times, by auscultation, a whizzing noise could be heard in it. The surrounding bone appeared to be healthy to the touch, and no opening of perforation could even be felt in the cranium. It became the question whether this tumour was the result of the so-called "hydrocephalus meningeus herniosus," or was really a cyst communicating with the falciform blood channels. The small size of the head was opposed to the former supposition, whilst all the symptoms above described, and the firm contents, which appeared to be coagulum, favoured the latter one. The child died some time after, with symptoms of cholera and collapse.

On post-mortem examination, the tumour was found to contain a layer of partly firm, partly loose coagulum of various colours, as in cases of aneurysm, and at the bottom a small funnel-like opening existed, communicating through the bone with the cranial cavity. The superior longitudinal sinus was full, and at a part corresponding to the extra-cranial cyst, was much dilated; into this part a probe passed from the outer cyst penetrated. The walls of the cyst, in addition to the normal scalp, consisted of connective-tissue membrane, with much elastic fibre, very like the sinus-walls, and an inner thin layer of loose connective tissue, full of stellated cells and nuclei, the inner surface being covered with laminated, flattened, and spindle-shaped epithelium, very like that of the sinus. On removing the surrounding soft parts on the outside of the skull a process of the lining membrane of the cyst was found dipping into the falciform sinus.

II. *New Clinical and Anatomical Researches upon the Disease termed "Ataxie Locomotrice Progressive."* By Dr. HIPP. BOURDON. (Archives Générales de Médecine, Avril, 1862, p. 385.)

In this communication, which was made to the Société Médicale des Hôpitaux, the author, after alluding to a former paper on the same subject, furnished by him to the 'Archives' for November 1, 1861, arrives at the following conclusions:

1. That independently of disturbances connected with muscular anæsthesia and cutaneous anæsthesia, a morbid phenomenon exists, essentially characterized by want of co-ordination of complex movements.

2. This symptom, which is the "ataxie locomotrice," may, like the paralysis of movement, the paralysis of sensibility, or every other functional disturbance of the same order, appear in a number of nervous diseases, asthenic, diathetic, &c., or in intoxications, and may exist without material appreciable alteration in the cerebro-spinal organs.

3. The same disorder, connected with the muscles, may be originated by various anatomical lesions occupying the posterior columns, the roots, and the posterior horns of the spinal marrow.

4. A certain number of post-mortem examinations give rise to the impression that a morbid entity exists, having a slow and progressive progress, a termina-

tion almost certainly fatal, and connected with a particular degeneracy of the spinal cord, occupying the seat above indicated, and capable of extending to the isthmus of the brain, and reaching the motor nerves of the eye, the optic, and perhaps other cranial nerves.

III. *Endemic Degeneration.* By W. A. F. BROWNE, Commissioner in Lunacy for Scotland. (Pamphlet.)

Under this designation the author describes the condition of a remarkable family which he met with in the "Wild Tirie," or land of Iona, of which island the inhabitants are extremely poor. The family consisted of "nine persons, descended from an apparently healthy stock, of whom two were not viable, one died in infancy, and one when twenty-one years old; of whom four are hydrocephalic, four are dwarfs, one diminutive; all save one of small size, feeble and puny; four are distorted and diseased, five are idiots; four had the head partially covered with a cork-like substance (a skin eruption); four cannot speak, two cannot walk, two walk slowly and laboriously, four cannot wash nor dress themselves, four would not escape from fire, and of four the senses are either hebete or rudimentary, or limited in range! and one is healthy, robust, and intelligent, and has arrived at maturity; and all, with this one exception, are so deformed, so hideous in countenance and aspect, as to suggest the last stage of some frightful plague, the type of the lower animals, or that they are the last members of a distinct and degenerated race of mankind." Of this strange society of beings, Dr. Buchanan, who has settled in the island, promises further investigations, photographs, &c.

The author looks upon all the features described as symptoms of a common disease, or degeneration, or signs of a vitiated nutrition; and refers to the cachectic condition as resembling that described by Billod,* as existing in lunatics who are the subjects of pellagra recently discovered in some of the French asylums; and to the combination of the skin disease and the cachexia "with the grave lesions of the nervous centres," as being very similar to the third stage of the pellagra of the north of Italy, described by Dr. Holland,† as preventing scorbutic cachexia, impairment of voluntary affections, pains in the limbs, diarrhœa, dropsy, insanity, &c. He by no means proposes to identify these affections, but suggests that it is more instructive to contrast the differences which may fairly be attributed to climate, special hereditary tendencies of race and surrounding circumstances.

Then follows a description of pellagra, and subsequently of cretinism, through which we will not here accompany the author, along with a running comparison between these affections and the condition of the family before described; and reason is given for connecting them with cretins.

The pamphlet closes by a consideration of the psychological peculiarities presented by the cases reported.

IV. *On the Regeneration of Bone.* By M. HAMEL and M. SEDILLOT. (Académie des Sciences, Juin 24 and Août 12. Quoted in Archives Générales de Méd., Août, 1861, p. 241, and Sep., p. 373.)

This observer adduced five remarkable cases, of which four had been under his own notice, in which the periosteum had obliterated a large perforation of the frontal bone, had reproduced the right half of the inferior maxillary bone, a large portion of the humerus, a portion of the body of a femur, and almost an entire tibia. The most interesting case was that of a man, aged thirty-six

* De la Pellagra, p. 16.

† Med. and Chir. Soc. vol. viii.

who for a long time had a wounded opening in the frontal bone, capable of accommodating the thumb, and very roughened, along with hernia of a part of the brain and dura mater.

The hernia was reduced and kept down by a pad of leather, and many years after, the patient having died of apoplexy, M. Hamel found the opening in the frontal bone obliterated by a whitish periosteal membrane, of new formation, of tolerable thickness, and of cartilaginous appearance, and in apposition with the outer part of the dura mater, to which, towards the centre, it was adherent.

The following were the facts brought out by the observations of M. Sedillot.

1. The superiority of operations in which the connexion between the periosteum and the subjacent osseous structure is maintained.

2. Insuccess of those proceedings in which the periosteum is dissected off, and isolated from the osseous surfaces.

3. Insuccess of the attempts at osseous regeneration by periosteum detached from splinters in fractures.

4. Absence of osseous reproduction by portions of periosteum preserved around amputated bones.

5. Absence of osseous regeneration in cases of pseudo-arthritis treated by resection with maintenance of a periosteal sheath.

6. Insuccess of the regeneration of bone by portions of periosteum isolated and kept in the wound after resection of limbs.

V. *Lumbrici passed through the Abdominal Parietes.* (L'Union Médicale, Oct. 31, 1861. Quoted from the Gaz. Med. Venete, No. 29, 1861.)

The three following cases are related in a communication by Dr. Garnier upon tæniafuges, with special reference to the cucurbita pepo, male fern, pomegrante, kousso, Rottleria tinctoria; their description follows the quotation, among other cases, of one in which lumbrici had been vomited, and had at last caused death by obstruction of the cæcum.

Case I. was that of a girl, aged twelve years, who was seized with symptoms of gastro-enteritis, fever, and emaciation; about a month afterwards, a painful tumour, of the size of the fist, appeared at the umbilical region, and eight days subsequently, it opened spontaneously, and gave exit to twelve lumbrici, and in course of time, to as many as twelve hundred, mixed with fæcal matter and gas, of from five to six times the length of the thumb. The symptoms ceased, the outward opening healed, and the child did well.

Case II. was that of a woman, aged fifty-six, who was seized with gastritis. Numerous lumbrici were removed by anthelmintics, but in spite of this, she experienced much pain in the right hypogastric region, increased on pressure, and a limited amount of hardness. On the fourth day a tumour appeared, with much pain and fluctuation. An incision was made, and after the evacuation of a quantity of darkish serum, two very long and large lumbrici escaped. On the eleventh day after, eleven others came out by pressure, and subsequently the wound healed well in the course of a month.

Case III. was that of a man, aged forty-five, who suffered from slight gastric disturbance and pain at the right groin, which became reddened and painful on pressure, somewhat as if in a state of phlegmonous erysipelas. After treatment, fluctuation appeared and bloody pus was evacuated by incision. After poulticing for two days, the skin about the opening became gangrenous and suppurated. The sore mended under bark and creosote application, but many days after, a lumbricus passed through the opening, and in the course of the fifteen following days, nine others came out on pressure. The suppuration dried up, and in two weeks, the patient was quite well.

Dubini, celebrated in Italy as an helminthologist, considered that in the last two cases an abscess had formed subsequent to the escape of the worm from the bowels, and to the adhesive cicatrization of the latter, with the abdominal walls; and that it was the presence of the worms alone which had determined the abscess.

VI. *On Internal Incarceration by True Intestinal Diverticula.* By Professor W. GRUBER. (Petersburgen Medicinische Zeitschrift, Band. i. Heft 2, p. 33.)

The author first alludes to six cases of internal incarceration which he has at various times communicated to German periodicals, of which three were varieties of volvulus of the sigmoïd flexure; one consisted in the adhesion and spiral-like twisting of the colic omentum round the ileum; whilst the two others arose from the adhesion to the mesentery of the end of the vermiform process which had wound around the ileum, or from its adhesion to the ileum, producing volvuli. He then goes on to relate the particulars of a recent case which he had observed, of internal incarceration produced by the encircling of a loop of the ileum by a true (congenital) intestinal diverticulum, not adherent at its extremity, which formed a kind of distended pouch. The case was that of a man, aged thirty-two, who on the 7th March, 1861, was in good health. On the 8th, he complained of constipation and pain in the belly. On the 12th, the pain about the navel was great and rendered more severe by pressure; at this part percussion gave a dull sound, but elsewhere a tympanitic one. Eructation, hiccup, intense thirst, and collapse preceded death on the same day.

After death the strangulation was found to be complete, the loop incarcerated being eight feet in length, of a bluish-red colour, and distended with chocolate-looking offensive fluid. The diverticulum arose from the free edge of the ileum, rather more than three feet from the cæcum, and had a small mesentery of its own, containing an artery at its lower part. Much is said with regard to the probable mechanism of this incarceration (but we forbear pursuing our author through his observations), and he then proceeds to quote and to give a *résumé* of no less than twenty cases placed on record, in which incarceration of the bowels was produced by a true intestinal diverticulum. In fourteen of these cases the diverticulum was attached at its extremity,* whilst in six cases it was quite free.†

The author, after examining the histories of all the afore-mentioned cases, arrives at the following conclusions:

1. The incarceration of bowel by intestinal diverticulum was apparently not even anticipated, much less diagnosed, in any of the cases.

2. The discovery and unloosening of the band adhering to the diverticulum in the first subdivision must be, during life, very difficult, if not impossible. In the second subdivision the puncturing of the dilated extremity of the diverticulum and its emptying, as recommended by Parise, would render more easy the liberation of the incarcerated loop of bowel.

3. The incarceration of bowel by the diverticulum appears more frequent in men than women, and most commonly between the ages of nineteen and forty.

4. The occasions of incarceration are extremely various, such as harassing walks, great muscular exertion, parturition, long standing, flatulent food.

* These cases are related by the following authors: Moscate, 1754; Doeveren, 1761; Donvignaud et Louis, 1768; Monro, 1803; Martin, jun, Rayer, Eschricht, 1830; Froriep, 1835; Rokitansky, 1836; Ulmer, 1850; Bouvier, 1851; Struthers, 1854.

† These cases are by Bougon, 1816; Regnault, 1816; Devy, 1845; Parise, 1851; Pasquier et Brigandat.

5. Most of the subjects of this affection were previously in good health; in others, symptoms existed previously.

6. In the first subdivision the disease lasted from two to fifteen days, and more; and in the second one from two to three days only; in the author's cases five days.

7. The first subdivision is more frequent than the latter in the proportion of two to one.

8. The incarcerating diverticulum was from 1 inch 8 lines to about 7 inches in length, and sprang from the ileum at distances varying from 15 inches to 3½ feet, from its termination in the colon. When the diverticulum was fixed at its extremity, this was almost always by means of a band or string, which might reach 3 inches in length, and the places to which it was affixed were chiefly the mesentery, but also the umbilicus, the ileum, the cæcum, omentum, &c. When it was not affixed, its extremity was always enlarged.

9. The portion of bowel incarcerated was always the ileum, and was from 15 inches to 6 feet 3 inches in length (but in the author's case 9 feet long); which in the first subdivision formed for the most part a main loop, but in some cases, two or even three; whilst in the second subdivision, with a single exception, only one loop was formed.

10. The loop of intestine incarcerated was not in all cases compressed; the passage being in one case obstructed by pressure of the ileum, which formed the ring along with the diverticulum, and in another by over-distention of the bowel, and consequent stretching of the diverticulum.

11. The mechanism by which this incarceration is brought about is stated to be, as regards the first variety of the affection, still obscure. The second subdivision is, however, explained under the supposition that the diverticulum forms a kind of spiral round the bowel, either, so to say, accidentally, or by reason of its structure and its getting distended with contents, and that thus a kind of foramen is formed by the passive diverticulum into which drops from above, first, a portion of the wall, and then the entire loop of small intestines, which by degrees gets distended and incarcerated, by reason of the powerful peristaltic action from above and by its own weight, the outlets of the contents not being so facile through the ascending portion of the intestinal loop. This explanation differs from that suggested by Levy and Parise, whose cases are enumerated by the author in his collection before alluded to.

VII. *Cases of Acute Dysentery treated by Large Doses of Ipecacuanha.* By Surgeon-Major A. Blacklock. (Madras Quarterly Journal of Medical Science, No. 4, p. 256.)

Ten cases were selected out of a number of twenty-four, for treatment by large doses (generally a scruple) of this remedy. All these were recent or acute cases, and after the use of the drug, in most cases owing to change of type, &c., tonics, quinine, iron, &c. have been resorted to, and other remedies to meet emergencies of the cases. We cannot give space to the reporting of these cases, but will quote one or two conclusions arrived at by the author regarding this disease and the medicine in question. Ipecacuanha is spoken of as being of more signal service than any other drug, as "none other brings the system into a condition favourable to the restoration of healthy function in the diseased intestine, and by its employment we have the great advantage of being enabled to dispense with bloodletting and other means which are, to say the least, very unsatisfactory in their consequences." Venesection is reprobated in almost all cases, as though shortening the acute stages, it appears to debilitate the heart and so lead to subsequent local engorgements, and impair the functions of depuratory organs, thus also inducing impurity of the blood.

The object spoken of in treatment should be to prevent vascular distention by keeping the blood pure, and that by preventing arrest of secretion, and ipecacuanha possesses all the properties required for these purposes; being a "general depurator and promoter of secretion, occasioning as it does at first a rapid formation of secretion in the skin, kidneys, fæcal glands and liver," it prevents undue work being thrown on the liver, and any overflow of bile by which the intestines would be irritated. Too long continuance of the drug does harm by the weakening action which it naturally has upon the heart, and which diminishes the power of the heart, in advanced stages, of resisting the congestion which exists in the capillaries of the diseased intestines. With regard to the later stages, for the purpose of meeting debility, the tincture of sesquichloride of iron is spoken of in the highest terms, not only in the engorgements of the bowels in dysentery, but with or without nitric acid, according to the state of the liver; also in fever of a typhoïd type, commencing without local complication, but presenting ilio-cæcal gurgling with the prostration and brown flush accompanying this infiltration. The author, however, looks upon ipecacuanha as only useful when the disease has acquired a phlegmonous character, the submucous cellular tissue having become affected, with a corresponding asthenic condition of the system. When the stools begin to consist of thin mucus, non-coagulating blood, with or without feculence, the ipecacuanha, if not omitted, should at least be intermitted, and a tonic sedative used.

As regards the hepatic implications in dysentery, the author observes, "not only does the liver not induce dysentery either by defective action or over-action, but it is the organ to whose resources we chiefly look for the removal of the sthenic forms of the disease." It is called into activity for the carrying off excrementitious matter, "whilst we give rest to the lungs, kidneys, and fæcal glands, and save the colon."

Other considerations exist in the paper regarding several points unnoticed in the history and treatment of dysentery, but for these we must refer the reader to the original.

VIII. *On Rickets.* By Dr. A. Friedleben. (Jahrb. f. Kinderheilk. und Physische Erziehung iii. Hefte 2 and 3.)

The views of Friedleben quoted from the above-mentioned source are given at some length in the Medizinische Jahrbücher, 1861, Heft 2, p. 128. Among other conclusions, he arrives at the determination that this affection depends upon an increase in the cartilage molecules, and not in any way upon a diminished addition, or increased removal of earthy salts; it rather arises from some obstacle to the union of the earthy salts with the cartilage molecules. The author does not throw out suggestions as to the influence of this theory upon treatment.

IX. *A short Account of Cardiac Murmurs.* By W. J. Gairdner, M.D. (Reprint from the Edinburgh Med. Journal, November, 1861.)

We find it impossible to do more than to draw attention to his pamphlet, as throwing out several new and important suggestions regarding the value and discrimination of cardiac murmurs; in the elucidation of which several novel forms of diagramatic illustrations are resorted to.

X. *Leukhæmia in Children.* By Prof. Löschner. (Jahrb. f. Kinderheilk., iii. 1. Quoted in the Medizinisch. Jahrbüch, 1861, ii. p. 136.)

Four cases are alluded to as having been observed. Of these one, a boy, aged nine years, and the other a girl, only three years old, belonged to what is termed the primary form (swelling of the glandular apparatus connected with the formation of the blood), and two belonged to the secondary form (following tuberculosis.)

XI. *Ulceration of the Internal Carotid Artery in the Carotid Canal, owing to Necrosis of the Temporal Bone.* (L'Union Médicale, Aug. 20, 1861.)

The case was that of a soldier, aged twenty-three years, who came under the care of M. Baizeau, with symptoms of pulmonary phthisis and struma of various organs, fistulous openings in many parts, owing to abscesses, and one especially under the left zygoma in front of the ear, consecutive to disease of the inferior maxillary bone. Moreover, for ten months, there had been a slight discharge from his left ear, owing to necrosis of the middle ear, the membrana tympani being perforated. One day, after severe coughing, a slight discharge of blood took place from the ear, which recurred several times for three days each time, ceasing spontaneously. Plugging with lint soaked in a solution of persulphate of iron, and injections of the same were resorted to. There was no occurrence of the hæmorrhage until the eighth day, when there was a return to a great extent, not only by the ear, but by the nostrils and mouth, the blood being of a bright scarlet colour. This flow of blood was arrested by the plugging as before. Three or four days later, the hæmorrhage returned again, and the patient became very weak. It was resolved to tie the common carotid artery, and this was done by M. Baizeau very shortly after hæmorrhage had again set in. The hæmorrhage was arrested, but the patient became affected with sharp pain in the left hypochondrium, a feeling of constriction at the lower part of the chest and of buzzings and tinglings in the ears. The hæmorrhage returned twenty-four hours after the operation, and again to a very large amount on the following day by the nasal fossæ. The patient soon died from loss of blood.

On post-mortem examination, a large communication was found to have taken place between the tympanum and the carotid canal, by necrosis of that portion of bone which is situated behind the promontory, and which separates the carotid canal from that of the Eustachian tube. This necrosed portion of bone had become isolated and separated into two irregular sequestra, one of the size of a grain of rice, and the other rather larger than a pea; the disease of which, situated under the internal carotid artery, on a level with its first bend, had induced two circular perforations of the artery.

Allusion is made to M. Chassaignac's notice of a similar case in his 'Traité de la Suppuration,' tome i. p. 529; and to M. Legouest's case of perforation of the vertebral artery by caries of cervical vertebræ.

XII. *On Indigo in the Urine.* By W. Gilchrist, M.D. (Edinburgh Medical Journal, December, 1861, p. 535.)

The patient, a woman, aged fifty-eight, suffered twenty-four years previously from a fall, pitching on her back, and two years after that from hæmaturia, lasting two years and a half. Fourteen years previously, when supposed to have a heart disease, she first remarked a blue deposit in the urine, which became at the same time very offensive; and this condition had continued ever since. Amaurosis for last nine years. The symptoms now complained of (she is still under observation by Dr. Lillies of Chudleigh) are dyspnœa and palpitation at night, very frequent micturition, pain about the neck of the bladder, anæmia and debility, anasarca. There is a bellows-murmur with the first sound of heart, chiefly at the base. The urine passed is about three pints in twenty-four hours, of 1010 sp. gr. On straining, it deposits a dirty-greenish sediment, which to the naked eye seems to be composed, 1st, of a heavy white crystalline deposit; 2nd, of whitish flakes of organic matter; and 3rd, adhering to or mixed with the latter, particles or patches of a deep blue colour. On microscopical examination, the white deposit is found to consist of triple phosphates, the flakes of organic matter of tesselated epithelium, which are swollen, granular, and opaque; and the blue matter appears amorphous, and presents two distinct shades—a dark and a light blue—both resembling the shades of Prussian blue. There are also finely molecular masses of a yellowish-brown colour. "Neither the blue nor the brown matter is altered by water, acetic acid, ammonia, or cold alcohol. Both solubule in warm alcohol, and in strong nitric acid, which dissolves them, destroying the colour. On adding sulphuric acid to a small portion of the clear urine (after Carter's method), a faint pink or copper-coloured tint is produced, thus proving that only a small amount of uroxanthin is present; heat or nitric acid produces no coagulum."

Dr. Gilchrist asks the questions regarding the uroxanthin or indican of natural urine (a neutral substance, capable of being resolved by acids or ferments into red and blue indigo and sugar), "What is it which, in certain cases of disease, determines the resolution of this substance into indigo? and where does the change occur?" and suggests that most likely either a ferment or a mineral acid, and probably in the bladder itself, effects the transmutation. He also ventures the supposition that the greenish or bluish mottled material sometimes seen on the surface of pus and other albuminous substances after exposure to the air, is the result of a similar transmutation of indican by the action of a ferment into uroglaucin.

XIII. *Researches regarding Infantile Pulmonary Emphysema.* By Dr. Hervieux. (Archives Générales de Méd., Juin et Juillet, 1861.)

These observations formed the subject of a communication to the Hospital Medical Society. The following propositions may be deduced therefrom:—

1. Infantile emphysema is much more frequent in the early months of extra-uterine life than at any other period of the existence of children.

2. Pulmonary emphysema, considered anatomically, exists in two distinct forms—vesicular and cystous, or interlobular.

3. The principal characteristics of the vesicular form in young children are, (*a*), the occupation of about the anterior third of each lung; (*b*), a very decided whiteness of the lung, owing to absence of blood; (*c*), the production of a feeling on pressure as of a ball of cotton covered by silk or satin; (*d*), the presence on the surface of the lung at one time of myriads of small transparent shining points like small air-bubbles, attaching sometimes to the sides of a glass full of water; at another time of small polygons symmetrically placed in a mosaic-like manner.

4. In the interlobular form the villæ are disposed, when numerous, in long linear series.

5. In young children the emphysema may invade the mediastinum, and thence infiltrate the subcutaneous cellular tissue of the entire surface of the body.

6. The most common pulmonary accompaniments of infantile emphysema are, (*a*), alterations of tissue indicative of pneumonia; (*b*), pleuritic adherences or effusions; (*c*), pulmonary apoplexy; (*d*), tubercles; (*e*), gangrene.

7. The heart does not show any of the lesions commonly observed in emphysema of the adult.

8. In the other organs of the body, variously affected, no traces exist of changes related to the emphysema.

9. The phenomena observed during life in young subjects affected by emphysema, are not so constant or well-marked as to deserve the name of symptoms.

10. The most common complications of this disease in the order of frequency are progressive algidity, with or without sclerema-icterus in the newly-born, measles, tubercular diathesis, erysipelas, rickets, and the syphilitic diathesis.

11. This disease appears to be not always curable, but may at least be rendered compatible with a more or less continued existence.

12. The causes of the disease are predisposing and immediate. The first are gastro-enteritis, and the other diseases mentioned above as complications. The immediate ones are various lesions from pneumonia, tuberculization, pulmonary apoplexy, &c.

XIV. *Syphilitic Affections of Various Internal Organs.* (Medizinische Jahrbücher. Wien, 1861, Heft 5, p. 116–117.)

Several cases of such supposed diseases are quoted and described, as, for instance, of the tongue, by Zeipl;* of the liver and lung, by Pleischl and Klob;† of the bowels, by Dr. Huët;‡ &c. We will only particularize an interesting case which is related in connexion with Dr. West's case of syphilitic stricture of the œsophagus.§ It was that of a man who had previously suffered from secondary syphilis, in whom, in Prof. Piorry's Klinik, a tumour at the constricted part was indicated by percussion. In this case, dilatation and the use of anti-syphilitic remedies produced so much improvement in the space of fourteen days, that he could swallow solid food, and the tumour entirely disappeared, as ascertained by the pleximeter.

XV. *Apoplectiform Intermittent Fever.* By Dr. CRUZADO. (Siglo Medico, 1861, p. 231.)

Under this title is related the case of a man, aged twenty-five, who was found accidentally, lying insensible, with conjunctivæ injected, face reddened, pupils dilated and insensible, lividity of the eyelids and lips, frequent and hard pulse, heat of skin, &c. He was bled and leeched along the jugulars, and cupped over the epigastrium, &c. On the following day he was sitting up in bed smoking, but in the afternoon, a fresh attack, with coma and dilated pupils, came on. He was treated as before.

On the day after, quinine was given, but very soon after, coma and stertor came on, and he was again treated as mentioned above.

On the two following days, similar attacks came on, in spite of large doses of quinine. On the day afterwards, quinine was given by injection, but the attack recurred. He was bled a fourth time, but the attack lasted until the following day. He was placed in a bath for a length of time, and this was followed by great perspiration in the night and slight fever the next morning, but there was no cerebral attack. A second bath was resorted to. Accessions took place, but finally ceased under the influence of hydro-ferrocyanate of iron and calomel given in the intervals.

* From the Wien Wochenblatt, 1861, No. 12.

† Wien Med. Wochenschr., 1860, Nos. 8, 9, 10.

‡ Behrend's Syphilidologie, ii. § See Dublin Medical Journal, No. 29, 1860.

XVI. *Case of Diabetes exhibiting remarkable Phenomena.* By E. A. Kunkler. (L'Union Médicale, Juillet 29th, 1861.)

The case was that of an American, aged twenty-six, who had been the subject of diabetes for seven months. Notwithstanding the use of morphine, iron, bitters, exclusively animal diet, &c., he got worse; the emaciation, thirst, want of sleep, frequent epistaxis being excessive. After consulting numerous physicians without benefit, the relater of the case, bearing in mind the hypothesis of Scharlan, that the origin of the disease was to be found in the vertebral column; and the discovery by Bernard, that irritation of the fourth ventricle of the brain caused the appearance of sugar in the urine and blood, thus connecting the disease with some derangement of the nervous system; and moreover finding that in this case the superior part of the dorsal portion of the spine was tender at many points, applied a certain number of cupping-glasses. Their application was followed by a disappearance of this morbid sensibility, but there was no abatement of the disease itself. Immediately, however, after a blister upon the neck, the presence of sugar and the unusual excretion of the urine ceased most unexpectedly, as if by enchantment. A slightly disagreeable sensation, which hitherto the patient had experienced at the lower part of the brain, also ceased at once. After continuing for ten or twelve days blisters upon the neck and behind the ears alternately, the affection had entirely and permanently disappeared, and this in spite of a varied diet.

The particulars of the above case were sent to M. Bernard, with the hope that as Paris offered more cases of diabetes than the locality where Dr. Kunkler lived (Placerville, California), he would give the plan of treatment an extended trial.

The quoter of the case in 'L'Union Médicale' recals to notice the recital by Becquerel of an instance in which glucosuria occurred in a case of acute myelitis, and in a case of spinal meningitis with tumour of the pia mater and softening and cyst of the cerebellum; also by Lebert, of diabetes in a hæmorrhagic affection of the spinal cord; and by Scharlan, of the post-mortem examination of two cases of diabetes, in which softening of the cord, with other changes, were found. He wisely suggests that the cure of diabetes by the means above related, must not always be looked for, inasmuch as although generally being developed under the influence of the nervous system, its modes of origin are very various.

XVII. *Preternatural Anus, owing to Scrofulous Ulceration of the Bowel.* By Dr. F. Schott. (Wochenblatt d. Zeitschr. d. k. k. Gesellsch. d. Aerzte zu Wien., October 9th, 1861.)

The author prefaces the description of his case by stating that, out of the 2293 deaths which in 1860 occurred at the General Hospital at Vienna, 837 (that is more than thirty-six per cent.) died from tuberculous disease of some kind or other. The patient in question was a man, aged thirty-nine years, who having suffered from cough, &c., for half a year, had been vomiting and constipated for six days. On examination, there was dulness at the apices of the lungs, and an opening of the size of a hemp-seed, from which a thin purulent fluid escaped, was found in the right groin, the surrounding skin being of a bluish-red hue. A probe introduced passed about five inches in an upward and outward direction without meeting anything like rough bone. A swelling in the groin had existed, according to the patient's statement, for a year, and four weeks before admission into the hospital it had opened spontaneously. After admission, the vomiting ceased, and was succeeded by purging, which gave way to opiates. One day the patient observed fæculent matter passing through the opening, but this only lasted a short time, and then bloody substance passed.

Vomiting returned and preceded death. On post-mortem examination, healed tubercle was found in the lungs; the peritoneum was covered by a pseudo-membrane, matting together the abdominal contents; and very extensive ulceration of the bowels existed, &c. Great destruction of the mucous membrane of the cæcum and ascending colon existed, and perforation of the muscular coat connected with a fistulous passage filled with ichorous fluid, which, running along the iliacus muscle underneath Poupart's ligament, opened upon the surface of the groin, as before described. Branching off from this was also a second passage leading to necrosed iliac bone.

The author concludes by adverting to the anatomical connexions of the cæcum, noticing the frequency with which it is not surrounded by peritoneum, and the consequent facility with which, in deep ulceration and perforation of the posterior wall of the bowel, the tissue uniting it with the iliac fascia may be involved, and thus a fistulous course opened along the iliac muscle.

XVIII. *Two Recent Cases of Spontaneous Rupture of the Spleen.* By Prof. ROKITANSKY. (Wochenblatt d. Zeitschr. d. k. k. Gesellsch. der Aerzte zu Wien, Oct. 16th, 1861.)

The subjects of this affection were two workmen, aged respectively forty-eight and nineteen years, of Vienna, who were affected by the same kind of disease, and died within a day of each other. It is also remarkable that in both cases the spleen was the seat of a leukhæmic tumour, but no trace existed of the dark pigmentary state commonly found in inveterate cases of intermittent fever and its consequent cachexia. In one case the liver had a decidedly leukhæmic appearance.

In both instances, several pounds of clotted and fluid blood were found in the abdominal cavity, and in both the spleen was from five to six times as large as it ought to have been.

In neither of these cases had any mechanical force brought about the rupture in question.

XIX. *Case of Cold-stroke.* By Dr. H. HARTSHORNE. (American Journal of Med. Sciences, October, 1861, p. 432.)

The author, alluding to cases in which sudden and baneful results followed the subjection to extreme cold, recites the case of a boy, aged fourteen, previously in good health, who in the night, during a very cold, piercing wind, rose from his bed and went barefoot and in his nightshirt to the window of the next room, which he opened and stood by for a few minutes, looking out on account of an alarm of fire. The next morning he felt ill, and became worse in the course of the day. At five P.M. he was suffering from headache, drowsiness, and vomiting; the skin moderately hot; pulse hard and quick. Aperients, leeches to nape of neck, and cold to head, were resorted to. On the next day the skin was cool, and there was less headache; bowels open. He, however, vomited twice. Subsequently he became restless and incoherently delirious, recognising only his mother. There was no clonic convulsion, but a disposition to clasp the fingers tightly against the palm, and to throw out the hands and feet with impatient suddenness. He could not be got to take medicine, and a blister was applied to the nape of the neck. On the following morning he suddenly died, but unfortunately no post-mortem examination could be obtained.

The probable causes of this suddenly fatal illness were investigated closely, and it was determined that the most likely one was the "stroke" of cold applied to the surface.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Waterproof Bandages.* By Dr. A. Mitscherlich. (Archiv für Klin. Chir., vol. ii. pp. 585–621.)

In a former communication,* Dr. Mitscherlich communicated his plan of rendering plaster of Paris bandages proof against the action of tepid water and other fluids, by saturating their dried surface with a solution of shel-lac in alcohol, Portland cement mixed with a solution of water-glass being substituted for plaster of Paris when great solidity of the bandage is required. In the present paper, an account is given of the various cases in which the bandage has been employed, the author having in the interval experimented on different substances in order to ascertain whether any of them are preferable to the shel-lac solution. Only one of these seems to be so, on account of the rapidity of its action and the amount of resinous matter deposited after its application, and this consists of three ounces of Dama resin dissolved in a pound of ether. This is applied over the plaster of Paris as long as any continues to be absorbed, protecting any wounded parts by cerate, and avoiding any portion of the bandage that may be intended to be fenestrated. According to the nature of the case, the bandage will have to be kept on for a greater or less time, but when this exceeds a month, it should be renewed. The author cites many cases of excision and compound fracture, in which the bandage has been found of essential service, keeping the parts quiet and undisturbed, and preventing recourse being had to amputation, which without its aid would have been necessary. In consequence of the quietude secured, even in bad cases of compound fracture, the fever was only moderate. In inflammation of the joints, it fulfils the double indication of keeping the parts in a state of rest, and admitting of the application of local or general baths. For the treatment of fractures of the thigh in children, often so difficult of management, the *cement-bandage* is an admirable apparatus, furnishing the necessary amount of support.

II. *On the Treatment of Cataract by Repeated Evacuation of the Aqueous Humour.* By Dr. Sperino. (L'Union Médicale, No. 53.)

A good deal of attention having of late been excited by the statement that Dr. Sperino had met with great success in the treatment of cataract by repeated evacuation of the aqueous humour, Dr. De Pietra Santa went to Turin to witness his practice in the ophthalmological hospital of that city. He was much struck with the cases which he saw there, and at his request, Dr. Sperino furnished him with some account of his procedure in anticipation of a complete work which he is preparing on the subject, and in which he will go fully into the question as to which forms of cataract are best suited to this mode of treatment. M. Sperino had long derived great advantage from the repeated evacuation of the aqueous humour in cases of iritis, interlamellar keratitis, severe hypopion, staphyloma of the sclerotica, congestion of the choroïd and retina (even when attended with exsudations), opacities of the vitreous humour, and in pseudo-membranous deposits in front of the lens. Even in some desperate cases of glaucoma, attended with commencing cataract, this treatment, useless as regards the lost sight, was of use, a diminution of the opacity of the lens following the evacuation. It was in September last, however, that M. Sperino seems to have first employed this as a direct means for the cure of cataract.

* British and Foreign Medico-Chirurgical Review, Oct. 1861, p. 547.

A lady, aged eighty-one, had complete cataract in her right eye, and one which was much advanced in the left. The great age of the patient, her liability to cerebral congestion, and the contracted condition of the anterior chamber, made him little desirous of performing an operation, but he determined to puncture the cornea, and evacuate the aqueous humour at intervals of twenty-four and forty-eight hours. The improvement was progressive, and in two months the opacity had disappeared, the patient being able to read without glasses Nos. 3 and 4 of Jaeger's scale, at the distance of from ten to twelve centimetres, and even thread a needle. The improvement thus far has continued permanent. After each evacuation, which was made through the same aperture in the cornea, ice was applied to the eye for some hours. M. Sperino observes that it was fortunate that this, his first essay, happened to be made in that variety of cataract, which his subsequent experience has shown him is precisely the one which yields best to this mode of treatment. He has since practised it with success in various cases, some of which have been witnessed by M. de Pietra-Santa. He employs Guerin's knife for subcutaneous myotomy, introducing it at the extreme edge of the cornea, generally on the outer side. This does not evacuate the humour, which is done by means of a small silver probe having a blunt extremity, and which serves also, without preliminary incision, for future evacuations, the point of puncture being easily found again, and no inconvenience resulting from its being reopened. This is done daily, leaving a day now and then for rest. Iced compresses are applied for a few hours afterwards, and the patient is well fed, and enjoined to take exercise in the open air.

III. *On the Employment of prolonged Warm-water Baths.* By Dr. EBERMANN. (Petersburg Med. Zeitschrift, vol. ii. No. 3.)

Zeis, Szymanowski, and other German surgeons, have represented that the employment of local tepid baths uninterruptedly for hours or even days together, is attended with the most beneficial effect in various surgical affections. In the present paper Dr. Ebermann gives an account of the trials he has made of them in twenty-one cases, occurring in the female surgical wards of the Obuchoff Hospital. The cases comprised six of ulcer of the extremities, nine of phlegmonous inflammation and ulceration, four cases of deep-seated panaris, one of superficial necrosis of the tibia, and one of ingrowing nail. The results were very favourable, the pain undergoing great relief, preliminary incisions being usually required in phlegmon and abscess. In panaris the relief of the pain was very remarkable, the ease continuing even for some hours after removing the limb from the bath. The pain ensuing upon incisions was also much mitigated by the bath. Febrile action speedily subsided, and the appearance of ulcerated surfaces as rapidly amended, so that a highly favourable change in the condition of the pus was sometimes observed even on the second day. The pus globules saturated the substance of the granulations, giving the surface of the ulcer a greyish appearance, which, within twenty-four hours' after removal of the part from the water, or even in less time, became converted into a fine red. The separation of dead tissue in phlegmonous inflammation was expedited; and the rapid regeneration of the epidermic cells was very remarkable. The redness, induration, and hypertrophy of the edges of the ulcers were soon exchanged after the baths for a softer and more pliable texture, admitting of cicatricial propagation. In only two of his cases did the author find the baths act disadvantageously. He thinks that the determination of the temperature to be maintained should be left to the feelings of the patient. As to the time the inflamed parts should be allowed to remain in the bath, Zeis, after amputations, directs that it should be from eight to twelve days, and Szymanowski five or six days; and the author found his cases of phlegmon, panaris, &c., required from five days as a minimum to thirty-eight days as a maximum. The signs which indicate that the baths may be discontinued, are softening of the edges of the ulcer and the filling of its cavity with granulations, together with the formation of a delicate cicatrix round its edge. In abscess, there should be either an adhesion of its walls or a strong disposition to this.

IV. *On the Employment of Compression in Tumours of the Breast.* By M. Broca. (Bulletin de Thérapeutique, vol. lxii. Nos. 4, 5, and 6.)

In this paper M. Broca gives an account of the advantages which he has derived from the employment of compression in *adenoïd* and *irritable tumours* of the breast. Very large and very soft adenoïd tumours are unsuited to this means, both because they often exhibit a tendency to ulcerate, and their usually irregular surface renders the application of uniform compression difficult. For the effectual employment of compression, much attention and assiduity is required upon the part of the surgeon, and some resignation on that of the patient. Accustomed to the use of stays, women for the most part breathe by means of the upper part of the chest, and at first they suffer much from any constriction which prevents its dilatation. The best way is to commence the pressure somewhat gradually, though still effectually, even from the first. M. Broca applies it by binding on plates of agaric, strips of adhesive plaster making, he thinks, less uniform compression, and being liable to induce irritation. The bandages securing the agaric, and others covering these, must vary somewhat in their disposition, according to the stoutness of the woman; but it is essential, as they may have to remain on even for weeks, that they should be well secured to the agaric and to each other by means of pins. As many as fifty pins are sometimes required, the bandage thus acquiring great solidity and not slipping.

In treating of *adenoïd tumours of the breast complicated with neuralgia,* M. Broca observes that in fact any of the varieties of tumours of this organ may be thus complicated, but he has never met with an example of A. Cooper's irritable breast occurring independently of any tumour. Most of the mammary neuralgias are connected with tumours, which are the consequence of various forms of hypertrophy, and especially adenoïd tumours. M. Broca does not agree with Velpeau in the slight value he attaches to the employment of compression in those irritable tumours. He regards this treatment as the most rational, seeing its great success in the simple adenoïd, the neuralgia disappearing when, by its aid, the atrophy of the tumour has been secured. Of course it will not always succeed, for there are cases of simple adenoïd which resist its influence, and all irritable tumours are not adenoïd; but judging from the four cases which have occurred in his own practice, M. Broca regards it as the most efficacious of all remedies yet essayed, with the exception of amputation—a humiliating resource. M. Broca's experience having been thus far confined to adenoïd tumours, he is not prepared to say to what extent compression may prove advantageous in other forms of irritable tumours, although he anticipates that the same advantages may be derived from those of them which are dependent on any form of hypertrophy and chronic inflammation.

V. *On Traumatic Emphysema.* (Bulletin de la Soc. de Chirurgie de Paris, 2 Sér., vol. ii. pp. 301 and 312.)

M. Broca brought this subject before the Society by the relation of a case of a man whose leg was shattered by the fall of a large stone. Besides a compound fracture of the leg, there was emphysema of the limb, and amputation was performed at some distance beyond where the gaseous infiltration had extended, there being ossification of the arteries, although the patient was scarcely forty-seven years of age. Gangrene seized the stump next day, and at the end of a week the patient died. M. Broca inquired of the Society its opinion as to the origin of the emphysema: whether it was the result of putrefaction, or of simple exhalation, as also what relation it bore to the production of the gangrene of the stump?

M. Chassaignac observed that he had remarked that the bodies of persons who had exhibited this emphysema became rapidly decomposed. It has also been observed that the bodies of persons who have committed suicide decompose very rapidly, and soon give rise to an abundant gaseous production. M. Chassaignac is disposed to believe in cases of traumatic emphysema that there is a kind of immediate poisoning, which may be compared with poisoning from the virus of a serpent. The effects of violent traumatism may be compared with the stupor observed as a consequence of gun-shot wounds, and which was attributed by the earlier writers to poisoning of the projectiles. This nervous shock and stupor explains the exhalation of gases in certain cases of emphysema, as well as the rapid decomposition after death.—M. Morel-Lavallée looked upon the production of emphysema immediately after accidents attended with breach of surface as a mere mechanical result, a kind of suction action, through the agency of which the external air penetrates amidst the wounded tissues; and it would be of more frequent occurrence were not the entrance of the air opposed by aponeuroses, the flaps of cellular tissue, &c. He regards the occurrence of the gangrene in M. Broca's case as a mere coincidence, possibly somewhat dependent upon the ossification of the arteries. He also thinks that the gravity of the cases in which emphysema suddenly supervenes has been exaggerated, having seen many instances of recovery.—M. Legouest observed that this gangrenous state of the stump supervening upon amputation was not rare in army practice, where it was usually attributable to overcrowding; and he can see no reason for seeking for cause and effect in the development of the gas and the production of the gangrene.—M. Broca confessed the great difficulty he felt in explaining this case, and referred to M. Maisonneuve's theory in similar cases, according to which, in violent cases of traumatism, gangrene, although not perceptible, is produced at the moment of the occurrence of the injury, the parts being struck with death, and decomposition taking place at once. This would ill explain in this case the gangrene of the stump at a distance from the injury, no gas having been present where the knife passed. So rapid was the progress of the gangrene that M. Broca could but regard it as a relation of cause and effect, although unable to decide whether the emphysema gave rise to the gangrene, or the gangrene to the emphysema.—M. Velpeau considered that the most rational explanation of the rapid occurrence of emphysema of the limb after compound fracture is, that air obtains admission from without, by reason of a sort of aspiratory movement taking place amidst the lacerated tissues. He cannot believe in the possibility of the gas being produced by gangrene in the course of a quarter of an hour. In the patients who have been cured after presenting this complication, and even in those who have succumbed, traces of gangrene have not usually been found. When gaseous exhalation occurs as the result of gangrene consequent on injury, it does not do so immediately after the accident, but subsequently, after reaction has been set up. As to the prognosis of these cases, M. Velpeau does not regard it so unfavourably as he formerly did, the gravity of the case depending in fact upon the nature of the wound, rather than upon the emphysema which complicates it. The emphysema is, however, an indication of an extensive and deep-seated lesion, accompanied by great effusions, detachments, and attritions; and in this sense it constitutes a grave complication. In M. Broca's case, the amputation was so practised that the emphysema could have taken no part in the production of the gangrene.

VI. *On Operations for Stone performed during* 1861. By M. Civiale. (Gazette des Hôpitaux, 1862, No. 23).

In presenting to the Academy of Sciences the annual account of his lithotrity operations, M. Civiale takes occasion to advert to the slow progress which this operation is making in England. Thus he states, that of 467 stone patients occurring in little more than three years, only 35 were operated upon by lithotrity, and of these only 22 were saved. The account of his own operations for 1860, which he published last year, made, he tells us, a great impression in England; and several English surgeons have since then repaired to Paris in order to obtain the instruction and instruments necessary for the due performance of the operation.

During 1861, M. Civiale has had 66 patients under treatment for stone; 52 of these being for the first time, and 14 recurrences of the disease after prior operations. Of these, 49 occurred in private, and 17 in hospital practice. Of these 66 patients, 51 were operated upon by lithotrity, which succeeded in 49 cases; and 10 by lithotomy, 6 being cured and 4 dying. The remaining 5 patients were not operated upon, because the stone was too large, and their organs were in too diseased a condition. The patients did not all present the same favourable conditions for treatment. The most fortunately situated were 31, who (a small stone constituting their sole malady) were promptly and easily relieved. For patients of this description, lithotrity, both as regards diagnosis and treatment, has attained great perfection; the operation under these circumstances being well nigh certain of success. In 35 of the cases the stone had existed too long before treatment was sought for, morbid conditions having been set up, which influenced in various degrees the execution and results of the operation. In many such cases a perversion of the sensibility and functional disorders of the urinary organs constitute the principal complication. The operation may be easy enough if the stone is small, but the irritated and exhausted organs bear the manœuvres with difficulty, and special adaptation of the mode of treatment is required to secure a favourable result. In other cases we may have a hard and large stone, contained in a bladder of contracted capacity, the space within which is still further encroached upon by tumours springing from its inner surface or its neck. The main difficulty here is the diagnosis, not of the nature of the morbid changes themselves, but of the exact extent of the complications. Even when these do not prohibit the execution of the operation, they may render it difficult and painful, and the results achieved may be imperfect. When the stone is very large and the lesions are greatly developed, the operation can only be performed with the greatest circumspection, and may have even to be declined in favour of lithotomy. M. Civiale, indeed, finds that one-fourth of his stone-patients require to be so treated, the three-fourths being amenable to lithotrity. In 52 of his lithotomy operations, large stones and tumours of the bladder simultaneously existed.

The cases of 1860 and 1861 united make a total of 120 stone patients, 115 males and 5 females. Of these, 88 were lithotritized, 3 dying, 79 recovering, and 6 still suffering under functional disturbances which did not depend either on the stone or the operation. There were 17 lithotomy operations, 8 of the patients being cured, 2 retaining fistulæ, and 7 dying. Of the 88, 15 underwent no operation, 6 of these are dead, and 9 are still living.

VII. *On the Operation for Umbilical Hernia.* (Gazette des Hôpitaux, 1861, Nos. 137, 146, and 149.)

M. Huguier, introducing this subject to the notice of the Paris Society of Surgery, laid down this proposition. In strangulated umbilical hernia, whether intestinal or omento-intestinal, it is perhaps preferable to abandon the case to the efforts of nature, watching for and treating any complications that may occur, rather than perform the operation. He founds this statement on the great mortality which has attended the operation for umbilical hernia in his own hands and in those of some of the most distinguished surgeons of Paris. He divides the causes which render this form of hernia and the operation for it more dangerous than other forms into two categories, primary or anatomo-pathological and consecutive or surgical. 1. As *primary* causes he enumerates the following:—(1) The abdominal aperture being narrow, resisting, and thin, exerts a contusing and almost a cutting action; (2) the mushroom shape of the hernia gives rise to a narrow pedicle, corresponding to the great mass of herniated parts; (3) the thinness of the coverings of the hernia, facilitating the production of inflammation by the taxis or other means, or even their rupture; (4) the absence or the small quantity of serosity in the sac; (5) the frequent gangrene of the sac; (6) the large size of the tumour, rendering it frequently irreducible and liable to contract adhesions; (7) the near vicinity of the strangulation to the cavity of the peritoneum. 2. *Surgical or consecutive causes.* (1) The danger of injury to the herniated parts by reason of the thinness of the coverings, the absence of serosity, and the frequency of adhesions. (2) The necessity in the great majority of cases, in order to reach the intestine, of injuring the omentum. (3) The sudden irruption of the herniated parts through the incision of the coverings, preventing the seat of stricture being reached. (4) The difficulty and sometimes the impossibility of reducing the hernia after liberation of the stricture. (5) The necessary injury to the peritoneum which takes place, inflammation being propagated, too, from the sac much more inevitably and rapidly than in the other forms of hernia. (6) From the position of the enlarged aperture after liberation of the blood, pus and decomposed fluids readily infiltrate into the peritoneum. M. Huguier has known a small quantity of blood determine a rapidly fatal peritonitis. From the above considerations it results that we should try every means to obtain the reduction of the hernia; and should only practise the operation for small, recent, and hitherto reducible hernia. In most of the other cases, we should simply facilitate the spontaneous opening of the tumour, and if necessary open the intestine to facilitate the discharge of fæcal matters, without attempting to liberate the stricture or effect reduction.

M. Goyraud, a provincial practitioner, is not disposed to take so unfavourable a view of the operation, having himself operated three times with success. M. Richet, also, believes that M. Huguier has somewhat too hastily concluded as to the general want of success of this operation in Paris. Several surgeons have there met with successful cases, although the generally dangerous character of the operation cannot be doubted. The thinness of the coverings of this form of hernia, M. Richet regards as a reason for operating early, before the intestine has become too much injured by the taxis or other causes of irritation. When the intestine has been exposed, its liberation should be effected rather by dilatation than incision, and penetration of fluids should be prevented as far as possible, either by puncture or obturation of the herniary orifice. The subsequent inflammation is best kept down by the aid of refrigerants.

MM. Boinet and Verneuil each related cases illustrating the benefit of temporizing, rather than resorting to early operations, as recommended by M. Richet.

VIII. *Osteoplasty applied to the Restoration of the Nose.* By M. OLLIER. (Comptes Rendus, Nov. 11, 1861.)

In this paper M. Ollier brings before the Academy of Sciences a practical application of his researches on periosteal grafting or transplantation. It is well known that new noses, constructed from the skin of the forehead or cheeks, when unsupported by sufficient remains of the bony parts of the nose, retract and gradually diminish, until they form as revolting an appearance as the deformities which they have been intended to repair. The case of a scrofulous young man, who had lost nearly all the osseous framework of the nose from congenital syphilis, came under M. Ollier's notice, and he resolved to employ his osteoplastic process in supplying such of the bony structures as could not be obtained from the adjoining bones. To this end he brought down a cutaneous flap from over the frontal bone, the periosteum being dissected off with it as a source of the future supply of bone. The ossification did not take place immediately, but by two and a half months the portion of periosteum had hardened into a firm, resisting support, which did not yield to pressure, having, too, adhered with such firmness to the maxillary bone as to prevent the fear of an occurrence of new deformity.

M. Verneuil (Gazette Hebdomadaire, 1862, No. 7), speaking of this case at a later period, with the plaster models and photographs before him, represents its success as very complete, and a true triumph of conservative surgery.

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QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.

Lecturer on Midwifery at St. Thomas's Hospital, Physician to the Royal Maternity Charity, &c.

I. The Non-Pregnant State.

1. *On the Pathogeny of Retro-Uterine Hæmatocele.* By Prof. C. Braun. (Monatsschr. f. Geburtsk, Supplement, 1862.)
2. *Mercurial Salivation following Cauterisation of Cervix Uteri with Nitrate of Mercury.* By Dr. Elleaume. (Mon. f. Geb., 1862.)
3. *Dangerous Bleeding from a Varicose Ulcer, after Suppression of Menses.* By Dr. Fr. Mosler. (Virchow's Archiv, 1861.)
4. *Inversion of the Hairs of the Labia Pudendi.* By Dr. Meigs. (Amer. Journal of Med. Science, April, 1862.)
5. *On the Lesions which accompany Genito-Urinary Fistulæ.* By Jobert (de Lamballe.) (L'Union Méd., July, 1861.)

1. Professor Braun published a valuable memoir on retro-uterine hæmatocele in 1860, in which the histories of three cases are recorded. The present memoir continues the former, and embodies seven additional cases. Of the present series,

Case 1 appeared to be an example of retro-uterine hæmatocele, occasioned by hæmorrhage from the right Fallopian tube. Recovery took place after puncture and removal of five ounces of blood-serum.

Case 2.—Metrorrhagia during seven weeks, and extreme anæmia. A retro-uterine hæmatocele formed, of the size of a child's head. To diagnose from ovarian cyst, an exploratory puncture was made. This gave issue to some drops of necrosed blood. A puncture was therefore made by a large trocar, and above two ounces of brown necrosed blood escaped. Slight collapse of the tumour ensued. On the following days masses of necrosed blood, containing much ammoniaco-magnesian phosphate, escaped by the punctures until the fourth day, when suddenly symptoms of internal hæmorrhage appeared, indicating a renewed retro-uterine blood-extravasation. Two days later, above two pounds of dark brown fluid escaped, the tumour suddenly shrank, an offensive discharge at last purulent, ceased on the fourteenth day. The tumour quite disappeared. The patient recovered.

Case 3.—In this instance the hæmatocele was ante-uterine, projecting through the vulva, protruding the anterior wall of the vagina. A puncture below the meatus urinarius gave issue to two pounds of chocolate-coloured fluid. Warm injections were thrown into the empty cyst for eight days. Gradually the discharge became purulent, and ceased at the end of five weeks. The patient was discharged well. The source of the blood appeared to be from the bursting of a vessel in the retro-peritoneal cellular tissue.

Case 4.—Metrorrhagia and peritonitis during pregnancy. Hæmatocele caused by extra-uterine gestation (tubal). Death.

Case 5.—Left ante-uterine hæmatocele during pregnancy. The patient was in her first pregnancy. The pelvis was filled with a fluctuating tumour, resembling a vaginal cystocele, immediately behind the urethra and the anterior wall of the uterus. After the first pains had arisen, an exploratory puncture was made; blood, partly fluid, partly coagulated, escaped. It being thought that the tumour might impede delivery, a trocar was used, and the opening enlarged by a bistoury. Whereupon, under pressure upon the tumour, and the passage of two fingers into the cavity, a large quantity of necrosed blood came away. Labour proceeded naturally. Warm injections into the cavity of the tumour were used, and under nourishing diet the patient recovered.

Case 6.—Retro-uterine hæmatocele of the size of a man's head, accompanied by metrorrhagia. Repeated exploratory punctures were made; and then a trocar was introduced; it was necessary to enlarge the opening with a bistoury, and to introduce a whalebone rod seven inches into the cavity in various directions to break up the blood-mass. Offensive necrosed blood escaped. After some weeks no tumour could be felt through the abdominal walls. Behind the cervix uteri, there remained a small hard spot.

Case 7.—Ovario vaginal hernia (hæmatocele). Incarceration of the tumour during first labour. Replacement of the tumour. Birth of a living child. Migration of the pelvic tumour during a year from the Douglasian sac into the anterior sac. Through frictions with iodine in glycerine, complete disappearance of the moveable pelvic tumour. Successful issue of second labour.

The author gives the following summary. The diagnosis could be made out with certainty in eight cases, and only with probability in two. In nine cases complete recovery took place, in one case death followed, caused by extra-uterine pregnancy and peritonitis. Puncture gave happy results in seven cases, and complete evacuation of the blood-tumour was always followed by recovery. Under expectative treatment these cases recovered. The hæmatocele was six times retro-uterine, four times ante-uterine. The hæmatocele contains a distinct framework which is easily broken down with the finger. The quantity of blood varies from a few drachms to several pounds.

2. Dr. Elleaume relates two cases in which mercurial salivation followed the application of nitrate of mercury as a caustic to the cervix uteri. In both cases the affection of the gums and mouth came on within twenty-four hours.

3. Dr. Mosler's case was that of a woman, aged forty-one. In youth she had suffered frequently from epistaxis. Menstruation had been copious. She had borne several children. The menses had been arrested during flow through a fright. She had previously suffered from varicose veins in the right leg, and an ulcer had formed. Most profuse hæmorrhage set in from this sore, the supposition of a mechanical cause being excluded. Extreme prostration attended. Being closely observed after recovery from this condition, it was found that at every menstrual period, the dilated veins of the leg became much swollen, in spite of firm compression.

4. Dr. Meigs describes a case of severe pruritus of the vulva in a young lady, which resisted the ordinary modes of treatment. He found the margins of each of the labia studded with long, straight, stiff hairs, just like eyelashes, all directed inwards, and so constantly irritating the mucous surface. The offending hairs were gradually eradicated with tweezers, and the patient was cured.

5. M. Jobert makes some useful clinical remarks upon the complications of genito-urinary fistulæ. He describes two forms of vesical hernia into the vagina: in the one, the mucous membrane alone forms a small tumour through the fistula; in the other, all the coats of the bladder are concerned. This latter form is far the more frequent. In the simple hernia of the mucous membrane there is a little sub-mucous œdema, which ought to be removed before attempting an operation. Astringent injections should be used for ten days; if this is not successful, slight cauterization or superficial scarifications are necessary. Another complication is spasm of the bladder, entailing fre-

quent desire to pass urine, and pains in the hypogastric region: a sound cannot be kept in the bladder. This, M. Jobert says, is more frequent in Italian and Spanish women than in others. If the operation for closure be performed during this condition, the contractions of the bladder will tear the wound open. Belladonna, camphor, and injections are of no use. Compresses wetted with cold water applied to the hypogastrium and vulva are useful. In most women affected with fistula, the bladder becomes much contracted. M. Jobert has four times seen the cavity of the bladder reduced to the size of a walnut-shell.

II. Gestation.

1. *Case of Early Maternity.* By Dr. J. G. Wilson. (Edinb. Med. Journ., Oct. 1861.)
2. *Some Observations upon Extra-Uterine Gestation.* By Dr. P. U. Walter, of Dorpat. (Monatsschr. für Geburtsk., Sept. 1861.)
3. *On the Origin and Growth of Tumours during Pregnancy.* By Dr. Lücke. (Monatsschr. f. Geburtsk., April, 1862.)

1. Dr. J. G. Wilson, physician to the Glasgow Lying-in Hospital, narrates a case of early maternity. J. W—— was thirteen years old on the 14th July last. She menstruated for the first time in January, and regularly till the end of April. On the 11th January she was delivered, after an easy labour, of a full-grown female child. She made an excellent recovery. Conception must have taken place when the girl was twelve years and nine months old.

2. Dr. Walter, of Dorpat, in an able memoir on extra-uterine gestation, resists the recent doctrine which denies the reality of a proper ovarian gestation. He relates one case with great minuteness, in which the evidence of dissection seems to prove that the ovary was the original seat of the development of the ovum. The following is a condensed account: A primipara, aged forty, left her husband in August, during the menstrual flow, and remained separated for six or eight weeks. The day after leaving she underwent a very rough journey in a wagon, suffering much from fear, and with difficulty keeping her seat. Menstruation did not return, but pains came on in the right lumbar region. These increased in May. Examined, there was found a globular ball filling the small pelvis. The patient felt movements of child in June. In August she complained much of abdominal pains; no fundus uteri could be felt. The fœtus could be felt through the abdominal walls, and auscultation discovered the fœtal pulsation. Extra-uterine gestation being diagnosed, the Cæsarean section was resolved upon. In the meantime, symptoms of irritative fever set in, and suddenly peritonitis. The woman died before the day appointed for the operation. *Autopsy:* A full-grown fœtus was found in the abdomen. The left ovary and Fallopian tube were normal; the uterus was somewhat enlarged; the right Fallopian tube was vascular, covered with plastic exudation pervious down to the uterine orifice, where it was closed by the decidua. (?) The fringes of its abdominal end were quite free, the ostium open; no adhesion. The right ovary was changed into a long tumour, its long axis parallel with the axis of the corpse. Its upper end reached three inches above the fundus uteri, bearing on its anterior surface the Fallopian tube. The greatest length of this tumour was six inches; width, four inches; greatest thickness, three inches. On the posterior surface of this tumour was a cavity 4″ 3‴ long, 3″ 9‴ wide, 1″ 6‴ deep, plainly clothed with chorion and amnios, which were torn, the shreds hanging in the abdominal cavity. The umbilical cord was fixed in the fundus of this cavity, where its vessels parted, and formed, with the parenchyma of the ovary, the placenta. Prof. Bidder

minutely examined the placenta. He was also of opinion that not the remotest doubt could exist that the placenta had been developed in the original ovary. Of the ovary itself not a trace remained; it had entirely passed into placental structure. Dr. Walter concludes that conception took place in January; that the spermatozoa reached the ruptured Graafian follicle in the ovary itself before the escape of the ovum; that the pains suffered in May, the fourth month, were due to the bursting of the fœtal membranes, and escape of the contents of the ovum into the peritoneal cavity; that the effused liquor amnii was absorbed, and the peritonitis healed; and that when the fœtus grew larger, pains returning, the peritonitis was rekindled, destroying mother and child. Careful drawings of the parts are given. The preparation is preserved in the Pathological Museum of Dorpat.

The memoir contains the description of two other cases of abdominal gestation.

3. The memoir of Dr. Lücke is not confined to the study of tumours connected with the pelvis or pelvic organs, but embraces the general influence of pregnancy in originating or accelerating the growth of tumours in various parts of the body. He relates a series of cases illustrating the impulse given to certain tumours by gestation, and the return to an indolent condition in the non-pregnant intervals. He considers that the period of pregnancy during which the greatest activity of growth is imparted to be from the sixth to the seventh month. He puts the question whether it would not be desirable as a rule in practice to extirpate tumours at the fifth month of gestation.

III. Labour.

1. *On Eclampsia.* By Dr. Behm. (Mon. f. Geb., Supplement, 1862.)
2. *Report of a Case of Successful Cæsarean Section.* By W. Jurran. (Mon. f. Geb., April, 1862.)
3. *Does the Uterus retain its Contractility some time after Death?* By Dr. Arbeiter. (M. f. G., April, 1862.)
4. *Subcutaneous Myotomy of the Constrictor Cunni for the Prevention of Laceration of the Perinæum.* By Dr. Cohen. (Mon. f. Geb., Supp., 1862.)
5. *Fatal Pericarditis and Peritonitis during Labour.* By Prof. Simpson. (Edinb. Med. Jour., May, 1862.)
6. *Recovery from Puerperal Cerebral Embolism, and Sudden Death in a subsequent Pregnancy.* By Prof. Simpson. (Op. supra citatum.)
7. *On the Fœtal Pulse.* By Dr. V. Hüter. (Mon. f. Geb., Supplement, 1862.)

1. Dr. Behm gives a tabular summary of 20 cases of puerperal convulsions, and deduces conclusions by the method of statistical analysis. He found that 75 per cent. of the cases were primiparæ; that the first fits came on in 7 cases before the normal term of gestation, causing premature labour in 5; that in 13 cases the fits first appeared at the full term of pregnancy—namely: during the stage of dilatation, or before the rupture of the membranes, in 2 cases; after escape of liquor amnii, and before birth of child, in 7 cases; during the expulsion of the placenta, in 1 case; after completion of labour, or during the puerperal week, in 3 cases. In 11 cases in which the fits arose during labour, they persisted into the puerperal week.

The head presented in 16 cases; breech in 2 cases, 1 being a twin; and the cross-births were 3.

The issue for mother and child was in recovery in 11 cases. In 2, both mother and child died, in 1 the mother being undelivered. In 6 cases the mother alone recovered; in 1 case the child alone recovered. Thus 17 mothers and 12 children survived. Of 8 dead children, 2 were found macerated and premature.

Labour was terminated by the natural process in 8 cases; by forceps, in 9; by turning, in 2.

In treatment, Dr. B. especially relies upon bleeding and calomel.

2. The subject of Mr. Jurran's Cæsarean section was a woman, aged twenty-seven, living at Penig. She conceived June, 1859. On the 5th March, 1860, labour set in. No progress being made, bark and borax were given. This being ineffectual, Mr. Jurran was called in. He ascertained the following condition of the pelvis: the ossa innominata were only seven inches apart; the trochanters, ten inches; the conjugate diameter, under $2\frac{1}{2}$ inches. Pains continuous. Cæsarean section was performed, with assistance of Dr. Jancovins, under chloroform. A living child was extracted. The labour completed, the abdominal cavity was carefully cleansed of blood-clots by a sponge. The steps of the operation offer nothing peculiar. Some fever and peritonitis followed, but mother and child ultimately did well.

3. Dr. Arbeiter relates a case which illustrates the properties of the uterus, and serves to explain those remarkable cases in which the fœtus has been expelled after the mother's death. A pregnant woman died of internal hæmorrhage, the result of laceration of the cervix uteri and fundus of the vagina. Dr. Arbeiter, arriving after three-quarters of an hour, found all the signs of death. He turned, and removed the after-birth by hand. During this operation the uterus was flaccid and yielding. After being completely emptied, it contracted to the usual hard ball, as after natural labour.

4. Dr. Cohen refers to a paper published by him in 1860, in which he combated the theory that perinæal laceration was due to excessive distension of the perinæal skin, and suggested that the first cause was rending of the fascia and muscles at the outlet of the vagina. He now proposes to obviate the risk of laceration by subcutaneous division of the constrictor cunni in cases where the accident seems impending. His method of proceeding is as follows: During a pain, the constrictor muscle is, he says, stretched or contracted into a string-like form, and is easily felt near the clitoris. He introduces a fine tenotome on the outside of the muscle about the middle of the nympha, and if the pain persists, then turns the edge of the knife on to divide the muscle, taking care not to cut through the mucous membrane internally. The knife being then turned on the flat, is withdrawn. Dr. Cohen relates 2 cases in which he says the effect was surprising.

5. Professor Simpson has related a case in which a woman, who had arrived at the full term of gestation, died suddenly some hours after the commencement of labour. The abdominal cavity was filled with serous fluid; the intestines were loosely gummed together; the fimbriated extremities of both Fallopian tubes were highly congested, and covered with inflammatory deposits; the ovaries presented a similar appearance; the uterine parietes were unusually soft and friable; the fœtal pelvis was tightly jammed in the maternal brim. No local cause for the peritonitis could be discovered. The pericardium was also distended with serum. Dr. Simpson was not aware that there was any such case upon record of a woman dying of acute peritonitis during parturition, with the exception of one related by Dr. Clarke, regarding which Dr. Hamilton used to aver that they must have examined the body of the wrong patient in the dissecting-room.

6. Professor Simpson records the history of another sudden death in pregnancy which is invested with unusual interest. A patient, five years ago, in

the eight month of pregnancy, after walking upstairs, was found in an almost senseless state. Hemiplegia of the right side followed. The carotid was beating more powerfully on the left side than the right; the mitral orifice of the heart was contracted. It was concluded that a vegetation had been detached and carried into the cerebral vessels. The patient recovered, went her full time, and bore one or two children afterwards. She walked lamely, and had only imperfect use of the right hand. When in the seventh month of pregnancy, her breathing became oppressed; she was cold and collapsed, but sensible; the pulse was small and fluttering; action of heart tumultuous. In two hours she died. At the base of the brain on the left side, below and to the external side of the left ventricle, a considerable cavity existed, which involved the lower and anterior portion of the corpus striatum, and was lined by a very vascular serous-looking membrane. No obstruction could be discovered in the vessels leading to the part. It was conjectured that the vessel in which the embolus had lodged five years before had undergone atrophy and absorption. The left auriculo-ventricular orifice was so contracted as barely to admit the point of the forefinger; the valves were shortened and thickened, and on their free surfaces were warty-looking projections overlaid by fresh coagula. The pulmonary arteries were quite free.

7. Dr. Hüter has carefully observed the rate of the fœtal pulse under various conditions. His observations were made in the lying-in institution of Marburg. He counted during five seconds, repeating the observation several times. In the case of a woman who wanted nineteen weeks of term, 19 observations gave from 11 to 12 beats in the five seconds, or from 132 to 144 in the minute. In 2 cases taken seventeen weeks before term, the average of 34 observations gave about the same frequency. In 2 cases taken sixteen weeks before term, in 4 cases taken fifteen weeks, in 1 case taken fourteen weeks, in 8 cases taken thirteen weeks, in 6 cases taken twelve weeks, in 8 cases taken eleven weeks, in 8 cases at ten weeks, in 12 cases at nine weeks, in 9 cases at eight weeks, in 17 cases at seven weeks, in 16 cases at six weeks, in 18 cases at five weeks, in 16 cases at four weeks, in 20 cases at three weeks, in 31 cases at two weeks, and in 21 cases taken at one week before term, multiplied observations gave a range from 10 to 14 beats in the five seconds, by far the most frequent number being 11, or 132 in the minute. In all cases care was taken to eliminate the influence of movements of the fœtus, which are known to accelerate the pulse. In 1195 observations, the rate was 132 in the minute 825 times. 310 times the rate exceeded 132, but in 73 of these instances the rise was observed in relation with movements of the fœtus. Sixty times the rate was below 132, but it was never below 120. This result accords with those of other observers.

Dr. Hüter extended his observations to the period of labour in the 200 women who had been the subjects of the preceding countings. He found that during labour the frequency of the fœtal pulse was 12 beats in ten per cent., 11 beats in eighty-three per cent., and 10 beats in seven per cent.

If it be ascertained that in a pregnant woman the normal frequency of the fœtal pulse is 10 in the second, if it should rise to 12 it may be concluded that fœtal movements are the cause of the increase. It may rise under this excitation to 15, but generally falls rapidly to the normal rate when the movements cease.

Dr. Hüter then examines the influence of other conditions upon the fœtal pulse. He cites the observations of Hohl and others concerning the effect of fever and other diseases of the mother. He relates in detail several cases of his own. Under febrile movement of the mother, the pulse of the fœtus commonly rose to 12 and 15 beats in the five seconds. As to the effect of protracted labour, Dr. Hüter observed that in a case in which the rate was 11 before the rupture of the membranes, it rose to 12 and 15 under pressure of the head in the pelvis, that it fell to 5 and 6 during labour-pains, rising again in the intervals. [This effect of labour-pains upon the fœtal pulse accords with the observations of the Reporter.—R. B.]

For want of space, reference is made by title only to the following papers:

On a Peculiar Hyperplasia of the Decidua. By Dr. Strassmann. (M. f. G., April, 1862.)

On the Use of Injections of Matico in Blennorrhagia and Vaginitis. By M. Guibout. (L'Union Méd., Feb. 1862.)

On Turning by the Feet in Cross-presentation, with Prolapsus of an Arm. By Dr. Spöndli. (M. f. G., April, 1862.)

On the Internal Measurement of the Pelvis, with a Description of Two New Instruments for this purpose. By Prof. Germann. (M. f. G., 1862.)

On the Cæsarean Section on the Dead. By Dr. Schwarz. (This memoir is chiefly devoted to an historical survey of the laws of different countries as bearing upon the question.)

On a Successful Delivery by Decapitation. By Prof. Martin. (M. f. G., April, 1862.)

On the Frequency of Twistings of the Umbilical Cord, and the Influence of this Condittition on the Birth of the Child. By Dr. Veit. (M. f. G., April, 1862.)

Contributions to the Anatomy of the Pelvis. By Dr. Schwegel. (M. f. G., 1862.)

THE LATE EDWARD STANLEY, F.R.S.

Edward Stanley was born in London, in July, 1792. His father was engaged in trade, and not rich. He was educated at Merchant Tailors' School, and in 1808 was apprenticed to Mr. Ramsden, one of the surgeons to St. Bartholomew's Hospital. Probably his destination to the profession was determined by the success of his uncle, Mr. Thomas Blizard, from whom, late in life, he inherited a considerable property. In 1812, on the death of Mr. Ramsden, he became the apprentice of Mr. Abernethy, who easily saw in his devotion to anatomy and his love of dissecting and of collecting museum-specimens, the qualities of such an one as he wanted, to help him in his anatomical teaching. Accordingly, soon after Mr. Stanley had passed the College of Surgeons in 1813, Mr. Abernethy made him Demonstrator. In the ten years during which he held this office, he bestowed great labour in augmenting the anatomical collection which in 1828 was presented by Mr. Abernethy and himself to the Governors of St. Bartholomew's, and became the foundation of that now great museum. In 1816 he was appointed assistant-surgeon to the hospital, but he still gave himself almost exclusively to anatomy. In 1826, he shared with Mr. Abernethy the lectures on anatomy and physiology, and from 1829 to 1843 was sole lecturer. In 1838, he became surgeon to the hospital, and from that time the ardour which he had shown in the anatomical schools was equalled, or even surpassed by that with which he devoted himself to the duties of the wards.

Some years before his election to the surgeoncy of St. Bartholomew's, Mr. Stanley had been chosen on the Council of the College of Surgeons; in 1844 he became one of the Examiners; and he was twice President. He was made a Fellow of the Royal Society in 1830, President of the Medico-Chirurgical Society in 1843, and Surgeon-Extraordinary to the Queen in 1858. In 1861, he resigned the surgeoncy of the hospital, chiefly because he felt some signs of failing health, and believed that he could not safely continue to do all the work which, in his judgment, the office demanded. He knew, indeed, that he could do as much as most men in the same position are content with doing; but this was not enough to satisfy his sense of duty. On his retirement he was appointed consulting-surgeon to the hospital, and in the duties of this appointment—the highest of which he could ever have been ambitious—he met death. On the 24th of May, after witnessing a lithotomy, performed according to his own teaching, he went to a consultation with Mr. Lawrence and others of the staff. He had just given his opinion with his usual care and scrupulous balance of considerations, when he fell senseless in apoplexy. After a momentarily-recovered consciousness, he passed into a deep insensibility, and within an hour died.

The narrative of the chief events of Mr. Stanley's professional life may seem only such as might be written of many of the successful hospital surgeons of the day. His may look like the ordinary career from hospital apprenticeship, through the teaching of anatomy, to the hospital surgeoncy, and to good private practice and the gains and dignities of the surgical profession. Yet they who knew him well can read out of his life a lesson which may be useful to some of those who are ambitious of a similar success. For his career—at least in all its earlier part—was a very difficult one; he had to overcome such obstacles as most men would have stopped at. In early life he was poor, and the strife for station in the hospital was hard and very bitter. He had little of what men commonly call the gifts of nature, and till he proved his own power and worth, there were very few who willingly helped him.

The quality by which, before all, he secured success, was his power of incessant and laborious work. This—surely the best of nature's gifts—never failed him; as a student (and he was a student all his life), he was always laboriously learning; as a lecturer, he laboriously taught; as a hospital-surgeon, he gave more time and thought to his work than did any of his day; in every office that he held, he grudged the pleasure that withdrew him from his duty. Besides, he was a man of very honest purpose, and thoroughly trustworthy. In his professional relations he was always fair and upright. Whatever office he filled, he gained the confidence of the best and the chief among those he worked with. And he was a warm-hearted man, active and genial in his friendships, readily sympathizing with his patients, and keenly alive to all the responsibilities of his profession—so keenly, that he felt with the most painful bitterness even those failures which could not be averted, and often lost, in an excess of anxiety and doubt, the happiness which he had honestly earned, of feeling that his duty was well done.

With these good qualities Mr. Stanley attained and honourably filled the highest offices in his profession, and one of the best positions in private practice; and this he did, though he lacked that self-reliance without which so few succeed; for he was naturally timid, and many events of his early life made him over-anxious, and too ready to follow others when he might better have guided himself.

As a teacher of anatomy, Mr. Stanley was safe and sound. His knowledge was less than that of some of his contemporaries, but what he had was very good, and he taught it with such emphasis and repetition, that every one learnt of him. Clear and slowly fluent, though without eloquence, his lectures were, at least for the purpose of teaching, among the very best of the time. Their extent and general character may be judged by the Manual of Anatomy which he published, and which was for many years the guide of nearly all his pupils in the dissecting-room.

Among his published surgical writings, the best are his 'Essay on Lithotomy,' and some of his papers in the 'Transactions of the Medico-Chirurgical Society.' It would be difficult to find, even in that admirable collection, any essays in which single strong points are better proved than in his papers "On Irritation of the Spinal Cord and its Nerves in connexion with Disease in the

Kidneys" (vol. xviii.); "On Dislocations, with Elongation of the Capsule and Ligaments;" and "On Congenital Tumours of the Pelvis," (vol. xxiv.) His larger work, the 'Treatise on Diseases of the Bones,' published in 1849, cost him enormous labour. He worked at it for more than thirty years, and, with his usual self-distrust, delayed its publication till it was less useful both to himself and to others than it would have been if printed many years earl It is an honest record of good facts plainly narrated, and the illustrations are among the best ever published with a surgical treatise; but having been begun on the narrow base of a Jacksonian essay, it could not be enlarged and changed at the same rate with the progress of surgical science while it was being written.

Mr. Stanley's excellence in surgery was in cases which admit of the best illustration from plain anatomy, such as fractures, dislocations, hernia, lithotomy, and diseases of bones and joints. On all these his opinion and his clinical lectures were of great value, and his practice was safe and usually resolute. He was less happy in cases of more difficult pathology, and in those requiring medical as well as surgical knowledge. For these his early education had been defective: he had been too long and too much engaged in anatomy. In operating he was not dexterous, but he was perfectly cool and collected; nothing ever seemed to shake either his hand or his mind; he compensated by steadiness and perseverance for much of his want of skill, and his undertakings usually ended better than in their progress they seemed likely to end. In no position did he show the good points of his character more than in assisting his colleagues in their work. When he had to take the sole or chief responsibility of a case, he was often too self-distrustful, too apt to rely on others, too timid about possible consequences. But in consultation he would usually give his whole mind to his duty, would give his own opinion honestly and clearly, and would take his full share of a difficulty or a responsibility with no other desire than that his colleague should have the whole advantage of the success that might be gained.

Such was Edward Stanley; and if we could write down the whole sum of life and happiness which he gained for others in his forty years of studious practice, the knowledge which he imparted to more than three thousand pupils, the example of constant work and of honest love of truth which he daily set before all among whom he lived, his stores of facts accumulated and perpetuated in his essays and museum, and his punctual discharge of every official and social duty, there would surely be such a record of good work achieved as could be written of only a very few of the best of those whom we have known.

THE LATE PROFESSOR SCHROEDER VAN DER KOLK.

Our present number contains notices of two of the numerous contributions which the indefatigable industry of the above distinguished physician, removed since they were written, in the midst of a career of beneficence and usefulness, from the scene of his earthly labours, has from time to time made to medical and physiological science. It would ill become us, therefore, to pass over in silence an event which has inflicted so severe a loss upon the interests of humanity, and has left in the ranks of the profession a chasm which will not easily be filled.

In a letter at present before us, dated 20th December, 1861, the late Professor complained that he had long been impeded in his work by symptoms which it is now evident were premonitory of his last illness; but he added that these were again abating. The improvement thus alluded to proved, however, deceptive, and the disease under which he laboured—ulceration of the colon and rectum—soon assumed the most alarming aspect. Nevertheless, the skill and attention of those about him seemed for a time to overcome the intensity of his malady, and to be crowned with success; "a new life, as it were, opened before him, of which his active mind had already marked out the useful direction," when fever, the result of taking cold, speedily reproduced the whole train of symptoms, under which he sank on the evening of the 1st of May, 1862, in the sixty-sixth year of his age.

During the early years of his professional life, Schroeder van der Kolk's attention was directed chiefly to the study of pathological anatomy. While Resident Physician in the Suburban Hospital at Amsterdam, he spent his leisure time in the preparation of minute injections, and formed a collection representing in different series most of the lesions to which the human body is liable. Being possessed of artistic powers, most of his publications are illustrated with drawings from nature by himself, and he also executed the lithographs in his well-known 'Observationes Anatomico-Pathologici et Practici Argumenti,' published in March, 1826—a work occupied chiefly with his investigations respecting phthisis and other pulmonary diseases.

In 1826, Schroeder van der Kolk was appointed Professor of Anatomy and Physiology in the University of Utrecht, and in 1842 he became one of the two Inspectors of Lunatic Asylums in the kingdom of Holland. In each of these capacities he laboured as usual to confer the largest amount of benefit possible on those around him. In the University he was in the habit of giving his observations in anatomy and physiology to be wrought out by his best pupils—a plan which resulted in the production from that source of a large number of valuable dissertations. As inspector, he brought the institutions for the insane placed under his control to such a state of order and efficiency, that they are now, we believe, the most perfect models extant of what such asylums ought to be.

Professor Schroeder van der Kolk's important researches on the minute structure and functions of the nervous system, and on the influence exercised by the nervous centres upon the nutrition of the body, have been made so familiar to the English reader by the recent publication by the New Sydenham Society of three of his works, that we need not dwell upon them here. Of his essay 'On the Formation and Extension of Cancer Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation,' an abstract will be found in the fifteenth volume of this Review.

His essays on the presence of elastic fibres in the sputa of phthisical patients, and on the origin and formation of tubercle, have also, with some minor papers, been translated into English. The most important of his works which has not been so translated, is his 'Observations on the Structure of the Human Placenta, and on its Circulation,' which was published by the Royal Netherlands Institute in 1851.

Of Professor Schroeder van der Kolk's more popular writings, two—namely, an essay 'On the Independence of the Soul,' evidently directed against the errors of Materialism and of Rationalism, and an essay 'On Maternal Love in Nature'—have appeared in an English dress in the pages of the 'Psychological Journal.'* These essays prove their author to have been a man of the most varied capabilities and of the utmost refinement of mind. They show also that he was one who rested not in second causes, but who looked from Nature up to Nature's God, and who learned humility from each fresh acquisition to his varied store of knowledge. "If it is, then," he says, "the language of the Creator which we read in the book of nature, where everything bears the mark of the most elevated love and truth, perfection and order, let us, who can penetrate only to the outer covering of nature, gaze with reverential admiration at the wisdom, love, and greatness of the omnipotent Maker who has created all with the word of His power, 'Let these things be.'"

* July, 1860, and Jan. 1861.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1862.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Geschichte der Medicin in Russland.* Entworfen von D. W. M. Richter. 3 theile.—*Moskwa,* 1813. 8vo.
History of Medicine in Russia. By Dr. W. M. Richter.
2. *L'Histoire de la Civilisation en Russe.* Par M. Gerebtzoff. Two Vols.—1858. 8vo.
History of Civilization in Russia. By M. Gerebtzoff.
3. *The Roll of the Royal College of Physicians of London, compiled from the Annals of the College and from other Authentic Sources.* Edited by W. Munk, M.D., Fellow of the College, &c.

An impressive scene was enacted in Russia at the beginning of last century. The redoubtable Czar Peter, in whose hands the Russian people were like clay in the hands of the potter, having heard of a patient, a certain Frau Borst, who refused to submit to a surgical operation, marched to the spot where she lay, overcame her resistance, performed the operation of paracentesis abdominis with his own imperial hands, and in the presence of the small group then constituting the Russian medical faculty, drew forth twenty-four pounds of water. The monarch was indeed a resolute reformer in medicine as in other matters. He improved himself in the art of surgery as he improved himself in the art of war—by constant practice. And if by many defeats at the hands of Charles XII. he learned to conquer the Swedes on the field of Poltava, we have a misgiving that ere he achieved the successful tapping of a dropsical patient, he had suffered many mischances in surgery at the expense of his victimized subjects. Thus at least thought his relative, the Duchess of Mecklenburgh, who hearing that his Czarish Majesty had cut away an abscess from a merchant's foot, fled with all the nimbleness in her power from the city in which the august operator was staying, in dread lest the lancet which he always carried about him should be applied to her own foot, suffering from a like disorder.

The violence and extent of the changes wrought in Russia by the exercise of Peter's sole and unresisted will, have led to the very common opinion that the Muscovites were before his time a nation totally devoid of science or learning. Such an opinion is erroneous. The subtle influences of civilization, letters, science, and art, had penetrated from Constantinople far into the dominions of Russia centuries before, when Russian princes dictated terms of peace to Byzantine emperors, and formed matrimonial alliances with them. Afterwards came a deluge of Tartaric invaders, which covered the land with darkness and misery for two hundred years, until the yoke was cast off, and the Czar of Muscovy became in the days of our English Tudors a potent prince, courted by many of the sovereigns of Europe. A disastrous interregnum and civil war ensued at the beginning of the seventeenth century, which terminated in the election of the first Romanoff to the throne. Again art and science began to flourish, and a civilization of an original, Oriental type was slowly developing itself with European aids applied in homœopathic measure, when suddenly the wilful Peter took the nation by the neck and flung it upon Europe.

The course of medical history and medical literature in a state of things thus loosely indicated was not fruitful of interest. The materials of such a history are scanty in the extreme. We can extract nothing of scientific value from the records within our reach, and rely rather upon biographical details to show how slowly the noble science of medicine progresses even among an intelligent though semi-barbarous race. The diligent and patient Richter has eked out the greater part of his first volume with reasons for the non-existence formerly of physicians in Russia—namely, the extreme salubrity of the country, and the consequent robust health and remarkable longevity of the people. These reasons would be all the more satisfactory, were it not for a ghastly list extending over fourteen printed pages of the various plagues and pestilent visitations from which Russia suffered between the years 1090 and 1656. The pictures suggested to the imagination by this methodical tabular statement are truly appalling. It is amusing, at the same time, to read Richter's laboured eulogies of the *physique* of the people among whom he lived, and the conclusion to which he is led, that in the good old times before the corrupting influences of luxury came in, a doctor would have been a superfluity in Russia. It is true the Sclavonian races are large, well-made men, of good average health and strength, but few individuals among them possess the stamina of a well-fed, well-trained Englishman. It is pretty well ascertained now that the troops of the Czar sustained heavier losses through privation and disease under the severe trials of the Crimean campaign than did the English troops. Boldly assuming, therefore, that the Russians of the Middle Ages inherited a share of the ills which visit mankind, we pass from the primitive state of society in which the father of the family is the medicine-man, to the age when priests followed the example of Luke, the beloved physician, and healed the bodies while they strove to comfort the souls of their converts. Of such was Ephraim, who accompanied Anna, sister of the Emperors Basil and Constantine, from Constantinople to Kieff, and there founded a house for the sick. Cave, in his 'Historia Literaria' (ii. 113), gives an account of Johannes Smera Polovecius, who flourished in 990, and whom he styles "medicus et rhetor" to the Grand Duke Vladimir, husband of the (Greek) Princess Anna. When Vladimir had resolved to abandon the Pagan religion and impose a new faith on himself and his people, he thought it wise and becoming in a Russian Prince to obtain accurate information on a subject of so much importance. He sent confidential agents into the various countries within reach to explore the religious world of that day, and from the reports thus furnished he decided on rejecting the Mahometan and Roman Catholic creeds, and adopt the ritual of the Greek Church. Smera was one of the deputies employed on this delicate mission. His peregrinations extended as far as Egypt, a letter from whence, written to the Grand Prince on brass tablets, was long preserved in the principal monastery at Kieff. It was printed in the sixteenth century, and is remarkable as containing a strong condemnation of the Greek Church, and a threat that if Vladimir should adopt the faith of that Church, he, Smera, would not return to Russia. The royal convert, unlike Louis XI., was not over-terrified by his physician's menace, and Smera we hope died peaceably in Egypt. The next hero of the healing art—for we are not authorized to call him "medicus" by the solitary operation recorded in his favour—was the Father Confessor of Sviataslaff, the son of Jaroslaf, who flourished in 1076. He cured a dangerous bleeding of the nose to which the monarch was subject, by stopping up the nostrils with some kind of rag. With equal absence of scientific pretension, St. Olympius, in 1087, restored a rich inhabitant of Kieff by anointing his face with ointment. The plain sense of St. Agapyt exposed the quackery of a celebrated Armenian leech, who pretended by a glance to ascertain the precise day and hour of a patient's death. The worthy father, before he was a saint, saved the life of a boyar, which had been thus foredoomed, and also effected in 1113, by the use of simples, the cure of Vladimir Monomach, one of the most eminent of Russian princes.

The gravity with which Richter narrates puerilities of this kind veils but feebly the poverty of the subject of early medical history in Russia. The names and no more of three physicians of the fourteenth century have been preserved—Avram Palitzin, Pafnutin Borovskoi, and Pimen Postnik. In the fifteenth century, we are assured that Prince Dmetri, the son of George, was also cured of a dangerous bleeding at the nose by Father Josias; that Prince Vassili Vassilievitch underwent the needlessly painful operation of cauterization for consumption; and that, in 1490, the son of Ivan Vassilievitch was cupped. The operation was performed by Leo the Venetian, whose arrival at Moscow in that year constitutes an era in Russian physiography, if we may coin the word. He came from Italy with Andreas, brother to Sophia Phominishna, the second wife of Ivan Vassilievitch. Under the stern and wily rule of this Prince, Russia was again raising her head. Ambassadors from many countries paid their respects to the Muscovite sovereign, and in their train came skilled artisans, engineers, and men of science of various degrees. Leo was a Jew by birth, and his miserable fate long deterred other learned foreigners from venturing into Russia. His illustrious patient, Ivan Ivanovitch, the Czar's eldest son, was suffering from a species of gout in the foot, and Leo, rashly confident in his skill and his remedies, staked his life on the cure of the Prince. The treatment by decoctions of simples taken internally, and the outward use of cupping, is thus described by the Russian annalist:—"The doctor began his cure by giving the Prince simples to drink; he began to burn him about the body with glasses into which he had poured boiling water; thereupon the Prince grew much worse and died." In those days the practisers of the healing art were popularly placed in the same category with magicians. Failure in a cure was regarded as the consequence of ill-will towards the patient. In Russia, this superstition was stronger than in any other part of Europe, and Dr. Leo, originally obnoxious as a Jew to all true believers, was doubly so as a foul sorcerer who had brought about the death of the heir to the throne. The forfeit to which he had boastfully bound himself was exacted to the full, and he was publicly executed on the 22nd of April, 1490. M. Lajetchnikoff, the Walter Scott of Russia, has founded upon this tragical incident his novel of 'The Heretic,'* in which the reader will find an admirable picture of Muscovite civilization in that day, deduced from contemporary chronicles.

The fifteenth century was fruitful in plagues and sundry kinds of disease inflicted on Europe, of which the venereal was the most fatal bequest bestowed on posterity. It entered Russia, through Poland, towards the close of the century, and under the name of the "French disease," excited so much alarm that the Czar gave special instructions to an ambassador going to Poland to obtain all possible information on the subject at the town of Viazma, whither it was said to have been first brought from Vilna or Smolensko. Collins, in his 'Present State of Russia,' alludes facetiously to the capture of this foul complaint "by the Russes during their conquests of many towns and provinces on the borders of Poland."

More to our present purpose is the circumstance that Richter takes credit to the Russians for being the first to make use of mercury as a remedy for this disease.

The first quarter of the sixteenth century is past ere we again find the names of any distinguished physicians in Russian history. Dr. Nicolai Looyeff, whom Richter supposes to be a Greek, and Dr. Theophyle, who came from Prussia, were both in attendance on the Czar Vassili Ivanovitch during his last illness. Theophyle had vainly attempted, in 1518, to obtain liberty to return home: the Czar would

* Published in English by Mr. Murray. 1844.

not part with him, though requested to do so by the Brandenburg envoy. Still there was no general advance in medical science.

The treatment of Vassili Ivanovitch, in 1534, for an ulcer in the bend of the thigh, was of the rudest description. After the application of salves, poultices, and baked onions, to the sore to induce suppuration, an ungentle fomentation of corn brandy was employed to get rid of the offensive fluid of a caries. The monarch's case being desperate, he called Nicholas Looyeff to him, and inquired if recovery were impossible. Richter quotes the little speech made in reply, as evidence of the physician's courage and straightforwardness:—"My lord and master!" said Looyeff, "when I was at home in my own country I heard of your great goodness and generosity, and left my father and mother to come to you. But am I able to raise the dead? I am not God." The good doctor doubtless remembered the fate of Leo, and would make no rash predictions. The Czar turned to his boyars, and said, "Nicolai has pronounced sentence of death on me!" Vassili died, but we are not told that either of his physicians suffered in consequence.

In the reign of his successor, Ivan Vassilievitch, surnamed the Terrible, medical science made slight progress in Russia. A Manual of Health, which as early as 1423, had been translated at Cracow, from Latin into Polish, was, in 1588, translated from Polish into Russian at Serpuchoff, by the directions of the Voevode Butourlin. A copy of this earliest Russian work on medicine, in manuscript, bearing the title of 'Lechebnia Knega," was in the possession of Dr. Bause, of Moscow, before the French invasion of 1812, but was destroyed in the conflagration of the city in that year. It contained a variety of ordinary facts relating to domestic economy, sickness, childbirth, &c., with directions for the use of simples and other remedies in disease. Some of the facts are startling, and the remedies naïve. The magnet is said to grow in India by the great ocean, and a certain cure for bad dreams is a ruby suspended round the body.

Ivan Vassilievitch, like our own Henry VIII., reigned in two characters. Previous to the death of his first wife, he showed himself one of the ablest and most virtuous princes that ever sat on the throne of Russia. His inhuman tyranny, engendered by suspicion, did not break out till some time after the death of his beloved consort. In the first portion of his reign he laboured assiduously to improve his country, corresponded with the Emperor Charles V., and with other potentates, and procured from them supplies of skilful artificers and men of science. His first favourite physician was Dr. Arnolfi, an Italian, whose close attendance on the person of the Czar was occasionally interrupted by official visits to those sick persons among the nobility whom the sovereign delighted to honour.

In the year 1553, the expedition of Richard Chancellor into the White Sea opened that friendly communication between England and Russia which continued without interruption for three centuries. The Russian boyar, Nepeja, who accompanied Chancellor to England as the Czar's ambassador to Edward VI., returned to his own country, in 1557, with the well-known traveller, Anthony Jenkinson. Among the persons who joined the travellers—in consequence, it is to be presumed, of the brilliant prospects held out to them—were a physician and an apothecary. The former of these, Dr. Standish, is described in the Roll of the College of Physicians* as of St. Nicholas Hostel, Cambridge, A.B. 1542, A.M. 1547. He served the office of proctor of the University 1551–2, and commenced M.D. 1553. On the 5th November, 1556, he was licensed by the College of Physicians to practise for one year only, in virtue of a bylaw which had been passed the previous year. In the following May, however, he was on his way to a region that had indeed been perilous to men of his calling, but where the jurisdiction of the College did not extend. He was most graciously received by the Czar, dined with him several times, received presents from him of sables, seventy roubles in money, and a horse to ride about the town. In an old document quoted by Richter, Master Standish is not named individually; but in the enumeration of gifts sent to the Czar by King Philipp and Queen Mary, appear, after a lion, lioness and cubs, various arms and armour, "very skilful and experienced doctors and miners," who arrived "all together, safe and sound." Richard Elmes, who must have been included in this incongruous group, and was probably the apothecary, long afterwards incurred the wrath of his Czarish Majesty by some offence unknown to historians. He was pardoned through the intercession of Sir Jerome Bowes, the English ambassador at Moscow, with whom he returned to England in 1584. We unfortunately know very little of the life and proceedings of these early medical emigrants to Russia, but that they inspired the ferocious Czar and his subjects with confidence is evident from the continued demand in Moscow for English physicians. In 1567, ten years after the arrival of Standish, a doctor named Reynolds, an apothecary, Thomas Carver (who perished in the conflagration of Moscow in 1571), with a surgeon, an engineer, a goldsmith, and a gold finer, reached Moscow, having obtained the express permission of Queen Elizabeth to offer their services to the Muscovite Prince. The doctor was salaried with two hundred roubles, the apothecary with one hundred, and the surgeon with fifty roubles. King Sigismund of Poland remonstrated with the Queen of England for holding intercourse with the Russes, thereby improving their condition and rendering them really formidable enemies. Ivan, on the other hand, laboured to obtain a secret alliance with Elizabeth against the Poles, and manifested his displeasure at her caution in the matter by lodging her ambassador, Randolph, in a house which no one was allowed to approach. Dr. Reynolds, however, contrived to visit his countryman clandestinely.

Dr. Elisæus Bomel, a charlatan of some notoriety, appears next in the long shadowy procession of medical practitioners in Russia. In the summer of 1570 he accompanied the Russian ambassador who bore from London a treaty of friendship between Elizabeth and Ivan

* Edited by Dr. Munk, vol. i. p. 51. 1861.

Vassilievitch. He was a native of Wesel, in Westphalia, had studied medicine at Cambridge, and passed for a skilful astrologer. He was even sent to the Queen's Bench Prison as a sorcerer by Archbishop Parker. He was about to be released, on condition of his immediate departure from England, when he wrote to the Archbishop, informing him that he could point out means for averting a great peril that threatened England. Parker communicated with Cecil, who also received a letter from Bomel and an extract from his treatise, 'De Utilitate Astrologiæ." In this learned work the Doctor attempted to show from history that great changes take place in a kingdom every five hundred years. That period having just elapsed since the Norman invasion, Bomel bid the wise minister beware. The unfortunate man did not live to hear of the invincible Armada (1588), or he might perhaps have plumed himself on the value of his astrological science. He was still in prison when he wrote to Cecil that the Russian ambassador had invited him to go to Moscow, and he promised to keep the Minister well informed of all that should happen at that Court if he were permitted to go. The permission was granted, and Bomel became physician and astrologer to the Czar, whom he soon contrived to perplex and mystify. After exercising a baneful influence over that monarch in his worst days, he became involved in a conspiracy of the Novgorodians, Poles, and Swedes, for the overthrow of Ivan. He was seized, and put to a cruel death. Having been put to the torture, he was bound to a stake and thrown on the fire. Jerome Horsey, the account of whose sojourn at Moscow has been printed by the Hakluyt Society, was present at the execution. Bomel, he says, "had deluded the Emperor, making him believe the Queen of England was younge, and that yt was very favorable for him to marry her. He lived in great favor and pompe; a skilful mathematician, a wicked man, and practiser of much mischief. Most of the nobles were glad of his despatch, for he knew much by them." When his bleeding body was roasted till life was thought to be extinct, it was cast into a sledge and brought through the Kremlin. "I prest among others to see him," says Horsey. "He cast up his eyes, naming Christ." "Being cast into a dungeon, he died there." Of the man who suffered this horrible doom, Dr. Strype says: "He was a physician of great fame, pretending to be skilled much in art, magic, and astrology, as well as physic; perhaps the son of Henricus Bomelius, a preacher of God's Word at Wesel."

The unfortunate man's widow, an Englishwoman, named Jane Richards, was permitted to return home in 1583, on the special application of the English ambassador, Sir Jerome Bowes.

Notwithstanding Bomel's treason, English medicine and its practitioners did not fall in the estimation of the Czar; for in 1581, the year after Bomel's death, Dr. Robert Jacob was sent with a highly flattering letter of recommendation from the Queen, in one of a fleet of merchantmen, which Jerome Horsey was conducting from England to Russia. The doctor was maintained by the Russia Company for some months, until Ivan appointed him a regular stipend. He it was who recommended Lady Mary Hastings to the Muscovite Prince for his seventh wife. Happily for the lady, the Czar died before the conclusion of the strange matrimonial negotiations which were begun under the sanction of the Queen of England. Jacob returned to England in 1584, very soon after the decease of Ivan Vassilievitch; but the succeeding monarch, Theodore Ivanovitch, had ere long to seek his aid in a matter of great delicacy and importance. Horsey, on reaching England in 1585–6, writes:

"I spent a good time inquiring of the learned physicians of Oxford, Cambridge, and London their opinions and directions concerning the Empress Irenia in some difficult matters relating to conception and procuration of children; she had been married seven years, and often conceived; with some other marriage matters, wherein I was charged with secresy."

He was instructed to find an experienced midwife, and having laid the matter before the Queen, that practical lady wrote a friendly letter to the Czarina, in which she says:

"We have not only sent you (as you lovingly requested us) an experienced and skilful midwife to assuage the pains of childbirth by her science; but we send you with her the aforesaid Dr. Jacob, our physician, who has been wont to take care of our health (a man previously known to you, full of faith in the medical art, in which he excels), in order that he may superintend the operations of the midwife, and faithfully tend your health."

A cruel political intrigue neutralized these friendly endeavours to benefit the Czarina's health. Irenia's brother, Boris Godunoff, a man of great ability and boundless ambition, possessing already great ascendancy over the Czar Theodore, whose health was never robust, aspired to the throne. Between the death of Theodore and the extinction of the dynasty of Rurik there stood but one feeble youth, the Czar's half-brother, Dmetri. He might be disposed of if Irenia should remain childless, and Boris might usurp the vacant throne. It was therefore his policy to prevent the skilful midwife from reaching Moscow; and we find that the passage from Elizabeth's letter which we have just given was omitted from the Russian translation, with the exception of a bare mention of Dr. Jacob, who reached Moscow in safety. The midwife was detained at Vologda, so that "the Empress never knew of her," and she was obliged to return to London in the autumn of 1587, leaving her business altogether unperformed. This chapter from the romance of medical history was the beginning of a series of events which terminated in the usurpation of the throne by Boris Godunoff and a frightful civil war. For further particulars of Dr. Jacob's career in England we refer to Dr. Munk's 'Roll of the College of Physicians.'

Muscovy would seem to have lost its charms for English doctors about this time. Dr. Mark Ridley, who was recommended by Queen Elizabeth to Boris Godunoff in 1594, as "one of her physicians, a man learned and expert in his profession, and fit for the service of a prince," returned in 1598 to England, where he died about 1623.

Dr. Timothy Willis was selected by Francis Cherry to succeed Ridley in 1599; but on his way to Moscow overland he executed certain commissions for the English Government at the Court of the King of Poland. This was enough to excite the suspicions of the Czar, who, taking occasion to complain of the doctor's want of drugs and surgical instruments which Willis had sent by ship to Archangel, dismissed the new comer. An apothecary, who about the same time had accompanied the Russian ambassador from London to Moscow, also returned home immediately. With some difficulty James Frencham, who had already sojourned in the land, was induced once more to leave England for Moscow with a cargo of medicaments, one of which, indeed the first on the list, was confectio erynges, to which the Russians attributed great virtues. Whether the excellence of these "medicine-men" was small or great, we cannot say, but they appear to have been kept in Moscow, when once there, with the most jealous care.

In the appendix to Richter's first volume are letters from Queen Elizabeth, begging for the return of Ridley and others; one from Henry IV. of France, making a similar request for "Paul Citadin de la ville de Milan," and a formal permission from the Czar for one Dr. Caspar Fiedler to travel.

It would appear that a physician was the most acceptable present a foreign potentate could send to Muscovy; and the article was not scarce. The German Emperor sent Dr. Carbonarius; the King of Sweden, Dr. Rosenburg. Denmark furnished Dr. Andersohn, while the Elector of Bradenburg supplied Dr. Engelhard. Prince Maurice of Orange was represented by Dr. Polidanus. From the Duke of Holstein came Dr. Sybelist; from Henry of Nassau, Dr. Pauw; and King James I. of England commanded Dr. Arthur Dee to go and serve the Czar. Dee was the son of the celebrated mathematician and astrologer once so high in the favour of Queen Elizabeth. When Arthur was still a child, his father went to Poland with his familiar, Kelly, and excited much ignorant wonder by performances akin to the spirit-rapping of our day. The Czar of Russia sent for him, and Dee subsequently asserted that patriotic motives prevented his acceptance of an invitation to reside in Moscow. Probably his services as "secret intelligencer" to the Queen's Government were more useful then in Germany than they would have been in Russia. After six years' wandering in Poland and Germany, Dee returned to England. His son Arthur, who assures us that he had played with golden quoits transmuted by the great projection, then went to Westminster School and to the University of Oxford. He afterwards began to practise medicine in London, but was interdicted by the College of Physicians for want of the necessary diploma.

This vision of the "grand projection" performed by his father in Poland fatally unsettled his studies, and carried him out of the regions of science; yet his residence abroad during boyhood was so far useful to him that it greatly developed his faculty for speaking various languages. Besides his native English, he was master of German, French, Hungarian, and Polish. Knowledge of this kind has always been highly estimated in Russia, and Dee's accomplishments as a linguist doubtless recommended him to the Czar and his Court. That

Court was just recovering itself from a period of eclipse. After a desolating civil war, Michael Romanoff had been placed on the throne of Muscovy under the guardianship of his father, the Patriarch Philarete. The letter of introduction which Dee carried from King James to Moscow is dated 11th June, 1621, and describes Dee as "a sworne servante of ours, one well approved for his worth and experience by our College of Physicians of the city of London." How the College came to approve him well, and yet not put him on the "Roll," does not appear. A letter, signed by King Charles, in 1626, repeats the same commendation, "the general approbation of his worth by our College of Doctors," and further states that Dee had been physician to Queen Anne, the mother of Charles, and had shown "faithful observance to her all the time of his service about her royal person."

Dee remained twelve years in Russia in personal attendance on the Czar and his father, the Patriarch, whose orthodoxy appears not to have been disturbed by astrology, spirit-crystals, and the philosopher's stone. While in Russia, Dee wrote his 'Fasciculus Chymicus Abstrusæ Hermeticæ Scientiæ Ingressum, Progressum Coronidem Explicans,' which was published at Paris in 1631, and afterwards translated by Elias Ashmole. Some particulars of Dee's stipend while in Moscow may not be uninteresting to the reader. He received about 1400 roubles a year, which, if multiplied by 5 to reduce it to the value of money in our day, amounts to 7000 roubles, or 1000*l.* In addition to this he was allowed 72 roubles monthly for provisions, besides rations of flour, wine, mead of various colours, and oats and hay for his horses. For residence he had a stone or brick house in Moscow itself; and a country-seat not far from the town. He was treated with the greatest consideration, and on his departure, in 1634, at the express desire of King Charles, who wished to make him his own physician, costly sables were presented to the doctor by command of the Czar.

Well might Olearius, in his 'Account of the Embassy from Holstein,' say: "The Russians love doctors, and medicine is in high estimation among them." Discrimination, however, was occasionally exercised. We have seen that Dr. Timothy Willis was unceremoniously sent back to his own country. Still more ignominious was the fate of three physicians and an apothecary—Drs. Damius, Kaufmann, and Dahleen, with George Crivæus—who, coming uninvited to Archangel in 1627, and petitioning the Czar for leave to visit Moscow, were ordered to quit the country by the next ship that sailed, because "they were unknown men, who had no authentic testimonials of their character to produce." Even to these rejected adventurers, however, fur and provisions were presented by command of the Czar. Nothing less than a royal sign-manual would satisfy his Muscovite Majesty of the skill and respectability of his foreign physicians, and no man could practise near the Kremlin in those days unless armed with a State paper recording his merits. On one occasion a royal letter reached the Czar, politely declining to accede to his demand for another English doctor. Michael Fedorovitch begged Charles I. to send the celebrated Peter Chamberlen to Moscow, to which the royal martyr demurred, inasmuch as "one Doctor Elmston, a native of Moscovia, and your Majestie's subject," wrote Charles, "having spent some time in these our Dominions, in the studie and practice of Phisick, is lately returned to Mosco, by whose service and attendance it is probable your Majesty may for the present be sufficiently supplied." As preparations had been made at Archangel for the reception of Dr. Chamberlen, the Czar was probably annoyed at this refusal, and we almost wonder at the polite consideration for the Stuart family which was manifested after the tragedy of 1649 by closing all the ports of Russia against Englishmen who were so unnatural as to put their own sovereign to death. True, it was Michael's son, Alexis, who then reigned at Moscow. Elmston was one of three persons sent abroad at the expense of Czar Michael, to study medicine for the benefit of Russia.

Jacob Arensen, born at Moscow, the son of the apothecary there, was sent in this way from 1616 to 1620, and on his returning home became the first *native* physician in Russia. John Elmston was the son of the interpreter to the English Embassy in Moscow. He was sent to England in 1629, studied at Cambridge, London, and Paris, enjoyed the protection of King Charles, and returned to Russia a Doctor of Medicine in 1642. The third medical student thus provided for was Valentine Byls, son of a physician to the Czar. He was absent in Holland sixteen years, pursuing his studies and fitting himself for the medical career which he subsequently followed in Russia. He took his M.D. degree at Leyden.

This diluted infusion of native blood into the faculty in Russia was not sufficient to create a national school of medicine. The first physician of Czar Alexis Michaelovitch, who ascended the throne in 1645, was Dr. Andreas Engelhardt, a native of Brandenburg, who remained ten years in the Czar's service. To him succeeded Dr. Samuel Collins, the author of a little book entitled 'The Present State of Russia,' 1671. Richter has erroneously attributed to him the 'System of Anatomy,' two vols., folio, 1685, which was published by Dr. Samuel Collins, the Gulstonian lecturer of 1675. The Russian Collins, after serving the Czar eight years (1659–1667), returned home and died at Cowley, in Middlesex, in 1685. Regard for his profession does not appear in his little book, where he avoids as much as possible *parler métier*. We have already mentioned his allusion to "mylady lues venerea" as being well known in Poland, and taken prisoner by the Russians. He expresses his disgust at the elfish lock which the Poles derived from *Plica*, which, he says, they take pride in and wear as a badge of nobility. One other observation only gives an intimation of the writer's profession. Speaking of the rigour of Russian fasts, he thus describes their jealousy of animal substances in their medicines: "If a medicine has in it *Cor. cervi*, *ungul.*, *Al.* or *Pil. lepor*, they will not take it though to save their lives."

Russian medical history is equally disregarded in the 'Account of Muscovy,' published in 1698 by Iodocus Crull, who was made Doctor of Medicine of Cambridge by royal mandate in 1681. He had not, indeed, been in Russia, and in compiling from printed sources his labours could not but be barren.

The plague of London, in 1665, threatened to put a stop to the migration of English doctors into Russia. Dr. Thomas Wilson went, furnished with good recommendations, in that year, but was stopped at Pleskoff for several months, then was cautiously allowed to approach Moscow, and after two years' wearisome delay was forced to return home without having obtained any employment. Dr. Frundek, a Dutchman, was rather more fortunate. After encountering obstacles and delays similar to those which beset Wilson, he was permitted to enter Moscow in 1666, having left his money and effects at Pleskoff, and buried his clothes in the ground there. The sanguine disposition which enabled him to overcome the difficulties of a Muscovite quarantine probably led him to write a book which might have been serviceable to the author of a 'Strange Story,' and which was published at the Hague in 1660, under the title of 'Tractatus de Elixirio Arboris Vitæ, seu Medicina Mea Universali.'

In his last sickness (1676), Alexis Michaelovitch was treated by Drs. Coster von Rosenberg and Laurence Blumentrost, both of whom have left works to testify of their medical knowledge, for an account of which we must refer to Richter's history. They both also left sons, who succeeded to their practice and to the favour of the reigning family. To the Princess Sophia the elder Blumentrost owed his life, when the riotous Strelitzes put one Dr. Gaden to death, and would have so disposed of all the Court physicians, on suspicion of their having abridged the days of the Czar Feodor Alexeivitch.

The list of physicians who flourished during the reign of this Prince (1676—1682) is composed exclusively of foreigners, unless we except Dr. Henry Kellermann, who, though of German blood, was born in Moscow, was sent abroad to study medicine, and on his return in 1678 was admitted Doctor by the Board of Apothecaries, a medical corporation founded at the commencement of the seventeenth century. It was not until the reign of Peter the Great that the same body conferred the rank of Doctor upon a genuine Russian. The Czar had, in 1692, sent to Padua, at his own expense, one Peter Posnikoff, the son of an officer of his court, for the express purpose of studying medicine. The progress made by the young man in all his studies was highly commendable, but especially so in the acquisition of foreign languages, an accomplishment that deprived Russia of the services of her first doctor. The exigencies of the Czar's diplomatic service diverted the best part of Posnikoff's time and attention from medicine to State affairs. He was called from Padua to Vienna, went thence to Amsterdam, and again to Italy and Venice. No record remains of his medical labours in Russia, after receiving the dignity of Doctor in 1701 from the hands of his countrymen, a dignity which he perhaps valued as much as the degree of *Doctor of Philosophy and of Medicine*, conferred on him in 1696 by the University of Padua.

Gregory Volkoff was another Russian medical student sent by Czar Peter to Padua in 1698, with a thousand thalers in his purse, and a passport to bear him safely through his journey. Yet the foreign importation of medical men did not flag in Peter's reign. Richter tells of a batch of twelve surgeons despatched at one time by General Gordon from Germany to Russia, and of fifty sent from Amsterdam in 1697 by Admiral Cruys. A few further particulars of these worthy gentlemen destined for the army and navy will be found in Richter's second volume.

Peter the Great, as we have intimated at the outset of this article, felt a strong personal interest in medical science. Endowed with great natural capacity, this semi-barbarous monarch and enthusiastic reformer watched with intensely eager curiosity all the processes involved in the study and the application of the great art of healing. Whether in the dissecting-room he followed the swift revealing hand of the anatomist; or, at Paris, bade his favourite Dr. Erskine procure him an opportunity of seeing the operation for the cataract; or, at Petersburg, talked deep into the night with his Dutch physician, Bidloo, on the growth of medicinal plants, or astonished Dr. Termont by performing himself the operations that have been mentioned, the same active energy in the cause of medicine is always and everywhere apparent. The attacks of epilepsy to which he was subject from his youth helped, no doubt, to increase his sense of dependence on the skill of his physicians, a skill to which his frequent excesses would also compel him often to resort. He not only founded hospitals for soldiers and sailors, making all officers contribute a month's pay every year for their support, but he also opened a medical school for the instruction of Russian youths in the art he respected so much; and he purchased, at a cost which he could ill afford, the Anatomical Museum of the celebrated Ruysch, of Amsterdam, and the natural-history collections of Seba. Of the Anatomical Museum Richter writes with professional rapture, describing some of the best preparations, the gems of the collection, with a gusto that does him honour.* He further intimates that the Czar paid Ruysch 50,000 guldens to impart to Erskine the secret of injecting into mere mortal remains that which preserved them so long from hideousness and decay.

In the cabinet thus formed Peter would often pass his early morning hours. Here, on one occasion, he gave a first audience to the Ambassador from the Court of Vienna, after closing the mouth of his remonstrating Chancellor by these characteristic words—"Let him come here: the place in which I see him for the first time must be indifferent to him; he is sent on an embassy to me and not to one of my palaces, and can tell me whatever he has to say in any place." The audience took place at five o'clock in the morning.

Erskine, the curator of the cabinet, who figures in Richter under the abominable disguise of *Areskine*, was a Scotch gentleman of degree,

* The illustrious physiologist Von Baer read a Memoir before the Petersburg Academy in 1850, in which he stated that the Museum is still very much what it was when Ruysch sold it, and that very few preparations have been added since that date. This is an additional illustration of the indifferentism of native Russians and of medical men trained in Petersburg for the scientific department of their profession.

related to the celebrated Earl of Mar. His career in Russia commenced in the capacity of physician to Count Menstchikoff, and he was called by the Czar to preside at the Board of Apothecaries in 1706. He appears to have enjoyed the personal confidence of Peter, travelling about with him in Russia and abroad. Perry, in his 'Present State of Russia,' has the following passage:—

"Dr. Areskin, a most ingenious gentleman, who is chief physician to the Czar, and a Member of the Royal Society in England, with whom the Czar usually informs himself in the curiosities of nature, being, in 1709, with his Majesty on that side of the country, he went on purpose by the Czar's order to see the foresaid saints that are laid in the vault in a monastery near Kieff; and in discourse I have heard him say, that the Russes in their pretences to these miracles, sufficiently outdo whatsoever is pretended by all the Papists in Europe."

Erskine is also spoken of by Alexander Gordon, in his 'History of Peter the Great,' as "an agreeable, open-hearted, fine gentleman."

The account of the operation for cataract which the Czar witnessed, occurs in the 'Memoirs of the Reign of Peter,' by the so-called Nestesuranoi. It was when the Czar was at Paris in 1717, on the day he took leave of the King and the Regent, he ordered Erskine to bring a skilful oculist to operate on an invalid aged sixty-five, who had been blind since the battle of Hochstedt. An Englishman named Woolhouse was recommended by Du Vernay, and came to the Hôtel Lesdiguieres, where the Russian monarch was residing. On seeing the needle planted in the eye, Peter turned his head away for a moment, but recovered himself immediately, and closely watched the operation to the end, when he held his hand before the invalid's eyes, and proved the reality of his restoration to sight. Greedy of every acquisition that might benefit his country, he requested Woolhouse to undertake the charge of a Russian pupil whom he would send to be educated as an oculist. To return for a moment to Erskine. Some obscure hints are given of his being concerned in the notable plot concocted by Count Görtz of Sweden, the Earl of Mar, and others, for a Jacobite descent in England, which terminated in the feeble expedition of the Pretender in 1715. The doctor wrote a justification of himself to Lord Stanhope, but was nevertheless under a cloud for some time, and ended his life in 1718, at Olonetz, far away from the capital and the Court. The Czar appears to have sincerely lamented him, and gave him a public funeral in the Pantheon of modern Russia—the convent of St. Alexander of Nevsky, near St. Petersburg. Erskine's library of four thousand curious books, and his collection of curiosities were bequeathed to the kunstkammer. and now lie, we believe, at the Petersburg Academy of Sciences. The degree of his intimacy with the Imperial family may be surmised from the bequest he made of his principal estate to the Princess Nathalie.

Dr. Blumentrost the younger, who succeeded Erskine as chief Court Doctor, was not so high in Peter's personal regard as his Majesty's Dutch friends, Termont, the surgeon, and Bidloo, the physician, with whom the monarch, who could speak no foreign language but Dutch, would pass hours of instructive conversation. A capacity for strong potations was also an indispensable qualification for the enjoyment of the Emperor's society, and Holland has never been wanting in hearty topers.

The Czar had once to do violence to his generally strict sense of justice in favour of Termont, who, in the heat of passion, and under the influence of strong drink, ran his servant-man through the body. The Czar next day was amazed to see his boon companion fall at his feet in an agony of remorse and fear. Upon learning the cause of this strong emotion, he pardoned the rash surgeon, on condition that he provided for the wife and children of the deceased serving-man.

Stæhlin, in his 'Original Anecdotes of Peter the Great,' tells (on the authority of an army-surgeon, Schulz) a story of Termont's widow and her second husband, which displays the Czar's strong common-sense, his natural humour, and, which is not altogether foreign to our purpose, his ability to detect a false pretender to medical knowledge. Madame Termont had bestowed herself and her fortune on a gallant journeyman barber-surgeon who came from Dantzig, and the happy couple made haste to drive through the fortune of the defunct, literally in a coach-and-four. Folly of this kind was not long in coming to the ears of the Czar, who on a given day sent for the usurper of his dear Termont's place. The young man, elated with the prospect of a high situation at Court, decked himself in costly attire, and on reaching the palace was ushered into the Imperial presence, where he found a numerous assembly of boyars. Peter at once put him through a searching examination on surgery, and had no difficulty in proving the young man's utter ignorance of every branch of his art that went beyond shaving and bloodletting. He thereupon called in a number of footmen and peasants who lay in ambush in an adjoining room, and compelled Madame Termont's husband in all his finery to mow their stubborn beards. The barber-surgeon having completed his task, was suffered to depart with the same misplaced pomp which had marked his arrival.

Dr. Nicolas Bidloo was the nephew of Godfrey Bidloo, physician to our William III. of glorious memory. It will be remembered that Godfrey Bidloo made a vigorous attack upon W. Cowper, the English anatomist, for using the fine plates of Bidloo's book on anatomy, and passing them off for his own. Lambert Bidloo, the father of Nicolas, was a great apothecary and herbalist, and the latter acquired a double hold on the confidence of Czar Peter by his familiarity with the subject of plants and gardens. His advice was taken in the laying out of both ornamental and botanical gardens in several principal towns of the empire, and he was appointed director of that which was planted at Moscow.

The death of Peter the Great in his fifty-third year, of strangury, suggests a doubt as to the skill of his numerous medical attendants and friends. Boerhaave is reported to have exclaimed when he heard of the Emperor's death, "My God! is it possible? What a pity that so great a man should have died when a remedy of the value of a few pence would have saved him." We do not profess to understand what the great physician meant. The words are given by Stæhlin as they were repeated to him by a nephew of Boerhaave's. In his last sickness Peter was attended by Dr. Blumentrost, Dr. Bidloo, Mr. Paulson, a surgeon, and Mr. Horne, an English surgeon, who made use of the catheter repeatedly. The Czar appears to have suffered acutely, for during the operation he held Paulson and the apothecary with so tight a grip that he left livid marks on their bodies. Horne had spent many years practising in Paris and other parts of the Continent. He was made principal surgeon in the Petersburg Hospital, and head of the young school of surgery there. Richter gives a long list of physicians, surgeons, and apothecaries who flourished in Russia during the reign of Peter the Great, to which we refer the curious reader, with this one remark—that they are all foreigners. We would observe respecting the surgeon John Hovy, or Hoffy, as he is variously styled, that he was taken by the Czar from Amsterdam, enjoyed much of his confidence, was sent to Archangel to convey the intelligence of a peace with Sweden, on which occasion he received a splendid ovation, with public honours and many handsome presents. His son was a celebrated banker at Amsterdam, where his name, if written Hoffe, will recal the name of our own countryman, Hope, the celebrated banker of Amsterdam.

We must hasten to our conclusion. During the whole of the eighteenth century Russia lay at the feet of a cruel German bureaucracy, which repressed every symptom of national life that came into view. The capacity of the native race for science and art was purposely denied, as was every opportunity for its development. In Richter's lists of medical men we find scarcely one Russian name until we reach the period of Catherine II. That extraordinary woman had completely thrown off her German traditions, and identified herself thoroughly with the people she governed. Although even he could not emancipate herself from the bonds of that bureaucracy which had in its power the entire administration of the empire, she gave to native aspirants a fairer chance than they had ever had before. Accordingly we find flourishing in her reign a few physicians and professors of medicine at the universities who were genuine Russians. Yet to the Empress Elizabeth (Peter the Great's daughter) is due the credit of sending in 1761, the last year of her reign, ten Russians into Holland for the purpose of studying medicine. Among these, Dr. Pogoretzki subsequently published certain medical treatises of ordinary interest, and translated into Russian several articles of the French Encyclopædia. Dr. Jagelski, who became professor at Moscow, and physician to the hospital there, wrote on 'Preventive Measures against the Plague,' 1771; Dr. Timkoffski, who was appointed surgeon to the Marine Hospital at Petersburg, wrote a 'Dissertation de Morbis Incurabilibus,' 1765; and Dr. Fialkoffski, an army surgeon, distinguished himself by his inaugural discourse, 'De Methodo studii Medici.'

The Empress, nevertheless, thought fit to meet her end under the medical care of an Englishman, Dr. Jacob or James Mounsey, who, after practising successfully for ten years in Moscow, was called to the Court in 1760. Richter tells us that he married Johanna, the daughter of James Grieve, a Scotchman, who for some years was city-physician in Moscow. Grieve was not, it may be supposed, embarrassed with any excess of modesty; for when about to retire to England, he begged his son-in-law to ask the Emperor Peter III. for an Imperial recommendation to the Court of England, to enable him to secure a place there. The recommendation was given with all formality through the Russian chancellor and the ambassador in London, but, as was to be expected, without any results, and Grieve died in Moscow about 1763.

We cannot omit from an account of the medical men of the Czarina Elizabeth that historical personage, Jean Herman Lestocq. He was the son of a French Protestant surgeon who, after the revocation of the Edict of Nantes, had resided at Hanover. Attracted by the brilliant accounts received from Petersburg of fortunes made and successes achieved, the young man, who had studied medicine, went thither in 1713, being in his twenty-first year. He was appointed one of the surgeons to the Court, and accompanied the Empress Catherine the First on her journey to Holland in 1716. He was a light-hearted dissolute fellow, and incurred the serious displeasure of Peter by some misconduct, the nature of which is not stated. He was exiled to Kasan, where he remained till the accession of Catherine, in 1725, when he was recalled, and appointed medical attendant to the Princess Elizabeth, a young girl of sixteen. With all the gaiety and rattle of a Frenchman, Lestocq was a schemer, and after he had obtained a certain ascendancy over Elizabeth, he directed her thoughts to the throne. On the death of Peter II. he urged her to declare herself Czarina, but she, preferring the freedom and pleasures of life to power, refused. Another ten years elapsed, and the throne was again vacant, having been bequeathed to Ivan, a child in his cradle. As Elizabeth hesitated to break her oath of fidelity, Lestocq grew more and more urgent. One of his devices was to paint a picture representing Elizabeth seated on a throne of flowers, upheld by cupids; on the other side, dressed as a nun, and surrounded with instruments of torture. "Choose," said he, "to-morrow, the purple or the rack." She chose the purple, and rewarded Lestocq for his services with the rank of Privy Councillor, Physician to her Majesty, President of the Medical College, and a pension of 7000 roubles. The Emperor of Germany sent him a patent of nobility, making him Count of the Empire, and the King of France likewise forwarded a handsome gratification. But Lestocq wanted the ballast of a true politician. He intrigued enough, but the Russian chancellor, Bestucheff, out-manœuvred him, and in 1748 the unfortunate surgeon was imprisoned, deprived of rank and fortune, knouted, and finally banished to Usting, in the government of Archangel. He survived his ungrateful sovereign, and was restored to liberty and some fortune by Peter III. and his wife Catherine II. The goods and chattels of which he had been despoiled having fallen into many hands, could not easily be given back to him.

Peter said, jestingly, that he might take his own wherever he could find anything. The notion jumped with the Frenchman's humour. He found it excellent sport to go to the houses of his spoilers when the masters were out of the way, and carry off, in the name of the Emperor, any bit of plate or jewellery, china, vase, or picture, that he could lay his hands on. Retiring to a small estate in Livonia granted him by Catherine II., he died there in 1767.

In the same year, 1767, there was published in London, by Dr. Thomas Dimsdale, 'The Present Method of Inoculating for the Small-pox,' a work which passed through seven large editions in the course of three years. The reputation of the author reached the ears of the Empress Catherine II., who summoned him to Petersburg for the express purpose of establishing inoculation in Russia. She was herself the first to submit to the operation which filled many with dread. In one of his publications, Dimsdale gives an interesting account of his proceedings, and of the Empress's mingled anxiety and resolution. Her son, the Grand Duke Paul, was the next patient, and by slow degrees inoculation came to be recognised among the enlightened part of the Russian community as a smaller evil than the small-pox. The more ignorant peasantry have not yet overcome their repugnance to this form of interference with the supremacy of disease. A few years ago we were in a Russian village near the Volga, where a child died after vaccination undergone in obedience to the strict commands of the proprietor of the village. The mother had picked out the vaccine matter from the infant's arm in such a way as to bring about a fatal result.

Dr. Dimsdale received a splendid reward for his success with the Empress and her son. He was dubbed a baron, with augmentation of his coat of arms by a wing of the Russian eagle, or. He received a present of 12,000*l.*, and a pension of 500*l.* a year as physician to the Empress. Well might he on his return to England set up a bank in Cornhill, and get returned M.P. for Hertford. He went again to Russia in 1781, for the purpose of inoculating the young Grand Dukes Alexander and Constantine. Before Dimsdale left Petersburg, the Empress founded an inoculation hospital for poor children, and appointed as superintendent an English physician named Halliday, who had practised many years in Russia.

Catherine, who had a characteristic fondness for grand designs, not only founded several hospitals, but organized a medical faculty in Petersburg, where she established "The Medico-Chirurgical Academy," which flourishes to this day. To its authority are subjected all the medical institutions and all the medical men of the empire, except the court physicians. The Academy confers degrees in medicine, surgery, and the veterinary art, a privilege that, before its formation, had belonged only to the universities of Petersburg, Moscow, and Kharkoff. It has fifteen professorial chairs, which, with rare exceptions, are always filled by native Russians or by Poles. The lectures are delivered in the Russian and Latin languages, and though nominally fixed for every day during ten months of the year, the large subtraction of red-letter days makes an average of not more than three lectures a week. The examinations take place during the two months' vacation. Five years' attendance on the lectures are required as a preparation for the examination for the degree of M.D., which can only be conferred on candidates who have not attained the age of twenty-four. Doctors who pass their examinations with great credit receive, in addition to their rank, medals and honorary diplomas. One or two individuals also are selected every year by the Senatus Academicus to travel abroad, at the expense of the Crown, for the purpose of completing their education by visiting foreign universities. On their return home, these travelling Fellows are obliged to serve for three years in one of the large hospitals, after which they are eligible for election to a professor's chair in the Academy. Yet, spite of these and many other advantages offered to the medical student of Petersburg, which we have not space further to detail, the number of Russians who reach the highest honours of the profession is astonishingly small. Of the average number of sixty who graduate every year at the Petersburg Academy, very few take the degree of Doctor in Medicine and Surgery. The Government students of the Academy, who are fed, clothed, and educated at the expense of the Crown, seem satisfied with the lowest status which will admit of their practising. This is partly due, no doubt, to the penalty exacted for the gratuitous education. For one year of such assistance the recipient has to render three years' unremunerated service in an establishment of the Crown; for two years' assistance he must render five of service, and for three years, six.

The Medical School of Moscow is but a branch of the Petersburg Academy, yet the Moscow University has an advantage in its medical faculty over the University of the modern capital of Russia. To Sir James Wylie, for thirty years President of the Academy, is due the independence of the University enjoyed by the Petersburg medical faculty. He also justly claimed the merit of organizing an efficient medical staff in the Russian army, and as a reward for his services, both in the civil and military departments of medicine, he united for a long time in his own person the honours and duties of the principal medical offices in the empire.

The singularly prosperous career of this gentleman, whose history has lately been brought into public notice by law proceedings in a disputed will case, claims a moment's attention. Born in Kincardineshire, he went to Russia about the year 1790, and was engaged, we believe, as medical attendant to a Prince Galitzin. He was subsequently appointed surgeon to a regiment of household troops, from which situation he emerged into the sunshine of imperial favour by a lucky accident, of which he availed himself with ready boldness.

Count Kutaisoff, the valet and favourite of the Emperor Paul, was very ill with a tumour in the neck, which, as it enlarged, pressed upon the windpipe and threatened suffocation. The Emperor was greatly distressed by the inaction of the court physicians, who hesitated to risk their reputation by cutting in the delicate region of the throat.

Paul casually mentioned his anxiety to the Colonel of Wylie's Regiment, who asked permission to send the *Anglichanin*, or Englishman. Permission was accorded, and Wylie, who could not lose much and might gain everything by fearlessness, cut the abscess and afforded instant relief to the patient, who recovered. In 1798, he was made Physician to the Court, and travelled to Moscow and Kasan with the Emperor, who, in 1799, made him his Surgeon in Ordinary, and Physician to the heir apparent Alexander. On the tragic death of Paul, Wylie was employed to embalm him, a compliment to his discretion which he never belied, for nothing could ever induce him to say what traces of violence he had seen on the Emperor's body. In 1804 he drew up the *Status Medicus* of the Medical Academy. Two years later he was made General Inspector of the Board of Health of the Army, and, 1812, Director of the Medical Department of the Ministry of War. He accompanied the Emperor Alexander through his campaigns, and had the melancholy distinction of amputating both legs of General Moreau. The reader of Sir Robert Wilson's Diary will find his name turn up continually. He was knighted by the Prince Regent at Ascot Races, with the sword of the Hetman Platoff, and was created a baronet the same year at the request of the Emperor. Russian, Prussian, and Austrian orders were showered upon him. He fixed the regard of the Emperor Nicolas even more than that of Alexander, and evinced his gratitude at his death, in February, 1854, by bequeathing his large fortune to the Emperor for the establishment of a charitable institution bearing his own name, and to be at the same time commemorative of his "august benefactors, the Emperors Paul and Alexander, and the Grand Duke Michael."

His relatives in Scotland have endeavoured, and thus far successfully, to lay an embargo on some 70,000*l.* invested in the English Funds, the disposal of which is not explicitly or with sufficient detail set forth in the deceased Baronet's will. That document recites somewhat ostentatiously the history of the testator in the following words:

"Recalling to mind the course of my past life, I can say from my own convictions, without any pride, that in the sphere of my activity I fulfilled my duties conscientiously and honestly, not without advantage to Russia. More than sixty years I was in the service of four monarchs of Russia, and every commission bestowed on me I performed with real ardour and steady devotedness. I laboured unremittingly for the organization of the medical department, and for the education of physicians in Russia, as my second country, and the thought that my endeavours have not been unsuccessful is not a vain consolation in the last days of my life."

A silver vase and marble pedestal, which had been presented by the Russian physicians to the old man on the fiftieth or jubilee anniversary of his public service, he bequeathed to the Petersburg Medical Academy, where a statue has been or is being erected to his memory from funds which he prudently bequeathed for that purpose.*

* At page 502 of the twelfth volume of the 'Dublin Quarterly Journal' (1851), the reader will find an account of the various military and medical services rendered by Sir James Wylie to Russia, as also of the numerous honours conferred upon him. The translation of an Imperial Rescript addressed by the Emperor Nicholas to Sir J. Wylie there given is, we believe, from the pen of Dr. Moore, of Dublin.

Our task is for the present over. We have traced as best we could the history of medicine in Russia, and in so doing have been necessarily confined to a narrative of the adventures of English, German, French, and Dutch practitioners of the art. The history of Russian medicine only begins in the nineteenth century; and though the names which give distinction to the Russian school of medicine—Von Baer, Jacobi, and others—are German names, the hearts of these gentlemen are, we believe, as Russian as the place of their birth. Piragoff, the eminent surgeon and operator, is both in heart and name a Russian of the most patriotic character.

The cradle of the best medical school of the country is doubtless of German origin. Dorpat, in Livonia, possessed an University at the commencement of the seventeenth century, when Russia lay for a time at the feet of Poland. A century later (in 1704) it passed under the sway of Peter the Great, its University having been extinguished. Another century elapsed, when in 1803 the Emperor Alexander revived the University, which rapidly gained ground, and is now, if judged by real achievements in science and learning, the chief seat of knowledge in the empire.

REVIEW II.

Studier i Läran om Lymphkärlens Ursprung. Akademisk afhandling. Af ADOLF KJELLBERG.

Studies respecting the Theory of the Origin of the Lymphatics. An Academic Essay. By ADOLF KJELLBERG.—*Upsal.* 8vo, pp. 32. With a Plate.

PREVIOUSLY to the appearance, in 1802, of Rudolphi's anatomico-physiological essays, it was generally supposed that the process of absorption was carried on by means of open mouths in the extreme ends, or rather in the first commencement, of the lacteal and lymphatic vessels. Even when physiologists were gradually led to adopt the view enunciated by Rudolphi, that such open mouths did not exist in the lacteals, much difference of opinion still prevailed as to the mode of origin of the vessels in question, some supposing that they began in the form of loops in the villi, others in networks like the arteries, others in simple central spaces, &c. The two last-named theories were those most generally received.

Before proceeding further with this subject, the author passes to the consideration of the epithelial cells on the intestinal villi, which are of great interest in reference to the theory of the origin of the lacteals.

After the discovery of these cells, it was at first thought that they fell off during the act of digestion; but it was soon ascertained that they not only remained, but during digestion became in a great measure filled with fat. This latter phenomenon seems to have been observed about simultaneously by Goodsir and by Gruby and Delafond; but while the first-named writer still believed that the epithelial cells were rubbed off in each digestive act, and therefore neglected the connexion between the observation he had made and absorption, the two latter said that the molecules of fat passed through the inferior narrow opening in the epithelial cells, in order to reach the lacteal vessels lying in the centre of the villi.

The influence of the bile in promoting the absorption of fat having been ascertained, Wistinghausen, in his researches into the mode of operation of this secretion, found that fat, in the form of emulsion and mixed with bile, passes with ease endosmotically through a membrane; while without the addition of bile, its transition takes place only under considerable pressure. To this observation Brücke objects, that Wistinghausen's results were obtained by experimenting with a complex membrane—namely, the intestinal mucous membrane—and that they therefore did not apply to a simple cell-membrane, where the pores must be supposed to be extremely minute. Finally, Brücke comes to the conclusion that the epithelial cells must be open in the extremity facing the cavity of the intestine.

Moleschott and Marfels inferred from their experiments that small, solid particles in the intestinal canal are capable of penetrating into the cells closed with "yielding plugs," whence they enter the villi, and make their way further into the lacteal vessels, the thoracic duct, and, lastly, into the blood.

Funke was decidedly opposed to Brücke's hypothesis, and from the results of his experiments concluded that only the fat which becomes fluid at the temperature of the body, or is previously so, can pass from the contents of the bowels into the epithelial cells, that the absorption of the fat takes place only endosmotically, and that the cells through which it passes are not open, but are closed by a membrane impenetrable to solid bodies. He had not as yet perceived the importance of the pore-canals observed by him in his investigations, in the broad extremity of the cells.

Donders, who from his first experiments on this subject, with flowers of sulphur, powdered charcoal, and quicksilver, was inclined to admit the possibility of the passage of small solid bodies from the intestinal canal into the blood, subsequently instituted new and extensive investigations by which he was led to an opposite conclusion. Like Moleschott, he repeatedly injected the blood of sheep into the stomachs of frogs, but could not subsequently recover a single blood-corpuscle of the sheep in either the vessels of the mesentery or the heart. Similar negative results were obtained with subdivided eye-pigment. In consequence of these and other experiments, Donders inferred that the statements of Moleschott and Marfels respecting the penetration of small solid particles into the epithelial cells were extremely doubtful.

Wittich is of opinion "that the transition of solid bodies, as powdered charcoal, pigment, starch, blood-corpuscles, &c., from the intestine into the blood, is possible only through rupture of the mucous membrane and of the blood or chyle vessel."

Rindfleisch considers that the passage of solid particles into the blood depends upon a pathological condition.

There is another view of the structures of these cells—namely, that in the free extremity of the epithelial cells there are extremely small and delicate openings, so-called pore-canals, which exhibit themselves under the microscope as lines in the basal edge of the cylinder-cells. This discovery was made simultaneously by Kölliker and Funke in the intestines of rabbits, and has since been observed both in a number of other animals and in man.

Another signification than that of pore-canals is given by Brettauer and Steinach to the lines in the basal extremity of the cells. The latter are, according to them, composed of palisade-like, parallel rods, standing close to one another, the boundaries of which are indicated by the lines observed by Kölliker and Funke. Between the rods, which stand in a more intimate connexion with the contents of the cells than with the cell-wall, are extremely minute spaces, constituting the connexion between the cavity of the intestine and the interior of the cell.

Heidenhain takes the same view of the lines as Brettauer and Steinach; while Donders, Welcker, and others assume the existence of pore-canals, an assumption which the author for his part considers to be correct.

As to the slender extremity of the epithelium, by means of which it is connected with the villi, it has already been stated that Brücke, as well as Gruby and Delafond, hypothetically assumed that an opening must exist in it, leaving to the fat which has entered the cell a free and open passage to the parenchyma in the villi. This hypothesis met at first almost exclusively with opponents, but subsequently found a vigorous advocate in Heidenhain, who endeavoured by his investigations to prove the correctness of the hypothesis. He found, in fact, that the epithelial cells at their small end were continued into efferent processes passing into an oblong swelling, in which a nucleus was for the most part discovered. Hence it would appear that the epithelial cells were connected, by means of efferent processes, with deeper-seated elements; and accordingly, Heidenhain, on injecting fat into the stomach of frogs, found drops of fat both in the epithelium and in the efferent processes and the cells connected with them.

To return to the lacteals within the villi, it has already been shown that after physiologists had tolerably unanimously come to the conclusion that the lacteals commenced with closed extremities, they still entertained various opinions as to their form, the principal being, on the one hand, that the vessels in question presented in the villi a network like the arteries; on the other, that they commenced as central spaces. Among those who adopt the former view are Krause, Goodsir, E. H. Weber, Zenker, &c. The latter opinion is held by a larger number—as Henle, Donders, Kölliker, Gerlach, Bruch, Frey, Frerichs, &c. Funke formed, as it were, an intervening link between the two. He assumed the existence of a blind central canal, in all probability provided with a proper membrane; to this central canal the free drops of fat passed through the parenchyma.*

* The reader will find various references to these minute anatomical relations of the lymphatics in the several Chronicles or Reports on Micrology in previous numbers of our Review.

It would, however, detain us too long to follow the author through the whole literature of the subject, and through the details of his own investigations. We shall, therefore, conclude our notice of this portion of the work by stating, in his own words, the results at which he has arrived.

"Teichmann has thus by direct injections obtained the same results as I arrived at by my examinations of intestines loaded with chyle. These results must, in my opinion, be brought into accordance with one another, in order to give a correct idea of the origin of the lacteals. *The epithelial cells furnished with pore-canals must be considered as properly the origin of these vessels. These are connected by means of their offsets with other cells lying in the peripheric part of the villi, which again, through their prolongations, open into the central canal, furnished with proper walls. This discharges itself into a vascular net-work to be found immediately beneath the villi and around Lieberkühn's follicles, with which, in all probability, the epithelial cells existing between the villi are directly connected by means of their offsets ; and this vascular network subsequently unites in the sub-mucous tissue to form the lacteal vessel furnished with valves.* Such is the view to which my investigations and studies on this subject have finally led me."

"In the *stomach*," observes the author, "the investigation of the lymphatic vessels and their origin is attended with much greater difficulties than in the small intestine; for under normal conditions only very little, if any, absorption of fat takes place in the stomach, and the contents of the lymphatic vessels are therefore, in this situation, almost colourless, on which account most of the statements made respecting the origin and course of these vessels are based upon the results of direct injections. Most histologists assume, after Fohmann, that in the mucous membrane of the stomach there are two lymphatic vascular networks, one superficially more minute, and a coarser one lying at a greater depth. This opinion is held by Kölliker, Frey, Gerlach, &c. Teichmann, too, from the results of his own injections, assumes the existence of a superficial network lying in the mucous membrane under the pepsin-glands, and a deeper one between the mucous membrane and the muscular coat; and as a singular but indubitable fact, he adds that lymphatic vessels are absent in all the interspaces of the pepsin-glands. Brücke has applied to the lymphatics of the stomach the results of his investigations respecting the lacteals, and therefore assumes that the former commence in this part in the same manner in which he supposes the latter to begin in the villi. I have myself been led by my investigations to another opinion, which also agrees perfectly with the view I hold respecting the origin of the lacteals." (p. 21.)

The opinion thus alluded to, the author expresses in the following words:

"*That in the stomach also the epithelial cells are to be regarded as the primary origins of the lymphatic vessels, these cells being connected by means of processes with other cells lying in the mucous membrane, which anastomosing with one another, unite into a network in the submucous tissue, which seems to pass directly into the proper lymphatic vessels.*" (p. 25.)

Reasoning from analogy, the author supposes that in the large intestine also the lymphatics arise in a manner similar to that above described.

As to the origin of the lymphatics in other organs than the stomach and intestinal canal, many pathological facts would appear to refer it to the corpuscles of the connective tissue. How far these bodies really form the origin of the lymphatics, is still, however, an open question, the answer to which must be supplied by future and continued investigations.

From the foregoing it will be seen that in his interesting essay the author presents us with a succinct account of the opinions of those who have before him written on the subject of which he treats, and that his own investigations have contributed considerably towards removing the obscurity in which the origin of the absorbents has hitherto been involved.

REVIEW III.

Epilepsy: its Symptoms, Treatment, and Relation to other Chronic Convulsive Diseases. By J. RUSSELL REYNOLDS, M.D. Lond., &c. &c.—*London*, 1861. 8vo, pp. 360.

FEW diseases have of late years received so much attention at the hands of the physician as epilepsy and the allied forms of nervous disturbance. The works of Drs. Sieveking, Radcliffe, &c., and the important physiological and pathological researches of Schroeder van der Kolk, Brown-Séquard, Kussmaul and Tenner, and others, have begun to throw light upon the true nature of this important class of affections.

Dr. Reynolds, the writer with whom we have now more immediately to do, renders full justice to the labours of his predecessors; so much so that his work, besides being acceptable as containing the results of his own experience, acquires great additional value as a full and reliable book of reference.

As to the scope of the work, the author, having stated his reasons for preferring the "clinical" to the local or pathological classification of disease, observes that—

"Upon the principles laid down—viz., that disease is the sum-total of modifications in structure and function; that its measure is that of the degree to which life is limited or its actions perverted, and that its classification is most naturally based upon the mode in which it effects this limitation or perversion—we come to regard 'special diseases' as groups of modified function and structure, although the names by which they are denoted sometimes express only the one and sometimes the other.

"Chronic convulsive diseases are a very definite group; they are, with few exceptions, readily recognised as such; and I propose in the following work to treat of them all, pointing out wherein they differ and wherein they agree, and advancing, by the discovery of the conditions upon which they depend, to a knowledge of the treatment which is appropriate to each." (p. 6.)

The following is the author's classification of convulsive diseases:

"I. Idiopathic convulsions; including—
Epilepsy proper; 'idiopathic epilepsy.'
Eclampsia puerorum; 'idiopathic convulsions' of children.

"II. Secondary, or eccentric, or sympathetic convulsions:
'Sympathetic epilepsy,' uterine, gastric, &c.
'Sympathetic convulsions,' in children.

"III. Diathetic, or cachectic convulsions; from general nutrition-changes:
Healthy in kind, but morbid in degree; puberty, &c. Morbid in kind and degree; tuberculosis, scrofulosis.

Toxæmiæ, arising from—
Retained excreta; urinæmic convulsions, 'renal epilepsy.'
Metamorphosed plasma; pneumonic convulsions, rheumatic, &c.
Poison introduced from without; 'syphilitic epilepsy,' lead, variola, &c.

"IV. Symptomatic convulsions; from centric disease—
'Symptomatic epilepsy,' from disease of meninges; tubercle, syphilis, traumatic, &c.
Disease of nervous centres; tumour, softening."

In the present volume, Dr. Reynolds confines himself to the consideration of epilepsy proper, or, in other words, of that form of idiopathic convulsions to which alone he believes the name of epilepsy ought to be applied.

Hence it will be seen that he employs the term epilepsy in a much more restricted sense than most other writers have done. He defines epilepsy, in fact, to be "*a chronic disease characterised by the occasional and temporary existence of loss of consciousness, with or without evident muscular contraction*," and excludes "all those cases in which the fits were evidently due to one or more of the several conditions enumerated, retaining only those in which there was no reason to believe in the existence of anything beyond an idiopathic affection, characterised by those essential features of the disease described in the definition."

The third chapter is devoted to the consideration of the *symptoms* of epilepsy. These are studied under the heads of the inter-paroxysmal and paroxysmal symptoms: the former being subdivided into the mental and emotional, the sensational or animal, the motorial or animal, and the organic or vegetal; the latter into the premonitory, the actual or those occurring during the attack, and thirdly, those succeeding the attack.

The conclusions drawn by the author as to the mental condition of epileptics during the intervals of their attacks are as follow:

"1. That epilepsy does not necessarily involve any mental change.

"2. That considerable intellectual impairment exists in some cases; but that it is the exception, and not the rule.

"3. That women suffer more frequently and more severely than men.

"4. That the commonest failure is loss of memory; and that this, if regarded in all degrees, is more frequent than integrity of that faculty.

"5. That apprehension is more often found preserved than injured.

"6. That ulterior mental changes are rare.

"7. That depression of spirits and timidity are common in the male sex, but not in the female; that excitability of temper is found in both." (p. 46.)

With respect to the immediate *prodromata* of epileptic paroxysms, there were, in 81 cases observed by the author, in 33 positively no prodromata, in 13 the existence of premonitory symptoms was doubtful; of the 35 remaining cases, the prodromata were in 16, sensational (vertigo, headache, nausea, pain, weight at cardia, pleasing sensation in legs, tightness in throat), in 9, they were mental and emotional (fear, depression, excitement, &c.), in 7 they were motorial, and in 3 they were extrinsic (darkness under eyes or ears).

As to its *pathology*, Dr. Reynolds agrees with Schroeder van der

Kolk and other recent writers in considering the medulla oblongata, or the upper part of the spinal axis, to be the organ affected in epilepsy; he believes, moreover, that its disturbance in such cases is functional, and that this disturbance is of degree, not of kind; "misplaced in time, in combination, and altered in degree, those functions are, but no new property is conferred upon the organ, nor is any natural power changed in the quality of its exercise." During the onset of the epileptic paroxysms there is over-action of the nervous centres, and such over-action is the proximate cause of the paroxysms.

"In the first place, nutrition is affected dynamically and temporarily; there is no recognisable departure from textural integrity, there is merely the difference that exists in health and in all organs between action, over-action, and repose; after a time, and by frequent repetition of attacks, the changes, induced temporarily, become permanent; and the texture, which is the product of foregone nutrition-processes, is altered statically and persistently. Nutrition-change is altered, it is more rapid than in health; and the result is, as van der Kolk has shown, enlargement of the capillaries, and fatty or granular degeneration of the medulla." (p. 250.)

Epilepsy is, according to the author, to be *diagnosed* from, 1, simulated epilepsy; 2, syncope; 3, hysteria; 4, catalepsy; 5, eccentric convulsions; 6, convulsions that are the expression of diathetic diseases; 7, organic lesions of the cerebral and spinal centres.

The points most reliable in the detection of simulated epilepsy are, the condition of the skin, the dusky and pallid tint of which in the genuine affection cannot be assumed, and the dilated state of the pupil. In syncope the loss of consciousness is not absolute, nor so sudden as in epilepsy. There is usually some immediate cause of the sensation, and the horizontal position affords speedy relief.

As to the *prognosis* of the disease, the author gives the result of his experience in the following passage:

"Of 115 cases of chronic convulsive disease which have fallen under my own care, all of which more or less closely resembled epilepsy—many of which had been termed epilepsy, and had been treated as such for several years—there were twenty-one individuals who perfectly recovered—i.e., eighteen per cent. Of these 115 cases there were 81 examples of true epilepsy, but of the epileptics only 8 absolutely recovered, equalling only ten per cent. Of the cases not truly epileptic, there were 23 which belonged to the category of organic cerebral disease, or so-called 'symptomatic epilepsy;' of these 10 were cured, 7 ameliorated, 4 were lost sight of, and 2 remained in *statu quo*. In 43 per cent., therefore, of so-called 'symptomatic epilepsy,' a cure was effected." (p. 312.)

"The danger to life in the epileptic is a somewhat remote contingency. It rarely happens that the fits have proved directly fatal. Instances are to be found of death from cardiac disease and from cerebral complication, and I have known a case in which a pauper was said to have died in an attack, but the real cause of asphyxial convulsions and of death in whose case was discovered, *postmortem*, to be a piece of meat sticking in the rima glottidis.

"Again, so far as my own observation extends, the attacks rarely leave behind them, in true epilepsy, either paralysis or other change of motility; and still less frequent are any notable injuries to the organs or functions of special sense." (p. 317.)

In the *treatment* of the disease, chloroform, according to Dr. Reynolds, appears to delay the attacks for a time, but to exert no permanently good influence. Bromide of potassium, recommended by Sir Charles Locock in those cases of epilepsy where the attacks recur only at the menstrual periods, has been found by the author to be of much use in some cases of hysteria, though since Sir Charles's suggestion appeared, he has not met with a case in which epileptic seizures were limited absolutely to the menstrual period.

Dr. Reynolds is opposed to the employment of issues and setons in the treatment of idiopathic epilepsy. "In cases of chronic meningitis, which have been confounded with epilepsy, both these modes of treatment have proved of service." The shower-bath he has never seen productive of any definite good, but often of very distinct harm. "Sitz-baths and sponging baths of a few seconds' duration have generally in this, as in many other maladies, been grateful to the sufferers, and of value in increasing the feeling of health, the enjoyment of food, and the capacity for exercise."

In the *mental* treatment, if we may so term it, of the disease, the most important point is to find "some occupation in which the patient takes interest, which requires some concentration of mind, but not much thought, and which may occupy many hours of the day. Drawing and painting, under the conditions mentioned, are of great value; the fabrication of various articles, amusement with a fernery, garden, vivarium, &c., may also be recommended."

We have thus touched upon some of the leading points in Dr. Reynolds' volume sufficiently to give our readers a general idea of its arrangement and of the nature of its contents. Epilepsy has recently been so frequently brought under our consideration, that a more detailed examination of the present work appeared to be unnecessary. We have already expressed our opinion of its value, not only as a book of reference, but also as containing some original philosophical reflections and useful practical suggestions.

Review IV.

On the various Contrivances by which British and Foreign Orchids are Fertilized by Insects; and on the good Effects of Intercrossing. By Charles Darwin, M.A., F.R.S., &c. With Illustrations.—*London.* pp. 365.

Gilbert White's objections to botany, or rather to botanists, on the score that too much attention was paid to the mere systematic classification of plants, and too little study bestowed on the laws of vegetation and the practical application of botanical knowledge to the wants of man, still continue in force, though possibly to a less extent than at the time when the amiable vicar of Selborne penned his well-known letters.

The elder De Candolle, by giving his sanction to the botanical essays of Goethe, did much to promote the study of morphology, and thereby greatly amended the systems of classification, and placed the whole science on a much more philosophical basis.

In this branch of botany there has been much progress, but the march of vegetable physiology is indeed slow. How little do we know of the life-history of any plant! How scanty is our knowledge of the functions performed by the several parts of plants! What little we do know of these matters has been taught us for the most part by " outsiders," by physicians and physicists, country clergymen and chemists, rarely by those who are ranked as botanists. Mr. Darwin, again, would probably shrink from being termed a botanist, and yet he is but another example of the fact, that we have to thank others rather than botanists for the little we know of plants as they live and grow and reproduce themselves around us.

There are many points mentioned in Mr. Darwin's recent work* which have a more or less direct bearing on animal physiology and pathology; while no student of nature can fail to find matter for speculation in the very numerous and carefully elaborated facts which the patient, sagacious observation of Mr. Darwin has elicited. The book is written for a purpose; it forms part of the many-sided argument wielded by the author in his previous work; but, we think, with rare honesty, the facts and the inferences from them are kept sedulously apart, so that the reader of the present volume is not made an unwilling partisan, but may, if he choose, occupy himself solely with the observations recorded by the author. The subject of the book is sufficiently well indicated by its title. The four first chapters are more especially devoted to the British Orchids, the fifth and sixth to sundry of the exotic species, while the final chapter is occupied with an elaborate discussion on the homology of the flower in these singular plants, on the causes of the great diversity in form in parts adapted to fulfil the same purpose, and with a general summary on insect agency.

The grotesque appearance of some Orchids, the beautiful colouring and rich perfume of others; their rarity, except in certain favoured spots, furnish abundant reasons for the partiality so generally evinced towards these plants by lovers of flowers. They will now be rendered tenfold more interesting to the student, when he learns that the extraordinary forms assumed by these plants have a definite purpose; that organs which he may have considered as trifling and unimportant, have a hidden meaning and a special import; that they may, in fact, be compared to the springs which, when touched, set in motion the wheels of some curious piece of mechanism.

To those whose delight it is to dwell upon the manifold instances of intelligent design which everywhere surround us, this book will be a rich storehouse—rich in facts, richer still, if possible, in the promise of further wonders to those who will pursue the subject with the necessary patience and skill. By way of illustration we will select two plants, whose conformation and mechanism are not the least curious of the many similar instances described by Mr. Darwin. The first is *Orchis*

* On the various Contrivances by which British and Foreign Orchids are Fertilized by Insects, and on the good effects of Intercrossing. London, 1862. 8vo.

pyramidalis—a plant not uncommon on limestone soils in various parts of England. The flowers of this Orchid may be described as having three sepals, two at the side of the flower and one at the upper or hinder part; within these are three petals, two at the sides equal in size and shape, while the third, called the "labellum" or lip, is placed at the lower or foremost part of the flower, and differs greatly from the other two petals. This lip at the distal end is broad and divided into three lobes, its central portion is narrow and marked by two prominent ridges, which leave between them a groove directed towards the column in the centre of the flower, and continuous with the long, hollow, spur-like nectary which forms the hinder part of the lip. Quite in the centre of the flower, standing up above the orifice of the nectary, is the "column," resulting from the fusion of the stamens with the styles, organs which in most other plants are perfectly disunited. The upper part of this column is formed by the two anther-pouches, which are nearly parallel one to the other, and contain each a club-shaped pollen mass on a little handle or stalk, the two handles being tied together below by a small saddle-shaped piece of membrane called the "viscid disc," which is enclosed in a little bag called the "rostellum." The remainder of the column is constituted by the "stigma" and the "rostellum." The stigma, unlike that of most orchids, is two-lobed, the two lobes diverging one from the other, like the arms of the letter V. Between the two lobes of the stigma, or rather at their point of convergence, is the "rostellum," a little membranous pouch, containing fluid, and overhanging and partially blocking up the orifice of the spur-like nectary before mentioned. Such is the machine and its parts; let us now examine the working thereof.

If a bristle or a needle be gently passed into the centre of the flower, it is guided by the ridges of the lip to the orifice of the nectary, where it comes into contact with the rostellum; instantly the rostellum opens by a little chink, leaving the saddle-shaped disc that ties the two pollen masses together exposed; this disc is bedewed with viscid mucus, which causes it to adhere to the bristle, and thus, when the bristle is withdrawn, the two pollen masses are withdrawn with it. The adhesion is partly due to the viscid mucus, which, like a cement, becomes hard as it dries—partly to a curious contractile property that the viscid disc has, whereby its two ends curl round the bristle, and firmly attach the disc to it. If by some chance the bristle fail to remove the pollen masses, the chink of the rostellum instantly closes, and thus the disc is kept moist, ready for a future more successful trial; if this closure did not happen, the cement would dry and harden, and the pollen masses would not be so easily removable. The two pollen masses, at the time of the removal, are nearly parallel; but within a very short time of their liberation from the anther-pouches, they are seen to diverge one from the other, and to change a nearly erect for the horizontal position; thus, while each at first forms a right angle with the bristle, it speedily forms an acute one by the bending downwards and outwards of its upper club-shaped end. It will be thus seen, that if a moth alight on the lip for the purpose of sucking the nectar in the nectary, its proboscis will follow the same course as our imaginary bristle, and the insect will fly away with the pollen masses adhering to its proboscis. Anon the insect will visit another flower, and now the object of the divergence and depression of the two pollen masses is seen. If the pollen masses remained in their original nearly parallel, erect direction, they would, when conveyed by an insect to another flower, be pushed up against the anther lobes of that flower—a situation where they would be useless; as it is, they impinge upon the V-shaped stigma already described; here some of the pollen-grains remain adherent to the viscid surface of the stigma, and ensure the fertilization of the ovules, while the bulk of the pollen mass still remains to fertilize other flowers withal.

Could any one have imagined so ingenious a set of contrivances to exist in these pretty weedlings? Is this mere theory, or is it true? is a question we have been frequently asked since the publication of Mr. Darwin's work. Whenever practicable, we have shown the bristle experiment. Mr. Darwin, moreover, figures the head and proboscis of one unlucky moth, with seven pairs of pollen masses adherent to it; and he gives numerous experiments and facts to show that the pollen-masses are removed by insects, and that if their access to the flowers be prevented, fertilization is not effected.

In the case of the Orchid we have selected for illustration a difficulty arises, from the fact that the nectary seems always to be empty, although there is plenty of fluid in the walls of the nectary; hence, Mr. Darwin surmises that the nectar may be secreted between the two membranes of which the walls of the nectary are composed, and that the moths penetrate the thin walls with their proboscides for the sake of obtaining the nectar; but this does not seem at present to have the support of ascertained facts, nor do we see that the presence of nectar is essential, as the peculiar odour of the plant, as Mr. Darwin suggests, might attract some insects, and the colour of the flowers be an inducement to others, to visit the flower. Supposing the fluid in the walls of the nectary to be really nectar, Mr. Darwin suggests that one reason of its being confined there is to allow sufficient time for the hardening of the cement of the disc of the pollen masses on the proboscis of the insect, this time being gained during the process of penetration of the proboscis into the tissue of the nectary.

Perhaps the most remarkable of all the orchids mentioned by Mr. Darwin are some species of the genus *Catasetum*. Space will not allow us to enter into many details; nor is this necessary, for the general plan of construction is the same in this genus as in the genus *Orchis* selected for our first illustration. In *Catasetum saccatum* the rostellum is provided with two long horns, called by Mr. Darwin "antennæ," one of which projects in front of the upper part of the lip, while the other one is bent downwards and outwards away from it. The pollen-masses have one broad, boat-shaped stalk, common to the two, representing the slender handles of the pollen-masses in *Orchis*, and attached below to a thick cushion-like disc; the stalk of the pollen masses is bent outwards, and the cushion-like disc is enclosed within a cavity in the upper part of the stigma. When an insect visits one of these flowers it alights on the lip, comes into contact with the antennæ placed in front of that organ, when immediately the pollen-masses, with their curved spring-like stalk, and the heavy disc at its end, are jerked or filliped out with considerable force to a distance of two or three feet, and adhere to whatever surface they may come in contact with, by means of the viscid disc. So firm is this adhesion, and so strong the stalk, that Mr. Darwin records that the stalk supported, without breaking, a weight of nearly three ounces for a few seconds, though itself very thin, and not more than the twentieth of an inch in breadth. Supposing the disc to become attached to the body of an insect, no force that the insect could exert would be likely to detach the stalk of the pollen, while, however, ample provision for the application of the pollen grains to the stigma of another flower is afforded by the ready disintegration of the pollen mass and the adhesion of its constituent grains to the stigmatic surface. In the flower under notice, only that antenna projecting in front of the lip has this sensitiveness to touch; when the other antenna, or indeed any other portion of the flower, is touched or shaken with force, the pollen masses remain *in situ;* but once let the point of the sensitive antenna be touched, and out fly the pollen masses, as just described. The antenna is an inch or more in length, and this extreme sensitiveness is instantaneously conveyed along its whole length, till the viscid disc of the pollen mass is detached in the forcible manner already described. This appears to us to be the most extreme degree of sensitiveness or vegetable irritability, or whatever it is to be called, that we yet know of in the vegetable kingdom. Let us hope that Mr. Darwin, or some other able physiologist, will turn his attention to this remarkable sensitiveness of simple cellular tissue; surely much light might be thrown on the contractile tissues of animals, by an investigation into the nature of the same phenomena as shown in plants. No one need be at a loss for materials, the processes of opening and closing of the commonest flowers are scarcely understood at present.

Connected with the genus *Catasetum*, is a circumstance which bewildered botanists, and, as Dr. Lindley remarked, "shook to the foundation all our ideas of the stability of genera and species:" this is the occasional presence on the same plant of two or three very different forms of flowers. These forms, indeed, were well known as existing on separate plants, and were referred to three distinct well-defined genera. No wonder, then, when specimens were brought to this country from abroad, and others were produced in hothouses in this country, showing two or three different kinds of flowers on the same spike, that the botanists were astonished and perplexed beyond measure. We quote from Mr. Bateman's splendid work on these plants the following, referring to the genus *Cychnoches,* which is closely allied to *Catasetum.*

"Among Mr. Skinner's earliest Guatemala collections, attention was particularly directed to the specimens of a plant which to the habit of a *Cychnoches* joined the long pendulous stems of a *Gongora*, and for the possession of which in a living state no small anxiety was entertained. Some plants were speedily transmitted by Mr. Skinner, but these, on flowering, proved to be only the old *Cychnoches ventricosum.* A mistake was of course suspected, and Mr. Skinner being again applied to, sent over a fresh supply of plants for the authenticity of which he vouched, but they were scarcely settled in the stove when the flowers of *C. ventricosum* were again produced. Mr. Skinner, importuned for the third time, and being then on the point of returning to this country, determined to take one of the plants under his special protection during the voyage, which, flowering on the passage, seemed to preclude the possibility of further confusion or disappointment. The specimens produced at sea were exhibited, and the plant itself placed in the stove at Knypersley, where it commenced growing with the utmost vigour. The season of flowering soon arrived, but brought with it a recurrence of the former scene of astonishment and vexation, for the blossoms, instead of the coveted novelty, were not distinguishable from the old *C. ventricosum.* These were still hanging on the stem, when the inexplicable plant sent forth a spike of a totally different character, and which was, in fact, precisely similar to the specimens gathered in Guatemala and to those produced on the voyage."*

Mr. Darwin supplies us with the solution of this enigma, by showing that some of these flowers are male only, some female, some hermaphrodite; that these are generally borne on separate plants, but are sometimes borne on the same plant. We cannot follow him into the very singular details by which he establishes these facts and supplies the explanation of such remarkable phenomena.

What pains are taken to secure cross fertilization in these plants! how perfectly is the end attained! If some great good were not intended, would all these pains be taken? Have we not, as students of human and animal physiology, something to learn here?

Cross fertilization by insect agency appears to be the rule in these plants. There are a very few exceptions to it, one of the most remarkable being in the case of the bee Orchis (*Ophrys apifera*), in which evident provision exists for self-fertilization—why, we know not. It may be asked what occurs supposing an insect with pollen of one flower attached to it visits another flower of a different kind? The answer is twofold. In the great majority of cases the structural arrangements are such that no effect is, or can be, produced; but when the form and position of the parts do allow of fertilization, hybrids are formed. These are, so far as we know for certain, rare; but it may well be that some of the forms we reckon as species may have had such an origin, and this is the more likely as some of them rarely form seeds, but are propagated by the formation of tubers, &c. In the 'Amœnitates Academicæ,' vol. vi. p. 120, it is stated that if the new bulb of the Orchis be destroyed, the seeds will ripen; but we are not aware whether this assertion has been confirmed of late years. The history of these tubers, too, is almost as remarkable in its way as the history of the flower, and it explains a fact which must have struck many—viz., the abundance of Orchids at a particular spot in one year, and their almost complete absence (above ground) in the following year. After a lapse of years, favourable conditions again

* See also Carpenter's Comparative Physiology, fourth edition, p. 643; Trans. Linn. Soc., vol. xvii. p. 522; Lindley's Botanical Register, fol. 195; Vegetable Kingdom, p. 173.

give rise to the production of numerous flowers, &c. We cannot enter into this subject, but will merely refer those of our readers who may be interested in this subject to a paper of M. Fabre, in the 'Ann. des Sc. Nats.,' quatrième sér. tome iii.

The last chapter of Mr. Darwin's book, 'On the Homologies of the Flowers of the Orchids,' belongs more especially to the botanist, and has less interest for the student of animal physiology; hence we merely commend it to our botanical readers, and to those who may be interested in seeing how the facts mentioned in the earlier chapters are cited in support of the well-known views of the author, as to the origin and progressive modification of existing species.

Review V.

Impaired Vision; or, Long, Short, and Weak Sight, and their Treatment by the Scientific Use of Spectacles. By T. Soelberg Wells, M.R.C.S. Lond., M.D. Edin., Ophthalmic Surgeon and Lecturer on Ophthalmic Surgery to the Middlesex Hospital.—*London,* 1862. 8vo.

Our German brethren have long been known and justly appreciated as exhaustive workers of the mines of science; frequently, it is true, they have been greater in analysis than in synthesis—not necessarily clear, but always profound; not always successful in conveying the knowledge they themselves have acquired, but patient, faithful, and earnest labourers.

This is not less true in ophthalmology than in literature, art, criticism, and philosophy, with the additional advantage (not always attending their labours) of being eminently practical.

In this school, and under its best master, the author of the work before us—Mr. Wells—has laboured long, assiduously, and intelligently, and has given in a condensed yet full measure the results of those studies. Yet this most useful and practical little book is not a mere translation (nor, indeed, a translation at all) of what the German ophthalmologists have written and taught; this is not one of those bare reproductions by a young hand, designed to launch his name on the stream of professional literature—not a book written to learn instead of to teach the subject.

Mr. Wells has carefully and, as here shown, successfully studied under the indefatigable Professor of Ophthalmic Science at Berlin, and mastered the principles on which he has founded so much that is useful as well as new in this branch of our profession.

Under this able tuition, Mr. Wells has acquired a thorough knowledge of ophthalmic disease, and ophthalmic treatment, and in the book before us has selected one branch only of it, but that one so frequent and increasing, and which interferes so much with the every-day comfort of life, as to make it a very important one.

He has done good service to the profession by placing the whole subject of refraction and accommodation on a sound practical basis, and to the public in rescuing the spectacle question from the hands of the advertising adventurer.

While speaking of what has been done in this field by the Germans, we in no way forget or overlook what has been written by Sichel, Bowman, White Cooper, and others on the same subject; but those who are conversant with their works, as well as with those of the German school, must admit how much new light has been shed on it since their treatises appeared.

Mr. Wells has commenced his investigations as to the condition of the eye as an optical instrument on a perfectly clear and intelligible principle, and has so laid down the method, that any one who will be at the trouble of mastering it thoroughly once for all, will not lay it aside. It is true it requires some little time and study, but when once acquired, it possesses the certainty of a mathematical demonstration, and varies not.

At first sight it appears more easy and simple to fit a patient with glasses till he says he can see better to read or work, but this is so far from being all that is wanted (except to sell spectacles), that the chances are, that more harm than good is done, and at the end of a few months, or even weeks, the second state of the patient is worse than the first.

This process is simple, rapid, and at the moment satisfactory to the dealer and purchaser, while the plan of investigating the actual condition of the eye and its powers of accommodation requires knowledge of the eye as an optical instrument, of its changes in form and tension, and of its pathological conditions. But this knowledge once obtained, noted, and compared, the process of adaptation is simple and certain, and a pair of spectacles suitable for the eye and its wants can be as accurately ordered by prescribing the number as a dose of medicine or a form of diet. That this facility, combined with certainty, may be obtained, it is requisite that one constant scale of numeration for glasses shall be agreed upon and adopted: it is as essential as the inch in measurement or the weight in chemical calculation.

The basis of the whole of Mr. Wells's book is the knowledge of the power of the eye to accommodate itself to what it has to look at; till this is learned—and it is not difficult—the causes of imperfect vision cannot be mastered.

The more the question of accommodation is studied, the more marvellous does the structure and mechanism of the eye appear; and it is only natural that the philosopher, the physiologist, the moralist, and the poet, should alike be enamoured of the subject.

That the same eye should be able to take in a landscape of enormous extent—as, for example, that from the top of the Great Pyramid—and next moment inspect the minute insect that is creeping in the crevices of its time-worn masonry; that it should be able to do this an unlimited number of times without injury to its powers; that its movements, though purely voluntary, are yet so accurate and easy that no cognizance of them is taken by the mind, fills the thinker with wonder and the Christian with reverence.

How this marvellous adaptation is accomplished is still an unsettled point, and various opinions have been advanced and abandoned again and again. The notion that a change took place in the shape of the lens under pressure was entertained by that profound philosopher, Dr. Young, but it was thought by subsequent investigators that the light thrown by the microscope on the structure of the lens refuted this idea.

Now, however, Cramer and Helmholtz appear to have settled this, at least to their own satisfaction; also, we believe, to that of their followers. We think, however, that so much depends on the accuracy of the instrument by which Helmholtz measures the changes in the curve of the surfaces of the lens, as to make it very desirable to have his experiments repeated.

We do not propose to enter into the controversy on this point. Mr. Wells has said as much on the various theories as is required for the elucidation of the subject of accommodation, and agrees with H. Müller that the iris, in conjunction with the ciliary muscle, is the chief organ of accommodation. Mr. Wells proceeds to develope his views, step by step, with careful mathematical precision, and, as will be seen, those whose education began, *eheu, fugaces*, twenty years ago, must polish up their elementary mathematics.

"As the term *infinite distance* constantly recurs in these pages, it will be well to explain its signification at the onset. We consider an object to be at a *finite* distance as long as rays emanating from it fall in a divergent direction upon the eye. Of course, rays from even a very distant object do really diverge, but this divergence (which naturally decreases in extent the further the image is removed) is already so slight when the object is placed at a distance of eighteen or twenty feet, that the rays from it impinge, to all intents and purposes, parallel upon the eye, and we therefore consider rays coming from an object further than eighteen feet as parallel, and as emanating from an object at an *infinite* distance. Rays coming from a nearer object are divergent in proportion to its proximity, and are considered as coming from a *finite* distance. (p. 2.)

He then proceeds to define and divide accommodation into positive and negative—the former for near, the latter for distant objects, and to state that they depend on a different mechanism, chiefly on the external muscles of the eyeball, according to Von Graefe, but also on the action of the ciliary muscle, according to Henle.

The range of accommodation is thus stated:

"1st Proposition.—Where the eye has assumed its highest state of refraction, it is accommodated for its nearest point of distinct vision; when its state of refraction is, on the other hand, relaxed to the utumost, it is adjusted for its furthest point.

"In normal eyes the nearest point of distinct vision lies at about eight and a half to four inches from the eye; this varies, however, according to the age of the patient. The near point recedes further and further from the eye with advancing years. For continual work at near objects—engraving, for example—the near point lies at about five inches. Few eyes, indeed, can bear to work for any length of time with the object nearer than this. The further point of distinct vision in the normal eye is at an infinite distance." (p. 19.)

"2nd Proposition.—The distance between the furthest and nearest point of distinct vision is called the territory, or range of accommodation."

Donders determines the range of accommodation thus:

"I take the liberty, therefore, of proposing a standard as a general expression for the range of accommodation. The range of accommodation A (which may be calculated through the distance which P and R, the nearest and furthest extreme point of accommodation, are from the anterior surface of the crystalline lens,) is given by the focal distance, A, of an ideal lens when placed upon the anterior surface of the crystalline lens would afford to rays emanating from the near point, a direction as they came from the far point. We must suppose this lens to be a meniscus placed upon the anterior surface of the crystalline, because the accommodation depends almost exclusively upon a change in the convexity of the anterior surface of the lens." (p. 20.)

Here we see the application of Donders' theory of a change in the shape of the lens, and not of the cornea, as the means of a change of accommodation.

"We can easily understand that a may be approximatively found by the formula, $\frac{1}{P} - \frac{1}{R} = \frac{1}{a}$ and that $A = \frac{1}{a}$." When once this formula is mastered, all the applications of it to the investigation of abnormal accommodation follow as a matter of course.

Mr. Wells illustrates this in the following manner:—

"A means range of accommodation; r far point; p near point (these had better have either been the initials F and N, or the words "remote" and "proximate"); ∞ means infinite distance, 1 foot, $''$ inch, $'''$ line." (p. 20.)

"Normal eyes which can see from an infinite distance up to 5″, from the anterior surface of the crystalline lens, have their far point (r) at an infinite distance ∞, their near point (p) at 5″. In order to find the range of accommodation of such an eye, we apply the above formula: $A = \frac{1}{p} - \frac{1}{r}$ in our case $r = \infty$, $p = 5''$, therefore $A = \frac{1}{5} - \frac{1}{\infty} = \frac{1}{5''}$.

"2. Let us test the range of accommodation (A) of a short-sighted or myopic eye. Let us suppose that its far point (r) lies at 8″ from the eye, its near point (p) at 4″, A therefore $= \frac{1}{4} - \frac{1}{8} = \frac{1}{8''}$.

"3. A presbyopic or far-sighted eye having the far point (r) at an infinite distance (∞), and its near point at 10″, has $A = \frac{1}{10} - \frac{1}{\infty} = \frac{1}{10}$." (p. 21.)

It is requisite to commit to memory these formulæ, and then the examination of eyes as to their normal power becomes a certainty; there is no question as to what is their condition, no baffling uncertain statements by the patient can mislead the judgment.

There are other methods of ascertaining whether the eye is myopic, presbyopic, or hypermetropic, which equally call for the consideration of the student, and are carefully stated by the author. His definition, too, of these three distinct conditions of the eye are well and clearly put.

"1. Normal of emmetropic eyes in which, when the eye is at rest, parallel rays are brought to a focus on the retina. When the normal eye is in a state of rest, the focal point of its dioptric system is situated on the bacillar layer of the retina.

"2. Myopic or brachymetropic eyes which are adjusted when in a state of rest, for divergent rays. In this case parallel rays are, even when the eye accommodates itself for its farthest point, brought to a focus before the retina, so that distinct images are formed on the retina only of those objects the rays from which impinge divergently upon the eye.

"In the myopic eyes, when in a state of rest, the focal point of the dioptric system lies before the retina.

"The hypermetropic eyes are adjusted for convergent rays. In this case parallel rays are brought to a focus behind the retina when the eye is at rest, for in hypermetropic eyes the focal point of the dioptric system lies when the eye is at rest behind the bacillar layer of the retina." (p. 26.)

This condition of hypermetropia, new to most of our British ophthalmologists, will require further notice, both from its influence in affecting the working power of the eye, and its connexion with an important pathological condition.

The chapter on myopia is capital, and full of instruction. Speaking of its origin, the author makes this warning remark:—"Myopia is often hereditary, but the seeds of it are also frequently sown in childhood." (p. 29.)

This is an important truth, and when we see children bringing the object before them very near their eyes, it should never be neglected or passed over.

Very often, also, amblyopia, or weak sight, is confounded with myopia or short sight. Weak-sighted persons, in order to obtain large retinal images, hold small objects very near the eye, but they cannot, like short-sighted persons, distinguish very small objects, and instead of concave glasses enabling them to see further, they see less well through them.

Short-sightedness is so often regarded as of no consequence that it is important to correct so grave an error.

"Every progressing myopia is threatening to the eye. If it progresses, the eye will become less and less useable, troublesome symptoms at the same time showing themselves, and not unfrequently vision is irrecoverably lost, at fifty or sixty, or sooner, through the detachment of the retina, extravasation or atrophy, and degeneration of the yellow spot." (p. 35.)

The dependence of myopia on insufficiency of the recti interni muscles—a recent discovery of Von Graefe's—is carefully treated. The truth of this theory, and the certainty with which its cure by tenotomy of the external rectus muscle, we have had several opportunities of testing. It is a most important point, and removes the operation of tenotomy for squint from the place of merely ornamental to the rank of curative surgery. We well remember the rise, raging progress, and subsidence of this operation. It now takes its true position as a cure for diplopia combined with or causing myopia and asthenopia.

The amount and extent of the division of the muscles, its method of execution, and the caution to be used in performing the operation, are fully given, and prove that this is not an operative proceeding proposed merely for display, but having a sound philosophical, therapeutic basis for it.

The length to which our observations have extended, and the necessity for reserving a space for one or two important points, compel us to limit our remarks on presbyopia to the concluding paragraph of that chapter.

"In conclusion, I may be allowed to call the reader's attention to the very important fact that a very rapid increase of presbyopia is one of the premonitory symptoms of glaucoma. If, therefore, a patient tells us that his far-sightedness has rapidly increased within a few months, so that he has had repeatedly to change his spectacles during that time for stronger and stronger ones, our suspicions should be aroused, and we should without fail examine him as to the presence of other premonitory symptoms of glaucoma, e.g., rainbows round the candle—periodical obscurations due, in Von Graefe's opinion, to intraocular pressure and flattening of the cornea." (p. 80.)

One of the most troublesome and wearisome to treat of all the affections of the eye, is that which has gone by such a variety of names, such as asthenopia, hebetudo visus, muscular amaurosis, and such like. It is sometimes no doubt due to an exhausted frame; hæmorrhages, over-nursing, and other like conditions of the system, may induce it; but in a great majority of instances it has nothing to do with it, and hence, though in the conditions above stated, tonics, rest, sea air, and good diet will effect a cure, yet in the majority of cases no good is effected by these means, or by the use of their opposites, such as purgatives, alteratives, and counter-irritants. The reason is, that the condition does not depend on constitutional causes, not even on over-work alone, though this, no doubt, is a frequent adjuvant, but it depends upon a condition which is here treated of and denominated "hypermetropia."

"By hypermetropia is meant that peculiar condition of the eye in which the refractive power of the eye is too low, or the optic axis (the antero-posterior axis) too short. We may, however, also have both these causes coexisting. The effect of the too short axis, or of the too low refracting power of the eye, is that the focal point of the dioptric system is behind the retina, so that in a state of rest even the parallel rays are not brought to a focus on the retina, but behind it, and only convergent rays are united upon the latter." (p. 82.)

"In the myopic eye, it will be remembered, the state of refraction is too great, or the optic axis too long, so that when the eye is in a state of rest the focus of the dioptric system lies in front of the retina, and parallel rays are brought to a focus before the retina, and only more or less divergent rays are united upon the latter. Now in hypermetropia we have just the reverse of this; the refracting power of the eye is so low, or its optic axis so short, that when the eye is in a state of rest parallel rays are not united upon the retina, but behind it, and only convergent rays are brought to a focus." (p. 83.)

This chapter is replete with interest and instruction. The causes, the ophthalmoscopic signs, the diagnosis, prognosis, treatment, and cure of hypermetropia are ably and amply treated. Its importance cannot indeed be well over-estimated, and when we say that in the majority of early cases the cure consists in possessing two pairs of spectacles—one for proximate and one for remote objects—it is obvious that *le jeu vaut la chandelle*, and that some trouble, loss of time, and patience devoted to accurate examination and ascertaining the precise condition of the eye, is well laid out, and amply rewarded by escaping the tedious infructuous treatment of former days. Moreover, this is the form of disability of vision that affects that large and increasing class of workers who depend on their eyes for their daily bread, whose want of *good* sight involves starvation, like the compositor, sempstress, engraver, and others.

Mr. Wells has given due, but only due, prominence to the importance of the ophthalmoscope in aiding the diagnosis of the internal condition of the eyeball. On this subject there has been the usual exaggeration and depreciation, assertion and denial; as ever truth has lain between the two, though the balance has been rather in favour of the former.

The great importance of this aid to investigation is now established, but as the use of the stethoscope or microscope consists not in being able to hear or see by the instrument, but in knowing what you heard and saw, so in the use of the ophthalmoscope it is to know what you are seeing, that is of primary consequence. This instrument comes before us in Mr. Wells's work as a matter of course, but especially as the only means by which we can pronounce with certainty on a disease which is the parent of all those intractable conditions which end in total loss of vision, and which is here called sclerotico choroiditis posterior.

This is a disease which, if we understand its symptoms and the proper treatment at the outset, and if we duly bear in mind all the grave complications that attend it if ignorantly or carelessly treated, may be so managed as to preserve the eye in many instances. It is here handled in a masterly manner. It is not easy, indeed, to overestimate the importance of its early recognition, for it is to its nondetection and its subsequent intractability that so many cases of so-called amaurosis are due—cases in which separation and atrophy of the retina, vitreous opacities, intra-ocular hæmorrhage, and the drop serene occur.

Till we were made acquainted with this disease by Mr. Wells and his master at Berlin—though for thirty years our attention has been largely turned to ophthalmic diseases—we were ignorant of the true pathology of this worst form of them.

We feel pretty sure that there are many others in the same predicament, and we earnestly recommend the careful study of this condensed and well-written chapter to them.

Since we became acquainted with this disease, several cases have come before us in which the early recognition of the symptoms have, we believe, led to the arrest of the disorder, but it is one which demands a great degree of careful management, a calm reliance on the treatment as well by the practitioner as by the patient, and an avoidance of conflicting contradictory advice; it needs also a contented acquiescence in apparent inactivity not often found in patients who, tormented by kind friends, hurry from one oculist to another, till they finally leave their eyesight at the doors of some miserable charlatan.

To the practitioner who is content to study afresh by the new lights of science—and unless he is so content, he must remain a mere routineer—we recommend the perusal of this book as the best and most concise work on the subject in our language; to the student, who qualifying himself for his future career, will make and avail himself of the careful step-by-step process by which he is led to the comprehension of those deviations from a healthy condition which he is one day to treat.

The author might, we think, have made some of his chapters a little more explicit: he has taken for granted that all his readers, or a majority of them, are well versed in the minute anatomy of the eye as brought down to the latest discoveries, but we can well imagine his desire to make his book concise and condensed.

We, however, congratulate the profession on the appearance of a work which puts them in full possession of what their Continental brethren have achieved in this branch of study.

Review VI.

1. *Report from the Select Committee of the House of Lords, appointed to inquire, Whether, having regard to the rights of property of the Crown and Individuals in Salmon Fishings on the sea-coast and in rivers and estuaries in Scotland, it is just and expedient that any and what legislation should take place for the regulations of such Fishings, so far as regards the use or prohibition of bag nets, stake nets, cruives, and other fixed nets and engines, and so far as regards close times or otherwise; and to Report to the House; together with the Proceedings of the Committee, Minutes of Evidence, Appendix, and Index.* Ordered by the House of Commons to be printed, 16th July, 1860. pp. 497.
2. *Report of the Commissioners appointed to Inquire into the Salmon Fisheries (England and Wales), together with the Minutes of Evidence.* Presented to both Houses of Parliament by command of her Majesty. 1861. pp. 545.
3. *A Bill, intituled an Act to Amend the Laws relating to Fisheries of Salmon in England.* Ordered to be printed, 29th July, 1861. pp. 17.

The two voluminous Blue Books which, with the short Act, form the heading of this article, are interesting and important, and not a little curious from the varied nature of the information which they contain. They will well repay a patient reading, and are especially deserving of the attention of the ichthyologist, of the political economist, and of the statesman and lawyer—of the latter, because the evidence is so often contradictory; of the ichthyologist, for the particulars scattered through them, communicated by practical men, respecting the habits of the salmon and its congeners; and of the political economist and statesman, on account of the demonstration they afford of the deplorable state of our salmon rivers, and of the sad waste of an article of diet, which, if judiciously cared for and properly protected, would be second scarcely to any other, no fish being more sought after than the salmon, no one more wholesome and nourishing, and no one more needing legislative protection.

After going through the minutes of evidence taken before the Commissioners in their circuit through England and Wales, one is not surprised that the salmon-rivers have fallen off in their produce, but rather that they have not altogether ceased to be productive. According to one evidence—that of a gentleman who has paid particular attention to pisciculture, and whose property has been greatly benefited by it—whilst the extent of the salmon rivers in England and Wales is about one-fourth more than in Ireland, the value of the former as to produce is vastly less, that of the Irish rivers being estimated at 300,000*l.* annually, that of the English and Welsh at even less than 10,000*l.* per annum. The falling off, the deterioration of the English rivers, is more strikingly shown by the same gentleman, Mr. Thomas Ashworth, by comparing the produce of certain rivers in Scotland and Ireland with certain in England. Of the many brought forward, the following is an example: The river Tay, with an area of 2800 square miles, produces salmon the annual value of which he estimates at 30,000*l.*; whilst the river Ouse and its tributaries, comprising an area of 2400 square miles, yields probably salmon of a value not exceeding 400*l.*, the rental being no more than 122*l.* 15*s.* The Duke of Richmond's fishery on the Spey, his alone, brings him in 12,000*l.* a year; and yet in times past, and those no very remote times, there is ample proof that the rivers of England were not less productive than those of Ireland and Scotland at present; indeed, weighing the evidence, there is reason to infer that they were even more productive. Even the Scotch rivers, many of them, are reported on as having deteriorated—the Tweed is a striking example, and also several of the Irish. The account we have of the abundance of salmon formerly in many localities is almost fabulous, as is indicated by engagements on the part of masters not to give their apprentices and servants a meal of salmon oftener than twice a week, and by the cheapness of the fish when brought to market.

The causes of the great change do not appear to be far to seek or difficult to understand. The evidence on the subject, wherever the Commissioners made their inquiries, is very clear, and on this point at least consistent. The chief causes seem to be connected with an increased population, a vast extension of our manufactories, mines, and chemical works, and a very improved agriculture. Owing to the first, the demand for salmon has augmented, its price has risen, and there has been a greater temptation to illegal fishing and poaching; owing to the second, the rivers have been injured, obstructions innumerable placed in their course, and their waters polluted and often poisoned; and owing to the third, which we hold to be the least of all the evils, the higher streams have been deteriorated as breeding streams, partly by the effect of drainage and the culture of moorland wastes, and partly by the lime and artificial manures applied to the reclaimed land liable to be washed into the streams. Of all these causes, poaching, and the obstructed and injured condition of the rivers and their waters, are certainly the chief. We have it proved in evidence that many rivers in which salmon were plentiful are now even without trout—this latter, a hardier fish, having been poisoned and utterly extirpated by noxious substances in solution or suspension derived from manufactories, chemical works, gas works, and mines. The illegal taking of fish, poaching in its various ways, it would appear, has been carried to an almost incredible extent. The slaughter is chiefly of the spawning fish and of the young fry. In many rivers we are assured that scarcely a single fish that ascends from the sea ever returns, being captured either by net, gaff, or spear. Of the fry, we learn that in many places it has been the practice to show them no mercy, taking them, when they are most easily caught, in their descent, and in such quantities as to render them valueless, excepting as pig's meat and for manure. Even by angling there has been a vast destruction of the young fish. One witness, accounting for the diminution of salmon in the Wye from the reckless destruction of old breeding fish and salmon fry, describes the mode of angling for the latter in use. He says: I have known nearly four hundred taken by a rod in one day, but not by fair angling. The poacher uses from ten to a dozen flies, and on each fly a maggot. He does not, like an amateur, pull out a fish directly he catches one, but leaves it as a lure, and "does not pull it out until he has got eight or ten fish on his line." And this is a practice which is followed when the young fish are "schooling," a term expressive of their assemblage on their migratory descent. This wasteful destruction of the salmon, we find, is not restricted to our English rivers; wherever the circumstances of society are similar, the evil seems to be equally felt. The following quotation from the writings of a distinguished naturalist, M. de Quatrefages, shows how this is the case in France, and happily shows also how, by proper means, rivers, now become almost worthless, may be rendered a source of vast revenue:

"Par suite de la destruction du poisson, le frais du fermage est tombé si bas, que pour le Rhône il n'est que de 7 francs et pour le Durance de 2 francs par kilomètre. Encore reste-t-il plus de 200 kilomètres qui n'ont pas trouvé de fermier. Aussi le revenu de ces 1500 lieues de cours d'eau est-il seulement de 521,000 francs. En les ramenant à peu près au degré d'empoissonnement que prennent les canaux et les rivières bien entretenues, et en calculant au plus bas, ce revenu s'éleverait à cinq millions au moins."*

Before proceeding to the consideration of the Act of Parliament that recently passed for checking the evil in question, it may be well to offer some remarks on the natural history of the salmon and its congeners—a history which, in many respects, is peculiar, and altogether very interesting. It is a subject which will doubtless have the attention of Mr. Darwin, and we shall be glad to find him able to reconcile it, in all its difficulties, with his favourite hypothesis, accord-

* M. de Quatrefages: Récherches sur la vitalité des Spermatozoïdes: Ann. des Sci. Nat., tom. xix. p. 360.

ing to which all the several species of the genus salmo, so different in their habits and various in their qualities, are merely varieties; the charr breeding in lakes, inhabiting the same waters as the trout; the trout, like the salmon and sea-trout, quitting their usual feeding places for spawning, all three choosing for the purpose the smaller streams; and the two latter still more differing, in a seeming necessity they are under after a time to migrate from the waters in which they have been bred to the sea, and after awhile to return to their native streams.

All fish of the salmon kind appear to require for their existence cool water, and the nearer, probably, to 40° the better. None of the salmonidæ are found in a tropical sea; none even in the Mediterranean, though there are rivers, both on the European and African side, descending from cool mountain altitudes in which at least the common trout flourishes. We have found, we may further remark, by experiments made expressly for the purpose of testing their power of enduring heat, that a temperature of about 80° is unendurable, proving rapidly fatal to those we have tried—viz., the common trout, the charr, and the young salmon.

Another peculiarity of these fish, and of the migratory as well as those which are confined to fresh water, is, that sea water and even brackish water deprives their ova of vitality. Our experiments on this point, and other experiments—an account of which we have been favoured with, in which brackish water was used, of the specific gravity 1015—are perfectly clear and conclusive.

In the evidence given to the Select Committee of the House of Lords, and to the Commissioners appointed to inquire into the salmon fisheries, we find assertions boldly made to the contrary: that salmon not only spawn in the tide-way, but also in the open sea; in brief, wherever they happen to be at the time they are ready to perform the spawning function. The only reason assigned that has the least plausibility is, that the fish with mature ova have been taken in the sea, without considering whether the fish thus taken had a retentive power over their ova or not; or whether circumstances may not have prevented those ready to spawn from entering rivers; or whether the ova, if deposited in the salt water, were there hatched. We have in the details of evidence, other and almost as striking examples of the association of ignorance with confidence and dogmatism, and of little regard also for accuracy of statement, where the bias of self-interest has come into plea. Nor, if that be any palliation, are there wanting instances of absurd and oppressive legislation? Ireland, the prolific source of both, affords examples of the kind. One old law prohibits any one taking salmon who has not the qualification of 40*l.* a-year income: another inflicts a heavy penalty on a man if he allow his pigs to wander on the seashore, in the belief that they there devour the spawn and salmon fry.

Another marked character of these fish, harmonizing with their love of cool water, is that their great breeding-time is in the early winter, in November and December; and their favourite places for depositing their spawn shallow, running, clear, pure water, only a very few inches in depth, where there is loose, clean gravel to cover and protect the ova. A powerful instinct impels the fish to seek these spots, totally regardless of its own safety and totally contrary to its ordinary habits. How marvellous that a creature which leads the life of a gourmand in the sea, there rapidly growing and fattening, as after its first descent, should, under an innate, ignorant impulse, quit its feeding ground, enter fresh water, and ascend towards its source, though constantly resisted, swimming against a powerful stream, leaping up falls many feet in height, and not stopping, except for temporary rest, till it finds the locality suitable to its wants. Breeding salmon have not unfrequently been taken in rivulets so small as to afford scarcely space to turn; and in some of the great rivers of North America, it would appear, they have to ascend so far, so many hundred miles before the requisite condition of water can be found, that vast numbers of them are said to perish before they are able, owing to the distance, to return to the renovating salt sea.

Another peculiarity is deserving of mention, which belongs to their ova—viz., the power they have, when impregnated, of enduring any cold between 40° and the freezing point of water, without losing their vitality. A degree of cold below 40° merely retards their development; a beneficial circumstance this, inasmuch as it helps to keep back the hatching till a time of milder weather, when there is an increased chance of a good supply of food for the young fish. And in harmony with this is the fact, that the fry, for at least five or six weeks after liberation from the ova, can live without feeding, supported by the residuary yolk, which is in part external, contained in a pendant little bag—the contracted vitelline sack, and in part internal, communicating with the intestine by a special duct. By one of the persons examined, it is stated that the ova may be frozen and yet retain their vitality. This is a mistake: the trials we have made prove it to be so. In our experiments they have borne a reduction of temperature two or three degrees below 32° with impunity, without being frozen; but when the temperature has been further reduced, so as to freeze them, their death has invariably followed. The circumstance that the salmon frequents Arctic, or rather bordering seas, and breeds in rivers emptying themselves into those seas, only confirms what we have said of their fondness for cool water: those persons who use the occurrence as an argument in favour of the tolerance of the ova of congelation, are forgetful that water flowing from under ice is rarely below 40°, if coming out of deep lakes, whatever their situation; and under no other circumstances do rivers in those regions escape being frozen throughout.

Not the least remarkable part of the history of the salmon is that of its early period—that extending from the hatching of the ova to the change of element from the river to the sea. The hatching, as to time, is variable, depending, as already remarked, on the degree of temperature,—seldom under seventy days, seldom exceeding one hundred and twenty. Before the young fish migrate, they undergo a certain change, altering in appearance so much that in the different stages of the parr and smolt* (the parr the younger, the smolt the more advanced fish), they were long considered, and even by naturalists, as distinct species. The parr, in becoming a smolt, acquires a new coat of scales, these resplendent with a silvery nacreous lining, and which covering the old and smaller scales, hide the markings, the dark transverse bars which are peculiar to and are characteristic of the young fry. This alteration of appearance, we believe, is very much dependent on the feed of the fish, both as to quality and quantity, and accordingly as the food is plentiful, or the contrary, the conversion of the parr into the smolt is accelerated or retarded, as likewise their fitness for migration. This under favourable circumstances of diet (of which flies form a considerable part, those rich in phosphate of lime, which is essential to the growth of the scales) takes place in about twelve months from the hatching, or under adverse ones, is retarded to eighteen months, or it may be two years. In relation to the matter of the descent of the young salmon, there has been a good deal of discussion, one observer maintaining that the smolt when a year old becomes impatient of fresh water, and instinctively hurries to the sea; whilst another, having under observation fish less amply supplied with food, being equally confident that the fry are not in the smolt condition and fit for travelling until the end of the second year. What we have stated, founded on information obtained from various quarters, may account for the difference of opinion.

We have said that the parr and the smolt were until recently considered as distinct species; nor is this surprising, not only taking into account the difference of appearance of the two, the parr with its red spots more resembling the common trout than the salmon, but also the remarkable fact, now well established, that the male parr, before it becomes a smolt, has its testes fully developed, so as to be capable of impregnating the ova of the adult salmon, whilst in the instance of the female parr, the ovaries are found only in a rudimentary state, and so small as to escape notice unless sought for. A state of testes seemingly so abnormal is, if we may speak of final causes, no doubt a wise provision, well adapted to secure the impregnation of the ova, as the old breeding females we have seen seek the same shallow brooks in which the parr is to be found before it changes its coat and becomes a smolt. It may perhaps be asked why this paradox as regards the sexes? The only answer we can give is that vague one, that it is founded on the fitness of things, and involving the doctrine of final causes. It may be a question, if there were a development of ova in

* The number of names given to the fry of the salmon and its migratory congeners is very remarkable and not a little confusing. In the Act the following are specified—these including the fish of all ages:—"*Salmon* shall include all migratory fish of the genus salmon, whether known by the names hereinafter mentioned, that is to say, Salmon, Cock, or Kipper, Kelt, Laurel, Girling, Grilse, Botcher, Blue Cock, Blue Pole, Fork Tail, Mort, Peal, Herring Peal, May Peal, Pugg Peal, Harvest Cock, Sea Trout, White Trout, Sewin, Buntling, Guiniad, Tubs, Yellow Fin, Sprod, Herling, Whiting, Whitling, Scurf, Burn Tail, Fry, Samlet, Smoult, Smelt, Skirling or Scarling, Parr, Spawn, Pink, Last Spring, Hepper, Last Brood, Gravelling, Shed, Scad, Blue Fin, Black Tip, Fingerling, Brandling, Brondling, or by any other local name."

the little parr—these, the ova, about the size of the common pea—whether their formation would not exhaust the young fish, and if not exhausting it, whether their expulsion would be practicable. There are indeed a very few instances recorded of ova of a good size having been found in parrs, but these fish, we believe, had been unduly detained in fresh water, and had increased considerably in bulk, without becoming smolts, owing probably to a want of the proper food.

In a brief period after the descent of the smolt, from the quantity of rich nourishing food it finds in the sea, such as sand-eels, shrimps, and other small crustacea, it undergoes a further change, and from a fish of two or three ounces it grows into a grilse of from four to five and six pounds, and this often in the short space of four or five weeks, when it would appear that it again becomes impatient of change, and seeks again its native streams, the female now in a state fit for propagation, its ovaries active, the ova progressing slowly to maturity.

Fortunately for the preservation of the species, the prolific powers of the salmon are great, the number of ova increasing with the size of the fish in the ratio of about one thousand to the pound, so that a full-grown salmon, say of twenty pounds, will shed about twenty thousand, whilst the testes, the milt, even of the parr, must contain millions of spermatozoa,—microscopic objects so minute as to require a high power to bring them distinctly into view, and yet one of them penetrating an ovum may suffice for impregnating it. This is in accordance with general analogy, and quite normal according to our physiological views, but need we say is not less mysterious than the abnormal peculiarity as to sexual power before adverted to!

We need not enter into particulars of the spawning operations of the salmon: they have been so often described, that they are generally well known, though even respecting the minutiæ of these there is not a perfect agreement amongst those who have described them. It may suffice to notice an error once and recently maintained, that the ova are impregnated before their expulsion—an error confuted by the results of trials, and *à priori*, one may say, was almost manifest from the peculiarities of structure of the sexual organs.

It is this prolific quality of the salmon doubtless which preserves it from extinction, for there is no creature that has more enemies. Itself predaceous in a high degree, in its turn, wherever it may be, and in all its different stages, it is preyed upon. The water-ouzel, the wild duck, and other birds haunt its spawning beds in quest of its ova, as likewise do all kinds of fish, especially the trout and eel, and even the salmon itself—not indeed the parent fish at the time of spawning—does not spare them. Those which escape and are hatched, are exposed to equal danger. Whilst in the river, the poacher is their great destroyer, and next to the poacher, certain fish, especially the pike in our English rivers, and the larger trout. In the sea, their risks rather increase than diminish; they are the favourite food in their smolt state of so many fish; and when larger, when grilse and salmon, they are pursued and destroyed by seals, porpoises, and sharks. And besides living enemies, how many are the dangers to which they are exposed, from the ovum to the adult, from the elements, such as river-floods, and drought, and poisoned waters.

Before finishing these brief notices of the salmon, we may as well advert to some obscure points in its history:

1. Does the adult fish breed every year, or every second year? We are inclined to believe that none of the salmonidæ breed yearly, but rather every other year. This conclusion is founded on the fact, that in the examination of a large number of white trout and of the common trout, on the approach of the spawning season, we have found as many with the roe and milt extremely small as with these organs nearly fully developed. And there is ample evidence that even at the height of that season, in the instance of the salmon, there are many taken in a barren or fallow state, and these in their highest and best condition.

2. Can the roe of the salmon be fertilized by the milt of the common trout, so as to give rise to crosses—intermediate varieties? The few experiments which have been made appear to prove that the thing is practicable. It is certain that the ova of the salmon have been hatched after impregnation by trout's spermatozoa. Mr. Shaw gives one instance; and an instance has come to our knowledge of the ova of the charr having been successfully impregnated by the milt of the trout. It is very desirable that more trials should be made, and the results carefully recorded, especially when successful, of the appearance and habits of the young fish, the offspring. They may aid in explaining peculiarities occasionally met with in the salmon, the white trout, and the common trout, which at present seem anomalous.

3. Is the salmon capable of remaining in the sea an indefinite time, or in fresh water for an indefinite time? or, in other words, is an alternation of the two requisite for its perfect health and highest condition? The migratory habits of the salmon when not breeding, seem at present hardly to be referrible to any principle, almost as little as the habit in man when leaving his comfortable inland home for a sea-coast watering-place, and in so many instances periodically, with the full persuasion that the change, however disagreeable from the discomforts he will have to encounter with his family, is necessary somehow for their well-being. Judging from the evidence in the Blue Books, it seems to be shown that the salmon can remain a long time at sea with impunity; that it does not always enter fresh water under the spawning influence, and returns accordingly to the sea without having spawned. It seems also to be shown that the breeding fish enter some rivers when they are in a gravid state, their roe and milt mature, or nearly so; and in other rivers, earlier, when the organs in question are little advanced towards maturity. Various speculations have been offered relative to the motive-causes of these habits. Apart from the instinct connected with the preservation of the species, the hypothesis most insisted on is, that the leaving of the sea is to get rid of a parasite (*Caligus piscinus*), the sea-louse; and the forsaking of fresh water, to be rid of a parasite of another kind (*Brachiella salmonia*). But this view is opposed by the fact stated in evidence, that

hundreds of fish, all of them in the highest condition on their first entering a river, are without the sea-louse; and the further fact, for which we can vouch from our own experience, that the fish returning to the sea after spawning are not always infested, as presumed. We are disposed to think that the temperature of the waters may be concerned with this migratory propensity. The salmonidæ all appear to be peculiarly sensitive of degrees of temperature, as well as to delight in cool water. It may be for this enjoyment, when they are fully fed and loaded with fat, that they leave the warmer sea for the cooler lake or river; and that after a time, when impelled by hunger, and not under the breeding στοργή, they may be tempted to return to their ample sea-fare. It is, we believe, chiefly where large bodies of water, such as several of the Scotch and Irish lakes, communicate with the sea by short rivers of easy access, that a running forward and backward from the sea to the lake, and from the lake to the sea, are oftenest witnessed. But this would not account for the descent of the smolts, which seems to be an act of pure instinct, especially as it appears that a change of water, from fresh to salt, is not absolutely essential to the existence of the fish. Mr. Yarrell gives an instance in proof of fry put into a newly-formed pond, from which they could not escape, seemingly doing well. Some of them taken with the fly, after two years, were described as "from two to three pounds weight, perfectly well shaped and filled up, of the best salmon colour outside, and the flesh well flavoured and well coloured, though a little paler than that of the new run fish."* The same author also, on the authority of Mr. Lloyd, writes of salmon bred in a lake and confined by impassable cataracts. These fish are spoken of as small in size, and of inferior flavour. But respecting them some doubt may be entertained, inasmuch as a very good authority, speaking of these very salmon, gives one to understand that there is a communication, and that passable, with the sea.†

By some it is supposed that the salmon and sea-trout after entering the sea do not travel far, and the fish, when they return, seek their native streams. In the main, both these opinions may be true; they are supported by facts. But there are other facts, if we may give credence to respectable persons, showing that there are exceptions. We have heard of a salmon taken off Malta—one, it may be inferred, which had gone astray, and had been carried by inflowing currents from the Atlantic into that inland sea, and had there wandered till it was captured. And instances are given, in the minutes of evidence before the Committee of the Lords, of salmon taken in the sea many

* British Fishes, vol. ii. p. 21.

† In interioribus quibusdem lacubus, *Wenern*, Siljan hiemem degit, unde vere preterlapso fluvios adscendit. Lacubus igitur ut mare, pro hibernis utitur, aquam salsam nunquam attingens. Vernæ horum lacuum dicuntur pinguiores et carne saturatiores, quam marini, qui, itinere et impedimentis fatigati, dictos lacos interdum attingunt."—Nelsson, *Pisces Scand.*

If the majority of the salmon use the lakes as their feeding grounds, instead of the sea, and fatten in them, it is probably owing to their finding in them similar food. We have been informed that crawfish and shrimps abound in their waters.

miles from any river. One informant states that, being at the mouth of the Tweed in August, he saw salmon leaping in the sea so far as the eye could reach. Another states that "ships from Norway see salmon forty or fifty miles off the coast."

4. Can the fry of the salmon be distinguished by any characteristic marks from the fry of the white trout? Some anglers and naturalists believe that such marks are obvious, and that they can make the distinction. This we doubt, seeing what a powerful influence the colour of water has on the colour of the fish, and knowing that mistakes have been made by very experienced persons, who fancied they knew the characteristic marks of each kind, these marks chiefly restricted to the colour of the fins.

5. Is the grilse not a true salmon (*Salmo salar*)? That the grilse is not the salmon on its first return from the sea, is an opinion recently advocated by one gentleman, who maintains that it is a distinct species. This notion has met with no support or credence that we know of: it is an undeniable fact that a grilse has never been taken in a river not frequented by salmon; and it is equally certain that a large number of captured grilse, released after having been marked, have, when retaken, become acknowledged salmon.

6. Is there a parr not a young salmon? The opinion entertained that there is such a fish we believe to be as erroneous as the last; though we cannot go along with the Chairman of the Commission, Sir William Jardine, that "any one who will insist now that a parr is not a young salmon must have some warp in his intellect;" knowing, as we do, one person who holds this notion who is amply provided with shrewdness and common sense. It has been asserted, and by the person just alluded to, that there are two rivers in Scotland inaccessible by salmon, on account of insurmountable falls in their course, in the upper parts of which parrs are found. We have carefully examined one of these rivers, and have satisfied ourselves that the inaccessible impediment is a delusion. We compared parr taken above the fall—that a moderate height for a salmon-leap—and parr taken below it, and found them perfect in resemblance; and we were assured by a countryman living near the fall, that he had in the breeding season seen salmon in the stream high up beyond it.

On the value of salmon as an article of diet we shall now offer a few remarks. This is a part of the subject, we need hardly observe, that is especially deserving of the consideration of our medical readers; and the more so as, in common with so many other parts, it is not without its obscurities.

In relation to diet, the salmon and its congeners may be viewed in three or four different stages: we speak now of the adult fish, as, very properly, the killing of the delicate fry, these a luxury, is now strictly prohibited.

1. Of the fish not spawning, when taken in the sea, or in fresh water, just come from the sea, when it is designated by the angler as "a clean fish," and is in its highest condition and greatest beauty.—In this state its flesh is red, and when dressed is firm and crisp, with

layers of white curd between the muscles. Its rich colour is owing to a certain quantity of oil of the same hue, and which, with a certain proportion—a minute quantity indeed—of iodine, is wasted and disappears after spawning, and a continuance of any length of time in lake or river. This clean salmon—and the remark applies to the white trout in the same state—is excellent food, and nourishing in a high degree. The prices which it brings, when rare, as at the beginning of the season, sufficiently marks how much it is in request, and how highly it is valued. Forty or fifty years ago, when salmon were taken in the Thames, eleven and twelve shillings a-pound was not an uncommon price for it, the salmon of that river was so much esteemed; and Scotch and Irish salmon is now sold early in the year from three to four and five shillings a-pound as retailed by the fishmongers. The presence of the iodine as a constituent part, and the oil, probably contribute to its wholesomeness. If it have any fault, it is to be found in its richness; in consequence of which it does not seem acceptable as daily diet; of this we have had proof in the facts already adverted to, of its restricted use in the palmy days of its abundance in our rivers. The lusciousness of the fish appears to increase with keeping for awhile, as if the curd, from exposure to the air, underwent a change and disappeared, which it certainly does, and were replaced by oil.

2. Of the spawning fish.—When taken full of roe or milt, even if both mature, the fish seems to be but little deteriorated as food, especially if taken in the sea, or shortly after leaving the sea, when, it may be presumed, it has not become attenuated by fasting. Some Welsh epicures, we are told, are not satisfied with a salmon without the roe. It is, too, generally asserted that the fish when in this condition is unwholesome and unpleasant to eat. We cannot but hold this to be a mistake, neither warranted by fact or analogy. Is not the smelt, that delicate species of the salmon kind, in most request when in roe? And does not the remark apply to the cod, the herring, and so many other salt-water fish, all of which, it may be inferred, like the salmon fresh run, have had no want of feed before their capture? If, on the contrary, the salmon has been long in fresh water, and its roe or milt have there become developed, then we admit it may have become deteriorated. The ova and the milt in this case seem to be formed, not from new nourishment taken in, but from the substance of the fish itself, whence a wasting, especially of its curd and oil. It is remarkable how abstinent the salmon is in fresh water; and it is equally remarkable for the unfavourable change to which it is subject from being in fresh water; even in a day or two it is said to suffer, losing its brightness of hue, and acquiring when dressed a flavour less grateful to the palate. In the instance of the white trout, the alteration from change of element is not less remarkable. After a residence of some weeks in fresh water, this trout loses its distinctive whiteness, and acquires the colour nearly of the common lake brown trout; but falling off little in condition, which may be owing to its appetite not failing it in the same degree as the salmon.

3. Of the salmon after spawning.—Immediately after this impor-

tant event, the fish seem to sicken and suffer, becoming feeble, soft, and flabby, especially the female. Now it is in its worst state for the table, and probably hardly wholesome food. We have not heard of its having any injurious effect when eaten; in truth, it is difficult to obtain information on the subject, no one but the poacher ever taking it when in this state—and he is not the person to peach of his misdeeds and relate his experience. We have learnt, in conversation with a distinguished Arctic traveller and ichthyologist, that not only the men of his party, but even the dogs, suffered more or less from eating of "white fish," a species of coregonus taken just after spawning; and this though at the time there was no diminution of their fatness. Now, when we consider that what is unpleasant to the taste is commonly unwholesome, even setting aside the instance just mentioned, and the fact that almost all animals after parturition are sickly, we need not, we think, hesitate in pronouncing the salmon just spawned as a thing to be avoided, and that the use of it is hazardous.

4. Of the spent salmon, so called when on its way to the sea, and it may be many weeks after having spawned.—Is this fish unwholesome? We more than doubt it, for we know it is used by the peasantry, and without any bad effect on the health, and as a matter of curiosity in our fishing excursions, we have tasted it ourselves without its disagreeing. But though it may not be unwholesome, it has little to recommend it; it has lost its salmon hue, and has become pale and insipid, not unlike some indifferently fed river trout.

No one can be more opposed than we are to the taking of the salmon, except when in its highest condition and in perfection for the table. What we have stated in opposition to popular belief is for the sake of truth, all error, all exaggeration, having, we believe, an evil tendency. We are assured—the fact is much dwelt on in the evidence collected by the Commission and the Committee—that vast quantities of salmon out of season are exported to Paris. Surely this export would not be continued were the fish commonly found to disagree with those who partook of them. Let the Parisian, however, be persuaded that there is a risk of illness from eating fish just after their spawning, will it not make him averse from partaking of it, except when in season, of its best quality, and not under average price? As to French cooking making good that which is essentially bad—a notion thrown out in explanation—is a fiction founded on ignorance.

It is time now to speak of the Bill, the new Salmon Bill of 1861, designed to amend the laws relating to the fisheries of salmon in England, England in legal phrase including Wales and Berwick-on-Tweed. By the passing of this Act, as many as twenty-seven former Acts and portions of six others have been repealed, these dating from the time of Edward I. to the present reign. Brevity is one of the recommendations of the Bill; it is comprised in fifteen pages, and though so brief, it pretty clearly defines what is lawful and what is contrary to law as regards the capture of the salmon, and fish of its kind which, like it, are migratory, and thereby, we regret to think, excluding from its protection those which are not migratory, such as the common trout and the charr. We must refer those who are specially interested in the subject, and who wish to know all the powers of the Act, to the Act itself. We must confine ourselves to the notice of its more important clauses; these, in our opinion, are the following:

1. That there should be the same open and close season for all English salmon rivers, the open time beginning on the 2nd of February, and ending the 31st of August, after which the sale of salmon is prohibited. A grace, however, is granted to rod-fishers, to give them an interest in, and to encourage the protection of, the upper breeding streams; this lengthening of the open time to the honest angler, but without giving him the power of sending the fish caught to the market, is extended to the 1st of November.

2. The taking of salmon-roe or salmon-fry, or being possessed of them, and the use of fish-roe (we would read of any kind) as a bait or lure, are things strictly prohibited.

3. No permanent obstructions are permitted in rivers or tidal waters—that is, in estuaries, and where there are weirs of a lawful kind, "a free gap" in them is imperative, and also the opening of them once weekly—viz., "between the hour of twelve of the clock at noon on Saturday and the hour of six of the clock on Monday morning," during which time also the killing of fish, except by rod and line, is subject to a penalty.

The general superintendence of the salmon fisheries throughout England is vested in the Home Office, to which we regret to see application may be made by the magistrates assembled at Quarter Sessions to alter the close time of any particular river. We speak of this with regret, because if granted, there will be an end of the general close time; and a difficulty may arise as to the conviction of persons charged with killing fish out of season. With this one exception, taking the Bill in its entirety, we are sanguine that it will be of great service, and go far to effect the great object in view—the improvement of our salmon fisheries, both in fresh water and salt—i.e., the river fisheries and the sea fisheries.

In the preliminary inquiries, as shown in the minutes taken before the Select Committee of the Lords and the Commissioners, we witness a strong antagonism between the river-fishers and the sea-fishers. The former declare that the stake-nets in the sea are one of the chief causes of the diminution of the supply of salmon; the latter maintain that their fisheries tend to increase the supply, and that the decrease in such rivers as have decidedly fallen off, is owing to other causes, such as poachers killing the spawning fish and the ruthless destruction of the fry, and the obstructions, with other detriments, in the rivers. The Act, it is to be hoped, has been just to both interests whilst consulting the common good. It must, we believe, be admitted that the sea-fisheries, properly regulated, are the more important. It is by them that salmon in their highest condition are taken. It is in their service that most employment is given, and to a class of young, active men, well adapted by their habits to recruit our navy.

It is pleasant to think how large under this Act is the scope for improvement. We hope to see not only many rivers which were once famous for salmon again abounding in this noble fish, but also other rivers with an aptitude for breeding salmon converted into salmon rivers by the removal of obstacles, or the construction of ladders permitting the ascent of the fish — such as have been tried with success in certain Irish streams. We have shown by a comparison of the produce of Irish and the English fisheries, the sad state of the latter, and yet, large as is the produce of the former, it has by no means reached its maximum; it admits of immense increase. We have now a letter before us from one of the gentlemen — Mr. Thomas Ashworth — who gave information to the Commissioners respecting the amount of produce of the rivers of the two countries just referred to, of which we have made mention. Having had permission from him, we shall quote his words, written, as he concludes, in haste; their purport accords perfectly with the evidence he gave last year before the Commissioners:

"The Galway fishery has increased about ten times in its produce in less than ten years,* from cultivation and protection of the salmon in the spawning-ground, and by having a Queen's gap cut of sixteen feet wide, always open, in the centre of the weirs, thereby allowing a large quantity of fish to pass up and reproduce their species. And we close our fishing season about the 12th August (*very early*), the lawful time being the 30th August. We find that we have a larger quantity of fish this year than any previous year; last week about ten or twelve anglers caught 190 *salmon;* the previous week 105; and on one day they caught 100. Three of the anglers caught ten salmon each in one day, with an increased quantity by nets as compared with previous years.

"We artificially propagated 660,000 salmon ova last winter, and conveyed them into new rivers round Lough Mask, where no salmon had been before, and protected the whole from poachers."†

Now, if so much can be effected in one locality in so short a time, how great is the promise for England if the proprietors of salmon rivers are "up and stirring," attending to their own interests, and by so doing benefiting the country at large. That the success in England will not be, whatever exertions may be made, so great as in Ireland, may be taken for granted, keeping in mind the differences of circumstances of the two countries. The great salmon rivers of the latter have the advantage of being connected with large lakes, and of being situated for the most part in districts little cultivated, thinly inhabited, and without manufactories, so that they are easily protected,

* In another letter with which he has favoured us he states, "our angling is more valuable now than the whole fishery was ten years ago. The catch up to this day, July 9th, is 1647 *salmon*, by the rod and line, this season; and this quantity has been taken upon less than one mile in length of the river, by about seven or eight gentlemen."

† The fish-passage to these rivers has been facilitated by ladders. The new fishing district thus made is described as thirty miles by ten in extent, so equal to 300 square miles. Credit, we are informed, is due to the Messrs. Ashworth for having first attempted in the United Kingdom the artificial process of breeding salmon. In 1852 we had the pleasure of seeing it in progress at Oughterard, on the shore of Lough Corrib, in Galway.

are safer from the depredations of the poacher, and are nowise liable to have their waters rendered unwholesome and destructive. But even a moderate success would be highly remunerative; and some of the English rivers, and more of the Welsh, if they have not all the advantages of the best of the Irish, are in this respect inferior to them only in degree. It should ever be remembered that the culture of our rivers interferes with no other culture, and that the resulting profits are a pure gain. Pisciculture, a new and expressive term for an old art, of late too much neglected, is now attracting much attention on the Continent, and especially in France, where it is largely encouraged by the Imperial Government. The writer whom we have quoted to show how the impoverished rivers of that country may be improved and rendered productive, says, enforcing his argument, "on peut semer du poisson comme on seme du grain;" and nothing is more easy, all the requisites being the mixing of the mature ova and milt, and exposing them to the action of clear running water. And by the new Act leave is granted to this effect. In proof of the facility of hatching salmon ova, we may mention that we have succeeded, using a tumbler as the receptacle, and changing the water only once a day; and also, on a somewhat larger scale, substituting an earthenware pan for the glass, and not changing the water more frequently. In this latter way we have succeeded in the hatchment of many hundred ova, and have had the pleasure of witnessing their development.

We have expressed regret that other, the non-emigrating fish of the genus *Salmo* were excluded from the Bill. This was not the intention of the movers of the measure in the House of Commons, Sir George Lewis and Mr. Clive. Their Bill was, "To amend the Laws relating to Fisheries of Salmon and Trout in England." Had it been carried, great is the benefit it would have conferred in affording protection to our trout-streams, in enhancing their value, and in promoting the pleasing and healthy exercise of angling. It was in Committee that the trout was struck out, and various other changes made in the original draft, which in its details was more comprehensive than the one that has passed into law. We hope that the subject will be again brought before Parliament, and that for the common good, the trout, and charr, and grayling, and indeed, river-fish generally, will be adequately protected. Men of the medical profession, we are sure, are interested in the subject. Anglers amongst them there are many; and who are better judges of the value of fish as an article of diet? and moreover, who are more happily situated for agitating the matter, and for enlightening the public on the value of our fisheries? Were it not so, we should hardly have been justified in admitting this article into our Review.

P.S.—Since this article was written, a new Act has been passed regulating the salmon fisheries of Scotland. It bears the date of the 7th August, and is to come into operation and take effect from the 1st of January, 1863. It is constructed on the same principles as the English Salmon Act; and, we regret to see, as in that Act, it gives the Commissioners, acting under the Home Secretary, power to fix the close-time for any river or sea fishing (no general close-time being specified)—a circumstance, as we have already insisted on, likely to render conviction difficult for selling salmon out of season.

Review VII.

1. *Observations on the Human Voice.* By Manuel Garcia, Esq. ('Proceedings of the Royal Society,' vol. vii. p. 399. 1854–1855.)
2. *On the Laryngoscope, and its Employment in Physiology and Medicine.* By Dr. J. N. Czermak, Professor of Physiology in the University of Pesth. Translated from the French Edition by G. D. Gibb, M.D. (The New Sydenham Society, 1861.) pp. 80.
3. *Praktische Anleitung un Laryngoskopie.* Von Dr. Ludwig Türck, k. k. Primararzte in Wiener Allgemeiner Krankenhause.—*Wien*, 1860. pp. 67.

Practical Introduction to Laryngoscopy. By Dr. L. Türck, Senior Physician to the General Hospital at Vienna.

4. *Zur Anwendung des Kehlkopfspiegels.* Von Dr. C. Gerhardt. ('Würzburger Medicinische Zeitschrift,' 1860, p. 173.)

On the Employment of the Laryngeal Speculum. By Dr. Gerhardt.

5. *Nouvelles recherches sur la Phonation.* Mémoire présenté et lu à l'Academie des Sciences le 15 Avril, 1861. Par Ch. Battaille, Ex-Interne des Hôpitaux, Ex-Prosecteur d'Anatomie à l'École de Médecine de Nantes, Professeur de Chant au Conservatoire Impérial de Musique et de Déclamation.—*Paris*, 1860. pp. 104.

New Researches on the Production of the Voice. By C. Battaille.

6. *Di Functionen des menschlichen Schlund und Kehlkopfes.* Dargestellt von Dr. C. L. Merkel, Privatdocenten der Medicin an der Universität zu Leipzig. Mit einer Tafel Abbildungen.—*Leipzig*, 1862. pp. 156.

The Functions of the Human Pharynx and Larynx. By Dr. Merkel.

7. *Remarks on Laryngoscopy.* By E. H. Sieveking, M.D., Physician to St. Mary's Hospital. ('Brit. Med. Journ.', June 28, 1862.)

Whatever reproaches may be made to the science of medicine on the score of our limited knowledge of, and comparatively trifling advance in, the therapeutic branch of our profession, we may fairly rejoice in the steady progress that is being made in diagnosis. Nor would it be difficult or uninteresting in many points to rebut the charges against our remedial agents on the very ground of the undeniable achievements that have been made in the same field. To say nothing of preventive medicine as applied to small-pox, to typhus and typhoid diseases, to scurvy, we may with justifiable pride draw the attention of cavillers to the positive benefit to mankind derived from our improved methods of recognising diseases of the heart—idiopathic and symptomatic—in their early stages, and thereby controlling them; we may maintain, without fear of contradiction, that the diagnosis of incipient phthisis, the distinction of the various pulmonary disorders that have been classed together as asthma, the certainty with which we establish the causation of renal, hepatic, or cardiac dropsies, have been positive gains, not only to the abstractions of medicine, but to the sick and suffering of our race. There may be doubts as to the best mode of treating acute rheumatism, and as to the *rationale* of the action of lemon-juice, nitrate of potash, or the alkaline carbonates, but there is none that the recognition of valvular disease enables us to adopt steps which shall prevent permanent organic change, or modify and arrest its destructive tendencies. Whether the tincture of the muriate of iron or gallic acid is curative of granular disease of the kidneys in certain stages, may be a matter of discussion, but not whether we may by an early recognition of chronic albuminuria adopt various means, hygienic or medicinal, which shall improve the patient's chance of life, mitigate his sufferings, and check his downward progress. Again, it can scarcely be denied by any one who has seen much of the incipient stages of phthisis, that whether we believe or not in cod-liver oil, in naphtha, in the hypophosphites, for the cure of the disease in its later forms, we are enabled by an early diagnosis to secure to our patients a position, regiminal and therapeutic, which shall place the morbid power in a state of abeyance which shall restore strength and prolong the span of life. Could we not say the same thing of the improved diagnosis of gastric disease? Has no advance been made in the arrest of disease of the nervous system by the galvanic test, by the test of sensibility, by the elimination of many disorders with nervous symptoms which are now positively referrible to the heart, the kidneys, or other organs?

Feeling strongly persuaded of the real advance made of late years in the practical applications of medical science, and knowing how much we owe to the patient labours of those who often, under great trials, have prosecuted researches in the first instance not apparently fraught with immediate benefit, we have much satisfaction in placing before our readers a few of the results obtained recently by the method of investigating the physiological and pathological conditions of that portion of the air-passages which has hitherto almost eluded our ken.

The stethoscope, valuable as it is in unravelling the web of thoracic disease, has done but little for the larynx; and yet when we consider how important this organ is; how frequently it is affected, idiopathically and secondarily; how important and unceasing its functions, both as the portal to the lungs and as the main agent for all human intercourse; how wide its nervous and vascular relations in the economy, we may well receive gratefully all aids that are offered to us for the investigation of its physiological and pathological conditions. Although it would appear that in England the first attempts were made to throw light upon the cordæ vocales—a term that, in the present instance, is both metaphorical and literal—it is in Germany that we find realized the first practical method of turning the physical exploration of the larynx to practical account. We should not be doing justice to the subject if we did not associate very prominently with the whole subject of laryngoscopy the name of Professor Czermak,*

* The *cz* are pronounced like *ch*, as in the word choose.

of Prague, who though not absolutely the first to inspect the interior of the larynx by an arrangement of mirrors, appears, from all we have been able to ascertain on the subject—and our reading has embraced most of the brochures and articles that have been published thereon —to deserve the credit of having made laryngoscopy an aid to the recognition and treatment of disease, and to have been the one to whom its introduction among the practical appliances of medicine is mainly due.

But while we desire to do all honour to M. Czermak for the indefatigable zeal and success with which he has prosecuted the scheme and realized the idea, we claim for our countryman, Mr. Avery, long since, unfortunately, consigned to a premature grave, the priority of the invention. He, too, feeling the importance of the subject, contrived a concave mirror, to be supported by a stem held between the teeth, which threw its concentrated light upon a second mirror introduced into the fauces at an angle capable of at once receiving the rays of the ocular mirror, and the reflection thus produced of the subjacent parts. Mr. Avery's contrivance, which may yet be seen at Messrs. Weiss', in the Strand, presents all the main features of the methods since introduced for the examination of the larynx by Garcia, Türck, Czermak, and others. Mr. Avery's apparatus embraced a lamp, which was carried with the concave mirror; but although a good "notion," it was impracticable on account of its bulk and weight. A similar objection may be raised against the mirror, which he introduced into the fauces, because, being at the end of a hollow cylinder, there is necessarily great difficulty in introducing it and obtaining that consentaneous action of all the parts of the apparatus which is necessary to success. Mr. Avery's invention had the merit of realizing a good idea, but in such a way as to render it unsuited for general adoption. Even before Mr. Avery contrived his laryngeal mirror, Mr. Liston, in 1840, suggested the employment of a dentist's mirror, bent at a suitable angle, for the investigation of the fauces; but the way in which he speaks* of the matter shows that it was merely one of those vague suggestions which many have made besides him, and not one deserving of the name of an invention or a place in the annals of medical progress. We should not have alluded to the part Liston took in the matter, but for the stress Czermak lays upon Liston's suggestion. It is different with M. Garcia. Though not an Englishman, he resides in England, and the publication of his researches in the 'Proceedings of the Royal Society' naturalizes his work; and we cannot but express a regret that what he has done received so little attention, and that

* Practical Surgery. By Robert Liston. London, 1840. Speaking of the inflammatory œdema of the upper part of the air-passages, Liston observes: "A view of the parts may sometimes be obtained by means of a speculum, such a glass as is used by dentists, on a long stalk, previously dipped in hot water, introduced with its reflecting surface downwards, and carried well into the fauces." (p. 417.) This is all he says on the subject. Those who have occupied themselves with laryngoscopy will best know how very little could have been seen by this method alone. Certainly the author himself would not have claimed for the suggestion the honours either of a discovery or an invention.

from his not being a medical man, he was unable to follow out the subject to its pathological and therapeutic conclusions.

M. Garcia's method of examination consists

"In placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on, against the soft palate and uvula. The party ought to turn himself towards the sun, so that the luminous rays falling on the little mirror may be reflected on the larynx. If the observer experiment on himself, he ought, by means of a second mirror, to receive the rays of the sun, and direct them on the mirror which is placed against the uvula."

All observers agree that no artificial illumination is equal to that obtained by the sun; but, unfortunately, the frequent obscuration of this source of light renders it impossible for the medical man to rely upon its aid, while under the most favourable circumstances no one could command a house in which at all times of the day the sun's rays would be available for laryngoscopic examination. While all credit is due to M. Garcia for having, with such difficulties, made the observations that he has, his results are necessarily imperfect, because he was unable to see more than a portion of the vocal cords. He states that he saw the arytenoïd cartilages separate in inspiration, the glottis large and wide open; but, he says, "however dexterous we may be in disposing these organs, and even when we are most successful, at least the third part of the anterior of the glottis remains concealed by the epiglottis."

We shall see that this restriction no longer exists; a restriction which, in diagnosis, would be a most serious bar, as it necessarily is, to the attainment of positive physiological data. Mr. Avery's apparatus, which, we are informed by Mr. Yearsley,* was invented in 1846, but does not appear to have been brought before the profession by the lamented inventor, and could not therefore be known either to M. Garcia or to Professor Czermak and more recent laryngoscopists, was intended to meet this difficulty. By the kindness of Messrs. Weiss we have been enabled to examine one of Avery's larynx specula. It consisted of two distinct parts, the one comprising the illuminating lamp and mirror, both supported on a straight stem, which was to be held between the teeth; the other a cylindrical tube, with a small mirror placed at an angle of 45° at one end, so that light thrown upon this mirror would be reflected down upon the larynx, an image of which would therefore be formed in the glass. We have here all the elements required for laryngoscopy—in fact, the idea of combining the lamp with the reflector is peculiarly ingenious, but the objection to the contrivance is, that it is scarcely applicable in ordinary cases. Even assuming a medical man to be able to support between his teeth the very heavy apparatus combining a large mirror and a lamp weighing a good many ounces, few, if any, patients could bear the introduction of a straight cylinder into the mouth in the position and of the size necessary to obtain the required image. It would be impossible to avoid contact with the base of the tongue and the consequent excitement of

* The History of the Laryngoscope. By James Yearsley, Esq.: The Medical Circular, Nos. 520, 521, 522. 1862.

reflex action. Had Mr. Avery lived, he would doubtless have seen the difficulty, and have solved it; as it is, we can only point to him as an ingenious *avant-courier* of those who have made laryngoscopy practically available.

In Germany, the question as to the priority of the invention has been carried on with some acrimony. It appears to us that the rival claimants may safely trust to the medical world for a due recognition of their actual services to the cause of science and humanity. Great credit is due to Dr. Türck, of Vienna, for having very zealously worked at the subject, and contrived the instruments, which he employed at first exclusively on the dead body. We, however, as impartial journalists, cannot withhold a special meed of praise from Dr. Czermak for having by dint of great perseverance rendered laryngoscopy an aid to the diagnosis and treatment of disease by the medical practitioner.

The method which he employs, and which has been adopted by various well-known physicians and surgeons in London of late, consists in receiving the rays of a moderator or gas-lamp upon a concave mirror of about three inches and a half in diameter, the mirror being either supported by a band passing round the head or by the teeth, and having a small central uncovered spot, allowing the observer to see through. The lamp being placed near the head of the patient, who sits before the operator, the latter first places the mirror so that its focus falls on to the patient's fauces when the mouth is opened. This is the first stage, and one in which the beginner experiences his first difficulty; however, by very brief practice this is soon got over. The second thing to be done is to introduce the smaller square mirror, which is attached to a straight wire stem, over the root of the tongue, and rest it against the uvula, so as to throw the pencil of light conveyed to it from the larger concave mirror down into the larynx and trachea. The uvula in general bears the contact well, but any irritation of the base of the tongue is likely to interfere with the observation by the reflex action thereby excited. There is, however, no serious difficulty in this, and after a very few *séances*, few medical men fail to introduce the throat speculum without annoying the patient or disappointing themselves. When once the observer has acquired some confidence in himself and his instruments, all serious difficulty is overcome, and we cannot but regard the statements of some of the writers on the subject as to the difficulty of accustoming the patient to bear the speculum, and the training they require, as exaggerated, and calculated to debar medical men from the use of an instrument which all may become familiar with. The subject is not one to be made another specialty of. The unprofessional public need not believe that there is only one man who is capable of diagnosing diseases of the larynx or looking down the trachea. Probably no one in England has yet acquired the facility with which Professor Czermak introduces the mirror, but there is no real mystery about it, and as the Professor has imparted all that he could impart to a considerable number of gentlemen, we trust that soon there will be a sufficient number of trained laryngoscopists to prevent the occurrence of such a calamity.

Both in a physiological and a pathological point of view, much has been actually achieved on the Continent. Our French friends have been less active than the Germans, but an excellent monograph by Battaille, published in Paris in 1861, shows that the physiology of the larynx* has been very carefully and successfully studied in France by the aid of the speculum. M. Battaille has the advantage of being an old *interne* and prosector of anatomy, while, later in life, he has devoted himself entirely to singing as a profession, so that he is peculiarly well qualified for the task he has undertaken. We very strongly recommend his work to the attention of our readers.† The author is a careful anatomist and a cautious observer.

It is not our intention to go into the physiology of the human voice generally, as our desire is rather to speak of laryngoscopic studies in connexion with pathology and therapeutics. We cannot, however, forbear quoting the results of M. Battaille's direct and very careful observations of the phenomena accompanying the production of the chest and falsetto voice respectively. This, as our readers well know, has long been a moot point. He finds that the relative position of the vocal cords and their mode of action differ materially in the two cases. In the production of notes belonging to the chest register, he has observed the following phenomena. (pp. 36 and 68.) The arytenoïd cartilages approach one another and are in contact by the posterior and inferior third of their inner surfaces; the apophyses (cartilages of Sartorini) scarcely touch, and vibrate with the vocal ligaments. These are stretched, and vibrate throughout‡ their entire extent, including the sub-glottic, ventricular, and marginal regions. In passing from the deep to the high notes, the glottis is sensibly shortened from behind forwards. At the same time the vocal ligaments are raised and gradually stretched in their three regions. The arytenoïd cartilages, while they diminish the glottic opening pos-

* We have also the title of another French work on Laryngoscopy, by Moura-Bourouillou, 'Cours complet de Laryngoscopie,' Paris, 1861; but we have not yet seen the book itself, and are therefore unable to express any opinion as to its contents.

† We do this with a reservation; we cannot approve the title *Sur la Phonation.* Another writer coins the word *Anthropophonik.* Why is it necessary to be always encumbering science with clumsy, lumbering Greek and Latin terms, deprived of all their euphony?

‡ M. Battaille divides the vocal cords into three regions: the sub-glottic, the ventricular, and the free margins. The first he describes as being limited by the angle of the thyroïd in front, posteriorly by a line which would continue downwards the anterior margin of the arytenoïd cartilages, above by the margin of the glottic lips, below by a horizontal line stretching from the crico-arytenoïd articulation to the angle of the thyroïd. The ventricular region M. Battaille describes as being bounded behind by the arytenoïd cavity, in front by a space a little larger than the thyro-arytenoïd insertions, externally by an oblique line stretching between the external portion of the arytenoïd cavity and a point of the thyroïd placed on a level with, and a little external to, the thyro-arytenoïd insertions, and within by the lips of the glottis. The third portion, the free margin, placed at the intersection of the preceding planes, extends from the arytenoïd apophysis to the inner angle of the thyroïd, and is formed by the lips of the glottis. The reader is recommended to examine for himself the anatomical relation of the muscular fibres to these parts in order more fully to enter into the *rationale* of their functions as delineated by M. Battaille.

teriorly, at the same time reduce the extent of vibrating surface. When the voice passes from high to low notes, the glottic vibrations become fuller and slower. For all chest notes the lips of the glottis remain parallel, and the superior thyro-arytenoïd ligaments take no part in the production of sound; the ventricles of the larynx remain linear. In the production of the falsetto voice, the most remarkable phenomenon is the immediate conversion of the rectilinear orifice of the glottis, which prevails in the production of chest notes, into an ellipse, while the opening enlarges posteriorly; at the same time the vibrations cease in the lateral portions of the glottis, the longitudinal tension diminishes, and the ventricular region of the ligaments is, so to speak, drawn from within outwards by a lateral traction. The ventricular region and free borders vibrate sensibly, though less than in the chest register; the arytenoïd cartilages are partially open at their bases, while their apices approach one another. The moment the falsetto is again changed into the chest voice the glottis becomes rectilinear, the antero-posterior tension increases, the ventricular tension diminishes, and all those phenomena reappear which are characteristic of this register.

M. Battaille's observations are exclusively physiological, and were made partly upon himself, partly upon his pupils or artist-friends. The majority of the profession in London have had repeated opportunities of witnessing the remarkable precision with which it is possible to conduct auto-laryngoscopy, as demonstrated by Professor Czermak. This is a procedure which may be recommended to all who are desirous of fully appreciating the value of this method of examination, while it allows of unlimited opportunities in the practical application of the mirror. In some persons auto-laryngoscopy is rendered difficult by an unusual narrowness of the fauces, otherwise the difficulties are not such as need deter any one from making the examinations on his own person. The only additional apparatus that is required is a plane mirror elevated in such a way that the image received on the faucial mirror may be thrown upon it and thus be rendered visible to the observer.

We may take another opportunity of dwelling upon the physiology of the larynx as demonstrated by the speculum; at present our object is rather to draw the attention of the profession to its practical application in the diagnosis and treatment of disease. Hitherto we can scarcely be said to have possessed any very definite means of making a positive diagnosis in laryngeal affections. The character of the cough, the presence or absence of tenderness or hoarseness, the concomitant general symptoms, with an examination into the history of this case, may enable us to form a shrewd guess as to the nature of the individual disease; but we are just as often wrong, as is best proved by the very vague manner in which counter-irritants, astringents, antiphlogistic remedies, galvanism, and the various agents ordinarily employed, are had recourse to. It would be difficult to find a more pointed illustration of the truth of these remarks than a case published by Dr. Sieveking, in which a man affected with chronic cough, pain at the right infra-clavicular region, a marked systolic arterial murmur at the same spot, feeble right radial pulse, and persistent hoarseness, was diagnosed to have an aneurysm, the diagnosis being in a measure confirmed by the aphonia, which was supposed to be due to pressure on the recurrent laryngeal nerve. On applying the laryngoscope, however, the discovery was made that there were two growths, one above the other, attached to the anterior portion of the true vocal cords, which, while they could not affect the physical signs noted at the right thorax, gave a new version to the hoarseness, and indicated a special line of treatment. We need but examine a limited number of cases presenting laryngeal symptoms, to satisfy ourselves that the pathological conditions we have to deal with are more varied than we should assume without the laryngoscope; and at all events that a precision is given to our knowledge of the affections of the vocal cords and the adjoining parts, which would have been impossible without such ocular inspection. The first case given by Professor Czermak, under the head of Pathological Observations, is one observed in 1858, and also characterized by the presence of tumours in the larynx. It is entitled, "Complete Obstruction of the Larynx by two Tumours springing from the Mucous Membrane beneath the free Border of the Vocal Cords, the result of scrofulous infiltration, and necessitating laryngotomy." As the case forms a sort of starting-point of practical laryngoscopy, and moreover illustrates a mode of using the speculum to which allusion has not yet been made, the following abstract may prove acceptable:

A girl, of eighteen years, of scrofulous habit, had been subject to repeated enlargements of various glands in different parts of the body. Her health improved after the establishment of the catamenia at sixteen, but a sudden enlargement of the submaxillary glands in the spring of 1858 was followed by swelling of the larynx. Its outline became more prominent, there was little tenderness on pressure, there was neither redness nor swelling of the pharynx, nor fever; the voice, habitually weak, fell to a whisper. In September of the same year dyspnœa set in, inspiration and expiration became sonorous, the larynx tender, and on the 4th of October a suffocative attack, threatening life, supervened, requiring the performance of laryngotomy. The symptoms showed that complete obstruction of the glottis had occurred. Professor Czermak, having been asked to examine the patient, after persevering attempts, rendered necessary by the irritability of the patient's fauces, succeeded in establishing that

"The false vocal cords were slightly swelled and injected, but moveable without any appreciable distress. It was possible to bring them rapidly into contact one against the other in the median line, and afterwards to separate them, so as to permit the ventricles and the inferior vocal cords to be distinctly perceived. These possessed almost their normal white colour, and to my great surprise showed a remarkable mobility. We observed, particularly during deep inspiration, or when the patient endeavoured to emit a sound, movements of the posterior portion, in which are placed the processes of the arytenoïd cartilages. Nevertheless it was not possible to close the glottis by the approximation of the edges of the vocal cords; these were invariably separated between the processes. I perceived between the separated borders of the glottis a superficial furrow formed by two cushions of mucous membrane, which were oblong and of a dark colour. The seat of the occlusion of the larynx was now recognised. It was found beneath the free edges of the true vocal cords, of which the superior lamella was normal; it was also the same in the other visible parts of the larynx, independently of slight swelling of the false vocal cords, and very considerable enlargement of the arytenoïd tubercles."

As Professor Czermak was desirous of still further eliciting the nature of the obstruction, he adopted the method employed by Dr. Neudörfer in examining the larynx in dead bodies. Through a canula with a large opening at its inner and upper end, contrived so as not to impede respiration, he introduced a small mirror into the trachea by the orifice made by the surgeon to prevent asphyxia; he gave the mirror an obliquely upward and forward direction, and by concentrating the rays of a lamp by an ophthalmoscopic mirror upon the laryngoscope, the whole interior of the larynx was strongly illuminated. It was now apparent that the glottis was obstructed by two tumours of mucous membrane situated beneath the free border of the vocal cords, and that they arose from the side and back of the larynx. Drawings of the tumours, and an illustration of the mode of using the speculum laryngis just described, are given in Professor Czermak's book, to which we must refer the reader for many other illustrations explanatory of the previous and succeeding remarks on the laryngoscope.

We take this opportunity to state, that the works of Battaille, Türck, and Semeleder all have the advantage of lithographic, xylographic, or coloured illustrations, which materially assist in the explanation of the facts noted. The lithotints in Dr. Semeleder's book are so well executed as to deserve special praise on account of their elegance and faithfulness.

The second case given by Dr. Czermak is one again illustrating the vagueness of our prevailing laryngeal pathology. In a gentleman supposed for some years to have laboured under "nervous" hoarseness, the speculum readily demonstrated the true pathological condition, and therefore indicated the treatment. After a considerable effort of the voice, the aphonia had come on, and had always been aggravated by emotion. It was discovered that a round growth projected from the middle of the right vocal cord, and as it was of soft consistence, it probably changed in volume under the influence of variations of the circulation. In the third case a scrofulous affection of the soft and hard palate resulted in the following conditions revealed by the laryngoscope; the epiglottis was found to be enormously enlarged, very much injected and ulcerated. The entrance to the larynx was so constricted as to prevent the greater part of the vocal cords, which were inflamed, from being seen. In the progress of the case, a second ulcer was seen to have formed, so as with the first to have caused a division of the epiglottis into a three-lobed body. This ulceration, and the swelling of the vocal cords increasing, laryngotomy became necessary, and by examining the vocal cords from below, in the manner previously described, further interesting facts were disclosed. A complete recovery was eventually obtained.

The character of the lesions inducing incurable aphonia was revealed by the speculum in the fourth case given by Czermak. Here, in a woman of sixty-eight, who suffered from aphonia supervening suddenly, as it was affirmed, upon drinking a cold beverage, there was loss of substance in the velum, with numerous pharyngeal cicatrices, regarded by the physicians as undoubtedly syphilitic.

"The epiglottis had disappeared, but there remained only a short stump, irregularly excavated, especially on the right side. Whatever the patient swallowed produced a choking sensation. The right superior vocal cord was considerably thickened, its surface irregular and rough. The surface of the left superior vocal cord was likewise rough, cicatrized, and had suffered besides a great loss of substance at its external border. Consequently on the left we could perceive a large portion of the inferior vocal cord relatively normal, whilst on the right the vocal cord was deeply excavated behind, and almost covered by the false vocal cord. The other parts of the mucous membrane, especially the right aryteno-epiglottic ligament, presented considerable loss of substance, and also numerous cicatrices."

It is easy to understand that such a condition of things was incompatible with a due formation of sound; in fact, it was seen, when the patient was desired to emit a sound, how the true and false vocal cords approached one another, only allowing the air to escape with a hissing noise.

In the fifth case we find retraction of the right vocal cord, with swelling of its fellow false vocal cord; the cause of aphonia is an old case of syphilis. A partial destruction of the epiglottis, with considerable swelling of the false vocal cords, was discovered to be the cause of hoarseness and aphonia in the following one. This was one in which the whole interior of the trachea was visible down to its very bifurcation.

In the seventh case there had been aphonia of eight months' duration, occurring in a gentleman of thirty-two, subsequent to severe hæmoptysis, which had been attributed to mental emotion and drinking cold water. The laryngoscope demonstrated constriction and insufficiency of the glottis; the constriction being due to swelling and infiltration of the mucous membrane covering the arytenoïd cartilages, by which their mobility was impaired, while the insufficiency was due partly to the limited mobility of the parts, and partly to irregular notches on the edges of the vocal cords of a greyish-white colour. Besides other treatment, caustic was applied directly under the guidance of the laryngoscope, and it was remarkable to observe in this and other cases where the application was made to the vocal cords themselves, how little irritability they manifested.

In the following (eighth) case, hoarseness was shown to depend upon active congestion of the laryngeal mucous membrane, while in the ninth a syphilitic ulcer was plainly discerned on the superior vocal cord close to the arytenoïd cartilage. We pass over the next two cases, which resemble the two last spoken of, and come to the twelfth, in which the negative evidence obtained by the laryngoscope is apparent. In a female, aged forty, who had been long suffering under increasing dyspnœa, with stridulous breathing, attributed to an obstacle seated in the larynx, ocular inspection showed the larynx to be perfectly normal, the glottis widely open, and the trachea entirely free. The remaining eight cases given by Professor Czermak, with the exception of the last, describe growths of various kinds and sizes discovered by the laryngoscope on or in the vicinity of the vocal cords. In one of these the galvanic cautery was applied with success, but we do not understand from the description whether it was used for the removal of the growths occurring within the larynx, or only for those occupying the pharynx. Although Professor Czermak speaks of this method of treating intra-laryngeal growths more than once, he does not favour us with a description of the process adopted. The last case given by the same author is one of œdema of the glottis following typhoïd in a male adult.

"The epiglottis, the aryteno-epiglottic ligaments, the superior and inferior vocal cords, were all considerably swollen by œdematous infiltration; all these parts were very pale, with the exception of the infected epiglottis, the right side of which was the seat of a vesicle filled with fluid. The glottis was reduced by the junction of the infiltrated and innominate inferior vocal cords, to a small roundish triangular space which existed between the arytenoïd processes. The superior vocal cords, as well as the aryteno-epiglottic ligaments, although infiltrated and swollen, counted as nothing in the constriction of the space reserved for the passage of air."

Laryngotomy was performed; the parts were thus allowed complete repose, and health was apparently restored.

We could readily multiply evidence of this new aid to practical medicine. What has been given will, we trust, convince any one who may hitherto have doubted the value of laryngoscopy that it is a real acquisition. Nor is it one entailing so many difficulties in regard to its application, that it need be confined to the specialist. On the contrary, with a few precautions, any person of ordinary dexterity may speedily acquire the tact necessary for the employment of the instrument. The beginner will do well to apply to some person already versed in the use of the instrument for those practical hints with regard to the introduction of the mirror, the position of the tongue, the relative position of the observer and the person operated upon, and similar minutiæ, which can at once be learnt by demonstration, but which to some may present mechanical difficulties sufficient to debar them from a prosecution of this interesting mode of studying laryngeal affections, if they are left to their own resources. With such aid we do not doubt that they will confirm the favourable opinion we have expressed regarding the comparative facility of employing the laryngoscope.

To those who are desirous of becoming more fully acquainted with the subject, we strongly recommend the study of the work from which we have chiefly culled our extracts, as well as those other essays on the subject which head this article. In these the reader will find numerous illustrations which will render the subject still more intelligible, and which we regret it has not been in our power to reproduce for his greater convenience.

Review VIII.

1. *On the Nature and Treatment of Diabetes.* By F. W. Pavy, M.D., Fellow of the Royal College of Physicians, Assistant-Physician to, and Lecturer on Physiology at, Guy's Hospital.
2. *Studien über Diabetes.* Von W. Griesinger. ('Archiv für Physiologische Heilkunde,' 1859, 1860.)

Researches on Diabetes. By W. Griesinger.

3. *Lehrbuch der Physiologischen Chemie.* Von Dr. E. F. Von Gorup-Besanez.—*Braunschweig*, 1862.

Textbook of Physiological Chemistry. By Dr. E. F. Von Gorup-Besanez.—*Brunswick*, 1862.

4. *Untersuchungen über die Zuckerbildung in der Leber, und den Einfluss des Nerven-Systems auf die Erzeugung des Diabetes.* Von J. M. Schiff aus Frankfort, Professor in Bern.

Investigations on the Formation of Sugar in the Liver, and the Influence of the Nerve-System on the Production of Diabetes. By J. M. Schiff, M.A. of Frankfort, Professor at Bern.

5. *On Sugar in the Urine.* By H. Bence Jones, M.D., F.R.S. ('Quarterly Journal of the Chemical Society,' April, 1861.)
6. *On Sugar in Urine.* By E. Schunk, F.R.S. ('Philosophical Magazine,' March, 1862.)
7. *On the Saccharine Function of the Liver.* By George Harley, M.D. ('Proceedings of Royal Society,' Feb. 1860.)
8. *On the Formation of Sugar and Amyloïd Substance in the Animal Economy.* By Dr. Robert McDonnell. ('Proceedings Royal Irish Academy,' Feb. 13, 1860.)
9. *On the Phenomena of Diabetes Mellitus.* By the Rev. Samuel Haughton, M.A., F.R.S., Fellow of Trinity College, Dublin. ('Dublin Quarterly Journal,' vols. xxxi. and xxxii.)
10. *On the Occurrence of Glycosuria in Cases of Burn.* By William Robinson Hill, M.D. Edinburgh, Physician to the Eastern Dispensary, Bath. (Beale's 'Archives of Medicine,' No. vii. p. 172.)
11. *On the Clinical Examination of the Urine.* By Dr. Roberts. ('Lancet,' May 10, 17, 24, 1862.)
12. *The Synthesis of Cataract.* By Benjamin W. Richardson, M.A., M.D. ('Brown-Séquard's Journal de la Physiologie,' xi. and xii.)
13. *De la Cataracte Diabétique.* Par le Dr. Lecorché. ('Archives Gén. de Médecine,' Mai, Juin, Juillet.)—*Paris*, 1861.

On Diabetic Cataract. By Dr. Lecorché.

14. *Physiologie der Nahrungsmittel. Ein Handbuch der Diætitik.* Von Jacques Moleschott.—*Giessen*, 1859.

The Physiology of Food. A Handbook of Dietetics. By Jacques Moleschott.—*Giessen*, 1859.

Somewhat heterogeneous substances pass by the common name of "Sugars;" but differ widely as they may, they are yet not so various as the interests their common name appeals to. Statesmen and philanthropists have come to speak of sugar as the second necessary of life;

to the physiologist and physician, it is one of the widest subjects with which their respective sciences can deal. The question of favouring or discouraging the production of sugar in slave islands must be left to the consciences of ministers of state; and the uses to which sugar is put in the arts would, like its political import, be foreign to the pages of this Journal; but the purposes it serves in the individual animal economy, the modifications it there undergoes, the *rationale* of its production within the body, and our means for holding that process in check, fairly fall within our province.

To the uses of sugar the instinct of the million speaks in unmistakeably clear language. The net revenue from the now equalized sugar duties amounted in 1861 to no less than six millions; for home consumption by the thirty million inhabitants of the United Kingdom, 3,790,776 cwts. were imported.* In round numbers, each individual in these islands consumes annually 14 lbs. of sugar.

The experiments of Messrs. Lawes and Gilbert, recorded in the British Association 'Reports' for 1852 and 1854; and in the 'Philosophical Transactions for 1859,' inform us of the purposes to which this large quantity of imported cane-sugar is put in our animal system. Their careful and colossal investigations have shown that an animal may accumulate from four to five times as much fat in its body as it consumes of fatty matters ready formed in the rations issued to it; and though they do not deny that fat may be formed in the system from the nitrogenized principles of our food by the process called by the Germans "*Deamidisirung*," they have established, and unassailably, the position that the main source of the fat formed is the carbohydrate, starch and sugar series. It is true that out of sugar, art and man's devices in the laboratory have as yet been unable to build up compounds more complex than glycerine, as has recently been done by Pasteur, or butyric acid and fusel oil, as has long been a familiar fact in the history of fermentation; but the bee has always had the power of making wax out of simple sugar, though it is provided, so far as we know, with no special glandular apparatus for thus effecting what transcends the utmost powers of artificial chemical synthesis. And the slaves, whose labour, according to the returns already referred to, enters into the production of nearly, if not quite, half the sugar consumed in these islands, accumulate, as is well known, large amounts of fat upon their bodies whilst the sugar harvest is going on, free license being given by their masters and full indulgence by themselves to their excessive appetite for the substance they manufacture. Moleschott, in the invaluable work on Dietetics the title of which stands in the heading of this article, when speaking of the formation of fat, simply re-

* These figures we have taken from a return on sugar ordered by the House of Commons to be printed June 12th, 1862, Nos. 315 and 316. Mr. Wentworth Lascelles Scott, in a paper read before the Society of Arts, Feb. 1st, 1861, has put the quantity per head of our population as high as $32\frac{1}{4}$lbs. of the raw variety, besides between 13 and 14 oz. of the refined; and Prof. Moleschott, op. cit., p. 365, gives much the same estimate upon the authority of Payen, adding that the quantity consumed per head in France is less than one-third of that consumed in Great Britain, being 3·333 kilogrammes as against 16.

marks that the deoxidizing process, which is begun by the vegetable kingdom, is carried on and out in the animal economy, as if it were now allowed on all hands that the formation of those complex molecules known as fats, was not the exclusive prerogative of the vegetable world. With the remark that thus one more of those sharp lines of distinction between the great kingdoms in the organic world, in which man delights, but which Nature abhors, has been swept away, we may pass on to enumerate certain other uses which sugar subserves in the dietary of animal life.

Modern physiology teaches us to look for these accessory functions besides and beyond the primary end of forming and being deposited as tissue, in one or other or both of the two directions of aid in the transformation of food, or of increase of vital action in the system generally. We assimilate food, and lay it up as tissue; we rid our system of this self-same tissue, as it grows old and effete, or as we wear it out in the using; and for the carrying out of both of these operations help is needed, and help is furnished to us by our different kinds of aliment. Of the power which sugar has to increase the evolution of carbonic acid—our surest measure, perhaps, of the activity with which these processes are being carried on—we find Dr. E. Smith speaking thus:* "Sugar in every form is a powerful respiratory excitant;" and a few pages further on he classes sugar and tea together as being simply the most powerful agents of the kind. The exceeding rapidity with which an increase in the evolution of carbonic acid takes place after the ingestion of sugar, seems to point to increased vital action in the tissues as the *rationale* of such evolution; and the use of eau sucrée by foreigners in much the same way and for the same purpose as tea is sipped by its votaries in all countries, may serve as a popular illustration of their community of action as revealed by science. Nor is evidence wanting to show that sugar can promote the transformation and assimilation of food as well as other vital processes. In the 'Journal of the Society of Arts' (Jan. 14, 1859), Mr. W. Bridges Adams says:

"I know by experience the difference in nutritious effect produced by the flesh of tired cattle on a march and those slain in a condition arising from abundant food and healthy exercise. In the former case any amount might be eaten without the satisfaction of hunger, whilst in the latter a smaller amount removed hunger. But I discovered that certain other food of a different quality, such as grape-sugar and fruit, would help the tired meat to assimilate, and thus to remove hunger."

Persons, again, who are familiar with the habits of artisans, have told us that even the more self-denying and temperate members of the class spend what appears at first sight a disproportionately large amount of their weekly wages in confectionery, and that when remonstrated with, these persons will say they must have something corresponding to the various sweet-pudding and tart luxuries of their mentors, if they are to get the full good out of their food, and resist at the same time the temptations of the beer and spirit shop. Patients just discharged from a hospital, on exchanging its plentiful fare for the coarse

* Royal Society's Transactions, 1859, pp. 723, 739.

and scanty food of poverty, will often, when they dare, ask for sweets and preserves in preference to what is commonly held to be good and nourishing food; nor is it from caprice or from being spoilt or pampered, as is sometimes ignorantly and harshly hinted, but from an unerring physiological instinct that they do this. The wealthy classes in England may need, as these stories show, to be reminded of the importance of saccharine articles of food in our dietaries. Other nations have them, so strangely, as it seems to us, mingled up on their tables with other articles of aliment, that they cannot ignore the need there is for having them yet more intimately intermixed within their stomachs. That robust personage, *der fleischfressender Engländer* of the German physiologists, would deny, with more or less vehemence of asseveration, that he "ever touched sweets," and he would, as we have heard him, scoff at the notion of helping a convalescent by giving him or her "lollipops;" but all this while every glass of port he drinks may be helping him towards assimilating his food with as much as half a drachm of sugar.

We will now proceed to enumerate the changes which sugar undergoes, firstly, in the digestive tract, and, secondly, after absorption from it. For our knowledge of the first of these two series of metamorphoses we are indebted to a plurality of physiologists; for our knowledge of the second we have to thank Dr. Pavy alone.

The different products into which the sugar which escapes absorption as such is broken up in the intestinal canal, are to be found enumerated in the ordinary handbooks; but they have never been put forth with such clearness as in Moleschott's work on dietetics, already referred to. The recently appointed Professor of Physiology at Turin (op. cit. p. 66), after stating, as also does Gorup-Besanez (op. cit. pp. 758–760, 762), that the exact agency by which cane is made to pass into grape-sugar* is not quite clearly made out, sums up the results of the investigations alluded to in the following words:

"The particular stages of the digestion of the fat-producing substances (starch, sugar, dextrine, cellulose) may be made intelligible by the following formulæ: First of all, starch, or, more correctly, the intermediate stage between starch and sugar, viz., dextrine, takes up two equivalents of water so as to be metamorphosed into sugar—

$$\begin{array}{ccc} \text{Dextrine} & & = \text{Grape-sugar} \\ C_{12}H_{10}O_{10} + 2\,HO & & = C_{12}H_{12}O_{12} \end{array}$$

* It is somewhat difficult to understand how this statement can thus be repeated by different authors of repute, when the following experiment is of such easy performance and such conclusive meaning:—A solution of ordinary loaf-sugar will not decolorize the copper-test solution, or give any precipitate, save possibly, owing to some small quantity of it having passed into grape sugar, a little red anhydrous suboxide, which does not separate instantly, and which, when it does separate, gravitates at once to the bottom of the tube, where it forms a bright Cayenne pepper-like deposit, leaving the entire supernatant mass of fluid as blue as before. Now this self-same solution of cane-sugar, if it be mixed with saliva and then be tested again, will instantly throw down the well-known yellow precipitate which fills the entire test-fluid, and wholly destroys the blue colour; showing that saliva, at all events, is one agent able by itself to effect the same transformation in cane sugar which we all know it brings about in cooked starch.

"This is the stage of sugar production which may go on throughout the entire digestive tract from mouth to rectum. Then comes the splitting up of sugar into lactic acid and water,

$$\text{Sugar} = \text{Hydrated lactic acid},$$
$$C_{12}H_{12}O_{12} = C_{12}H_{10}O_{10} + 2\,HO,$$

which makes up the second stage, and is carried on to a certain extent in the stomach, but pre-eminently in the intestine, and has its special development localized in the cæcum. The third stage corresponds to the splitting up of the lactic acid into butyric acid, carbonic acid, and hydrogen,

$$\text{Hydrated lactic acid} = \text{Hydrated butyric acid} + 4\,H + 4\,CO_2,$$
$$C_{12}H_{10}O_{10} + 2\,HO = C_8H_7O_3 + HO. \quad + 4\,H + 4\,CO_2,$$

and it is in the large intestine especially that this process goes on.

"Now, as butyric acid belongs to the fatty acids, this history of its production justifies us in giving the name of fat producers to all the substances which can be changed by digestion into sugar. Here, unfortunately, the clue to the gradually progressing developmental changes slips from our hands, and as yet we are ignorant how and when the butyric acid becomes changed into the fats so much poorer in oxygen and richer in carbon than it is itself."

The German metaphysician, Brandis, was in the habit of pointing out to his class, that, in spite of the ordinary belief that the English were a practical people merely, and incompetent to philosophical speculation, nevertheless most of the great movements of mental philosophy had owed their rise to British teaching. It is gratifying to the English physiologist to be able to point to the names of his fellow countrymen as standing in a similar relation to the inauguration of fresh truths and the sweeping away of established error in the sciences with which he is conversant.

We cannot but think that Dr. Pavy's name will take its place in this list, and that his researches on sugar will be spoken of hereafter as those of Willis, though now nearly two hundred years old—or those of Cowley, which are nearly one hundred—are still. Our knowledge of the changes which sugar, when taken as food, undergoes in the liver, is entirely due to his researches. From them we learn that just as starch becomes sugar in the vegetable economy before it can be carried over the entire system of the plant, and becomes again insoluble as cellulose and lignine, so starch in the animal digestive tract becomes sugar, and soluble, but as a preparatory stage to becoming insoluble again as amyloïd, or animal liver-starch, within the liver. Further researches are needed and promised (op. cit. p. 67) by Dr. Pavy, as to the final disposal of the amyloïd thus laid up in the liver; for the present, he hints that it will be found entering both into the formation of bile and the production of fat. Dr. Pavy (op. cit., p. 29, seq.), in writing upon this subject, gives us an account of the observations and the experiments which the observations suggested; and it seems that the enormous size which the liver assumed after a vegetable diet was the first phenomenon of the series which excited his attention. The liver of the dog fed upon animal food exclusively is to its body as 1 : 32; of a similar animal fed upon vegetable food exclusively, 1 : 16. Other physical changes besides these alterations of relative weight were to be found in the livers of animals fed on sugar or other carbohydrates; they became paler in colour, less firm in consistence, less resistant to pressure; and, what is of some significance, these changes were imitated on a smaller scale in the Peyerian and solitary glands of the intestine.

The second step in the series of discoveries was, that the increase in the size of the liver was mainly due to increase in that one of its physiological constituents which is known under the different names of "glycogen," "hepatine," "amyloïd," "liver starch." A full account of this substance will be found in No. liii. of this Journal, Jan. 1861, p. 63. It was discovered independently, and well nigh simultaneously, by Bernard, Hensen, and Pavy; and glycogen, the name proposed for it by the first of these investigators, as denoting, on the one hand, its origin from, and on the other, its tendency to revert to, sugar, would be an exceedingly appropriate one for it, had it not become known that, whilst it may owe its origin to the splitting up of albuminoïds, its tendency to revert into the soluble form most usual to carbohydrates —viz., grape sugar—is, during healthy life, at all events, held in check. Amyloïd, then—for that is the *name* employed by Dr. Pavy, who has taught us more about the *thing* than has any one else—has been found by him to make up 7·19 per cent. of an animal's liver whilst it is confined to flesh diet, and to make up as much as one quarter of the entire weight of the organ when the animal has been kept for several days to a diet of bread and potatoes. On a mixed diet of animal food and sugar, the latter substance being administered to dogs in the shape of sausages, so as to secure its being swallowed, the relation of amyloïd to liver was, as would be expected, a little lower than that held by it upon a purely vegetable diet, though twice as high as that upon a purely animal one. So far as this, then, we have traced sugar ingested as aliment; and we have seen that after absorption it undergoes a process the reverse of that which it underwent itself in being transformed out of insoluble starch. So far as to the point at which it reassumes an amyloïd condition we can follow it surely; its further disposal is as yet not a matter of such positive certainty. At what it will probably be found to be, we will proceed to hint before we speak of what it has demonstrably been shown not to be. The single solid substance, amyloïd, is henceforward our *terminus a quo;* to it, therefore, and not to sugar ingested as aliments, which is only one of the sources whence it may be elaborated, we shall direct our attention.

As to the final disposal of the amyloïd substance, we think Dr. Pavy has scarcely given sufficient weight to the suggestions of a positive conclusion which his interrogations have already evoked from Nature. At p. 69 of his recently published work on diabetes, Dr. Pavy says:

"That there *must* be some other mode of transformation to which amyloïd substance is susceptible in the liver, besides conversion into sugar, is proved by the results I have obtained after the introduction of the carbonate of soda into the portal system during life. A solution of carbonate of soda has been injected into one of the branches of the portal vein, and in a few minutes even has created a total disappearance of amyloïd substance from the liver without any evidence of the production of sugar."

This self-same carbonate of soda Dr. Pavy has elsewhere* shown to possess the power of largely increasing—in fact, of doubling—the percentage of fat within the liver; and, but that facts in therapeutics are something much less stable than facts in physiology, we might couple its reputed cholagogue properties† with this its power of producing fat, albeit the two lines of working have been usually held to be mutually antagonistic. Meanwhile, we wait for the fuller proofs which Dr. Pavy promises us of the connexion which may subsist between the production of fat, the secretion of bile, and the disappearance of amyloïd from the hepatic cells.

Leaving now the consideration of probabilities, and the anticipation of positive conclusions, we will proceed to give an account of the negative results at which Dr. Pavy has arrived as to the final disposal and purpose of this amyloïd substance. The positive conclusion at which Bernard had arrived the name glycogen was intended to denote; and that amyloïd is glycogenic, that it can be transformed, and that with the very greatest rapidity, into sugar, no one can deny; the question is as to the how and when. And Dr. Pavy must, we think, be held to have demonstrated that this transformation is a post-mortem and pathological, not an ante-mortem and physiological process. The glycogenic theory rested upon the assumption that the condition of the blood and of the liver after death was the same as during life; and this assumption, by catheterism of the right ventricle, and by a more complicated but not less convincing process for the liver, Dr. Pavy has proved to be erroneous, both as regards the blood and the hepatic cells. The results of Dr. Pavy's experiments we will give in his own words, premising, firstly, that by the phrase "Trace of sugar," he means such quantities as the fractions $\frac{47}{1000}$, $\frac{58}{1000}$, $\frac{73}{1000}$, of a grain represent; and secondly, that muscular disturbance and respiratory embarrassment supervening during the operation, may cause these infinitesimal fractions to assume the dimensions of integrals, and destroy the value of the experiment.

"The following quantitative determinations conducted upon blood derived from the dog show that, although there may exist but a trace of sugar in the blood during life, yet, that sugar may be found to a notable extent in the blood derived from an ordinarily conducted examination of the same animal after death. We have here a proof of the difference I have insisted upon, between the blood belonging naturally to life, and that removed ordinarily after death, a difference, as far as I am aware, that has never been alluded to by others. Looking to the column showing the results of analysis of the blood removed after death, it will be observed that in the five instances, the proportion of sugar varied from half a grain to nearly one grain per cent.: there having been only a trace of sugar in the arterial and right ventricular blood collected during life. Four out of the five livers belonging to the animals were submitted to analysis, and, as shown, were richly charged with sugar:

* Guy's Hospital Reports, 1861.
† Bidder und Schmidt: Die Verdauungsaefte, p. 397.

	Blood from the carotid artery, and likewise from the right ventricle during life.	Blood (defibrinated) from the right side of the heart after death, per cent.	Liver a short time after death, per cent.
No. 1.	Trace of sugar.	$\frac{7}{10}$ths gr. of sugar.	Not analysed.
No. 2.	Trace of sugar.	$\frac{65}{100}$ths gr. of sugar.	4·10 grs. of sugar.
No. 3.	Trace of sugar.	$\frac{5}{10}$ths gr. of sugar.	3·39 grs. of sugar.
No. 4.	Trace of sugar.	$\frac{94}{100}$ths gr. of sugar.	2·45 grs. of sugar.
No. 5.	Trace of sugar.	$\frac{7}{10}$ths gr. of sugar.	2·44 grs. of sugar."*

To the existence of similar quantities of sugar in the blood during life, and that life healthy life, Dr. Harley, in the 'Proceedings of the Royal Society for Feb. 2, 1860,' and Dr. Robert McDonnell, in the 'Proceedings of the Royal Irish Academy for Feb. 13, 1860,' bear their evidence also.

Dr. Harley's experiments, indeed, as recorded in the pages of this Journal, July, 1857, by himself, have had nothing added to them in the 'Royal Society's Proceedings for 1860,' so far as the blood is concerned. Nothing has occurred to shake his conviction as expressed in 1857, that healthy arterial blood may contain as much as from ·18 to ·24 per cent. of sugar, and that little, if indeed any, more sugar is to be found in the right side of the heart than there is at the same time in any one of the systemic arteries. Four several experiments upon animals immediately after death—meaning by "immediately" such an interval of time as twenty seconds—have enabled Dr. Harley to confirm Bernard's statements as to the absence of sugar from the portal vein, at a time when saccharine impregnation to the extent of as much as ·333 per cent. may be detected in the hepatic blood. But the saccharine impregnation of a liver left for some hours after death in the body of the animal was found to rise from ·333 per cent. up to 1·55 per cent.—i.e., to be all but quintupled. Drs. Pavy and Harley are, in fact, well nigh in complete accordance the one with the other; both alike hold that if we find a great quantity of sugar in the blood, we must look upon it as either a post-mortem or a pathological product, but that an infinitesimal quantity is normally present there during life, and that the seat of this scanty glycogenesis is the hepatic tissue.

Dr. Robert McDonnell's account of his own experiments is especially valuable, as he employed catheterism of the heart in the living animal as his method, one less likely to lead to fallacy than the speediest possible process for extinguishing life. He says:

"In making experiments on the tissue of the liver immediately after death, no matter what rapidity, precision, and care are exercised, it must be confessed that results are met with which seem contradictory. However, the object being to ascertain the condition of the hepatic blood during life, I have had recourse to catheterism of the right side of the heart—an operation which, in the hands of others, has given results corresponding with those to which I now allude.

"1st. In twelve experiments made on dogs, for some weeks before fed exclusively on meat, traces of sugar were found in the blood of the right side of the heart in five; there was no sugar discoverable in the blood of the remaining seven.

* Op. cit., p. 46.

"2nd. In four rabbits fed on boiled eggs, meat, and butter, for some days, no sugar was detected in the blood drawn from the right side of the heart.

"3rd. In three dogs fed on mixed diet, and three rabbits fed on carrots, potatoes, &c., sugar was found in the blood of the right side of the heart, and in equal quantity in the blood taken from the carotid.

"4th. In three rabbits fed on vegetables, sugar was found in the blood of the right side of the heart drawn during life; but double, and in one instance more than treble, the amount was found in the blood sucked from the same locality after the animals were killed (one by pithing, two by hydrocyanic acid). Hence one seems in some degree justified in concluding that in vegetable-eating animals the food is normally saccharine, but that the liver does not appear during life to form and pour out into the blood of the hepatic vessel sugar specially derived from the transformation of the amyloïd substance into that material." (pp. 137–8.)

Sugar, it is well known from the experiments of Von Becker,* cannot rise in the blood to a higher per-centage than ·5 per cent. without giving rise to diabetes, but there seems to be some show of reason for holding that smaller quantities of sugar than this may be broken up in the blood itself into simpler compounds, under the influence of oxygen and alkalinity. As the correctness of this physiological theory may influence such a therapeutical process as that of the alkaline treatment in diabetes, we will give it in the words of Gorup-Besanez, who has furnished us, in his excellent 'Lehrbuch,' and elsewhere,† with the best line of evidence in support of it. That writer (p. 204, op. cit.) says:

"The view holding the possibility of sugar being directly oxidized in the blood, is favoured by the fact observed by me, that grape-sugar, like cane-sugar, passes in weak alkaline solutions, constantly, though slowly, when acted on by ozone, into carbonic and formic acids, and that in this experiment the production of other intermediate products is never to be observed. Sugar, in its relations to ozone, belongs to a series of substances which in neutral solutions are not distinctly acted on by it, but in alkaline go through a kind of process of decay, that is to say, speaking generally, are attacked by it very slowly, so that a part of it is always left undecomposed, whilst another seems already completely oxidated. This process, however, after all, is not so slow that an argument can be drawn from the time it takes against the existence of an analogous process in the blood."

As in the blood, so in the urine of health, sugar is to be detected in infinitesimal quantities. Professor Brücke, of Vienna (whose convincing researches on the causes of the blood's coagulation appeared in this Journal in the year 1857), Dr. Bence Jones, and Dr. Schunk have recently brought evidence to prove this, based on processes which only masters in the art of chemistry could undertake, and only well-grounded scholars understand.

Some of Professor Brücke's results have been called in question in an Inaugural Dissertation recently read at the University of Dorpat by N. Iwanoff, who, whilst allowing the occasional appearance of sugar in healthy urine, demurs, nevertheless, to the uniformity or constancy of such a phenomenon. Dr. Bence Jones's paper, contained in the

* Zeitschrift für wiss. Zoologie, Band v. p. 176, 1853.

† Annalen der Chemie und Pharmacie, Band cx. p. 86, 1859.

'Transactions of the Chemical Society for April, 1861,' gives an account of two sets of experiments, in the first of which sugar was added to, and in the second sought for, in healthy urine. The scientific value of each set of experiments is equal, but in neither will the report of the processes adopted admit of being abridged. The practical bearing, however, and the actual results of the second set of researches, we will put before our readers in Dr. Bence Jones's own words:

"The presence or absence of sugar in healthy urine is not only of great interest in relation to the true comprehension of the nature of diabetes, but it is also of importance in respect to our knowledge of the chemical changes which occur in the body in health. If sugar exists in the urine in health, as Brücke maintains, then diabetes must be considered as an exaggeration of a healthy state, and not as a distinct and peculiar condition of the system; and it will be necessary to admit that in health and in diabetes the same chemical changes take place in the system, but that the greater amount of change in the one case constitutes health, and the lesser amount in the other case is called diabetes."

We must anticipate somewhat, and say that Herr Griesinger, of whose views we shall later give a detailed account, would agree in all of this, but would transpose in the last sentence the words "greater" and "lesser." Summing up his results, Dr. Bence Jones says:

"These experiments therefore fully confirm Professor Brücke's statement, that sugar exists in healthy urine. By obtaining alcohol from the fermented fluid, and by never failing to find rotation, provided sufficient" (one litre *ad minimum*) "urine had been taken for the experiments, I have added to the evidence given in his original paper."

Dr. Schunk has succeeded by the aid of acids in breaking up the three several extractive matters which he has isolated in the urine into nitrogenized resinoïd bodies, on the one hand, and into a species of glucose, on the other; and he hints that the glycosuria which Dr. Pavy has shown may be produced by the injection of phosphoric acid into the circulation, may possibly be due to an analogous working of that acid upon these bodies. The composition of these extractive matters is such, that in the case of two of them, sugar and acetic acid, and in the case of the third, sugar and formic acid, coupled in either case with hippuric acid—an acid well known to exist in healthy urine, and found by some to abound in the urine of diabetics—may be taken to represent the compounds into which they may be easily split. This analysis of Dr. Schunk's is in wonderfully close accordance with the theoretical views which have been held as to sugar and hippuric acid being two ordinary results of the disruption of those compound molecules the albuminates; and if we couple the analysis and the theory, on the one hand, with the fact shown by Kühne and Hallwachs,* that the liver is the seat of the production of hippuric acid, and on the other with the asserted phenomenon of increase of hippuric acid in diabetes, we get together an aggregate of facts and hypotheses which harmonize with each other and with much of the pathology of

* Nachr. d. k. Gesellschaft der w. zu Göttingen, 1857.

the disease just mentioned so well as to win for themselves much of the appearance of truth. That there is a second possible, and indeed actual, source for the production of glycogen and glycose—the nitrogenized principles, namely, of the animal body—is brought forcibly home to us by these discoveries of Dr. Schunk; and the present may be a fitting place for glancing at the other arguments which may be brought forward in proof of the double origin of these carbohydrates. To this double origin we have the double testimony of physiology and pathology. We know, indeed, from the experiments of Professor Bernard and of others, that the livers of animals confined for some time strictly to flesh diet, still laid up amyloïd and secreted sugar, but Dr. Robert McDonnell has recently made these experiments even more complete and convincing by showing that amyloïd is not to be extracted from the meat itself upon which such animals are fed, and that consequently the objection that the carnivore really gets his liver-starch ready-made in the food he takes, falls to the ground. Dr. Pavy, it is true, has succeeded in obtaining an amyloïd substance from the lungs and from muscular tissue, though he has failed in obtaining it from blood, kidney, or spleen, but he seems to think that the small quantity obtainable from such sources is by no means sufficient to account even for the 7·19 per cent. of amyloïd which the liver of a dog kept to animal food is found to contain. It has long been known that the incubated egg contains sugar, which the freshly-laid egg does not, and Dr. McDonnell has recently found that cats, as well as other carnivora, when fed exclusively on meat for days, still continue to form milk containing sugar. From a purely chemical point of view the question presents no difficulty—indeed, as the following passage from Gorup-Besanez will show, there is even a third source possible besides the starch and albuminoïd principles already spoken of.

"If one considers," says Gorup-Besanez, when writing of the production of grape-sugar in the animal body (p. 203), "that a portion of the albuminates of the blood, according to the researches of Lehmann, disappear in the liver, and that according to all that we know of the constitution of albuminates, they must be looked upon as coupled compounds, one of the constituent elements of which is a carbohydrate, it will appear, to say the least, not improbable that the sugar which is produced in the liver is split off the albuminate principles. We would not, however, hereby wish to exclude the possibility of this sugar originating from the glycerine of the fats of the liver—a possibility which must be kept the more in view since Berthelot has obtained fermentescible sugar from glycerin, and Pasteur has observed this very product to originate in the alcoholic fermentation of sugar."

It is unnecessary to remark that Gorup-Besanez is entirely unacquainted with Dr. Pavy's discovery that the sugar ingested into the stomach is one of the chief sources of the starch-like glycogen of the liver.

No pathological argument in support of the double origin of diabetic sugar is needed by the physician who has observed the extreme emaciation of diabetic patients, who is aware that an excessive increase in the absolute amount of urea daily excreted is one of the phenomena of the disease, and who knows that urea is but one of several fragments into which such complex molecules as the albuminates can be broken up, and that these other fragments contain all and more than all the elements necessary for the constitution of such simple compounds as glycogen and glycose. The argument from the pathological side has never been stated better and more clearly than by the Rev. S. Haughton, op. cit. p. 32, 33; and to his interesting paper we have much pleasure in referring our readers.

Before leaving this part of our subject, we would remark that the changes which normal urine frequently, (as diabetic urine pretty nearly always, and to a much greater extent,) undergoes in what has been called the acid fermentation, bear clear testimony to the existence of a saccharine substance in the healthy secretion. The very visible deposit of urates is precipitated, not because the fluid has cooled, but because its acidity has been intensified by the development in it of lactic acid. Vigorous health and active exercise, as well as states of life resembling them in nothing save increased waste of tissue, will be found to produce urine which runs readily into hyperacidity. The products of wasting tissues are thus seen to be linked with substances of the carbohydrate type in a series of phenomena standing midway betwixt those which the micro-chemistry of Dr. Schunk has detected as being purely normal, and those which the disease diabetes presents to us all too obtrusively on the great scale.

Sugar, then, may take its origin from two different classes of substances within the animal economy, resembling herein so many other physiological, so many other pathological products; that it is not produced in the great quantities supposed by Professor Bernard during healthy physiological life, it is Dr. Pavy's great merit to have, as we think, demonstrated; and that it is produced in infinitesimal quantities during such life, it is one of the feats of micro-chemistry to convince us. This infinitesimal glycogenesis may be exasperated into diabetes, and this either by art and man's device in experiment, or by the more obscure, less easily traceable hand of disease. And we have yet to speak of it as brought about in experiment, as supervening in disease, and, thirdly, as affected by therapeutical agents.

A diagram, such as that which Mr. Darwin has given us in illustration of the kinship of species, would be very serviceable in illustration of the true relationship subsisting between the results of experiment and the generalizations of pathology. The two series may start, it is true, from one common point; but they may be leaving it only to continue ever after diverging from it. The limited range of our eyesight may prevent us from seeing the direction they are really taking; and errors analogous to those of Lamarck in classification, may enter into and taint pathological science. We do not look upon experiment as premises from which all pathology must necessarily flow, any more than we regard less highly-developed and specialized zoological types as for that reason necessarily the genealogical predecessors of the more highly developed and perfected. The two sets of phenomena are not related as rungs in a ladder, or links in a chain; it is *reticulatim*, not *catenatim*, that they are apposed and connected.

The truth we have been trying to express in the language, and illustrate by the history, of zoological science, was expressed some years ago by Professor Savory in the short phrase:—"In experiments nature is often but coarsely caricatured." The criticism this metaphor carries with it will be felt to be exceedingly applicable to much of the experimentation we shall now proceed to pass in review. These experiments fall into three classes, of which the third is the freest from fallacy. The first set of experiments are directed to the nervous system; the second disturb the mechanical and hydraulic balance of the circulation; the third, giving perhaps a coarse caricature, yet such a one as, we think, may enable us to recognise an object, alter, either directly or mediately through the digestive system, the chemical composition of the nutrient fluids.

Many other nerve lesions have been found to produce diabetes since the one first discovered by Professor Bernard to have that effect. How they produce it is not as yet definitely settled. Dr. Pavy says (op. cit. p. 91) that he does not now believe, as he did formerly, that the diabetes thus produced depends on a simple interruption to the transmission of nervous force between the medulla oblongata and the liver. Professor Schiff, who discusses this question at considerable length (op. cit. pp. 90–110), concludes by stating it as his opinion that the liver hyperæmia which he supposes to precede a saccharine state of the blood and urine, is at the beginning of the experiment an expression of nerve-irritation, and at the conclusion the result of paralytic dilatation of the bloodvessels.

The following passage will be found to contain his views, and an experiment strong in confirmation of them:

"We can distinguish the diabetes due to irritation, as I have done in the rabbit, from that which is due to paralysis; but a somewhat severe operation is necessary to show this. You must lay the interval between the sixth and seventh cervical vertebræ bare, which can be done with little loss of blood. Then you must divide the membranes and destroy the posterior strands of the cord. Usually the abundant sugar secretion in the urine ceases in from five to six hours.

"When the diabetes from irritation has gone off, then any further destruction of the posterior strands remains always without any effect. But if the rest of the cord is cut through, the diabetes of paralysis is set up, which remains unaltered up to death."

But some pages further on (p. 123) we find him saying:

"Just what in the lungs is neuro-paralytic hyperæmia, that in the liver is increased secretion of sugar; it is in either case the result of paralysis of vessels whilst the heart's action continues. We must ascribe the phenomena observed by Bernard to this paralysis, and not to an excitation of the *energie vitale.*"

Dr. Pavy, on the other hand, failed to make the urine saccharine either by division of the cord itself, or by division of it, together with the vagi; but we are not told that he took measures for protecting the animals thus operated on against that loss of heat which, occurring in lower animals after this operation, his own experiments, as well as those of Bernard and of Schiff, have shown to cause the disappearance alike of sugar and of glycogen. By adopting certain precautions against cold, Schiff has kept rats, with their cord destroyed over a space as broad as that protected by a vertebra or two, at a point corresponding with their first thoracic or lowest cervical vertebræ, alive and diabetic both, for as long as twenty days (op. cit. p. 109). Rabbits and guinea-pigs were found less able to resist the violence of the operation, and died, the first within ten, the latter within five days. Men, after accidents analogous to the severance of the cord, are protected from loss of heat; and in three several cases of fracture of the upper dorsal vertebræ, Professor Schiff has thrice observed diabetes to supervene in company with albuminuria. How long the diabetes lasted we are not told; the cases of the animals instanced above seem to show that diabetes of a somewhat enduring character may be set up by nerve lesion; with what difficulty, however, the following quaint sentences from Schiff will show: "I do not give count of the dead bodies which fell as victims, but where life and strength held out, the albuminous urine was constantly and abundantly loaded with sugar." The temporary character of the diabetes set up by the other nerve-lesion experiments detracts much from their physiological and therapeutical value; a nerve lesion which produces a diabetes lasting for four days in a frog, and for but as many hours in a mammal, cannot be held to be a very successful imitation of the pathological lesion which the physician battles with for month after month. This drawback both Dr. Pavy and Professor Schiff put most plainly and honestly forward.

Of experiments modifying the liver and its circulations mechanically, none can be more simple than those recorded by Professor Schiff, as performed by himself (op. cit. p. 126) upon rabbits. He passed long needles through the body walls, and having moved them to and fro in the substance of the gland when thus introduced into it, he left them there in two instances for a few minutes, and in a third for an hour and a half. The animals showed scarcely any outward signs of being inconvenienced by this procedure, but all alike, and the third most markedly, became diabetic. In like manner, and working by the setting of the amyloïd free into the circulation after its escape from ruptured hepatic cells, the kick of a horse has been observed by Professor Bernard* to produce a temporary diabetes in the human subject. Over-filling of the blood-vascular system of the liver may be effected in two ways, either by increased injection of the portal, or retarded efflux by the hepatic veins. In both cases the outflowing current has been found to be impregnated with sugar. If we introduce a syringe-pipe into the portal vein of an animal just killed, and turn a stream of water through it, and collect the mixed fluid which flows out of the vena cava inferior, a vessel of much greater relative length in the lower animals than in ourselves, and consequently easy to be secured, we shall find that the first washings which come over will contain a large quantity of blood. But as the injection of the liver proceeds, the blood impregnation of the outflowing stream becomes less and less, and at

* Léçons, p. 346. Paris, 1855.

last the liver itself assumes a pellucid, jelly-like, quivering appearance, whilst the fluid issuing out under a pressure thus so plainly testified to by the naked eye appearance of the gland, will be found to contain not sugar, but that variety of starch which runs with such rapidity into it as to be called glycogen. The washings may contain this starch-like glycogen in such abundance as to be milkily opalescent, but under any circumstances of health, if they have blood or saliva added to them, the copper test, previously silent, speaks instantly to the presence of sugar, and to the possibility of producing a saccharine state of the circulation by mere mechanical, or rather hydraulic pressure. Congestion from the portal side in the living animal is a thing not readily to be effected at will, but Professor Schiff has discovered a way to bring this about. The renal system has not wholly lost all anastomosis with the portal even in ourselves; but in such an animal as the frog, the factors of the two venous trunks, each alike inferent, the kidney portal and the liver portal, anastomose very freely indeed; and what is of equal consequence, through one or other of these two great stems, interlacing by their roots, and indeed continuous by them, all the blood of the lower half of the body, viscera, parietes, and extremities, must pass. Ligature of the one therefore inevitably overfills the other system, and ligature of the vena renalis advehens overfills the vena advehens of the liver, which takes a course from the allantoïd along the anterior surface of the abdomen to take up the vessels of the chylopoïetic viscera at the portal fissure. And such an operation Professor Schiff says has been in his hands a sure and often repeated plan for producing diabetes. We do not know in what way Professor Schiff proceeded to ligature the renal portal veins, but by making incisions along the outside of the elongated ossa ilii, the veins in question are easily exposed from above, and lying, as the vena cardinales always do, along the outer surface of the primordial kidney, they are as easily ligatured. A little care is necessary to avoid including the ureter in the ligature. This operation we have repeatedly performed upon frogs; and they have survived it for some days, but no diabetes has ensued upon it. And we are, in consequence, tempted to think that Professor Schiff, when engaged upon this series of experiments, was working, as he tells he discovered upon another occasion he was (p. 110, op. cit.), with a Fehling's solution which had been spoiled by exposure to light and air, and which consequently would give a reduction on boiling by itself. Dr. Roberts' rules, which we shall give further on, would have prevented such an error as this.

Hepatic congestion and its sequela diabetes can be artificially produced with much less trouble, and without any recondite acquaintance with renal portal circulations. "We can," says Professor Schiff (p. 127), "produce them both in ourselves *per aspirationem*, in the course of a few hours;" and Dr. Pavy (p. 77), by simply limiting the amount of air supplied, short of producing asphyxia, for an hour, has brought about the same result in the lower animals. This latter observer is of opinion that the saccharine state of the urine which he and others have detected after chloroform, is to be ascribed rather to the struggling and congestion consequent upon its administration, than to any direct action of the anæsthetic; and Schiff, who once entertained the same views, seems to us to have given them up somewhat overhastily, on finding that a hedgehog, an animal notoriously requiring a large quantity of anæsthetic agents to be brought under their influence, evacuated after etherization what might seem to be a correspondingly large quantity of sugar. But surely the muscular exertion and right heart congestion of a resolutely rolled-up hedgehog is as truly muscular exertion, and as likely to bring about hepatic congestion, as any other kind of struggling. Pertussis, coma, pneumonia, have all been seen to bring diabetes in their train, impeded respiration, and mechanical congestion being, as it would seem, the one property they all possess in common. Lastly, the mechanical relations of the two sets of capillaries within the liver, the one carrying blood, and the other containing liver cells, may be deranged by an entirely different procedure, but, as it is said, with precisely the same results. Ligature of either portal vein or hepatic artery will make the urine diabetic. Andral gives us indeed an account of a case of diabetes in which, after death, the portal vein was found* to have been actually occluded during the lifetime of the patient. We have ourselves, struck with the clearness and lack of ambiguity with which these statements are made, repeatedly ligatured the subcutaneous abdominal vein in frogs, being as it is an easily accessible and exceedingly important constituent of the portal system, but our results were by no means of the character which these observations had led us to anticipate. And against any application to human pathology or therapeutics of any of this second series of hydraulic or rather of hæmostatic experiments, militates the fact that such heart disease as we have in tricuspid regurgitation may gorge the hepatic circulation to almost any extent, and yet never give rise to any diabetes.

Thirdly. Qualitative alteration of the liver blood has been found experimentally to bring about a diabetes more surely and lastingly than either of the two lines of procedure already described. Less stress has been laid upon this than upon either of the other two methods, yet it perhaps is the most important of the three. Irregularities of food, and especially of drink, are well known to have evoked the actual disease into activity, if not into being, in the human subject. Regularity in food and drink is insisted upon by every writer of repute, from Dr. Prout downwards, as the first and chief point to be attended to in the treatment of the disease when it is set up, and experiments with diverse alimentary substances, as they may be tolerably close reproductions of actual human eatings and drinkings, would seem not unlikely to be the most instructive we can put into play. Now the portal blood may have its composition altered either by the direct injection of the modifying substance into one of its factors or radicles, or mediately by the absorption into it from the digestive tract of the modifying agent. The second way, as being the more simple, we will give an account of first. Dr. Harley, whose experiments in both

* Comptes Rendus, tom. xxxiv. p. 468. 1856.

directions were put on record in the twentieth volume of this Journal, July, 1857, neglecting the maxim, "Fiat experimentum in corpore vili," found that by ingestion of an abundance of asparagus he could produce in himself a diabetes which would last for several days; and the Brothers Schiff, with a similar self-sacrifice to science, twice produced in themselves a diabetes, lasting upon each occasion for several days, by the simple plan of reducing their usual exceedingly abundant allowance of animal food to one-third or one-fourth of its ordinary amount, and substituting for it a large supply of bread. Somewhat less personally perilous were Professor Schiff's other experiments, but the indications they give are not less lucid. Rabbits and guinea-pigs whilst confined to a diet of sweetened mashes of carrots (p. 45), or to taraxacum and chrysanthemums (p. 128), were found to become diabetic, and to continue so, so long as they could be got to take these kinds of food. In the two latter cases Professor Schiff is inclined to refer the working of these two members of the Syngenesious family to their common possession of inulin *vice* starch, and inulin he considers as the vegetable carbohydrate of nearest kinship to the liver carbohydrate, glycogen. He does not think of inulin as an irritant and indigestible article of food. But the common Jerusalem artichoke is rich in inulin, and it is in the possession of an exceedingly bad name among the large class of dyspeptics; and we ourselves have discovered that inulin, when once washed free from sugar, which clings to it, however, with extraordinary tenacity, will resist, when thus to all physical appearance a pure white starch, the influence of the same saliva which turns ordinary starch into grape-sugar. Indeed, to cause it to manifest its starchy nature by undergoing metamorphosis into sugar, it is necessary to act upon inulin with sulphuric acid. Hence it may possibly be that the diabetic properties of such plants as taraxacum are not to be ascribed to any action upon the liver, which old writers ascribed to them, and which Professor Schiff, with a faith larger than is ordinary in experimentalists, believes in, but to their abounding in an indigestible variety of starch, which sets the ordinary reducing agents at defiance. Ether and alcohol, mixed each with equal parts of water, were, in Professor Bernard's hands, and nitrate of uranium in M. Leconté's, found to be equally potent in producing diabetes in dogs; and Dr. Harley states that cheese and other indigestible substances have been observed to set up a diabetes in man.

The composition of the portal blood may be more directly, though less easily, modified by throwing different substances into its radicles in the way of actual injection. Dr. Harley (loc. cit. p. 195), by the injection of ether, chloroform, liquor ammoniæ, methylated spirit, into branches of the portal vein, rendered the urine saccharine. And to this list Dr. Pavy's experiments, recorded in the 'Guy's Hospital Reports' for last year, enable us to add the somewhat incongruous member, phosphoric acid.

The best account, perhaps, which we have of the pathology of diabetes is to be found in Griesinger's writings, as given in the heading to this article. The antecedents of both intercurrent and of permanent diabetes he has most carefully collected from the medical literature of all countries, and massed together for us in a most complete and philosophical paper. His paper, however, we are informed, is but the shadow of a coming and larger book upon the same subject. Griesinger is of opinion that inordinate indulgence in saccharine foods may lead to permanent, as we have already said it is known to do to temporary, diabetes. In two, however, of the cases which he records, we observe that the condition of getting wet through, or of getting thoroughly chilled, was combined with the more voluntary evil of excessive sugar eating; and as the former of these conditions has been repeatedly seen to be a full and sufficient cause by itself for the production of the disease, not only by Dr. Prout, but also by Griesinger himself, we are inclined to think that his somewhat scanty induction from the history of sugar-refiners and lovers of confectionery is weakened by this complication of antecedents.

Of traumatic diabetes we find as many as twenty cases brought together in this paper; the author, however, whilst holding it as made out that mechanical shock and contusion may give rise to the permanent disease, thinks nevertheless that it is the general shock and universal contusion, and not cerebral lesion, which is the really efficient cause. In four only of the twenty cases were head symptoms and head lesions at all prominent, and in three out of these very four cases the supervening diabetes was not permanent, and consequently, if we are to suppose the permanent disease to be a separate entity, it may never have existed in these cases at all. That shock not localized to the head may produce diabetes, we think is certain—mental shock, indeed, has, not without reason, been held competent to the production of the malady. Dr. Hill has recently given us his experience of four cases of burns, taken as they presented themselves, in all of which the sugar reaction was given by one or other of the ordinary tests. One of the theories which he proposes for the explanation of these facts is, that the mellituria was due to the shock which an injury so sudden and severe as an extensive cutaneous burn must produce. And to the special working of localized intra-cranial lesion in the production of genuine and lasting diabetes, we have the testimony of such observers as Drs. Gall, Barlow, and Pavy; and it is in cases of insensibility dependent upon intra-cranial effusion or uræmic poisoning, and in no others, that Dr. Roberts has observed intercurrent diabetes to occur in the human subject. Indeed, some pages further on (p. 27, op. cit.) we find Griesinger, in speaking of the question whether carbuncle be a cause or an effect of diabetes, remarking as one point in favour of its being, sometimes, at least, in the former of these two relations, that in all the four cases in which carbuncle and diabetes coincided in the same individual, the carbuncles were situated about the head and neck, whence their influence would be propagated to the central nerve-masses with least loss and greatest sureness.

Griesinger, like other recent observers, has failed to detect a saccharine state of the urine in cases of epilepsy and hysteria, which, however, it has been stated to accompany; but he confirms the opinion which has connected its origination in women with the arrest of the catamenia and mammary secretions. His statistics, comprehending 225 cases, give a per-centage, however, of 76·4 males as against 23·5 females.

Dr. Prout's views as to the near relationship of dyspepsia and diabetes have found a warm supporter in Herr Griesinger. Even where he allows nerve-lesion to be the first term in the series of causes, he will insist that it is through the intermediation of the dyspeptic disturbance (which every one, even before these days of experimental physiology, knew it could produce,) that it brings about diabetes. The direct action on the liver is a *vera causa* for artificial, it is by no means so clear that it is so for natural diabetes. How can this physiological theory explain the clinical fact that a couple of potatoes will set up afresh all the glycosuria which a careful confinement of flesh diet has brought down to zero? The small quantity of starch thus surreptitiously swallowed will set up for days, and even for weeks sometimes, an excretion of sugar a hundredfold of its own equivalent. May this not be due to some derangement wrought in the mucous membrane of intestinal tube, changing its secretions and perverting their functions? Acute gastric disturbance, changing the character of these secretions yet a second time, is, we know, competent to arrest diabetes, as is often seen when acute febrile attacks intervene in its course: why should not chronic gastric derangement be able to produce it? The concomitants of chronic dyspepsia, its acidity, flatulence, water-brash, and other miseries, are often found to usher in the more deadly, if not the more wretched disease. The causes of chronic dyspepsia—over-work, drinking whilst hot, distress of mind, poverty of food—have all been considered, and with good reason, as causes of diabetes also. It is commonly held that excessive hunger is to be found as a symptom in all cases of diabetes, as well as excessive thirst; this, however, is by no means the case; the polydipsia, it is true, is always present, and from the very first onset of the disease; the bulimia is often wanting at the commencement, and not rarely throughout the course of the disease. Nor are proofs wanting that the secretions found in a diabetic stomach are something very different from those to be found in a sound one. The diabetic stomach will convert uncooked starch into sugar—a feat found to be impossible by the healthy stomach;—nay, more; it has been shown, according to Petters,* that the additional transformation of this sugar into alcohol and carbonic acid may be effected by this self-same gastric juice, or rather gastric mucus, even when pure from all admixture with saliva, either oral or pancreatic. It is upon the ingestion of starch, which is thus rapidly transformed, that the most striking symptom of diabetes, its polydipsia, rises up in especial fury. After animal food, and particularly after fat animal food, this thirst is not violent; it is when farinaceous food is swallowed and submitted to this rough and hurried transformation, that the unquenchable desire for fluid makes itself felt

* Prager Vierteljahrsschrift, Band lv. p. 90.

so forcibly. Herr Griesinger finally hints that the sugar found in fluids vomited from the stomach after a meal of exclusively animal food, is due not to any secretion of sugar from the walls of the stomach, but to a breaking up within it of the albuminates by the same potent ferment which acts so vehemently upon the amylacea. The pain so constantly felt at the stomach by diabetic patients, as well as the large quantity of food, amounting to as much occasionally as six pounds of meat, point, in his estimation, clearly to the existence of some alteration in the characters of the secretions of the organ.

Morbid anatomy certainly lends its countenance to the view of the disease being dependent upon functional derangement, the structural lesions observed after death may, possibly without exception, be considered sequelæ and manifestations, not causes and sources of malnutrition. Dr. Wilks, however, is of opinion, though Dr. Griesinger is not, that the liver of a diabetic patient possesses certain distinctive characters, being firm and tough to the touch, homogeneous and darkish to the eye. The records of its microscopic appearance are so exceedingly various as to prevent us from laying any weight upon any character in particular. Dr. Pavy informs us that he has observed the bile to present pretty generally certain well-marked and easily-recognisable peculiarities, resembling as it does a rhubarb mixture, but he has not given us any *rationale* of this appearance. And to his labours and those of Dr. Wilks we owe the discovery that the lung-disease under which, according to Griesinger, from forty-three to forty-four per cent. of diabetic patients sink, is in reality not of a tubercular, but of a sub-inflammatory character, analogous, in fact, to that which is sometimes seen in the diabetic kidney, taking the form of numerous little scattered abscesses, as recorded by Griesinger. Dr. Pavy states it as his opinion that the *rationale* of these deposits in the lungs is the same as that which he, Lohemeyer, and Lecorché are agreed is the pathology of diabetic cataract—a derangement, namely, of the relationship which should subsist between the tissues and the blood nourishing them. Much of the pathology and much of the therapeutics of diabetes has been built upon this theory, and it may be well in consequence to see what confirmation experiment producing, and observation recording diabetic cataract lends to it.

Dr. Richardson (op. cit.) has given us records of no less than 107 different experiments in the way of injection of various saccharine and saline solutions into the tissues, and also in the way of immersing in similar solutions the entire bodies of certain animals; and the cataractous condition which ensued in the lens when the injection was made with, or the immersion took place into, a fluid of greater specific gravity than the blood, he ascribes simply to the operation of the laws of endosmosis. "The mode by which the cataractous state is produced must be accepted, I think, as osmotic in character—i.e., as a direct physical effect on the lens through its surrounding and internal fluids, by which the arrangement of the lens, fibres, or tubes is changed." (p. 472.) In the same connexion Dr. Richardson observes that the opacity when produced was "quickly removed, or was prevented from forming at all, by the immersion of the animal in water." "The fact was further supported by the general effects of the dense fluid on the tissues of the body. There was scarcely a case in which the lenticular opacity was strongly marked in which there was not therewith a shrinking of the tissues, followed in most cases by convulsion, amounting even to tetanus." We are disposed to think that some confusion may exist here between physics and pathology, between endosmosis and neurosis. Dr. Richardson says on the next page, "By removing four of these lenses at the stage of perfect obscurity, and by macerating them, I obtained the most perfect reaction for sugar from the solution by Trommer's test." Finally, Dr. Richardson declares it to be his opinion that a relationship subsists between the natural and the artificial cataracts in question. To this last conclusion we find Dr. Lecorché demurring (p. 728, op. cit.), firstly, upon the general ground that Dr. Richardson's experiments are physical rather than physiological; and, secondly, for the following special reasons. He remarks that the cataract which accompanies diabetes, when once produced, is not removable by the removal of the cause which called it into being, as is the artificially-produced cataract; that the natural cataract arises some eighteen months or two years after the commencement of the disease, the fatal termination of which it usually foreshadows, according to Oppolzer, with well-nigh absolute certainty; and, finally, that in the only case in which a human diabetic's cataractous lens was chemically tested, it was found to be devoid of sugar. Certain experiments of Dr. Lecorché's, in which he injected the aqueous chamber in rabbits with solutions containing from 10 up to 100 grammes of glucose per 1000 grammes of water without once producing cataract, have the appearance of militating even more strongly still against the purely physical explanation which Dr. Richardson proposes. It is difficult also to understand how upon this view of its way of originating it should be nearly, if not quite, without exception, developed on the back and not on the front of the lens: and that like other morbid growths, it should have distinct and constant morphological characters disclosed in its structure under the microscope. The cataractous lens of natural diabetes depends upon cell-growth for its existence, and the place and time of that existence is determined and fixed. And in consequence we are inclined to class it with the falling hair, and the decaying teeth, the dry itching skin, and the necrosing phalanges, known to be correlated in the way of nutrition, or rather of mal-nutrition, with the diabetic state.

Judging, then, of the whole from the parts, and transferring our reading of these local diseases to the interpretation of the general systemic disorder, we suppose the tendency to death in diabetes to depend upon a perversion or alteration of the blood composition, which unfits it for its office of duly nourishing the tissues; this perversion of nutrition it is our business to detect, and when detected, to combat. With a notice, then, of the recent advances in the modes of testing for and of treating diabetes, we shall bring this review to a close.

The kidneys, hurrying out of the system whatever deleterious elements it may be in their power to separate from the blood supplied to them, are especially prompt in freeing the nutrient fluid from that element which, as we have just seen, has so much power for deranging the growth of so many and such various tissues. The blood will not bear more than 1-200th per cent. of sugar, and the very haste with which the kidneys free it from any sugar in excess of this infinitesimal quantity may lead the practitioner into the error of mistaking an intercurrent glycosuria, a thing even more common than an intercurrent albuminuria, for permanent diabetes. We have already mentioned several circumstances more or less pathological in character, which have been found to be accompanied by a transitory diabetes, and M. Dufresne tells us that any one can produce in himself a temporary diabetes by drinking at night a strong solution of sugar. We are inclined to suspect that the existence of diabetes has occasionally been inferred from a saccharine condition in the morning urine of dyspeptic persons who are in the habit of eating arrowroot or other such biscuits the last thing at night. Our first caution, then, in testing for diabetes, would be to avoid mistaking a temporary for a permanent glycosuria, and this we shall do by looking to the other symptoms of the case for the passing moment, and instituting a second examination of a second sample of urine passed at another hour, and after other antecedents in the way of food.

The sugar of diabetes and the sugar of the liver have always been allowed to coincide with grape-sugar in the great chemical characteristics furnished by the copper-test, by Moore's caustic potash test, in the direct and rapid fermentation they all undergo so much more quickly than milk-sugar, when in contact with yeast, and in their action upon polarized light. The sugar of diabetes, however, and true grape-sugar, have been thought by Dr. Owen Rees to resist fermentation for a longer period than the sugar of artificial diabetes; and Schiff (op. cit. p. 109), has observed this latter sugar, as obtained by him in the experiments upon rats already mentioned, to resist decomposition much more strongly than the sugar obtained directly from the liver. Bernard has striven to establish a difference between ordinary diabetic and the liver-sugar, upon the ground of their acting differently upon the animal system when thrown into it by subcutaneous injection. He found that one gramme of sugar dissolved in twenty-five cubic centimetres of water could be injected into the cellular tissue of a rabbit without showing itself in the urine, but if the quantity of this sugar in the water was increased by one-half, unaltered sugar was to be found thereupon in the urine. Diabetic sugar, however, was retained, and, he supposes, altered and consumed in the blood, when injected in the proportion of 1·5 grammes to the twenty-five centimetres of water, and liver-sugar behaved in a similar way even in the proportion of two grammes or half a drachm to the same amount of water. Schiff, however, has drawn attention to the fact that the grape-sugar employed by Bernard was impure, being that of commerce simply, and contaminated consequently with both sulphate of lime and sulphuric acid: and that the solution of liver-sugar was in reality but a concentrated decoction of the liver substance filtered through animal charcoal, and like the grape-sugar therefore mixed up with various saline matters, which, on Bernard's own showing, have been proved to have great effect on the absorption and excretion of saccharine matters when purposely added to them; and finally, Schiff's own experiments have led him to think that the physiological differences which Bernard thinks he has elicited between the working of the sugar from these several sources, have no real existence, or in German phraseology, that *organoleptically* liver and grape-sugar are identical. Lastly, Gorup-Besanez speaks of three several kinds, as they have been considered, of sugar under the one common name of grape-sugar, and mentions the view which distinguishes them but to disapprove of it.

Persons who are anxious to make themselves acquainted with the ways and means for the detection of this sugar, either when masked and disguised, as in the fluids and solids of the physiological laboratory, or when existing as an infinitesimal in the urine of healthy life, we must refer to an exhaustive review of the entire subject by Dr. Huppert, in Schmidt's 'Jahrbucher' for 1861, Bd. cxii., No. 10; October, and to the paper already mentioned in the 'Transactions of the Chemical Society,' by Dr. Bence Jones, April, 1861.

But we should not do justice to our subject if, in the interval between our pathology and therapeutics, we omitted to notice Dr. Roberts' two articles in the 'Lancet' of May 17 and 24 of the current year, on the qualitative and quantitative tests for sugar in the urine. His qualitative test is a modification of Fehling's solution, and is of the following composition: sulphate of copper, eight grains; tartrate of potash, half a drachm; liquor potassæ, one ounce; and he gives the following directions for its use, which will prevent the occurrence of such fallacy as we have already seen to arise through neglect of the precautions he recommends:—

> "The best method of detecting sugar in urine is as follows: pour some of the prepared hot liquor into a narrow test-tube to the depth of three-quarters of an inch; heat until it begins to boil; then add two or three drops of the suspected urine. If sugar be abundant, a thick yellowish opacity and deposit of yellow suboxide are produced, and this changes to brick red at once if the blue colour of the test remain dominant. If no such reaction ensue, go on adding urine until a quantity equal to the bulk of the test employed has been poured in; heat again to ebullition, and no change occurring, set aside without further boiling. If no milkiness is produced as the mixture cools, the urine may be confidently pronounced free from sugar, for no quantity above the fortieth of a grain per cent. can escape such a search, and any quantity below that is devoid of clinical significance."*

Dr. Roberts has based an easily applicable "Clinical Method" for the quantitative examination of diabetic urine upon the fact, that when the sugar of diabetic urine is destroyed by fermentation, reducing, as it is transformed into alcohol and carbonic acid, the specific gravity from as much, it may be, as 1050 down to even less than 1000, each degree of density lost corresponds to one grain of sugar in the

* Lancet, May 17th, 1862.

ounce of unfermented urine. His directions for its employment run as follows:—

"About four ounces of the saccharine urine are put into a twelve-ounce bottle, and a piece of German yeast, about the size of a cobnut or small walnut, is added to it. A great excess of yeast is used to hasten fermentation, but a little more or a little less does not sensibly affect the result. The bottle is then covered with a nicked cork, which permits the escape of the carbonic acid, and set aside on the mantelpiece or other warm place to ferment. Beside it is placed a tightly-corked four ounce phial of the same urine, without any yeast. In about twenty-four hours the fermentation will have ceased, and the scum cleared off or subsided. The fermented urine is then decanted into a cylindrical glass, and its specific gravity taken; at the same time, the density of the unfermented urine in the companion phial is observed, and the density of it ascertained. Fermentation is generally complete in about eighteen hours, if the locality is sufficiently warm; and it is desirable to remove the two phials into a cool place two or three hours before the densities are taken, in order that they may attain the temperature of the surrounding atmosphere.*

We pass from diagnosis to therapeutics, repeating an exhortation of Griesinger's, which will serve as a fitting stepping-stone between the one and the other; it is, to lose no time in suspicious cases in testing the urine, as diabetes is in its earlier stages amenable to therapeutical measures, which later it sets at defiance.

English physicians, holding by their great master, Dr. Prout, believe that the first and chief point to be attended to in diabetes is diet. Nowhere can fuller and clearer directions on the *juvantia ac lædentia* in diabetes be found than in the paper bearing that title by Dr. Camplin, himself a living witness to the excellence of his own treatment. The volume of the 'Medico-Chirurgical Transactions' (that for 1855) which contains it is referred to as authoritative by the latest writers upon the subject at home and abroad. Following their example, we will pass on to remark that Dr. Pavy, in his almond *bread*, has presented us with a new substitute for the carbohydrated bread of ordinary composition, so longed after by, and yet so deadly to, the diabetic. It will alternate advantageously in the dietary of the disease with Dr. Camplin's bran biscuits.

The blood of a diabetic debarred from carbohydrates comes very near to the normal standard of a nutrient fluid, and the patient's tenure of life approaches the normal standard in all but one important particular—and that is, that the slightest indulgence in food tainted with sugar or starch may again re-establish the old mischief, and by destroying the relation which should exist between the blood and the tissues, bring the durability of the entire system into peril.

To his twenty-six pages on the subject of Diet, Dr. Pavy has appended a list, showing the diabetic in a tabular form what to eat, drink, and avoid. To what he has written we may add, that Griesinger, after several experiments, came to the same conclusion as to the question of giving or withholding fluid—viz., that the latter of the two courses is as useless, and indeed hurtful, as it is obviously cruel.

But water acidulated with phosphoric acid, though recommended in

* Lancet, May 24th, 1862.

this country on the high authority of Dr. Watson, Dr. Griesinger has found positively injurious, as increasing the quantity of sugar excreted. One case, indeed, he records, in which diabetes was supposed to have been brought about by its employment in a gastric complaint. Griesinger remarks, that these observations are in accordance with what Dr. Pavy's experiments, recorded in the 'Guy's Hospital Reports' of ten years ago, might have led him to expect; and we may add, with what those recorded in the volume for last year teach even more clearly.

Pavy, Griesinger, Garrod, and Camplin are all agreed as to the working of strong and sugary alcoholic drinks in increasing diabetes; claret, however, as containing but a low per-centage of alcohol, and scarcely a trace of sugar, and freed under our present tariff from what was formerly an objection to its use—viz., its dearness, holds with dry sherry an honourable place in Dr. Pavy's list.

Prout, Griesinger, and Pavy all alike bestow high encomiums upon fat as an article of food possessing the power of diminishing the urgent demand for drink which torments the diabetic; and from this substance as an aliment to cod-liver oil as a medicine the transition is imperceptible. One of the recommendations which the almond has as a substitute for the amylaceous cerealia, is the large per-centage (twenty-four per cent.) of oil which replaces the starch so injurious in this disease, in which they abound. It is difficult not to connect such experiments as those in the 'Guy's Hospital Reports' for 1861, in which the per-centage of fat in a liver is found to be doubled after its owner has been subjected to dosing with carbonate of soda, with the effect which the same drug has in checking the secretion of sugar set up in artificial, and existing in real diabetes. These experiments, besides lending countenance to the view which regards the formation of fat as one of the normal destinations of the carbohydrates, seem to point very clearly to the employment of alkalies in a disease so deficient in fat, and so prodigally wasteful of the materials out of which fat is elaborated. It is consequently with a feeling something like disappointment that we miss any very warm commendation of the alkaline treatment in Dr. Pavy's last work, though he mentions incidentally that he has given the carbonate of soda to the extent of four drachms a day, the acetate of potash to that of half an ounce, and the citrate to that of six drachms, and Rochelle salts to that of an ounce. Griesinger, on the other hand, who tells us what we fully believe as to his disregard for Hufeland's theories of the disease, and a "Parisian Apothecary's" views of its therapeutics, speaks nevertheless in higher terms of praise of the alkaline treatment than his scanty faith in drugs allows him to apply to any other merely medicinal agent. This praise is, after all, not of the very warmest, but coming from such a quarter it will carry some weight with it. Carlsbad and Vichy both have their share of it.

Griesinger has succeeded in making the urine of diabetic patients alkaline—a thing usually found rather difficult; his doses amounted to as much as five drachms per diem of the bicarbonate, and six drachms of the carbonate of soda. The former of these two salts is the one he prefers, and he does not seem to have made any trial of potash. Under the use of these means the urine reassumed its natural colour, and the sugar very sensibly diminished; many of the subjective symptoms of the disease disappeared without being replaced by what would usually be the result of the persistent use for periods of as much as six weeks of such large doses of the alkalies. But though hunger and thirst, and dryness of skin, and nervous symptoms, all alike were diminished or destroyed, yet in every one of Griesinger's own cases a fatal result occurred. They amount, however, to but 7 out of as many as 225 of his own and other persons', the records of which are scattered throughout a wide range of medical literature. To be successful, the alkaline treatment must be put in play early; and Mialhe, to whom we imagine Griesinger alludes as the "Parisian Apothecary," and who is not likely to underrate the efficiency of this line of treatment, agrees in this fully with his German critic.

Griesinger's views as to perverted gastric secretions lying at the bottom of the disease, must have led him to anticipate striking results from the employment of genuine pepsine. This substance he had daily prepared fresh and fresh from the stomachs of pigs, and administered twice a day to his patients an hour and a half after their food. He tried it in two cases only, and his unfavourable results consequently should count for less than they otherwise might do. This last remark applies to his trials of the virtues of yeast. Permanganate of potash, and other oxidizing agents, such as ozone, gave negative results. The homœopathic or saccharine treatment of diabetes, as recommended by Piorry, seems to have been more potent for harm than any other plan adopted in this disease. Its champions, however, exhibit that unshakeable faith in their own invention which Swift has so happily ridiculed in his account of the Academy at Lagado. The dog dies upon the spot; but when Gulliver leaves the laboratory, the doctor is still trying to recover him by the same operation. In another experiment the same philosophers find the charge and trouble very great (as are the pain and misery of the saccharine treatment), and they have little or no result. "However, it is not doubted," says Gulliver, "that this invention may be capable of great improvement." In like manner we find a physician practising, not in Lagado or Laputa, but a good deal nearer home, after an enumeration of those most strikingly unsuccessful cases of treatment upon the saccharine plan, conclude by saying, that "much has yet to be learnt concerning the mode of using sugar in glycosuria." We must profess ourselves, as Gulliver did under similar circumstances, "not skilful enough to comprehend the experiments by which he has demonstrated this." Before leaving a subject in which the ludicrous and the painful are inextricably intermixed, we would mention, as instructive, an experiment of Griesinger with this sugar treatment. It produced wild dreams, mental disturbance in the waking state, bodily depression, and all the *malaise* of blood-poisoning; but in addition to these miseries, after some very fair trial of this very hurtful agent, a large boil developed itself upon the forehead—a sequela, one would say, clearly, and not a cause, in this case, of severe diabetes.

We will now, in conclusion, sum up what we think is fairly deducible from the experience of the writers we have been reviewing as to the treatment of this still enigmatical disease. Carbohydrates must be rigidly excluded from the dietary, carbohydrogens as resolutely introduced into it. The Arctic pemmican and the German sausage might serve for food, but the drink to be taken with them must not be stronger nor sweeter than the wines of Bordeaux. The urgent thirst will not and need not be stinted as to quantity of fluid, but as to its quality, we must have in it alcohol at a minimum and sugar at zero. Bread the diabetic may have, but it must be a bread chemically as unlike as physically it should be like what ordinarily goes by that name. Doses of alkalies, either in small doses, as at Vichy and Carlsbad, or in larger ones, as in prescriptions, calm and quiet the angry stomach. From their employment, the very best effects are found to arise—cures, namely, in commencing, great alleviation in long-established cases. Alkalies should not be put into an acidified but into a fasting stomach. On the other hand, whilst food is being ingested, *inter cibum*, as classically written and consequently often wrongly interpreted prescriptions run, acids may be taken more safely and conveniently, we believe, than in the intervals between meals. With acids, opium, quinine, and iron will combine as advantageously in therapeutics as easily in chemistry, and cod-liver oil will be found to benefit the patient.

REVIEW IX.

Uterus Duplex Bicornis cum Vagina Simplici. Tvende Födsler, begge Gange ved Vending af Fosteret; flere mærkelige Abnormiteter. Af Dr. F. C. FAYE, Professor i Födselsvidenskab, &c., ved Norges Universitet.

Double Bicornate Uterus with a Single Vagina. Two Deliveries, both times by Turning; several remarkable Abnormities. By Dr. F. C. FAYE, Professor of Midwifery, &c., in the University of Norway.—*Christiania*, 1861. 8vo, pp. 21. With a Plate.

THE above remarkable case was that of a woman, aged thirty-nine years and a half, in whom menstruation, which had commenced in her sixteenth year, had always been regular; in 1839 she had been under treatment in the Royal Hospital at Christiania for an abdominal tumour, supposed to be caused by hypertrophy of the uterus. At that time neither os nor vaginal portion of the uterus could be discovered on examination. At the end of three months she was discharged uncured. Several years later she was restored to health by using the chalybeate baths of Eidsvold, from which time she felt quite well, was not conscious of the existence of any abdominal tumour, and married at the age of thirty-six. In the course of about nine months she became pregnant, and was in due time delivered, by turning, of a child, in whom the arm had presented. The child died at the end of twenty-five hours; the mother's convalescence was uninterrupted. In May, 1860, she again became pregnant. She continued well until three days before labour-pains set in, when she was attacked with violent shivering, the movements of the child ceasing. The abdomen was most prominent on the right side. Pains commenced in the evening of the 31st January, the upper extremity again presented, and she was once more delivered by turning, the child (a boy) being small, and presenting the appearance of having been some time dead. The placenta was retained, apparently in consequence of spasm in the lower portion of the uterus, preventing the introduction of the hand, to effect which six different attempts were made. The following was the report of the patient's state twenty-three hours after the birth of the child:

> "The funis is uninjured, the orifice of the uterus is strongly contracted and is very tender, so that only the tip of the second finger can be introduced; the abdomen is highly distended and is very tender; there is frequent vomiting; the tongue is dry, and the pulse is 130. Leeches and oily frictions were prescribed."

Nothing was then heard of the patient, who resided at a distance, for fourteen days, when it was found necessary to call in additional assistance from Christiania. On the arrival of Dr. Vogt, the patient was somewhat better, the pulse was 108, and she could bear to lie on her back; the pains, however, still continued. There was thirst, the tongue was rather dry, but the abdomen appeared to be less distended. The fundus uteri reached to the umbilicus, and to the left side of the same was felt an oblong moveable tumour as large as a goose-egg, and extending up towards the false ribs. This tumour was tender to the touch, and on percussion gave a dull tympanitic sound. The bowels acted well without pain, but as passing water was distressing, the catheter was occasionally introduced. The renal secretion was scanty. The patient was next day conveyed by rail to Christiania, and was taken into hospital, where she was for the first time seen by Professor Faye. The rounded, half fluctuating tumour in the abdomen lay more in the middle, and extended up towards the umbilicus, while at the same time a smaller oblong swelling was felt to the right side and upwards, which seemed not to be connected with the larger tumour. As the smaller tumour yielded a dull tympanitic sound on percussion, it was considered to be a portion of the large intestine. Through the vagina (as had been observed before the patient's removal to town) was felt the sharply-defined opening of the inferior segment of the tumour forming an external os uteri with slender margins, through which a prominent swelling presented itself; a uterine sound could without difficulty be introduced some inches upwards and backwards between this external investment, which was considered to be the wall of the uterus, and the swelling, which was probably formed of the membranes. The thin margin of the ring-shaped opening, taken for the orifice of the uterus, having been incised in many places, and the opening having thus been made somewhat wider, an exploratory trocar was introduced into the middle of the highly-distended tumour, upon which a quantity of intensely fœtid fluid was discharged. The patient was put under chloroform, and the hand having been introduced into the vagina, an attempt was made with the fingers to separate the membranes from their external adhesions. This was effected with tolerable ease throughout an extent of about two inches posteriorly, but higher up and anteriorly the inner and outer walls of the sac were found to be so closely connected, that it was thought prudent to desist from all such further attempts. The operation having ended, the tumour had disappeared, and the patient felt considerably relieved by the evacuation of the contents of the sac. The pulse was 144. Next day (20th February) there was slight abdominal tenderness on the right side; there was no tympanitis; a round body (the uterus) was felt a hand's-breadth above the symphysis pubis. Pulse 92. The three following days the patient's state continued about the same, but in the evening of the 23rd she got a violent shivering, followed by heat and thirst. The pulse that evening was 120, next morning it was 108. There was milk in the breasts. In the evening of the 25th she complained of great oppression of the chest, the abdomen became again distended, bilious vomiting set in, and her consciousness, which had hitherto been clear, became clouded. The pulse became small and extremely rapid, and she died at eight o'clock on the morning of the 27th.

The body was examined next morning by Prosector E. Winge. Inflammatory effusions, &c., having been removed, there was seen in the entrance to the pelvis two uterine bodies, of which one (the most prominent) lay in the left, the other in the right side of the pelvis. To each uterus belonged only one ovary, Fallopian tube, and round ligament, attached to the external and posterior surface of the respective uterine bodies. Inferiorly the two uteri were found to be united by a thick septum. Into the bladder only one ureter opened, to the right, and only one kidney was found, occupying the ordinary place of the right kidney. It was much longer than usual (6″).

The right uterus presented about the normal shape; it measured from the fundus to the os uteri $4\frac{1}{2}''$, in breadth $2\frac{1}{2}''$, from before backwards $1\frac{1}{4}''$; the anterior wall was $7'''$, the posterior $\frac{1}{2}''$ thick. The left uterus was larger, more cylindrical, inclined with its fundus somewhat to the left. It measured from the fundus to the orifice a little more than $7''$, in breadth $2\frac{1}{2}''$, from before backwards $1\frac{1}{2}''$; the anterior wall was $\frac{1}{2}''$ thick, the posterior was somewhat slighter. The cavity on section presented an hour-glass constriction; its mucous membrane was here and there injected and swollen, covered with some deciduum-like membranous remains; in other parts it was smooth, of a rather yellowish-brown colour, but especially on the posterior and internal surface it was prominent, as if lined by a placenta-like uneven mass, intimately connected with the substance of the uterus. The marginal portion of the orifice was posteriorly and to the right side separated from the vagina, and through this rupture the finger could be introduced to the extent of two inches into the connective tissue between the uterus and rectum. Many of the details are given, and from the results of the examination Prosector E. Winge infers that the pregnancy must have existed in the right uterus.

In his remarks upon this interesting case, Professor Faye shows that there is much reason to believe that the accumulation in the left uterus commenced some time previously to the last pregnancy, and that it increased with the latter. The left uterus so filled began to contract immediately after the expulsion of the fœtus, causing the bearing-down pains and the tense condition of the inferior segment of the uterus. As to the origin of the accumulated fluid, appearances were in favour of the opinion that an ovum may have separated from the left ovary, and have become fructified in the cavity of the uterus. In this case, the placenta may have formed in the inferior part of the cervix uteri, and gradually have coalesced with the subjacent uterine substance, while the embryo would at the same time have coalesced with the inner surface of the uterus. To justify such an hypothesis, we must suppose that the fœtus died within the first two months, and that it putrefied and gradually dissolved in the surrounding fluid.

It is also worthy of note that in this case the upper extremity on both occasions presented, inasmuch as a horned and bipartite uterus can scarcely predispose to that variety of presentation, on account of the more limited space. In animals with many-horned uteri the young are always born lengthwise, each horn expelling its contents down through the common neck and outer opening.

As to the absence of the right kidney, it is quite uncertain whether this abnormity stands in any causal relation to the original double formation of uterus; at the same time it is right to mention that a similar condition has been observed in another instance. Professor Hasse, of Göttingen, has, in the 'Deutsche Klinik,' No. 24, 1860, described a case, reported by Dr. Sprengell, of cancer in a bicornate uterus, where the right kidney was entirely wanting, while the left was very large. The cancerous destruction had, however, in this instance, advanced so far, that it was impossible to decide to what extent the uterus had been divided, and how far an actually bicornate form had existed.

Professor Struthers has recently, in the 'Edinburgh Medical Journal,' described a case of bicornate uterus observed by him. In a slighter degree this formation is not extremely rare; thus some years ago Professor Faye observed a bicornate formation in a woman who died in the Lying-in Institution some time after delivery. In this case the bicornate shape was limited to the fundus uteri (*Uterus arcuatus seu bifundalis*), while both horns were about equally developed.

In the foregoing notice we have confined ourselves to endeavouring to give, in a short space, the leading points contained in the description of the very remarkable case observed by Professor Faye, and of his comments upon the same. We have thought this the best course to pursue, in consequence of the original being in the Norwegian language, and (not having been, so far as we are aware, translated) inaccessible to the majority of our readers. The most striking peculiarities of the present case appear to us to be the fact of the fœtus attaining its full term in one of two nearly equal uteri, and also the probability which exists that conception took place in each uterus, apparently at different times. Professor Faye's very interesting paper is illustrated with a plate, giving an accurate representation of the state of the organs implicated in the abnormity.

Review X.

1. *A Military System of Gymnastic Exercises for the Use of Instructors.* By Archibald MacLaren. Adjutant-General's Office, Horse-Guards.—*London*, 1862. pp. 194.
2. *The Value of a Gymnastic Training to the Soldier.* By Archibald MacLaren, Esq., the Gymnasium, Oxford. A Lecture. (From the 'Journal of the Royal United Service Institution,' vol. vi.)

A certain foreign physician, well known in his own country, has somewhat recently proposed and carried out a new method of treating various diseases by the actions of muscles at a distance from the part diseased, or by movements adapted, as he thinks, to counteract the progress of such diseases.

This system, which contains, it must be confessed, certainly a grain of reason, is carried out at the doctor's own establishment, if report be true, with some amount of success. The movements prescribed in most cases are calculated to give rise to a certain levity in the spectator, and an amount of perseverance is expected which few patients would ever give. The doctor acts upon the idea that excess of blood in one part is to be removed or its effect counteracted by increased muscular activity in another part. The system is, however, not without precedent; it is but an amplification of the theory propounded by Bacon. In the 'Novum Organon' he says:—"There is scarce any tendency to disease that may not be corrected by some appropriate exercise. Thus, bowling is suited to disease of the kidney, shooting with the long-bow to those of the lungs, walking and riding to those of the stomach:" and he complains that physicians have not given as much attention to this subject as it deserves.

It has been proposed to try the effects of this muscular treatment with reference to that unfortunate subject of ill-gotten disease, the British soldier. The vicious state of the various military depôts and camps is no longer ignored; the stink of these worse than Augean dens pervades the country, and the authorities have most judiciously determined to counteract the temptations to which soldiers are exposed by additional means of recreation. Gibraltar, thanks to Captain Jackson, has for some time had a soldiers' club, and with the happiest results; a reading-room has just been built at Chatham, and athletic sports are diligently encouraged at Aldershot.

Whether the foreign professor before alluded to would consider this method of curing vicious disease to be a modification of his system may be doubtful; at any rate, he ought to have some satisfaction in seeing the opening of a new chapter in support of his theory.

It is evident, that if healthful pastimes be supplied, the soldier will not be so prone to spend his hours in the ale-house and dancing-booth from sheer lack of some amusement. As a step towards providing a regular system of gymnastics, the military authorities have adopted the manual compiled by Mr. MacLaren of Oxford; and this capital little book has been arranged with much care by one who thoroughly knows his work. The art of a manual of this kind lies in so arranging the exercises that they may increase in difficulty and interest as the learner advances. In his introduction, this modern Amoros says:

"All exercises of mere position or posture have been avoided, for in no way do they furnish adequate exercise to the healthy adult; moreover, they are quite incompetent to maintain in their practice the pleasure and interest which are essential to the beneficial results of all exercise. And to receive these in the present system, the exercises are given in such great variety, that while every part of the complex structure of the human body may receive ample and suitable employment, the form of such employment may be varied almost daily."

As the value of the gymnasium will be proportionate to its popularity, it is perfectly necessary that the exercises should have sufficient variety to prevent apathy, and should evidently tend to diminish the difficulties of professional duties. The slow, plodding perseverance generally required in gymnastic exercise diminishes the attraction which a gymnasium ought to have, and it will be a difficult matter for the gymnasiarch to offer attractions capable of competing with those of the cricket-field and racquet-court.

Taking gymnastics in its limited sense, Mr. MacLaren is rather inclined to give too much prominence to the necessity of muscular development. If it be true, as some have pointed out, that bodily strength is of less value since the introduction of gunpowder, the training of the soldier must consist in calling out other powers of the body.

Our embodied idea of energy, activity, and strength is not so much the soldier as the policeman. For whereas in these days of long-ranging rifles soldiers seldom fight hand to hand (and we have learnt to disbelieve all reports from the other side of the Atlantic of repeated bayonet charges), the policeman has to match his muscles against those of the burglar or the murderer whom he may chance to meet; and muscular strength seems to be the requisite of a good thief-taker, but endurance and activity of the good soldier. The defeat in the first conflict at Bull Run was ascribed by the several regiments engaged to the pace at which the troops were brought to the field of battle. Some of them had to march twelve miles, and the men were exhausted when they reached the ground. From their want of endurance they can scarcely be said to have had the chance of trying their strength with the enemy. Nor must muscular strength and endurance be considered as necessarily going hand in hand: each requires special training, and powers of endurance are called forth by special practice, as much as power of a muscle is developed by exercise of that muscle.

The question, then, arises as to the best method of increasing the power to endure fatigue. Mr. MacLaren rides his hobby a little too hard, if he expects the gymnasium to present the best field for this kind of training. The space allotted is too confined, the exercises not sufficiently protracted for this; it is rather in the cricket-field and the racquet-court, in the boats and athletic games that the Englishman learns to resist fatigue. The Germans, who are constantly working in the gymnasium, are not able to stand as much fatigue as Englishmen. Compare, for instance, the pedestrian feats of Germans and of the members of the Alpine Club. The English, again, are far outdone by the savages and half-savage tribes of America.

The performances of the famous Captain Barclay are put to shame by hundreds of Indians and Indo-Canadian half-breeds. The Assiniboines, the most cunning of horse-stealers, will go night and day in quest of horses. The half-breeds will run all day with the dog-sleighs, making four miles an hour, and this for many days together. The theory which Mr. Richard Burton advances in his 'City of the Saints,' that "the fortitude and endurance of the Indian is the result, as in the prize-fighter, of undeveloped brain," may be laid as a flattering unction to our national pride; but, unluckily, it is but an hypothesis after all. Well-developed muscle is, it is true, an attribute of civilization. The muscles of the savage are not so round and prominent as those of the European, nor his torso so finely modelled; so that there is some reason in an Englishman's taking pride in the state of his muscles, and in feats of strength he would be more than a match for the ordinary savage. Speaking of the road to the Great Salt Lake City, Mr. Burton says, "there is only one Indian (a Shoshone) who can whip a white man in a rough-and-tumble." The Englishman owes much of his manly qualities to that love of out-door sports inherited from his Anglo-Saxon forefathers (curiously enough we still retain the Anglo-Saxon plega and gamen in the words play and game); and though the English stock suffers from being transplanted to a dry, hot climate, the American would probably have broader shoulders and be a finer man had he not so sadly neglected the games of the mother country. It is therefore earnestly to be hoped that the Government will not stop short in the good work on behalf of the soldier which it has begun. Not only should a gymnasium be furnished, but field sports should be encouraged. A field for cricket, hockey, and football would not be a very expensive addition; and if the authorities are liberally inclined, a racquet-court and fives-court might be built. It is much to be regretted that the fine old game of tennis is so expensive that few but the rich can afford to play it. The size of the court, the greater weight of the ball as compared with that used in the racquet-court, call for more power, and consequently tend to strengthen the whole frame better than any other game. If the gymnasium is intended to supply the lack of field sports, the building should be as much as possible open to the air. The athletes of Greece appreciated the advantage of practising under the pure heaven, and they worked in covered stadia only during the winter months. The advantage to be gained by working in fresh air can scarcely be over-estimated. The exhalation from a number of bodies in active work in a closed room must have some effect in fouling the purity of the air, and there are few who can boast that their bodies, like Alexander's, give off odours which savour of the gardens of the Hesperides. Any one who has worked in a close gymnasium will appreciate these remarks. Not only is one conscious of the impurity of the air whilst at work, but one feels a sense of heaviness afterwards which materially diminishes the value of the exercises. Were the sides of the building open, folding shutters only being supplied, which might be pulled down in case of rain, the ventilation of the building would be secured, and the expense of material diminished.

In drawing out this manual of gymnastic exercise, Mr. MacLaren has performed his duty to the soldier so ably, that it is to be hoped that he will turn his attention to the state of the volunteer. Some hints with regard to physical culture would scarcely be misplaced. Volunteers have been so much praised, and that deservedly, that they must not take it amiss if some fault be found with their general appearance. Many have been so well trained at public schools and the universities in boating, cricketing, and sports of all kinds, that their activity and agility are not to be equalled by the soldier, and skirmishing movements are executed by some of the London corps with even more rapidity than they are by the regulars. But volunteers are sadly deficient in one respect; they have neglected to persevere in those extension movements to which the soldier is obliged to give so much time. They consequently lack that erect carriage and elegance of bearing which belongs especially to the soldier. This is the one thing lacking. Were setting-up drill more vigorously practised, there would be less of that prancing action which is a lamentable characteristic of the march of the volunteer. We commend them to Mr. MacLaren's care.

Review XI.

1. *The Composition of the Urine in Health and Disease, and under the Action of Remedies.* By E. A. Parkes, M.D., F.R.S., Fellow of the Royal College of Physicians, Professor of Hygiene in the Army Medical School, late Professor of Clinical Medicine in University College, London, and Physician to University College Hospital.—*London*, 1860. pp. 404.
2. *On the Natural Constants of the Healthy Human Urine; and a Theory of Work founded thereon.* By the Rev. Samuel Haughton, M.A., F.R.S., Fellow of Trinity College, Dublin.—*Dublin*, 1860. pp. 34.
3. *On Urine, Urinary Deposits, and Calculi.* By Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital, Professor of Physiology and of General and Morbid Anatomy at King's College, London.—*London*, 1861. pp. 433.
4. *Reprints from the 'Journal of the Chemical Society':—*
    1. *On the Occurrence of Deposits of Crystallized Phosphate of Lime in Human Urine.*
    2. *On a Deposit of Crystallized Xanthine in Human Urine.*
    3. *On the Simultaneous Variations of Hippuric and Uric Acids in Healthy Urine.*
    4. *On the Composition of the Amorphous Deposits of Urates in Healthy Urine.*

    By Henry Bence Jones, M.D., F.R.S., F.R.C.P., F.C.S., late Physician to St. George's Hospital.

Dr. Parkes's work is divided into two books; in each book there is an introduction—that in the first treating of the various constituents of the urine in health, their mean amounts, and the quantities eliminated in the twenty-four hours, with other points connected with the physical and chemical characters of the secretion; that in the second giving an account of abnormal ingredients found in the fluid, whether in a state of solution, or forming urinary sediments. The first book is devoted to the consideration of the variations of the urine in health, arising from various physiological causes and the action of medicinal agents; the second book, to the changes in the urine induced by the presence of diseases of various kinds.

Before discussing the contents of this work, we may say a few words in reference to its general character and the purposes for which it is designed. Dr. Parkes, in his preface, states:

> "The title of the book expresses its exact nature; it is a mere enumeration of the alterations in the urinary constituents under various circumstances. I have not entered into the chemical history of these constituents, nor into the mode of determining their amounts. Works on chemistry deal better with the first subject; and the technical treatises of Neubauer and Thudichum have, for the time, exhausted the second."

We do not hesitate one moment to assert that this is the most complete work on the subject ever published, and gives to the reader, in a moderate compass, all that is really known upon the matter of which it treats; although the author says it is a mere enumeration of the alterations in the urinary constituents, we must take exception to the statement, for the observations are not only selected with profound judgment, but the remarks appended to each are full of intense interest, and exhibit the possession of a most philosophical mind, and one capable of weighing evidence with extreme correctness, and also of generalizing.

The mode in which Dr. Parkes has arranged his work differs altogether from that adopted by any other British writer on the subject. Instead of taking any single urinary constituent, and tracing its variations under all circumstances, he has adopted a plan which is much more conducive to the advancement of clinical study, and has endeavoured to trace out the variations of the more important principles in the urine under different physiological and pathological conditions. We cannot pretend in this review to present to our readers anything like an abstract of all the contents of the work before us; but we shall content ourselves with making such selections as we think will prove most useful, and which will lead them to desire to investigate the subject more minutely for themselves, and to seek the work itself to satisfy such a wish.

Dr. Parkes remarks that four methods have been employed in discovering the composition of the urine. In the first, the per-centage amounts of the constituents are alone determined; in the second, the per-centage amounts of the solids are ascertained; in the third, the total amounts of the different urinary constituents excreted from the body in a given time are obtained; and in the fourth, the results are determined not only in relation to a given time, but also to the weight of the individual—as, for example, the number of grains excreted to each pound's weight. The third and fourth methods are evidently those which can be most depended upon in arriving at results of value, either to the physiologist or pathologist; the first and second methods were more used in the older analyses, but, except in certain circumstances, they are not much to be relied upon.

Water of the Urine.—With regard to the quantity of urine excreted in the twenty-four hours by healthy individuals, there are many discrepant statements, and it appears that within the limits of health the amount of fluid may vary very considerably. From a table made by Dr. Parkes, in which the results of the observations of numerous experimenters, both English and foreign, are included, it would appear that in some cases only 35 fluid ounces of urine may be passed; in others, as much as 81 fluid ounces; and even in the same individual considerable variations are seen, which our author considers may range in health about one-fourth above or below the mean amount; but at the same time, he thinks that a greater deviation than the above, unless due to some powerful physiological condition, augurs the presence of disease.

Urea.—The urea is, perhaps, the most important of the solid constituents of the human urine, and its determination a subject of high importance. A very valuable table is given in the work, being a collection of all the more trustworthy analyses which have been hitherto made. The results of this table show that the mean amount of urea passed by adult individuals, living well and taking regular exercise, in twenty-four hours, to be 512·4 grains (troy) or 33·18 grammes; or per hour, 21 grains (troy) or 1·382 gramme. It will be found, however, that the amount of urea is subject to great variations within the limits of health, even from 286·1 to 688·4 grains. Dr. Parkes remarks on this point that the maximum and minimum amounts passed on any one day by an individual are usually about one-fifth above or below his mean amount.

Uric Acid.—The uric acid of healthy human urine is subject to still greater alterations than the urea. From a table given by Dr. Parkes, it would appear that the mean amount passed by healthy adult men is 8·569 grains or 0·555 gramme. The extremes in the same person Dr. Parkes thinks may be taken at from one-fifth to one-third above or below the mean.

Sulphuric Acid.—It appears from a very extensive table given by Dr. Parkes, and made up from the observations of himself and numerous Continental and other experimenters, that the mean amount of sulphuric acid eliminated in the twenty-four hours by healthy males between twenty and forty years of age, is 31·11 grains, or 2·012 grammes; but as with urea and uric acid, the difference between the extremes is great. Even in the same person, the variation is greater than in the case of urea and uric acid, as these latter principles are formed within the body, whereas the sulphuric acid is derived partly from within, and partly from without; that is, it is introduced with the food.

Phosphoric Acid.—The mean amount of phosphoric acid in the same class of individuals appears to be 48·80 grains, or 3·164 grammes; and the same remark applies to the phosphoric acid as to the other ingredients, perhaps even in a greater degree; for the difference between the mean and extremes is more considerable, probably, as Dr. Parkes remarks, because the quality of the food and the amount of phosphates passing into the body varies greatly in different localities. In the same person it may vary from 35 to 50 per cent. Some of the phosphoric acid in the urine is united to earths, some to alkalies. The ratio between the earthy and alkaline phosphates may vary from as 1 to 2½ to as 1 to 7.

Chlorine.—The mean excretion of chlorine, from an elaborate table, is given as 126·76 grains, or 8·21 grammes; but Dr. Parkes considers this average too high, arising from extreme cases in the table. He considers it safer to estimate the amount as from 6 to 8 grammes. The range of the chlorine is very extensive. Dr. Parkes mentions and gives the estimated quantities of many other ingredients, as the hippuric acid, pigments and extractives, creatin and creatinin, the different bases, &c.; but as we shall have few occasions to refer to these substances in this review, we shall content ourselves with a mere enumeration, without making any remarks upon them.

Most valuable information is afforded by ascertaining the quantity of each constituent excreted in the twenty-four hours by a definite amount of body weight, and Dr. Parkes has collected the results of different observers on this point. Lehmann and Vogel, for example, estimate the amount of urea excreted by each kilogramme of weight of the body at 0·500 gramme in twenty-four hours; that is, in the proportion of 3·5 grains to each pound avoirdupois. Dr. Parkes, in nine men, from twenty-three to thirty-five years of age, whose weights were known, as also their true mean excretion of urea, found the mean of the whole to be 0·459 gramme to each kilogramme, or 3·36 grains to each pound avoirdupois. A valuable table is introduced showing the amounts of the more important urinary constituents excreted in the twenty-four hours by a given weight of the body:

Constituents.	In 24 hours, 1 kilog. excretes in c. c. and grammes.		In 24 hours, 1 lb. avoir. excretes in drachms and grs.
Water	23 c. c.	...	2·9 f℥
Urea	0·500 gramme	...	3·53 grains
Uric acid	0·0084 "	...	0·059 "
Hippuric acid	0·0339 "	...	0·237 "
Creatina	0·0043 "	...	0·032 "
Creatinin	0·0064 "	...	0·048 "
Pigments and extractives	0·151 "	...	1·062 "
Sulphuric acid	0·0305 "	...	0·214 "
Phosphoric acid	0·048 "	...	0·336 "
Chlorine	0·126 "	...	0·875 "

In remarking upon these figures, Dr. Parkes states that the quantities of hippuric acid cannot be depended on; and the amounts of creatin and creatinin are from two cases only, estimated by Dr. Thudichum.

A short section is devoted to the consideration of the amount of the acidity of healthy urine, and another to its specific gravity. Dr. Parkes thinks it most convenient to express the acidity by stating that it is equal to so much crystallized oxalic acid, and estimated in this way the amount of acidity of the twenty-four hours' urine in male adults appears to vary from about thirty to sixty grains of this acid. The specific gravity of healthy urine has a mean of 1020, ranging, however, very greatly, from 1012, or lower, to 1030, or higher. The weight is dependent on the solids of the urine; but, as will be at once evident, great changes may take place in the relative proportion of the different constituents without an alteration in the specific gravity.

A section is devoted to the consideration of the origin of the various urinary constituents, and the conclusion is arrived at that the *water* of the urine is derived almost entirely from the water of the liquid or solid food, but that possibly a very small amount is formed in the system; that the *urea* is the product of the metamorphosis of the nitrogenized tissues, and that in the carnivora almost all the nitrogen passes off in this manner; but that in man a portion is eliminated in the form of uric acid, hippuric acid, creatin, &c.; that urea is probably formed from the breaking down of nearly all the tissues, although there may be certain intermediate compounds. That urea is formed from the tissues is proved by the fact that starving animals eliminate it; and from the recent researches of Bischoff and Voit it would seem probable that it is not produced directly from the plastic nitrogenized constituents of the blood, but from the effete products of tissues only. Dr. Parkes remarks that—

"This statement, if true, would settle at last the much controverted doctrine of the 'luxus consumption' of the albuminous food—that doctrine which teaches that the well-known increase of urea in the urine soon after meals, arises simply from the surplus albuminous material (which is not required by the system) being at once converted into urea. The enormous increase in urea has seemed to many persons to completely prove this doctrine; but Bischoff and Voit have now given strong grounds for believing that it is incorrect, and that in this case, as in all others, the increase in the urea proceeds from the increased metamorphosis of the formed tissues, and that albuminous food in the blood can no more be acted on by oxygen than the albumen in the blood of the fasting animal can be so acted on."

With regard to the origin of uric acid this is still a matter of great uncertainty; one also of great practical importance. Can it be formed directly from the food, or does it also result from a disintegrating of some tissue? Uric acid is found to increase in the urine after food, but so does the urea also; and if, as above stated, it is probable that this latter principle is derived from the tissues only, then we cannot well help coming to the conclusion that uric acid has a similar origin; there are certain difficulties with regard to the production of uric acid, interesting alike to the physiologist and pathologist, but which we have no time to enter upon in this review. Our author has noticed and weighed them carefully, and an attentive perusal of his observations will well repay the labour it may cost.

The true origin of the hippuric acid of the human urine has not been satisfactorily determined, and the same remarks applies to most of the other organic constituents. The explanation of the presence of the inorganic salts is more easy; thus it would appear that *sulphuric* acid is derived partly from the sulphates in the food, partly from the metamorphosis or oxidation of the organic compounds containing sulphur; the phosphoric acid also partly from the food, but partly, by oxidation, from the phosphorous tissues; the chlorine directly from the food. With regard to the alkaline and earthy bases, these are obtained from the food, although many of them enter into the composition of the tissues. It is curious to observe the selection of some of these bases; thus in the muscles and the blood corpuscles potash salts predominate, but in the serum of the blood and in bile the salts of soda are chiefly found; and lastly, the bones are extremely rich in lime salts.

Having in the introduction to Part I. spoken of the various constituents of the healthy renal secretion, our author proceeds in Chapter I. to describe the variations in the urinary excretion during health, arising from certain physiological conditions, more especially the influence of sex, age, weight, solid and liquid food, mental and bodily exercise, menstruation, pregnancy, and other minor circumstances.

Influence of Sex.—It would appear that the number and exactitude of the observations on the urine of women have been less than on that of men, and, in fact, little that is satisfactory can be found. There is an idea prevalent that the amount of urea, compared with the weight of the body, is somewhat less in females; but there is some doubt upon this point. As to the other ingredients, the ratio appears to be the same in the two sexes. The deviation in the urea from the mean amount appears to be quite as great in women as in men.

Dr. Parkes gives the following table, from the observations of Becquerel, Lecanu, Bischoff, Mosler, Beigel, and Ranke, made of the urine of twenty women between the ages of sixteen and forty years:

Urine of Adult Women in twenty-four hours.

Ingredients.	In cubic centimetres and grammes.		In fluid ounces and grains.
Urinary water	1303 c.c.	...	42·0 ounces.
Urea	24·61 grammes	...	390·0 grains.
Uric acid	0·473 „	...	7·3 „
Chlorine	6·41 „	...	98·9 „
Sulphuric acid	1·96 „	...	30·2 „
Phosphoric acid	3·64 „	...	56·2 „
Extractives, bases, and other ingredients	9·65 „	...	149·0 „

Influence of Age.—Considerable discrepancies exist in the accounts of the urine of the fœtus; it would appear to be devoid of urea, but it contains nitrogenous matter, which evolves nitrogen gas when treated with the hypochlorite of lime. Some observers have asserted that it is devoid of uric acid; others, on the other hand, have found in it much of this principle. It would appear to be very watery in character, and beyond this, little that is certain is known concerning it. In new-born children, the state of the urine has not been accurately determined, and it is only after children have arrived at the ages of four years and upwards, that we have any accurate analysis of their urinary secretion. Dr. Parkes has given an interesting table, showing the relative amounts of some of the more important constituents of the urine in children and adults, estimated in relation to a given weight (a kilogramme) of the body.

	In children between three and seven years.		In male adults.		Excess in children to each kilogramme.
Water	59 c.c.	...	23 c.c.	...	·36 c.c.
Urea	0·973 gramme	...	0·500 gramme	...	0·473 gramme
Extractives and volatile salts	0·279 „	...	0·151 „	...	0·128 „
Chlorine	0·308 „	...	0·106 „	...	0·202 „

From this table we gather some important facts—first, that in these young children the excretion of urea in proportion to their weight is nearly twice as much as in adults; the excretion of extractive matters is considerably greater in children; whereas the eliminated chlorine is almost trebled; all facts showing indisputably that in children the processes of metamorphosis goes on very rapidly, the tissues being quickly constructed and as quickly disintegrated. As the age increases, it would appear that the metamorphosis gradually lessens until from sixteen to eighteen years of age, when it approaches closely that which occurs in adults.

In middle age—that is, from forty to sixty years of age—very few analyses of the urine in health have been made: insufficient to compare with those of individuals between twenty and forty years. Dr. Parkes remarks that it seems probable that the diminution in the excretion of urea which is so marked in old age, is perceptible to a less extent all through middle life, and he considers that we may provisionally assume that the excretion of urea will diminish about five per cent. between the ages of forty and fifty, and ten per cent. between fifty and sixty.

In old age it would appear, from Lecanu's observations, that the amount of urea is much lessened, but concerning the other urinary ingredients there are great discrepancies.

Influence of Weight.—Dr. Parkes, from analyses made by Beneke, Kerner, and himself, on individuals of about the same age, and of known weights, sums up his conclusions to the following effect:—That there is an increased excretion of urea with an increase of weight of the body, ranging from 0·433 to 0·529 in the twenty-four hours for each kilogramme, but that this is not perfectly regular; the irregularity, he says, may be accounted for by the fact that the weight of the body may not always be dependent on the same conditions; in one man it may arise from large bones; in another from great development of muscle; in a third from fat, and so on; that the metamorphosis of fat produces carbonic acid and water, but no urea; the metamorphosis of bone is slow, and produces but little urea; whereas the change in muscular tissue produces urea in large amounts.

There does not seem to be any relation between the weight of the body and the amount of uric acid excreted.

Influence of Food and of Fasting.—We have next to notice the influence of food upon the urine in health; a correct knowledge on this point is of the highest importance, seeing that food produces a greater alteration than any other condition, and that little that is valuable can be ascertained concerning the changes in the urine in disease until we have made it out correctly.

Dr. Parkes makes the following remarks on the different functions of the solid portion of food:

"Food acts in various ways; it supplies substances which become parts of tissues, or, on the contrary, pass out from the body as rapidly as they entered it; it causes a retardation or acceleration of metamorphosis, according to its kinds, or furnishes substances which enter the body only to be themselves metamorphosed by oxidation or fermentation. Food is composed of different substances, which have different effects. The final result is, therefore, a consequence of very complex actions, and the several stages cannot be readily traced back."

It will be well, first, to consider the effect produced by fasting or more or less complete abstinence from food, either liquid or solid.

When the solid portion of the diet is much reduced, as when an individual who has been taking a full diet is placed upon one which is meagre or poor in character, we at once notice the following changes in the urine. The urea is diminished in quantity—often to one half the former amount, or even less; probably after a short time the fall in the urea is in relation to the wasting of the body.

The uric acid appears to be lessened, but no very trustworthy observations have been made on the subject. We have known it to be lessened or altogether absent after a fast of thirty-six hours; but, on the other hand, we have seen the urine of fasting men loaded with urates. In the latter case, it has been when the abstinence has been from liquids as well as solids. We have frequently confirmed this last statement.

The phosphoric acid is much lessened, also the phosphates of lime and magnesia; the sulphuric acid is to some extent reduced, probably by as much as is derived directly from the sulphates in the food. When the fasting is more severe or total, then the various urinary ingredients become still more diminished in amount, and some of the solids, as the chlorides, entirely disappear.

It is stated by Bischoff and Voit, that in starving dogs the bile-pigment appears in the urine, but not the true organic portion of the bile, as the glyco- and tauro-cholic acids.

When liquids are abstained from, there appears to be a diminution not only of the watery excretion, but also to some extent of the various solid constituents; this latter effect may partly be owing to retention in the blood, but partly to the diminution of the change of tissue which then probably ensues.

Dr. Parkes gives the results obtained by Wundt of fasting from common salt or chloride of sodium, which appear to show that not only is the urinary water lessened to a very considerable degree, but that after the third day the urine becomes albuminous; and on this point he makes the following remarks:

"The lessening in the water of the urine was very marked, and leads to the notion that chloride of sodium may play an important part in the diffusion of fluids. The production of albuminuria is a most important observation, and it is very desirable the experiment should be repeated. It seems to support the idea that the freedom of the healthy urine from albumen is owing to the albumen, during its transit through the renal tissues and epithelium, being rendered insoluble by the action of acids in the presence of chloride of sodium."

As we have seen that fasting produces certain effects upon the urinary secretion, we should naturally expect that the taking of food would cause the opposite condition: both the fluid and solid portions of the urine are found to be augmented by taking solid food, an effect which shows itself an hour or two afterwards, and continues for ten or even more hours, producing urine having the characters of the so-called "*urina cibi.*" The urea is uniformly increased, sometimes doubled; the increase commences even from the first hour, attaining its maximum from the third to the seventh hour after a meal. Our author here remarks:—

"The increase in the urea so soon after food has given rise to the famous controversy between Schmidt and Bidder, Frerichs and Lehmann, on the one hand, and Liebig and Bischoff on the other, as to whether some portion of the food is at once converted partly into urea (luxus consumption of Bidder and Schmidt), or whether it merely causes a vast increase in the rapidity of tissue-change, by the increase in the rapidity and force of the circulation, by the action on the nervous system to which it gives rise, or by an immediate chemical action on tissues. To reconcile these different views, F. Fuhrer and H. Ludwig suggested some time ago an intermediate view — viz., that the increase in the urea is not owing to direct transformation of food or to increased metamorphosis of the solid tissues, but to increased disintegration of blood-cells. The point is one of great importance in practical medicine. If urea be formed direct from food, it ceases of course to be a measure of tissue-change except during fasting-hours; and if during health some portion of the food is thus eliminated as urea, without taking any part in the formative processes of the body, it is quite possible that in disease the same thing might go on to a still greater extent, and that even the whole of the nitrogenous nourishment might emerge as urea without forming, even for a time, a component part of any tissue. It is conceivable, indeed, that there might be a large discharge of urea from directly metamorphosed food, with an absolute lessening in tissue destruction. However, the latest observations of Bischoff and Voit seem to have almost settled the controversy, and to have proved that urea is always a product of tissue metamorphosis, and that in flesh feeders, at any rate, it is an exact measure of this."

The uric acid is supposed by some to be less affected by food than the other urinary ingredients, but still it has been shown clearly, both by Dr. Bence Jones and Dr. Ranke, that it is increased after a nitrogenous meal.

Both the sulphuric and phosphoric acids are increased, especially the phosphoric, combined with earthy bases.

The sulphuric acid is partly derived from the sulphates of the food. The chlorine at first appears to be lessened—that is, when the digestion is very active, but it is afterwards excreted in large quantities.

The various bases appear to follow the law of the acids with which they are combined.

Much controversy has taken place with regard to the effect of food on the acidity of the urine, and the subject has within the last few years been carefully studied, and great light thrown upon it. Dr. Bence Jones, from numerous experiments, concluded that the acidity of the urine lessened during digestion, and sometimes even became alkaline. Others, as Winter, found results differing somewhat from those of Dr. Bence Jones. Beneke, again, from experiments on himself, rarely found diminution of acidity after meals, and denies altogether the influence of food. Dr. Roberts, from very numerous trials, found results entirely agreeing with Dr. Bence Jones; and lastly, Dr. Hermann Weber furnished the author with results of experiments made on his own urine, and carried over a period of two months, which to a great extent are confirmatory of the original statement. In Dr. Roberts' experiments, the acidity was most diminished the second hour after breakfast, and in the third, fourth, and fifth hours after dinner. The effect of mixed and pure animal diet was the same; vegetable food had a feeble effect at first, but when used for several days, its action was equally powerful. Dr. Parkes, speaking of the cause of this alkalinity, remarks that it must be due either to the increased introduction of alkali into the system (as no fixed alkali can be formed), or its dislocation from its usual acid. Both causes may be in play, and although the immediate effect of food is to lessen acidity, yet the remote effect is to increase it, for food increases the chemical changes in the body, which give rise to the production of sulphuric, phosphoric, hippuric, and uric acids.

After speaking of the influence of food in general, our author next enters into the consideration of special kinds of food.

Nitrogenized Animal Food.—The water is increased if pure albumen is taken, and there is no water in the diet.

The urea is much increased, apparently in proportion to the nitrogen of the food, when the body does not gain weight. Bischoff noticed that a dog which, on 1200 grammes of meat, gave daily 88 grammes of urea, passed 181 grammes on 2660 grammes of meat; without food, the same dog passed 12 grammes of urea; in man this point requires to be more fully determined.

The uric acid is also increased by animal food, though relatively less than the urea. The Rev. Mr. Haughton's observations (to be noticed afterwards) confirm these statements fully, both with regard to the urea and uric acid.

The hippuric acid is much lessened; sometimes it is said that it altogether disappears.

The sulphuric acid is much increased, partly from the increased introduction of sulphates, partly from increased oxidation of sulphur; and it has been noticed by Bischoff and Voit, that in dogs the unoxidized sulphur is also greatly augmented.

The same increase is found in the phosphoric acid, arising from the same causes, especially the greater ingress of phosphates. Dr. Parkes, in concluding this subject, states:—

"The importance of bearing these facts in mind when the urine of disease is examined, is obvious. In some diseases, as in diabetes mellitus, a strict animal diet is often enforced, and the large amount of urea and sulphuric acid which is sometimes found in this disease may be attributed to the diet alone. In fevers, too, the diet is often highly nitrogenous, and some portion of the large amount of urea found in these cases may be owing to this."

When non-nitrogenous food is taken.—The water is stated to be lessened.

The urea greatly lessens, becoming less than one-half, probably depending partly on the lessened ingress of nitrogen, and partly on the appropriation of the oxygen by the non-nitrogenized food.

The uric acid is also lessened to about one-half.

Nothing is known concerning the hippuric acids, nor anything certain of the sulphuric acid, phosphoric acid, or chlorine.

The free acidity appears to lessen, and the urine may even become alkaline.

When fat is added in excess to ordinary diet.—Our space will not allow us to enter fully into this subject; but we refer our readers to a most instructive summary of our author of Bischoff and Voit's latest experiments on the influence of fats on the metamorphosis of the tissues and on the urinary excretion. The practical result appears to be this—that in men on good diet an excess of fat will simply be stored up in the body, and will not be likely to cause any lessening of the urinary excretion, though in ill-fed or starving persons it would do so to some extent. Dr. Böcker, in his experiments on four healthy young men, using first a diet without butter, then one with from two to ninety grammes, found no influence produced by the fatty matters on the water, urea, uric acid, sulphuric acid, phosphoric acid, magnesia, soda, or potash of the urine.

When sugar is added in excess to ordinary diet.—In men, sugar, when taken in large quantities, can be detected in the urine.

It increases the flow of water, even when none enters the body.

The urea appears to be lessened, often considerably.

The uric acid is lessened, but the influence on the hippuric acid is unknown.

The sulphuric acid is probably diminished, and the phosphoric acid certainly decreased; and this applies more especially to the phosphate of lime.

Influence of vegetable food.—When the food consists solely of ordinary vegetables, it produces marked changes in the urinary constituents.

The water sometimes remains normal, is sometimes increased.

The uric acid is lessened somewhat.

The hippuric acid is largely increased, even though no benzoic acid or benzoyl compounds be introduced.

The sulphuric and phosphoric acids are probably lessened, and the chlorine increased from the larger amount derived from the food.

The different bases are augmented from the same cause.

The acidity is lessened, and the urine often rendered alkaline, and hence the frequency of deposits of the phosphates of lime and magnesia.

Oxalate of lime is often present when certain vegetables, as rhubarb, sorrel, onions, turnips, &c., have been indulged in. The above remarks appear to apply in general to fruits, as well as what are ordinarily termed vegetables.

Special articles of diet.—It is important that the influence of some of these should be determined, especially those in very common use, and we will select *gelatine.* It has been found that this body does not pass off as such in the urine, but is entirely destroyed, causing a large increase in the amount of urea. Dr. Parkes remarks:

"In being thus oxidized, gelatine appropriates oxygen, which would otherwise act on tissues, and thus, like fat and sugar, lessen metamorphosis. But Bischoff, from his last experiments, questions whether it does not play a more important part than this; whether, in fact, it does not enter into the composition of the tissues, and thus take, in part, the place of albumen. In either case, the practice of giving strong jellies in fevers, and in cases in which it is wished to arrest metamorphosis, is sound; for either gelatine is a true aliment, or, if not, it is an absorber of oxygen, and thus limits metamorphosis. The effect of gelatine on the other constituents of the urine is not known."

Lastly, we will notice the effect of liquids on the composition of the urine: and first of *water*.

A. *When large quantities of water (of mean temperature) are drunk in a fasting system.*—The excess is thrown out chiefly by the kidneys, producing a urine of low specific gravity and pale in colour, known by the name of urina potûs.

The urea is augmented at first, but afterwards sinks below the normal amount, producing a kind of compensation; it seems probable, however, that the urea is really altogether increased from an augmentation of tissue change.

The uric acid lessens or disappears, according to Böcker, but, as Dr. Parkes remarks, it is difficult to determine this with certainty when the total fluid is much increased. The sulphuric acid is not increased, the phosphoric slightly lessened, and the chlorine augmented at first, but it afterwards falls below the average, as if only carried away mechanically; this is rendered the more probable from an observation of Ferber, who noticed that 1200 cubic centimetres of water caused as much excretion of chlorine as 1800.

B. *When (the mode of life being equable) the diet is the usual one, or is regulated for the experiment, and when a large additional quantity of water is taken during the twenty-four hours, the following are the results:*—

The water is increased.

The urea is augmented. It was found by Genth that when very large quantities of water were taken, as much as 216 grains of urea per diem were passed, in excess of the amount passed with the same diet, but without the increase of water; and this for some time.

The uric acid is lessened, as when the water is taken by a fasting person.

The changes in the amount of hippuric acid are unknown.

The sulphuric acid is decidedly increased, the phosphoric acid less so, and the chlorine much augmented at first, but not afterwards.

Water, when taken in these large quantities, causes loss of flesh, unless the digestion and assimilation are good, and plenty of food be given to supply the place of the wasted tissues.

In actual practice we have often found much good effected by taking into consideration this action of water; it must be remembered also, that when it is taken largely with solid food, the digestion is often much impaired.

The effects of *alcohol*, in its different forms of distilled spirits, wines, and beers, are fully discussed; also the influence of tea, coffee, cocoa, &c., upon the urinary ingredients: these, however, are all sufficiently alluded to in an admirable summary which we cannot refrain from bringing before our readers, although somewhat lengthy; its importance, however, will fully justify its insertion.

"The various liquids ordinarily taken as food are thus divisible into two great classes, those which favour and those which more or less retard urinary excretion. To the first belong water and the lighter wines; to the second alcohol, strong wines (probably), strong beer, tea and coffee. The retardation of excretion produced by the second class is, however, not shown merely by the urine, but in several cases also by the pulmonary, cutaneous, or the intestinal excretions. It can, therefore, scarcely be doubted that the action of these substances is not merely that of retaining certain excretions in the body, but that of absolutely lessening their formation. They appear, in fact, to retard metamorphosis, though in different degrees and in different directions; some acting remarkably on the urine, but less on the intestines, as coffee; others influencing the urine very little, but the bowels more, as tea. These substances differ also in their mode of action, even on the urine: thus tea appears to slightly lessen the uric and phosphoric acids, while it scarcely affects the urea; while coffee not only affects the chloride of sodium, but lowers remarkably the urea, the uric acid, and (probably) the phosphoric acid. No doubt, also, other differences will be made out, and each substance will be found to have its specified action.

"Although the effect on the urine of these substances is thus tolerably well known in systems which are carefully arranged for the experiment, it remains to be seen how far in actual life their effects are counteracted by custom, habits, or peculiarities of race or climate. It is impossible to suppose that every drinker of spirits has always such a small secretion as occurs in the temperate man, who suddenly (for the sake of the experiment) introduces a large quantity of alcohol into his body. The effect of the alcohol may be counteracted by other conditions, such as the use of large quantities of water, of great exercise, or like agencies, which augment metamorphosis.

"The exact steps of the process by which this retardation of metamorphosis is brought about are not yet known; we see only the results. Is it from simple appropriation of oxygen, or from some more complex action of nutrition directly, or through the medium of the nerves? Is the process of the building of the tissues equally delayed with that of their unbuilding? In many cases, the use of these agents in large quantities lessens the desire for food, and the body maintains its weight on less nutriment than would have been the case with water or light wines.

"How far this effect is a good one, how far the normal rapidity of metamorphosis (such as occurs with moderate water-drinking) can be advantageously checked by the use of such substances, is a social problem of the highest importance. It seems to me that the obvious deduction from our present physiological knowledge is, that the more rapid the healthy metamorphosis of the body, within certain limits, the more urea and pigment are formed, the more perfect is nutrition, *as long as nutriment is supplied in sufficient amount*, and as long as the formative powers can use it. In the immense excretion of children and in the retarded metamorphosis of old age we see the two ends of the scale, and have proof that growth and progress are corollaries of rapid metamorphosis and elimination. Have we, then, a right to conclude that anything which impedes healthy metamorphosis is hurtful, and that, in checking disintegration, it will equally check formation? Perhaps without going at present quite to this length, we may believe that the most perfect condition of health is rapid building and rapid unbuilding; and all the most strengthening hygienic means, as exercise, sea-air, saline baths, and abundant nutritious animal food, act by forwarding both these processes. Appetite increases, but at the same time the action of the eliminating organs is also increased; the body gains weight, although there must be increased rapidity of the molecular currents and of chemical changes. The training for the ring may be taken as an illustration of my meaning. The prizefighter eats largely of animal food; he thus, if Bischoff's and Voit's experiments be received, increases both the formation and disintegration of tissue; and it is to be presumed that the excretion of urea during training must be increased. The prize-fighter brings into play another factor of elimination, for he gradually increases his muscular movements to an enormous extent; and by so doing, he must absorb much more oxygen than usual, and give out more carbonic acid.

"All the three great factors of metamorphosis—viz., the nitrogenous food, oxygen, and movements, are thus increased; and the amount of metamorphosis must also go on augmenting, up to a certain point, as the bulk of the tissues increases.

"So far the prizefighter may be said to follow the dictates of common sense; but now how does he act with regard to alcohol, and wine, and the substances usually supposed to give strength, and to limit the necessity for food? Why, he almost discards their use; he takes no spirits, no wine, only a little weak beer (which he might with advantage leave off), but drinks to any amount of pure water, or fluids equivalent to it; and thus taught by experience, he employs another most potent agent in elimination. Under this *régime* his health improves wonderfully; he can bear any fatigue, morbific causes are comparatively inoperative, injuries are more easily recovered from, and for the time he is the very type of health and vigour. That the class is not a healthy one is owing to the reckless living between the periods of training.

"If, then, nitrogenized food is abundant, the use of substances to check metamorphosis, within physiological limits, seems unphilosophical. If the food be insufficient, then the use of these substances may become desirable. Yet, as Liebig says, the use of alcohol, coffee, tea, and such substances, is so universal as to look like instinct; and some decided benefit must be at the bottom of a custom so general. The explanation usually given, that these substances are taken because, by lessening metamorphosis, they lessen the desire for food, and maintain the body at the same weight with less nutriment, may account for some, but not for all cases; and it is most probable that their immediate sensible effect on the nervous system is that which is wished for by those who take them, and that, as already said, any part of their action which might be injurious is commonly counteracted by other habits which are almost as instinctively adopted as is the use of these substances themselves. Yet I must candidly say, with regard to the stronger alcoholic liquids, that what study I have been able to give to this subject, and to the causation and treatment of disease generally, have led me more and more to adopt the view of Carpenter and others, and to believe that the use of alcohol in health is not only unnecessary, but is absolutely injurious. The effect of light wine, beer, tea, and coffee, in impeding metamorphosis, is, however, so inconsiderable, and their irritating effects on tissues so slight, that there seems nothing to urge against their moderate use."

Dr. Parkes goes on to speak of the action of liquids in disease; but on this subject we must refer the reader to the work itself, which will amply repay any time spent upon it. The latter portion of the first book is devoted to the consideration of many other points which we have not time to dwell upon. The more important of these is the effect of various mineral waters, and the variations in the urine during health from the use of medicinal agents, both organic and inorganic. These points are most elaborately treated, and every fact really determined concerning the action of the different agents will be found fully discussed.

The Second book of Dr. Parkes' work commences with an introduction, in which are described the various morbid constituents which have been from time to time discovered in the urine in disease, whether occurring in solution or in the form of crystalline or amorphous sediments. This portion of the work we must also pass over in silence, not, however, that there is any lack of interest in it, but from want of space, and we must at once proceed to investigate the changes of the urine during the progress of different diseases.

In commencing the first chapter devoted to the composition of the urine in disease (Chapter III. of the work), Dr. Parkes thus states the object of investigating the changes in this fluid:

"The urine is examined in disease for two purposes—1st, to discover the condition of the urinary organs; 2nd, to determine, as far as a single excretion will permit, the course of the abnormal metamorphoses of tissues in the body which lead to alterations in the composition of the several excreta. To determine this with complete exactness, we require to understand thoroughly the normal metamorphosis of tissue, and the changes produced in it by disease. We are still, however, almost entirely in the dark on this great topic, and the facts now known respecting the urine in disease can be regarded as merely the preliminary inquiries necessary to prepare the ground for subsequent complete investigation. It is of course extremely desirable, in disease as in health, that the urine should not alone be examined, but should be regarded in connexion with the excretions of the skin, bowels, and lungs. Everything in the body takes place with the most rigorous chemical exactness, and changes in one excretion must be intimately and accurately connected with changes in the others."

After adverting to the many difficulties which there are in investigating accurately the urine in disease, owing to the influences of the various physiological conditions which are active, to the frequent inability to weigh sick persons, especially when suffering from acute disease, and of knowing the amount of the excretion of the same individuals in health, Dr. Parkes gives certain empirical formulæ designed for calculating the urinary excretion in a sick person whose normal excretion is unknown. These are of so much interest that we will present them to our readers:

"1. Ascertain the weight of the person in pounds avoirdupois.

"2. Multiply the following figures by the weight; the result is the excretion in grains in *twenty-four* hours:

	In men between 20 and 40.	In women between 20 and 40.	In children from 3 to 8.	In children from 8 to 16.	In young men and women from 16 to 20.
Urea	3·53	2·96	6·83	5·20	4·39
Chlorine . . .	0·875	0·817	1·44	1·097	0·926
Sulphuric acid .	0·214	0·25	0·414	0·315	0·266
Phosphoric acid	0·336	0·65	0·65	0·495	0·418

"Make a further correction for middle and old age, by calculating according to columns 1 or 2, and then deducting 10 per cent., if the person be between forty and fifty; 20 per cent., if between fifty and sixty; 30 per cent., if between sixty and seventy; and 50 per cent. above seventy.

"These numbers are quite provisional, as the analyses are so few. Future observations will confirm or refute them.

"4. Correct for diet:—If the persons have been starving for two or more days (as in some fevers), deduct one-third from the calculation, when made according to the table. If the diet be meagre, deduct one-eighth or one-sixth; if pretty plentiful, but still below that of health, deduct one-tenth.

"3. Correct for movement:—If there be total inactivity, deduct one-tenth; if merely quietude, deduct one-twentieth.

"Corrections should also be made for the amount of fluids drank, the action of the external temperature, and the condition of the eliminating organs, &c.; but I am at a loss how to determine the proper figures for these agencies. Their amount, however, should never be disregarded."

Dr. Parkes quotes a case of his own, showing the working of the plan. A healthy man in the hospital weighed 114 lbs. Required, the physiological amount of urea in twenty-four hours:

$$3·53 \times 114 = 402·42 \text{ grains.}$$

Correct for moderate diet, deduct one-eighth:

$$402·42 - 50·3 = 352·1 \text{ grains.}$$

Correct for bodily inactivity, deduct one-twentieth:

$$352·1 - 17·65 = 334·45 \text{ grains.}$$

The physiological amount determined by actual experiment (mean of six days) in this man was 346·7 grains, being only 12 grains above the calculated amount. Should we not be able to ascertain the weight of any patient, a rough estimate can generally be made from his appearance.

Our space will not allow us even to allude to the very interesting remarks of our author on the best means of avoiding the many difficulties which surround the whole subject; but we shall at once pass on to the consideration of the alterations in the urinary secretion in some of the more important diseases considered in the work. The first acute affection spoken of is intermittent fever.

Chief of that which has been clearly made out concerning the changes in the principal constituents of the urine in ague is exhibited in the annexed Table from Mr. T. Ringer's paper, published in the 'Medico-Chirurgical Transactions,' 1859; and it will be interesting to observe that a very close relation exists between the temperature and the amount of urea.

Excretion of Urea, Chloride of Sodium, and Water on one day (Quotidian Ague, untreated).

Time.	Temperature.	Urea in grammes per hour.	Chloride of sodium in grammes per hour.	Water in cubic centimetres per hour.
From 3 P.M. April 6th to 5·30 A.M. April 7th	98° Fah.	0·684	0·029	36
From 5·30 A.M. to 7	97°·5	0·666	0·029	25
From 7 A.M. to 8 A.M. just before shivering	Rose from 98°·2 to 99°·8	1·361	0·384	80
From 8 to 8·30, cold stage, shivering at 8	Rose from 99°·8 to 103°·4	2·176	0·612	136
From 8·30 to 10·30, hot stage	Rose from 103°·4 to 105°, then fell to 102°·4.	1·287	0·187	62
From 11 to 5, sweating	Rose from 102°·4 to 103°·4, then fell to 98°·6	0·935	0·064	40

The weight of this man was 65 kilogrammes, his age was fifty-nine; the normal excretion may be calculated, according to the rule before given, as 23·16 grammes in twenty-four hours. It appears, therefore, that the urea in the apyretic hours was below the normal average, but greatly increased during the cold stage of the fever.

The uric acid appears to be lessened; of the other ingredients little is known. Albumen is frequently found during the fit.

The results of a very elaborate investigation of the changes in the urine in cases of typhoïd fever is given, and the various analyses of different Continental observers are carefully collected and discussed; but Dr. Parkes admits that "the study of the urine in typhoïd fever throws at present no light on *diagnosis*, as the characters are common to so many febrile affections." But again he states:

"The *prognosis*, in severe typhoïd fever, has appeared to me to be more favourable in proportion to the free excretion of urea and uric acid. As a certain amount of urea and uric acid must be formed in every case, in proportion to the fever (the temperature?), and to the susceptibility with which the tissues of the individual undergoes metamorphosis, it might be assumed that the excretion of these effete substances is a most necessary point. This is confirmed by observation; for although a large amount of urea and uric acid indicates a high febrile state, and therefore so much the more danger, yet it is undoubted that there is more peril in retention than in any amount of fever and formation with elimination. The greater the excretion in typhoïd fever the better; and as long as 500 to 700 grains of urea in men, or 300 to 500 in women, are being passed in each twenty-four hours, the prognosis is so far favourable. But whenever, when the fever continues, the urea falls much below these amounts, we may anticipate that, in consequence of it, or of the cause that produced it, a low typhoïd condition may come on; or that (probably) some local inflammation, as pleurisy, may take place, and relieve the blood for the time from some of the effete products."

Dr. Parkes states that in 7 out of 21 of his own cases of typhoïd fever, albumen occurred in the urine; in 5 it was temporary only; in 2 permanent, or at least present for a long time afterwards. In typhus fever, albumen is very much more frequent than in typhoïd; some observers have found it in all cases of the former disease. In the other exanthematous diseases, as variola, rubeola, and scarlatina, there have been no peculiar changes observed in the urine, except the extremely frequent presence of albumen in the latter disease, in which the kidneys became implicated. Under this head some interesting observations will be found in relation to the pathology of scarlatinal dropsy, and of the possibility of its occurring without the presence of albuminuria.

The next class of diseases from which we shall select, include acute affections of certain organs of the chest and abdomen, and we shall first speak of *acute lobular pneumonia.*

The more important results obtained in this disease may be thus summed up: the watery portion of the urine is much lessened, often to one-half the healthy average, and this is not dependent on any diminution in liquids drank, nor can it be accounted for by the sweating; it is probably retained, and then excreted after the pyrexia has ceased.

The urea is always greatly increased during the height of the disease. Dr. Parkes in one case found one day as much as 1321 grains, and other observers have detected a large increase beyond the healthy average. Now, that this is not due to food can be readily shown, for in the above-mentioned case the patient had been starving for several days; the augmentation is doubtless due to the pyrexia. In pneumonia the specific gravity of the urine is high, a circumstance arising from the diminution of the watery portion of the fluid and the large excess of urea.

The uric acid is also augmented, and amounts ranging from twenty to nearly thirty grains have been obtained. Sometimes, however, the increase is not found until resolution has taken place, a fact accounted for by supposing this acid to be retained in the blood, from the difficulty experienced by the secreting organs in its elimination. The chloride of sodium is greatly diminished, or even entirely absent, in the early stages, but reappears on the occurrence of resolution. It is a curious fact, that even when common salt is given in large doses in these cases, none appears in the urine.

With regard to abnormal principles, it would appear that albumen is frequently present, even, in a large proportion of the cases, as much as 45 per cent. Collecting the observations of German, French, and English physicians, it seems to occur most frequently during the height of the disease, possibly when the chlorides are most deficient; when it occurs before resolution has taken place, it seems to be an unfavourable sign.

Now and then cases are found in which the excretion of urea and other urinary solids is deficient, but it would seem that in these exceptional cases retention has taken place, and *not* diminished formation.

In *acute pleurisy* it appears that the urine acquires the condition common to all true febrile affections with exalted temperature, but in a much less marked degree than in pneumonia. In some cases of pleurisy the chlorides are very deficient, and the deficiency is probably connected with rapid effusion into the pleural cavities of a fluid rich in these salts.

Albumen probably never occurs in genuine pleuritis, if uncomplicated with antecedent renal disease. Our knowledge of the nature of the alterations produced in the urinary excretion by the presence of *acute bronchitis* is at present very imperfect. In some cases a considerable augmentation of the urea has been noticed; in others, a marked diminution. Dr. Parkes is inclined to look upon this last phenomenon as due to non-excretion, and remarks that, in diseases accompanied by considerable impediments to aëration, as pneumonia and intense capillary bronchitis, it is most commonly found.

The urine in severe bronchitis often contains albumen.

In *acute pulmonary phthisis* it seems probable that the urine has the same characters as in pneumonia, the water being lessened, urea and uric acid augmented, and the chlorides very deficient or altogether absent.

Passing over the consideration of the state of the urine in acute *hepatic* and *cardiac* affections, in which nothing very satisfactory has been determined, or which can be practically applied in the investigation or treatment of disease, we come to certain acute diseases of the joints, and especially acute rheumatism, or rheumatic fever and gout.

In *acute rheumatism* the quantity of urine is usually deficient, the specific gravity high, and there are very frequently copious deposits of urates, more or less coloured. The water is deficient; this may partly arise from copious perspiration, but it even occurs when the skin is dry and hot, showing the probability of retention.

The urea is increased, Dr. Parkes thinks from his own observations, about one-fourth or fifth over the physiological amount.

The uric acid is also somewhat augmented, sometimes nearly doubled in amount.

The chlorides are diminished; at times they even disappear; but upon the whole, the diminution of these salts is less marked in acute rheumatism than in acute pneumonia.

The sulphuric acid is likewise much increased in true rheumatic fever.

With regard to the acidity of the urine, it is still somewhat doubtful whether the total amount of free acid eliminated by the kidney is much greater than in health.

Albumen seldom occurs in the urine in acute rheumatism, unless from antecedent renal affection.

With regard to the presence of other abnormal ingredients—as lactic or acetic acids—nothing of a satisfactory character has been made out.

In *acute gout* the urine has been made the subject of extensive research. In order that no confusion should exist with regard to the nature of the disease, Dr. Parkes states:—" I define gout, after Garrod, as a febrile affection, with inflammation about the joints, leading to a deposit of urate of soda;" and most of the analyses are taken from Dr. Garrod's work on the subject, especially those having relation to the variations in the uric acid. The examinations of the urine have been made at three periods—1st, between the paroxysms; 2nd, during the paroxysm; and 3rd, after the paroxysm.

Between the paroxysms, gouty patients on an average throw out less uric acid than other individuals; and from examinations of the blood, it appears that it is frequently retained in that fluid.

During the paroxysm of acute gout, the water of the urine is a little below the normal amount.

The urea appears to be but little affected; the mean amount found by Dr. Garrod was 320 grains; in one case of gout, Dr. Parkes found 323 grains.

The uric acid is lessened from retention in the blood. Dr. Garrod found 3·62 as the mean of seven cases.

The sulphuric acid has been found in its normal amount.

Albumen is very frequently present in small quantities, usually only temporary; but in chronic gout there is often a slight permanent albuminuria.

There is frequently oxalate of lime in crystals.

Dr. Parkes in one case found indigo by simply adding a little hydrochloric acid.

After the gouty paroxysm there is not uncommonly a considerable discharge of uric acid, indicating the previous retention of this principle in the blood; all analysis of the circulating fluid confirms this fact. Dr. Parkes remarks, in speaking of the influence of medicines in gout, that "Dr. Garrod has proved conclusively that colchicum does not increase the amount of uric acid."

After having detailed the changes of the urine in acute diseases, Dr. Parkes draws certain general conclusions which are of considerable interest; and the following passages give in comparatively small space the more important facts—these may be studied with profit.

"As the general febrile state is common to all the diseases considered in the preceding section, so the condition of the urine, which is so closely connected with augmented temperature, is found in all. There are, however, some specific differences; and it is probable that, as inquiry advances, our knowledge of these differences will become much more precise than it now is.

"These differences may arise:—

"1. From the locality of the disease. Whenever an organ is affected, the metamorphosis of which furnishes any special products, such products may be found in excess in the urine. Thus the phosphates increase in inflammations of the nervous tissue; the bile acids in some cases of hepatitis; and, in all probability, the uric acid in splenic engorgement; the chlorides are especially lessened in pneumonic exudation, as they are poured out with the albuminous mass into the lung; in milk fever sugar is found in the urine, &c.

"2. From the presence of abnormal substances peculiar to the disease. At present it can scarcely be considered that we have sufficient proof of this cause. Perhaps some compound rich in sulphur may be present in rheumatism; and perhaps organic acids, unusual in healthy urine (valerianic, formic?), will hereafter be found there.

"3. From a perversion of metamorphosis special to a disease. On this head we at present know scarcely anything. The existence of leucin, tyrosin, and peculiar extractives, in the urine of acute yellow atrophy of the liver, is perhaps owing to the peculiar change in that organ.

"4. From the course of the disease, and especially from the variable action of other eliminating organs. For example, the presence of diarrhœa in typhoïd fever modifies the excretion of the urine. So also in acute gout, there are peculiarities of renal secretion which sometimes distinguish the urine of that disease from all others.

"5. From impediment to excretion from kidney implication, or without it (gout?). Kidney implication is not special to any disease, and may be absent in all; but it occurs more frequently in some, and sufficiently so, in fact, as almost to make specific differences.

"None, however, of these differences are so great, or so constant, as to aid us in differential *diagnosis*. The general febrile characters, so to speak, override the others; and at present, from the urine alone, we cannot tell whether the case be one of typhoïd fever, small-pox, rheumatism, or pneumonia. The great uses of the examination of the urine are for *prognosis* and *treatment*. The amount of metamorphosis is a good indication of the severity of the disease, and may indicate the necessity of restraining it; or, if exudations are to be got rid of, a copious excretion may show us that the chemical alterations ending in elimination are proceeding satisfactorily without aid. On the other hand, we may detect, by the urine, insufficient elimination in a disease, in which the thermometer indicates a great metamorphosis, and measures may have to be taken to augment such elimination.

"The urine has seldom been examined *before* an attack of pyrexia, except in gout and ague, neither of which diseases can fairly be taken to represent the probable course in other affections; because, in reality, between the fits of ague or gout there is no complete absence of the disease, but merely another form of it.

"It would, however, be important to know if in other diseases, prior to the actual commencement of augmented temperature, there is deficient urinary elimination.

"After an attack of pyrexia, as already said, the urine takes on characters entirely the reverse of those in the febrile period. The chloride of sodium and the water is augmented; the urea, uric acid, and pigment are below even the average of health. There is, indeed, a delayed metamorphosis, which is evidenced not only in this way, but also, probably, by the presence of oxalic acid in increased quantity; at any rate, oxalate of lime crystals have been shown by Walshe to be more frequent in convalescence than at other times."

Chapter IV. is devoted to the investigation of the urine in chronic diseases, which are provisionally divided into diseases of organs or tissues not renal, and diseases of uncertain origin, but which are distinguished by prominent urinary symptoms. We shall select a few of these, and show the more important deductions which have been arrived at.

In certain chronic nervous diseases, as *epilepsy, hysteria,* and *chorea,* nothing of importance is given; the well-known watery urine of hysteria is alluded to, and attributed to some peculiar condition of the nervous system influencing the kidneys. In spinal paralysis the alkaline state so commonly observed is referred to ureal decomposition caused by the cystitis, rather than to the secretion of true alkaline urine from the blood.

In chronic diseases of the lungs, as *phthisis,* the condition of the urine is said to differ little from the normal, except when complications arise, and these, as diarrhœa, copious expectoration, and febrile disturbance, produce a marked change for the time.

In *chronic cardiac* disease, again, there does not appear to be any constant deviation from the normal condition, but changes ensue when the disease is complicated with dropsical effusions; albumen is apt to be present when there exists beyond a certain amount of pressure in the renal circulation, and this may be altogether independent of structural disease of the kidneys.

In chronic diseases of the *spleen* and in *leucocythemia,* the uric acid appears to be markedly increased: hypoxanthine has been discovered in the urine.

In the various forms of *dyspepsia* depending on structural or functional disease, the urine becomes altered in different ways, and the various states in which the urine is found are thus enumerated by Dr. Parkes:

"1. *Quantity.*—As a rule, perhaps the urine is not lessened in quantity, unless there be either great vomiting or hepatic congestion.

"2. *Acidity.*—Alterations in free acidity are at present the more important signs connected with the urine. The normal condition of the urine during digestion may be considered to be a lessening of acidity. In some dyspeptic persons, however, the acidity of the urine after food is said not to follow this rule, but to increase. If so, there would be either a deficient supply of acid to the stomach, so that there is more secreted by the urine, or there is a production of abnormal acids. The experiments, however, are so few in number, and have been so incompletely made, that it is unsafe ground for conjecture.

"In some dyspeptic cases, especially those attended with torpid digestion or with acid vomiting, it has even seemed to me that the urine is more alkaline than usual—at least, without food being apparently richer in alkaline substances. I have seen the urine passed two or three hours after a meal so alkaline from fixed alkali, that the earthy phosphates were largely precipitated —much more largely, in fact, than I have ever seen in health. Are there, then, two conditions of the urine in dyspepsia—abnormal acidity and abnormal alkalinity?

"When the urine is more acid after food, it is also often scanty, and deposits urates and oxalate of lime. The symptoms connected with this state are chiefly cardialgia, nausea, and frontal headache.

"3. *Colour.*—The alkaline urine is usually pale; the acid urine is deepish in colour—perhaps, however, merely from concentration.

"4. *Urea.*—The amount of urea depends on the power of digestion. Sometimes it is very small. Often, however, it is relatively larger in the urina cibi, as the water is lessened.

"5. *Uric acid.*—It is often great in the urina cibi, and is said by Lehmann to be increased absolutely as well as relatively; but quantitative determinations are still wanting. Dr. Prout thought that badly and half-digested food gave rise to a large amount of uric acid.

"6. *Chloride of sodium.*—The amount depends very closely on the amount of food and digestion; it is small where digestion is imperfect, and as a rule it would appear that the more chloride is in the urine, the more perfectly must it be presumed that digestion has been carried on.

"7. The amounts of the sulphuric and phosphoric acids have not been determined; both are probably influenced by the very variable amount of food taken in different cases.

"8. Abnormal ingredients are not necessarily present. Albumen has been said to occur after certain kinds of indigestible food; but further evidence is required on this point. Sugar also has been found in some instances. Oxalate of lime crystals are perhaps more common than in other chronic diseases."

In chronic diseases of the skin, little that is satisfactory concerning the alterations in the urine has been made out; it was thought that where boils were present the urine was usually saccharine, but such is certainly not commonly the case.

In some diseases of the bones, especially in *rickets*, a very large quantity of the earthy phosphates have been often found in the urine, arising in all probability from increased metamorphosis of bone tissue. Again, in mollities ossium a peculiar protein has been discovered in the urinary excretion.

There are certain diseases of the blood which produce changes in the urine which have been investigated—such as *anæmia* and *chlorosis;* here we find a considerable diminution in the urine pigment—sometimes to one-fourth or one-sixth of the normal amount; the urine is also pale, less acid or neutral, of low specific gravity, and its quantity as much or even above the healthy average. The urea is not much decreased, but the uric acid is in small quantities only.

Dr. Parkes, in concluding this section, remarks that—

"The condition of the urine corresponding to the other quantitative changes in the blood—viz., increase, decrease, or alteration in the quantity of the fibrine, albumen, fat, and salts, is not yet known, as these diseases cannot be diagnosed as primary and independent affections at present. We only know, that in diseases attended with great increase of fibrine (pneumonia and acute rheumatism), the sulphuric acid, urea, and the uric acids are greatly increased; that in hypalbuminosis the amount of urea is small, as in the convalescence of fevers; that excess of salts in the blood generally appear in the urine, and that in excess of water of the liquor sanguinis (hydræmia) the water of the urine is generally large, unless there be, as is often the case, escape of water from other channels (dropsy of cavities or of the areolar tissues)."

The next division refers to some diseases of somewhat obscure origin, but with prominent urinary symptoms, and the more important of these are diabetes mellitus and diabetes insipidus, &c.; the chief symptoms being the presence of some abnormal ingredient, as sugar or cystine, or in the disproportionate increase of some normal constituent, as water or urea; we shall first speak of *diabetes mellitus*, or *glycosuria*.

It must be remembered that a trace of sugar appears to be always present in healthy urine: this has been long suspected, and Brücke, Dr. Bence Jones, and Schunk have recently established it conclusively;* but in the disease of which we have now to speak, this principle is always in quantities immeasurably larger than the trace alluded to. One of the most striking urinary symptoms in diabetes is the excessive quantity of water; from 100 to 400 fluid ounces or more may be passed in the twenty-four hours, but large amounts of sugar may be present without any augmentation in the water. It has often been supposed that the quantity of water passed has exceeded that taken into the body, which, if true, would necessarily lead us to the conclusion, either that water is formed in the system by the union of its elements, or that this fluid is absorbed from without by means of the skin and lungs. Dr. Parkes has investigated this point, and comes to the conclusion that at present there is no reason to think that water is absorbed or formed in the system, but that occasionally the body may lose more water than it receives, and will then lose weight. The body in diabetes will often retain water for some time, and if an observation of a single day only be made, it may happen that some of the water of the previous day may be poured out. In this disease, almost all the water drank passes off by the kidneys, and there is consequently a considerable diminution of the insensible perspiration from the skin and lungs, as likewise from the intestinal mucous membrane, giving rise to constipation.

With regard to the urea, many discrepancies in the statements exist, but if we look to the more trustworthy analyses, and those made on the urine of the twenty-four hours, we shall find that it is *probably* generally greatly increased in diabetes.

* See Review VIII. in this number, p. 359.

The analyses collected in a table by the author show daily amounts of urea varying from 421 to 1411 grains, some of the patients being on a mixed, others on a pure meat diet. It is right, however, to state that there are exceptional cases on record. This increase of the urea can scarcely be accounted for altogether by the character of the diet; it cannot be explained by an increase of temperature, for diabetic patients are usually cooler than ordinary subjects. May it be, as Dr. Parkes remarks, partly due to the great increase of water-drinking, which is known in other individuals to increase the amount of urea; also to the lessened egress of urea through the dry skin; or to the lessened action of oxygen on nitrogenized tissues, the sugar not being oxidized as under ordinary circumstances?

With regard to the uric acid, Dr. Parkes states that it is certainly not increased in quantity, but probably diminished; the large excess of water, however, renders its accurate determination a matter of difficulty. Hippuric acid has been asserted to be in increased quantity in some cases of diabetic urine. With regard to the reaction of the urine in glycosuria, it seems not to be very acid when first passed, but a change rapidly occurs; some acid, as lactic or acetic, is formed, and a great increase of acidity thereby induced.

The specific gravity of the urine is almost always high, ranging from 1030 to 1075; the more common weight, however, is about 1040.

The most important change, however, is the presence of a large quantity of sugar in the urine; sometimes this is very great: one or two pounds, or even more, are occasionally voided in the twenty-four hours, but at times this principle may be in comparatively small amounts. Dr. Parkes devotes some space to the careful investigation of the various causes which influence the amount of sugar. In every case of diabetes, the sugar in the urine is augmented by saccharine and amylaceous food, the increase being perceptible within two hours, and lasting from four to six, if the quantity of such food has been great; in some of these cases, all the urinary sugar is thus produced, and absence from it will completely free the urine from saccharine matter: probably this occurs only in the early stages of the disease. In the greater number of cases, however, the sugar is derived from other sources, as from meat or gluten, although the dependence of the sugar on the food is still quite evident; for it begins to increase soon after meals, and continues for some time, and if there be great abstinence, it may altogether disappear. In such cases, when the patients are placed on a mixed diet, part of the sugar is due to the amylaceous compounds, part to the nitrogenized.

Gum was found by Professor Graham not to increase the sugar, and this probably is explainable by the fact that its dialyzing power is extremely small; gelatinous tissues are asserted to yield more sugar than fibrinous.

Lastly, it appears that sugar is sometimes formed altogether independent of food—that is, during complete inanition, and it must then be furnished by the tissues themselves.

The details of some interesting experiments made by Mr. Sidney Ringer on this subject on diabetic patients are related by Dr. Parkes; during inanition—that is, from the ninth to the fifteenth hour after food in a male subject—the ratio of the urea to the sugar was as 1 to 2·24; after nitrogenized food, the first to the ninth hour—as 1 to 1·9; but when starchy food was given, the ratio between the urea and sugar was altogether altered, because there was then an independent source of sugar; for example, the same patient on a mixed diet passed urine in which the ratio of the urea to the sugar was from 1 to 3·9 to 1 to 20·4.

Excess of water-drinking seems to augment the amount of sugar in the urine, and abstinence from water to decrease it; but this latter effect is probably dependent rather on retention of sugar in the blood, than on any deficient formation of this principle; as when fluid is again given, an excessive elimination of sugar occurs. Patients are often made very ill from the sudden withdrawal of their accustomed fluids: and indeed no real good appears to result from stinting the consumption of fluid, provided that it be of a proper nature.

Febrile disturbance will often cause a diminution or even absence of the sugar, not altogether accounted for either by retention in the blood or from deficient food.

Dr. Parkes gives a full account of the known action of medicines on the formation of sugar in diabetes.

Omitting the consideration of *diabetes insipidus*, in which little light has as yet been thrown on its pathology; also of those affections in which *cystine* and other matters, as *fat*, *chyle*, &c., are present in the urine,—we pass on to the consideration of Chapter V. and last, in which those diseases are discussed which may be called truly renal—affections in which there exist structural alterations in the kidneys themselves.

In *acute Bright's disease*, the urine has all the characters which are observed in intense febrile excitement, the water being lessened, the colour intensified, and the urates in excess and in the form of a deposit; there is also the presence of albumen and blood, besides the various desquamated structures from the urinary organs and passages, as renal and vesical epithelium, pus, granular cells, and epithelium casts or cylinders; there is likewise very frequently deposited urates, and sometimes oxalate of lime is present. During recovery the urine becomes increased in quantity, and the albumen is lessened, or even disappears. In the early stages there is often proof of much retention in the blood of the various urinary ingredients, as urea, uric acid, &c. In the chronic form of the disease,* the water, as a rule, is perhaps a little lessened, but there are many exceptions; in some cases it is very abundant. Dr. Parkes gives a table consisting of nine cases, six of his own and three from Frerichs, from which it seems probable that when the kidney becomes much atrophied, the urinary water is lessened; in the one case, where the secretion was abundant, and ninety

* We must refer our readers to the work itself for the definition of these diseases.

ounces voided in the twenty-four hours, the kidneys were found to be hypertrophied, weighing ten ounces and a half and nine ounces and a half; when dropsy and symptoms of uræmic poisoning occur, the urine becomes scanty, although this last condition is not necessarily accompanied by dropsical effusions, for vomiting and purging, not uncommon in this disease, will lessen the amount of water; as will likewise a free action of the skin. It has been long known that the solids of the urine are usually much diminished in this disease, and it appears that the diminution affects *all* the solids, though in somewhat different degrees.

The urea is much lessened, especially when symptoms of uræmia are present. Many analyses are given in illustration of this fact. In one case related by Frerichs, less than a gramme, or about fifteen grains only, were excreted in the twenty-four hours. In a case of Dr. Parkes', five weeks before death, there were thirty-seven fluid ounces of urine, one hundred and thirty grains of urea, and sixty-five grains of chloride of sodium, eliminated in the day.

In some of the exceptional cases, however, the quantity of urea and solids has been very large; thus Dr. Parkes found in one case, as a mean of two days, 422·8 grains of albumen and 981 grains of non-albuminous solids; and Mosler, as a mean of three days' observations in an anasarcous case, found 620 grains of urea, and this in a patient having no exalted temperature. Dr. Parkes enters fully into the possible causes of these exceptions, for which we must refer to the work itself. The uric acid has been usually discovered to be decreased, and Dr. Garrod has ascertained its accumulation in the blood, but, as in the case of urea, it is now and then thrown out in large quantities. The other solids are also generally lessened, and the urine frequently acquires a peculiar odour, probably from a decomposition in the albumen.

With regard to this last-named principle, its amount varies very greatly, from 5 to 545 grains in the twenty-four hours, the average being about 220 grains.

The amount of albumen has a considerable relation to the food; it has been asserted that in many cases the urina sanguinis contained no albumen, while the urina cibi was rich in this principle. Dr. Parkes gives a table of two cases, indicating the increase of albumen caused by a meal. In an interesting case of albuminuria and glycosuria which we had an opportunity of watching for some time, the urine of early morning often contained neither sugar nor albumen, but after breakfast the sugar soon appeared, afterwards the albumen; the two principles gradually disappeared, or became greatly lessened in the same order. Nothing like an inverse ratio between the albumen and urea can be observed in this disease.

Dr. Parkes inquires into the causes of this increase of albumen from food, and in some lectures, published before his work, made use of the terms *food* and *blood* albuminuria to express the two sources of albumen. It is known that the injection of albumen into the blood will cause its appearance in the urine, and temporary albuminuria is not uncommon during the absorption of exudations. Our author inquires, Is the albumen in such cases like a foreign body which passes through membranes, as quinine and iron, or any other substance, may do? and he suggests that the albumen poured into the blood after a meal may be more diffusible than the *serum*-albumen in a fasting animal. Other substances, as olein, sugar, and altered pigment, are occasionally met with in these cases, as well as small traces of blood.

The condition of the urine in *pyelitis*, acute and chronic *cystitis*, is afterwards referred to, but nothing very novel is made out. There is, however, one point which is important both to physicians and surgeons—it is the diagnosis of pyelitis and cystitis, or rather the complication of cystitis and pyelitis. Dr. Parkes makes the following observations on this subject:

"It is often extremely difficult to know if, with the cystitis, there is pyelitis or disease of the kidney itself. Pyelitis must be distinguished by other symptoms, or by the existence of pelvic epithelium. Disease of the kidney can only be distinguished by the amount of albumen being manifestly greater than could be furnished by the pus, and by the discovery of casts or of renal epithelium.

"The urine in chronic cystitis, apart from the structures mechanically suspended in it, is not, as far as is known, altered in composition.

"The alkalinity in cystitis is often owing to urea decomposition; but in addition, as pointed out by Dr. Owen Rees, the mucus poured out by the mucous membrane is itself alkaline, and either lessens the acidity, or is sufficient, *per se*, to make the urine alkaline.

"A peculiar faint odour is often perceived, and appears to arise from changes in the mucus or albumen. Vibriones rapidly form after emission. If the cystitis arises from calculus, the appearances are the same, except that blood is in larger quantity, and is more frequently seen."

We have now given a short and very imperfect sketch of the subject-matter of Dr. Parkes's work; sufficient, we trust, to make our readers anxious to know more of its contents, and to study the volume for themselves. They will there find the results not only of a most patient and judicious selection of material from all known quarters, but likewise of much original clinical investigation. Above all, the work will prove most valuable from its containing the opinions of one most capable of weighing dispassionately and profoundly the conflicting opinions and observations which exist on this difficult and as yet imperfectly studied subject.

The little work or paper by the Rev. S. Haughton contains the substance of two papers read before the Association of the King and Queen's College of Physicians of Ireland, and published in the 'Dublin Quarterly Journal of Medical Science,' August, 1859, and August, 1860. Mr. Haughton states that the investigation was suggested to him by a remark of Bernouilli, to the effect that the total quantity of work of which a healthy man was capable appeared to him to be constant, no matter in what description of labour he was employed. Mr. Haughton considers that Bernouilli, owing to the deficient knowledge of his time, fell into error by taking simply the mechanical labour of which a man was capable; and he thinks the following to be a juster view of the subject:

"1. A man in health consumes, per day, a certain amount of food and drink, and excretes, per day, an equal amount of waste matter, supposing him to be full grown, in good health, and not gaining or losing flesh.

"2. The excretions are effected in four ways: per halitum, per cutem, per vesicam, per anum, and are more highly oxidized than the food and drink consumed.

"3. The difference of oxidation of the ingesta and excreta, converted into work, should account for, and be equal to, the total labour effected per diem.

"4. The work done per day may be divided into—

"*a.* The effort necessary to live, or the work spent on the performance of organic functions—*opera vitalia.*

"*b.* The work, converted into heat, requisite to keep up the animal temperature—*opera calorifica.*

"*c.* The mechanical work effected by bodily labour—*opera mechanica.*

"*d.* The unknown, and hitherto unmeasured, work done by the mind—*opera mentalia.*"

Mr. Haughton says that it is an opinion of physiologists that the animal heat is fully accounted for by the oxidation of food effected *per halitum*, and therefore there remain only three excretions to consider—that from the skin, the kidneys, and intestines; and three kinds of work—vital, mechanical, and mental. He divides his paper into four parts. The first part contains an account of the daily discharge of urea in the healthy urine of man; the second, the daily discharge of uric acid in the same; the third, the amount of phosphoric acid; and the fourth is devoted to the comparison of the foregoing with the daily work, bodily and mental, and with the daily food.

In the first part there are some useful facts obtained by himself, and which agree closely with the observations of several trustworthy observers, and with those obtained by Dr. Parkes from a careful collection of the numerous analyses upon record. The urea was determined by Liebig's volumetrical process, by means of nitrate of mercury, and which he finds to give the most accurate and consistent results. He divides the individuals upon whom he operated into two classes—well-fed, flesh-eating, wine-drinking men, and well-fed, water-drinking vegetarians. The figures obtained from the first set of men are seen in the annexed table.

TABLE A.—*Beef-eaters.*

No.	Urea per day. Grains.	Urine per day. Ounces.	Specific gravity.	Urea per fluid ℥. Grains.	Solids per day. Grains.
1.	465·09	34	1023·8	13·70	817·25
2.	677·25	62	1019·0	10·11	1065·69
3.	644·62	52	1018·5	12·40	998·55
4.	554·00	50	1015·8	11·08	818·00
5.	630·00	45	1028·2	14·00	1330·20
6.	484·30	41	1024·8	11·81	1051·25
Mean	575·87	47·3	1021·7	12·18	1013·49

Mr. Haughton gives the ages, weights, the mode of life, including the amount of exercise and of study, of the six individuals, and also mentions any peculiarity which any one possessed. He considers the deviations from the mean amount of urea is not dependent on any single circumstance, as weight, bodily exercise, &c., but upon a combination of circumstances which he hopes to be able to explain satisfactorily. All the specimens of urine in the above table gave, on the addition of nitric acid, at 50° Fahr., after standing twenty-four hours, a precipitate of crystallized nitrate of urea; and from this circumstance, as the individuals were all healthy, he does not consider this test a proof of the existence of excess of urea. In this conclusion we fully agree. The next table gives his results on the second class, the water-drinking vegetarians.

TABLE B.—*Vegetarians.*

No.	Urea per day. Grains.		Urine per day. Ounces.		Specific gravity.		Urea per fluid ℥. Grains.		Solids per day. Grains.
1.	367·50	...	70	...	1014·00	...	5·25	...	1012·90
2.	578·81	...	81	...	1014·76	...	7·14	...	1236·87
3.	315·00	...	45	...	1015·23	...	7·00	...	712·50
4.	366·12	...	56	...	1012·41	...	6·54	...	717·08
5.	342·55	...	43	...	1017·17	...	7·87	...	775·60
Mean	393·99	...	59	...	1014·71	...	6·76	...	890·99

It should be known that No. 5 represents the same man as No. 3, but the analyses were made a fortnight subsequently.

Mr. Haughton afterwards gives a table showing the relation between the specific gravity of health urine and the quantity of urea contained in it, also a diagram of the curve of specific gravity and the curve of urea in man, from which it is evident that the curve which expresses the relation of the quantity of urea in healthy urine to the specific gravity of the latter is not a right line, or the quantity of urea is not proportional to the specific gravity; the urea curve may be represented sufficiently near by a parabole whose parameter is 28, and from this we may deduce the following rule sufficiently correct for clinical purposes:—"*Half the excess of the specific gravity of urine (not containing either sugar or albumen), above* 1000, *is the number of grains of urea per fluid ounce.*"

In relation to the subject of urea, Mr. Haughton states, that he has examined carefully for urea, the serous effusion of the pericardium of two subjects, both women; the one had died of Bright's disease and dropsy, the other had perfectly healthy kidneys; in the first case, 3·05 grains of urea were contained in the fluid ounce; in the second, 1·31 grains; and he makes this, to medical men, very important remark:

"The difference in the two cases is sufficiently striking, and the total quantity of urea in the dropsical effusion of No. 1 must have been very great; yet the quantity in No. 2 is also so large as to make us hesitate as to the conclusions to be drawn from the qualitative determination of the presence of urea found, on a post-mortem examination, in the pericardium, or in the ventricles of the brain."

In the second part of the paper, the daily discharge of uric acid in healthy urine of men is considered; and, after speaking of the results obtained by different observers, he gives his own in the subjoined tabular form:

TABLE C.—*Uric acid per day* (*Beef-eaters.*)

No. 1.	1·02	grains per day.
No. 2.	11·88	"
No. 3.	1·04	"
No. 4.	7·40	"
No. 5.	5·29	"
No. 6.	0·71	"
Mean	4·55	"

TABLE D.—*Uric acid per day* (*Vegetarians*).

	Grains per day.	
No. 1.	0·50	
No. 2.	0·71	
No. 3.	1·69	partly hippuric acid.
No. 4.	2·48	
No. 5.	2·03	partly hippuric acid.
Mean	1·48	

From these results, Mr. Haughton draws the conclusion that, considering the quantity of urea passed by the persons in the Tables to be 576 grains and 394 grains respectively, we may fairly consider the uric acid discharged simply in the light of a minute fraction of nitrogen which has escaped oxidation, and as a matter rather of chemical than physiological interest. We must, however, differ entirely from Mr. Haughton on this point; why should the uric acid be disregarded because it exists in only comparatively small quantities in healthy human urine? Do we not find, in comparing the urinary excretion of different animals, that some eliminate all the nitrogen in the form of uric acid, as the serpent, birds, &c.; others throw out large quantities both of urea and uric acid, as the tortoise; that man throws out a large amount of urea, and but a comparatively small quantity of uric acid; and, lastly, the herbivorous animals only eliminate a questionable trace of this acid? Again, do we not see that many diseases to which man is liable are closely connected with uric acid? We have, therefore, no good grounds for discarding the consideration of it, or looking upon it merely in the light of an accidental product. Mr. Haughton, however, is not a physician, possibly not a comparative physiologist, and this may account for his neglect of this important principle.

The third part of the paper is devoted to the phosphoric acid, its daily discharge in the healthy urine of man, upon which, as Mr. Haughton observes, there is great difference of opinion; some looking upon it as accidental in character, and as dependent on food; others regarding it as the direct product of the disintegration of nervous tissue, and as a measure of nervous work done by the system. Two tables are introduced, as with urea and uric acid; in the first, the phosphoric acid is determined without reference to the bases with which it is combined; in the second, the relative proportions combined with the earthy and alkaline bases are indicated.

TABLE E.—*Discharge of phosphoric acid per day (Beef-eaters).*

No.	Phosphoric acid. Grains.	Weight.
1.	47·14	126
2.	43·28	126
3.	40·78	126
4.	38·10	174
5.	23·72	189
6.	29·43	145
Mean . . .	37·07	

On comparing the weights of the men and the respective amounts of phosphoric acid discharged, it will be at once evident that there is no direct proportion between them, as would be expected if the acid were simply dependent on the waste of the albuminous or other tissues.

TABLE F.—*Discharge of phosphoric acid per day (Vegetarians).*

No.	Total phosphoric acid. Grains.	In combination with earths. Grains.	In combination with alkalies. Grains.
1.	30·00		
2.	32·47	5·92	26·55
3.	22·78		
4.	27·54	6·30	31·24
5.	20·70	3·46	17·24
Mean . .	26·70	5·23	21·67

Mr. Haughton says he has reason to believe that the proportion of phosphoric acid in combination with alkalies to that in combination with earths—viz., as 1 to 4—is the same in well-fed men as in vegetarians, and that this ratio may be confidently used in drawing conclusions with respect to this subject; he also thinks that the idea of the phosphoric acid arising exclusively from the waste of nervous tissue is ill-founded, seeing that, if such were the case, the quantity of phosphoric acid obtainable from the nervous matter would only last about twenty days—a circumstance almost incredible, although the brain is better supplied with blood than any other portion of the body. He also thinks we may fairly conclude, that only the phosphoric acid in combination with the earths can be looked upon as having its origin in the wear and tear of the bony tissue; and he calculates that a man of 150 pounds weight, and having about 10 pounds of bone earth in his system, would require more than seventeen years to lose all the phosphoric acid from this source.

In the fourth part, Mr. Haughton deduces his conclusions; but to attempt to go into the details of his method would occupy too large a space, and we must refer our readers desirous of seeing them to the author's own paper. The conclusions are thus summed up:

"1. The quantity of urea passed per day by men in health varies with their food and occupation, the latter being the principal cause, and regulating the other.

"2. Men employed only in manual or routine bodily labour are sufficiently well fed on vegetable diet, and discharge, on an average, 400 grains of urea per day, of which 300 grains are spent in vital, and 100 grains in mechanical work. This conclusion is in conformity with the experience of the mass of mankind employed in manual labour in all ages and countries.

"3. When the work done is of a higher order, a better quality of food must be supplied, sufficient to allow of a discharge of 533 grains of urea per day, of which 300 grains, as before, are spent in vital work, and 233 grains in mental work and the mechanical work necessary to keep the body in health.

"4. The quantity of urea discharged per day varies also with the weight of the individual, which influences the vital and mental work.

"5. The habits, weight, and occupation of the individual enable us to account for a range of the diurnal quantity of urea, varying from 300 to 630 grains per day; and this discharge may be confidently predicted when the habits and weights are known. When in any case the discharge of urea exceeds that calculated from the preceding data, it must be attributed to ill-health, and most generally to that most fatal of all diseases to which man is liable—anxiety of mind—a vague and unscientific expression, which, however, denotes a most real disease."

Mr. Haughton's essay is highly ingenious, and may be considered as a step in the right direction, but we cannot yet accept all the conclusions he has drawn; the analyses, although valuable, have been too few in number, and made usually upon one day's urine only. Then he asserts that the kidneys excrete all the nitrogen from the system as urea, which is certainly not true in man; and with regard to the increased production of urea by mental labour, this cannot as yet be considered as proved. Mr. Haughton has calculated that each hour of severe study, consisting of high-class teaching and its preparation, causes the formation of 43 grains of urea, and that ordinary mental labour, such as office-work, only generates about 27 or 28 grains.

If mental work really gave rise to so large an amount of urea, we should expect to find that the lower animals, when their weight, mechanical labour, and other circumstances are taken into consideration, would excrete a much less quantity of nitrogen than man. It would be interesting to examine this point, and also to ascertain the relation between the excretion of urea in idiots and imbecile persons compared with healthy subjects. Mr. Haughton's work may be looked upon as extremely suggestive, and well worthy of careful perusal.

We have next to speak of Dr. Beale's work, which is chiefly composed of thirteen lectures delivered by him in his laboratory, most of them being already known to the profession from their having been published in the 'British Medical Journal.' Dr. Beale states that he has restricted himself as much as possible to those parts of his subject which are of practical importance in investigating the nature of actual cases, and he explains the reason for not discussing special questions connected with the treatment of disease, by stating that the lectures were given to practitioners, most of whom had had far larger experience than himself; and again, that almost the whole time was necessarily devoted to the practical examination of the urine and urinary deposits by the microscope, and by the application of the necessary tests.

Dr. Beale's book, as is the case with Dr. Parkes', treats of the composition of the urine in health and disease; yet notwithstanding the identity of subject, they are works of a very different object and character. Dr. Parkes, as we have already seen, selects a disease, and then traces the various deviations which the urine undergoes in such case; Dr. Beale, on the other, selects a urinary constituent, either normal or abnormal, and afterwards shows what morbid states of the system its presence or alteration of quantity is indicative of; moreover, perhaps the bulk of Dr. Beale's work is devoted to the consideration of subjects not touched upon by Dr. Parkes—namely, the various modes of analysing the urine.

To a student or practitioner but little acquainted with urinary research, Dr. Parkes' work would require a key, and such a one will be found in the volume under consideration.

Dr. Beale commences by giving in a clear and forcible manner general directions for the examination of the urine, the apparatus required, the mode of ascertaining the weight or specific gravity of the fluid, its reaction, and also describes the several changes to which the urine is liable. In this lecture, the subjects are treated minutely and ably, and all that requires to be known on the different points can be readily obtained from its perusal.

After this there is a full discussion as to the constitution of healthy human urine; each of the ingredients is fully described, both microscopically and chemically, and in most instances the mode of separating and estimating them is given. The physiological and pathological relations of the various constituents are also discussed; these occupy altogether three lectures: the next two are devoted to the urine in disease; all the abnormal constituents are spoken of and treated in the same manner as the normal ingredients; the state of the urinary secretion in several morbid states of the system is likewise alluded to. In all this part we have nothing very special to remark, as these subjects have been treated of in so many works, and there is no great room for novelty, even in the mode of putting the matter forwards; we have only to add, that all the desired information will here be found, and in a simple, intelligible, and instructive form.

The next chapter is on a subject not often found in works of this kind—it is on the anatomy of the kidney. The minute structure of this important organ is well described, and illustrated by exceedingly good woodcuts; the physiology of the secretion of urine is not overlooked. The remaining lectures are occupied chiefly with the characters of those substances possessing little or no solubility in water, and which are therefore frequently found as urinary deposits, or, more aggregated, as urinary calculi.

Out of so many subjects, it is extremely difficult to select one to illustrate the manner in which the work has been executed, and we will, therefore, as our space is limited, refrain from so doing, and content ourselves with recommending the volume to the careful perusal of our readers, feeling sure that they cannot fail to find it most useful whenever they have occasion—and who has not at the present day?—of making examinations of the urinary excretion and its products. The work throughout is illustrated by numerous and well-executed drawings on wood, showing all the microscopical appearances.

The remainder of our task consists in the examination of the papers by Dr. H. Bence Jones, in the 'Chemical Society's Journal.'

The first consists of about two pages, and in it Dr. Jones describes the characters exhibited by the urine of a young boy who had suffered a year or so before from violent sickness and pain in the stomach, lasting some days, and afterwards followed by the voiding of a urine of a dark colour, like that of blood, but unaccompanied by the passage of any calculus. Just before Dr. Bence Jones was consulted, he had caught cold, and had been somewhat delirious, and he then passed the urine the nature of which forms the subject-matter of the paper. It contained a small quantity of albumen when passed at night, but not when voided in the morning; there were no blood-cells, no casts, but it had a high specific gravity. After some weeks, the urine was again examined, and then was found to be thick and dark-coloured, and under the microscope exhibited a crystalline deposit resembling uric acid, and at first supposed to consist of this substance, but on heating the urine, the crystals were found to dissolve. On a closer examination, the deposit was shown to be soluble in water and hydrochloric acid, and, without effervescence, in nitric acid; the solution in hydrochloric acid crystallized on evaporation in prisms. The crystals were also soluble in alkalies, and in fact gave all the known reactions of *xanthine*. Subsequent examinations, however, of the urine did not exhibit a trace of this matter.

In another short paper Dr. Bence Jones confirms the statements of some chemists, that phosphate of lime occurs as a crystallized salt in human urine, and that it can be produced artificially, not only by adding a salt of lime to the urine after it has been passed, but likewise by giving preparations of lime as medicine, and the following conclusions are arrived at:—

That the presence of crystallized phosphate of lime in the urine does not depend on the amount of phosphoric acid in that fluid, but on the amount of lime, the amount of acidity of the urine being such as to allow its crystallization; and again, that the increase of the lime, or the decrease of the acidity, or the occurrence of both of these conditions, does not necessarily indicate any disease, but may be caused by food or medicine.

In another paper the results of numerous experiments and analyses are given, indicating that after food there is not only an increase of the *uric* acid of the urine, but that the *hippuric* acid likewise follows the same law; thus, seventeen observations on one man's urine gave the following mean results: 1000 c. c. of urine before food, sp. gr. 1015, gave, of hippuric acid, 4·51 grains, of uric acid, 6·05 grains; after food, the same amount of urine, sp. gr. 1017·2, gave, of hippuric acid, 5·94 grains, of uric acid, 9·48 grains.*

In disease, it is not much that is really known of the variations in the amounts of hippuric acid.

The last paper of Dr. Bence Jones to which we would call attention is devoted to the examination of the composition of the amorphous deposit of urates in healthy urine. At one time, this was assumed to consist of urate of ammonia, chiefly on the authority of the late Dr. Prout; afterwards, Heintz made the assertion that the salt was urate of soda, and not urate of ammonia; but his own experiments do not appear to be at all conclusive on the point, as in all his examinations he found ammonia, potash, lime, and magnesia, as well as soda. Scherer also discovered that the amorphous urate deposit was complex in composition. Dr. Bence Jones, seeing these discrepancies, thought the subject should be re-examined, and as the result of his analyses, concludes that not only are different bases, as ammonia, potash, soda, and lime, often present, but that there is usually an excess of uric acid—that is, more than is required to form acid urates, and that this excess of uric acid is held so feebly by the urates, that washing with cold water will set free crystals of uric acid. Finally, Dr. Bence Jones considers that the amorphous deposit has no constant composition.

These papers, though short, form a most interesting and valuable addition to our knowledge of urinology.

Review XII.

1. *Die Krankheiten der Handwerker, ein Beitrag zur Kenntniss zunächst der Verhältnisse in Copenhagen.* Von Adolph Hannover. (From the 'Monatsblatt für medicinische Statistik und öffentliche Gesundheitspflege.'—1861.

The Diseases of Artisans, with especial reference to the Statistics of Copenhagen. By Prof. Hannover, M.D.

2. *Traité Pratique des Maladies, des Accidents, et des Difformités des Houilleurs.* Par H. Boëns-Boissau, M.D.—*Bruxelles*, 1862. pp. 186.

Practical Treatise on the Diseases, Accidents, and Deformities of Colliers. By H. Boëns-Boissau, M.D.

One feature of modern civilization perceptible in every civilized community, is the attention of late years given to sanitary science, for as true civilization advances, individual life advances also in value, and it therefore becomes a political object with every government to promote measures calculated to improve the health of its subjects and to diminish the ratio of their mortality. But if, on the one hand, civilization enhances the value of life, on the other, the real and artificial wants of society, and the pursuit of wealth, involve the over-extension

* We hope shortly to be able to offer to our readers the results of observations recently made at Berlin, from which it would appear that certain articles of diet, and especially a particular kind of cheese, greatly increased the amount of hippuric acid in the urine.

of some forms of labour innocuous in themselves, and also the development of manufactures and of callings prejudicial to the health of those engaged in them; and hence there arises an increasing need of sanitary arrangements to counteract the injurious influences so called into existence.

In a thickly-peopled country like England, where by the effect of competition every trade and occupation undergoes subdivision for the sake of economy in labour, and nicety and refinement are sought in union with cheapness, and where a vast commerce calls for manufacturing agency of the largest and most varied character, and tends to collect men in large masses in factories and towns, creating an enormous class of operatives or mechanics, it is certainly a paramount object to foster this industrial population, and to augment its efficiency by all means calculated to prolong life and to improve health. As England has the largest stake in the commerce of the world, so it has the highest interest in advancing the moral and physical condition of its working classes, by whose labour the materials of that commerce are produced, and the wealth of the country accumulated.

By the persevering teaching and activity of medical men, these truths have at length gained credence with the government, and there is a general recognition of the importance of sanitary science, and a partial attempt to carry out its teachings; but much yet remains to be satisfactorily established before any considerable onward progress can be made, and the duty devolves on the medical profession, as guardians of the public health, to determine the causes detrimental to it, and to suggest remedies for them. For the furtherance of this object we regard the two contributions which head this article as highly valuable to all workers in sanitary science. Their object is similar—viz., to point out what diseases are peculiar to artisans, and what causes are productive of them; but as the mode in which the subject is treated in the two essays is different, and as the one takes up sanitary statistics of almost all mechanical occupations, and the other of one only, we must consider each in turn separately.

We will take Prof. Hannover's essay first,* and attempt a brief analysis of its facts and figures, which are placed before us in a painstaking and original manner. His object is to show to what diseases the various classes of handworkers or mechanics are severally most prone, and to contrast them in respect to these maladies with persons not occupied in any handicraft. This he does by the aid of various tables, the materials for which are collected from the statistics of the great town hospital of Copenhagen—the Frederic and General Hospital, which practically supplies medical aid to all the poorer classes in the city, to most of the artisans, and, indeed, to many of higher social status; for, as is pretty well known, the hospitals of the Continent have a far wider scope of operation among the community in which they happen to be placed than in this country, and number among their applicants classes of individuals who never seek their aid

* We took occasion to notice briefly this Essay by Hannover in our number for January last, p. 173.

in our land. This circumstance gives greater value to the information collected respecting the prevalent diseases of the working classes of Copenhagen, for it implies a wider and more satisfactory field of observation than could be obtained in a British hospital, and therefore so far diminishes the extent of the objections which might be raised against the deduction of such facts from hospital experience. Notwithstanding, however, this comparative advantage over English hospital data, several matters must be taken into consideration before assigning its true value to the information collected in the Copenhagen institutions. These we shall presently mention, but before doing so, desire to make a few preliminary remarks on circumstances attaching to Copenhagen as the field of observation affecting the whole subject in hand.

The city has no special trade or manufacture, no very large factories, the most important being the Royal Porcelain Manufactory, and a few cotton-mills, and in its position and construction is altogether a healthy city. In the absence of any special manufacture absorbing the labour of a large portion of its working-classes, we may presume that the relative proportion of mechanics to the population is about normal, whilst from the size of the city, almost every trade is represented. The preponderance of any one handicraft would necessarily materially affect the statistics of the hospital, and deductions drawn with reference to the proclivity of disease among the members of that craft in relation to that of other callings, would therefore not be correct. This circumstance has not been kept sufficiently in view by Hannover, for though no very large factories create a preponderating special class of operatives, as in many towns, yet certain occupations are more successfully and extensively carried on in Copenhagen than others, as indeed the tables show must be the case with cotton-spinning and with the brazier's business. So, again, on comparing the relative number of the several trades admitted into hospital in Copenhagen and in Vienna, we find that whilst in the former city the first four trades in the list are weavers, tanners, bakers, and coppersmiths, those in the latter are carpenters, shoemakers, smiths, and tailors. Hence these variations, which must obtain in every town, require to be allowed for, and the actual number of admissions of any trade into a hospital affords no precise criterion of the liability to sickness of that trade, standing, as it does, in direct relation with the number of persons engaged in it. To arrive at a knowledge of this point, it would therefore be necessary to ascertain the number occupied in each trade. Hannover himself remarks, indeed, that the only correct way of discovering the immediate influence of an occupation upon the health of the artisan would be to take a determinate number of workmen of sound health, of like age, and of similar physical conformation, &c., and to observe how long their health remained unbroken, and also the influence of the favourable or unfavourable conditions under which they were placed. Such data could be collected among a limited number of people working together in the same factories and using certain manufacturing materials, but are not to be obtained with regard to the several classes of artisans at large. It is in the absence of such precise information that Hannover seeks to establish the relative proclivity of the various trades to disease as far as practicable by an appeal to hospital statistics.

According to the Census of 1851, there were 61,568 persons of the male sex in Copenhagen, and of these 11,129 were engaged in various mechanical operations. The difference—50,439—represents the male population, including children, not belonging to any handicraft. From 1843–1847, 9835 males labouring under internal diseases were admitted into the medical wards of the General Hospital, and of these 3681 were artisans, the remainder, 6154, belonging to other classes: common labourers and servants. According to these totals, the mechanics constituted more than one-third of the entire number of medical patients.

The following table represents the proportion in every thousand of each trade occupying more than fifty workmen, admitted in the medical wards from 1843 to 1847:—

TABLE I.

Weavers	548	Shoemakers	314
Tanners	465	Ropemakers	308
Bakers	452	Butchers	302
Coppersmiths	370	Dyers	289
Braziers	365	Turners and Comb-makers	272
Carpenters and joiners	364	Masons	270
Basket-makers	359	Tinmen	263
Glaziers	349	Bookbinders	246
Blacksmiths, gunsmiths, and locksmiths	346	Glovers	231
Tailors	336	Saddlers	227
Goldsmiths	330	Hatters	225
Wheelwrights	327	Printers	219
Painters	325	Pastrycooks	188
Pinmakers	323	Watchmakers	180
Millers	317	Sailmakers	154
		Coopers	145

Now this table does not correctly represent the greater liability of some trades above others to disease; for, as above remarked, numerical results of this sort must be largely influenced in every town by the relative preponderance of certain occupations over others; and on making a comparison with the vital statistics of other towns, it is clear that in Copenhagen, weaving and tanning are the chief industrial occupations of that city, and that therefore tanners and weavers are not such sickly folk as the figures in the preceding table would imply. Moreover, there are other extrinsic circumstances mentioned by Hannover, which must materially affect the number of applicants of the different trades for hospital aid. One of the most important of these, says Hannover, is the greater number of unmarried mechanics in some trades than in others; for it is a fact that the married are more unwilling to quit their homes to go into hospital, being better off for nursing and home comforts than the single. Another circumstance is the nature of the occupation; for the one that is poorly paid contributes more inmates to hospital than another gaining higher wages. This, again, will operate by sending slighter cases in greater numbers, and chronic cases of disease at an earlier period, to the hospital. Further, the sort of occupation, with respect to the physical exertion it involves in its exercise, influences the frequency and period of application for hospital relief. For example, a lesser amount of disease disqualifies the smith for labour than would an artisan having a sedentary occupation, such as a tailor or shoemaker. Other minor circumstances are to be found in the customs of the various trades, and in the variable extent to which they make provision for their sick fellow-workmen by benefit associations and the like. Among others, for example, Hannover says that in Copenhagen it is the custom for the bakers to live in the houses of their masters, from whence, however, they are at once transferred when sick to the hospital. Lastly, we would add that where the number of those occupied in any trade is very small, the statistics of a hospital would be very fallacious in respect to their comparative proclivity to disease; for many accidental circumstances, including those above-named, will operate in an undue and irregular proportion upon their admission. And in reference to the prevalence of any particular disease among them, these statistics would ill represent the fact. Thus, for illustration, at the Wieden Hospital, Vienna, only five watchmakers were admitted, and of these four died from phthisis; now this cannot be assumed to show that phthisis cuts off four out of every five watchmakers; but the explanation chiefly is, no doubt, that this lingering disease induced these workmen to seek the assistance of the hospital, whilst that in the case of more acute disease, the majority of their fellow-workmen were willing and rich enough to procure medical relief at their own homes.

Hannover proceeds to show what forms of disease are most rife in the several handicrafts; and for this object tabulates two sets of observations: the one of 9835 males, of sixteen years of age and upwards, who were admitted into the medical wards of the hospital from 1843–1847, and subsequently discharged as cured, relieved, or dead: the other of 6111 males, also sixteen years old or upwards, who died in the medical wards between 1840 and 1859 inclusive.

Taking the first series of observations, we learn that there were of the 9835 men admitted, 3681 mechanics, and 6154 of other occupations not mechanical. Again, of the 3681 artisans, 3304 were discharged cured or relieved, and 377, or 10·20 per cent., died; whilst of the other classes of inmates, 6154 in number, 5144 were cured or improved, and 1010, or 16·4 per cent., died. According to these figures, therefore, the death-rate was much greater among those not ranking with mechanics.

Referring to his second series of observations, based on the 6111 deaths of males occurring from 1840 to 1859, we find that 1965 took place among mechanics, and 4146 among those not occupied in mechanical labour. Of these latter, common labourers, servants, and waiters formed 3182 of the number. At the Wieden hospital, Vienna, the proportion of mechanics admitted as in-patients to other inmates during 1860, was considerably greater; for of the entire number of 3536 males, 2258 were mechanics; or more correctly, deducting 936 from 3536 for boys under sixteen years of age (actually 1092 under twenty years of age), there were 2258 mechanics in 2600 admissions.

The general facts are shown by Hannover, together with the particular disease and the special occupation of the patients, in elaborate tables, which we cannot here reproduce. The following one, however, exhibiting the relative number of deaths to diseases of most kinds registered among artisans and non-artisans in 1000 of each class, is interesting.

TABLE II.

1343—1847.	Artisans.	Others.	1843—1437.	Artisans.	Others.
Feb. biliosa gastrica	114	91	Pleurodyne	13	9
„ catarrhalis	27	16	Lumbago	13	7
„ typhoïdea	48	64	Encephalopathiæ	6	13
„ intermittens	21	9	Morbi mentales	34	46
„ rheumatica	31	25	Epilepsia	8	9
„ scarlatinosa	10	19	Neuralgiæ Morbi-spinales	17	8
„ variolosa	50	53	Paralysis	8	4
„ morbillosa	11	22	Asthma	2	2
Erysipelas	14	18	Cardialgia	6	0
Phlebitis et arteritis	1	1	Splenalgia	1	0
Inflammatio cavit. cranii.	6	14	Colica	20	9
Angina	30	13	Diarrhœa, cholera	12	14
Variæ affect. colli et pectoris	2	4	Obstipatio alvi	4	2
Endo- et peri-carditis	1	2	Dyspepsia	10	4
Bronchitis	60	76	Gastritis chronica, &c.	4	10
Pneumonia	69	96	Helminthiasis	2	3
Pleuritis	17	18	Morbi syst. uriniferi	6	5
Inflammatio abdominis	7	4	Morbi chronici cordis	21	20
Apoplexia	8	14	Phthisis	94	60
Hæmoptysis	5	4	Icterus, Morbi-chronici hepatis	9	8
Hæmatemesis	1	2	Tumores abdominis	1	2
Hæmorrhoides	6	3	Cancer	6	9
Cæteræ hæmorrhagiæ	4	7	Leukæmiæ, Hydrops.	10	17
Delirium tremens	34	80	Cachexia, Senectus	1	5
Rheumatismus non febrilis	104	47	Suicidium, &c.	5	24
Cephalalgia	6	8			

So far as hospital statistics go, this table shows at a glance what maladies are more common among mechanics compared with other classes. Besides the so-called bilious gastric fever, the most prevalent diseases of artisans are rheumatism in its various forms, particularly chronic rheumatism, pleurodynia and lumbago, colic and phthisis—a series of disorders which might be *à priori* predicated from the operation of external agencies to which, as a rule, artisans are more exposed than others, and, so far as colic is concerned, from the direct noxious influence of materials used in trade. The greater frequency of intermittent fever among them is rather an accidental feature here in Copenhagen; for in the city itself this disorder is very uncommon, and its prevalence among hospital patients of the working classes is due to the custom many artisans have of travelling from town to town, and to the fact, that not a few of them are not natives; the disease, in fact, originates elsewhere in malarious districts, prior to their residence in the city. This is indicated by the fact, that it is most rife in those trades whose workmen come most from abroad, as in the case of the masons, many of whom are Germans, and, in a lesser degree, in that of the tailors. It is remarkable, that not one painter during the five years was admitted with intermittent fever—a circumstance which Hannover is disposed to attribute to the influence of the materials used in their trade.

The number of deaths in 1000 members of the several trades specified, from 1843 to 1847, is thus represented:

TABLE III.

Printers	186	Bookbinders	98
Painters	160	Goldsmiths	97
Butchers	143	Millers	96
Shoemakers	137	Carpenters and joiners	88
Saddlers	121	Tobacco (cigar) makers	88
Coopers	116	Blacksmiths, &c.	71
Masons	110	Weavers	64
Wheelwrights	108	Tinmen	54
Tanners	106	Turners	44
Tailors	102	Bakers	38

The differences between the several trades in respect to their mortality in hospital are very considerable, according to this table. But, as with foregoing figures of the same sort, it represents no absolute truths, for there are several circumstances which will materially modify the proportion of deaths to cases of disease, calculated upon the statistics of admissions, discharges, and deaths among the several trades; and there is no necessary direct relation between proclivity to disease and mortality. The differences remarked in the above Table will, as Hannover indeed mentions, be governed by those conditions or circumstances heretofore stated; such as, for instance, the greater readiness on the part of some artisans (*e.g.*, of the bakers) to go into hospital, than of others; and far slighter causes, whereby the proportion between deaths and cures is entirely altered; thus, the bakers show the smallest ratio of deaths to admissions. Moreover, no agreement is observable between the ratio of mortality and the absolute number of the different tradesmen admitted, as set forth in the first table quoted. The greater frequency of phthisis in some occupations than in others, will account for some of the variations indicated, and the greater or less ability in some trades to ward off a fatal result than in others, will supply another partial explanation of them. Nevertheless, as the author remarks, no good reason can be assigned why the mortality among shoemakers should be double that of the weavers, or why that among masons and tailors should be considerably greater than among joiners and smiths. Mr. Neison has observed that here in England, among the bakers, the liability to disease at the earlier and middle periods of life is smaller than usual, but the mortality greater; whilst among butchers the death-rate is high, but the frequency of disease rather small.

Let us turn now to Hannover's second series of observations relative to the fatality of the several principal diseases as shown by the hospital statistics of deaths and their causes from 1840 to 1859; and, as before, we will first adduce his table, showing the proportion per thousand of the several maladies causing death in the case of artisans and of others.

TABLE IV.—*Causes of death in every* 1000 *cases of those admitted.*

1840—1859.	Artisans.	Others.	1840—1859.	Artisans.	Others.
Feb. typhoïdea	74	68	Delirium tremens	61	103
„ scarlatinosa	0	1	Encephalopathiæ	20	27
„ variolosa	6	6	Epilepsia	6	7
„ morbillosa	1	0	Morbi spinales	10	6
Erysipelas	2	4	Diarrhœa—cholera	21	24
Phlebitis et arteritis	10	8	Gastritis chronica, etc.	3	4
Inflam. cavit. cranii	23	27	Morbi cyst. uriniferi	18	19
Variæ affect. colli et pectoris	3	4	Morbi chronici cardis.	64	59
Endo- et peri-carditis	4	6	Phthisis	348	230
Bronchitis	13	18	Icterus, Morbi chron. hepatis	12	13
Pneumonia	98	150	Tumores abdominis	2	2
Pleuritis	20	12	Cancer	60	56
Inflammatio abdominis	24	24	Leukæmia, Hydrops	32	35
Apoplexia	31	40	Cachexia Senectus	12	30
Hæmoptysis	1	1	Suicidium Veneficium	12	7
Hæmatemesis	1	2	Morbi varii	7	3
Cæteræ hæmorrhagiæ	1	4			

According to this table, the chief maladies which cut off artisans in a greater proportion than others are phthisis, pleurisy, chronic heart disease, and typhoïd fever. In this result there is a general correspondence with what is indicated in Table II. Phthisis in both tables appears especially as a more frequent destroyer of life among artisans than others, killing more than a third of their whole number; and chronic heart disease is very destructive, though in a much less degree. On the other hand, pneumonia and delirium tremens are more common causes of death among those not ranking as artisans.

Professor Hannover next proceeds to consider each disease in detail, and to discuss the relative proclivity of the several trades to its injurious influence. In this detail we cannot follow him, but will content ourselves with the more important facts he points out.

Bilious gastric fever affects one-tenth of all the males admitted, and artisans more than others. It prevails among the smiths, weavers, tailors, shoemakers, and tanners in about equal measure, and is very common among the bakers. It rarely attacks masons, cigar-makers, printers, and painters, owing, as Hannover surmises, to the materials of their trades—the fresh lime, tobacco, turpentine, and varnish. From five to six per cent. of the male patients suffer from typhoïd fever; but it is not so common among artisans, though as a cause of death more frequent. Painters and butchers are particularly exempt from it, but it is more fatal than common among carpenters, printers, smiths, saddlers, and bakers; and on the contrary very slightly so among masons. Among those not following handicrafts, it was most fatal in such occupations as contained the largest proportion of young persons, as candidates, students, and seamen. Printers and painters are more afflicted with rheumatic fever than carpenters, joiners, shoemakers, and bakers, whilst it is uncommon among tailors, turners, and tanners—mechanics, as Hannover remarks, having no characteristics in common. Bronchitis, as might be expected, is most frequent among artisans exposed to variations of temperature, such as bakers and smiths. Pneumonia is neither so frequent nor so fatal among artisans as others. But it is curious to observe, that whilst bronchitis is most rife among bakers and smiths, pneumonia seldom attacks them; an indication, as Hannover maintains, that the causes of these two diseases are different in kind. On the contrary, it attacks more coopers, millers, saddlers, painters, and turners, who on their part are less liable to bronchitis. Of the saddlers admitted, upwards of one-fourth had pneumonia. In reference to the mortality from pneumonia, coopers, rope-makers, goldsmiths, weavers, carpenters, painters, saddlers, tailors, and smiths suffered most, and bakers, masons, and joiners least. Thus, sometimes the proclivity to disease and the mortality from it are in direct ratio; in other instances, in inverse proportion.

Phthisis appears peculiarly a disease of artisans, and in order better to understand its causes and the means of limiting its ravages, an accurate investigation relative to the mechanics who specially suffer from it, to their conditions of life, and to the circumstances of their employment, is most desirable. Of 1000 patients in the Copenhagen Hospital, 94 artisans suffered with phthisis, and only 60 of other classes; of 1000 deaths, 348 were among mechanics, but only 230 among others. Of 2258 mechanics at the Wieden Hospital, 248 died of tuberculosis of lungs.

Hannover justly remarks, that in drawing conclusions respecting its relative frequency in different trades from hospital statistics, much caution is necessary, and many disturbing causes to be allowed for. Thus artisans whose work is laborious will be more numerous and earlier applicants at an hospital on account of chest disease, than those whose occupation is sedentary, and consequently a great excess of the latter class would at any time solicit particular consideration, and indicate a very great proclivity to tubercular disease of the chest among them; more so, that is, than if the like circumstance were observed with respect to the former. Thus, for example, as consumption is less incompatible with the occupation of tailors than with that of smiths, a large proportion of admissions of the former would claim more consideration. At the same time, we would add that the conclusion which Hannover points to in the preceding observations would be much modified by the undoubtedly greater number of tailors than of smiths in the category of operatives in any community. This and other modifying circumstances already insisted on need not to be recapitulated; another or two may, however, be added. For instance, in appreciating the statistics of phthisis in the various trades, a circumstance is pointed out by Hannover which might escape consideration—viz., the influence of the original physical constitution upon the choice of a trade, which will have a tendency to transfer the weaker in constitution to sedentary occupations, and thereby augment the proportion of phthisical patients in those callings. Another fact to be remembered is, that among hospital patients are found many weak creatures who are almost perpetual applicants for relief; such, whatever may be their occupation, swell the number in the statistics of admissions, though they appear only once in the table of mortality. To these modifying conditions we would subjoin another of some moment; for although, as Hannover says, there are no very large factories in Copenhagen, and consequently the employment of children is carried to no considerable extent, yet the relative age of the workpeople in any trade, as compared with another, is an item not to be omitted in assigning a proper value to statistical figures. For example, there are woollen and cotton-mills in Copenhagen, and juvenile labour is more employed in them than it is in the carrying on of almost all other mechanical operations, and when we inquire into the causes of the greater prevalence of such a disease as phthisis among weavers, the relatively more youthful age of these workpeople, considered collectively, must be taken into account.

Bearing the foregoing modifying circumstances in mind, let us proceed with Hannover's numerical statements as to the prevalence of phthisis in the several trades. From 1843 to 1847 no case of phthisis among coopers, basket-makers, brush-makers, plumbers, and chandlers, came into the hospital; and from 1840 to 1859 no death from that malady occurred among brush-makers, type-founders, pewterers, and tallow-chandlers. Certainly, the number of workmen belonging to those several trades was very small, and no deduction can be safely drawn from it; nevertheless, it is remarkable that of 43 coopers admitted in the hospital, not one suffered from phthisis; and of 32 deaths recorded among such tradesmen, only two were assigned to that malady. Few trades can, indeed, point to so favourable a sanitary condition. Hannover presents an interesting table of the proportion per thousand of the various trades coming under treatment, or overtaken by death, in the hospital, from which the following conclusions are drawn: The first general fact that strikes the student of this table is, that the ratio of cases labouring under the disease and that of the deaths from it stand in no direct relation. Thus, on referring to some occupations which rank highest in the table, we notice that phthisis is much less prevalent among bakers than painters; but at the same time that its mortality is nearly the same in the two trades; again, the mortality is lower among painters than bricklayers, though the disease is more common among the former. In the case of shoemakers and saddlers the death-rate from the disease is nearly equal, but its prevalence is much more marked among the former.

Now, this greater numerical frequency of cases of phthisis in one trade than in another is, on the one hand, either due to the repeated admission of the same person as a patient in the hospital, or else phthisis pursues a more rapid course, or is of shorter duration, among those mechanics in whom the mortality is greater than the proclivity to the disease, so that its sufferers appear but a few times on the register before they are cut off by death. The former explanation is imperfect, and must apply equally to all mechanics belonging to trades of a similar character. Of artisans among whom the frequency of the disease is small, and the death-rate from it high, may be enumerated tinmen, hatters, and goldsmiths. Among most mechanics the rate of frequency and of mortality is alike high, but of course in a variable degree. The following occupations are arranged in accordance with their relative mortality, beginning with the highest: Furriers, glaziers, needlemakers, glovers, tailors, bookbinders, watchmakers, stonemasons and sculptors, turners and brushmakers, tinmen, joiners, saddlers, shoemakers, coppersmiths, printers, sailmakers, butchers, goldsmiths, instrument-makers, weavers and hatters. After these come bricklayers, millers, smiths, wheelwrights, cigar-makers, painters, pastry-cooks and ropemakers. The lowest rate of mortality and frequency was among flax-combers, bakers, house-carpenters, tanners, dyers, braziers, and coopers.

On a consideration of these groups, it will be noted that mechanics are classed together in whose occupation no common feature or bond of connexion is discoverable, whether in the sort of labour involved or in the materials employed. Moreover, when the accepted causes of the disease are considered, no conformity is perceptible between them and this representation of its relative frequency and mortality in the different occupations. Many, indeed, of the assigned causes vanish before the teachings of experience. For example, it is generally admitted that phthisis is more common among workmen exposed to the inhalation of dust in their occupation; yet this rule is not without exception, for among the workers in tobacco factories this disease is not common; among the brassfounders it is uncommon, and therefore the dust of the charcoal used by them cannot be a cause of phthisis. So, again, its frequency is below the average among bakers, flax-combers, ropemakers, and in a less degree among millers. A partial explanation why bakers in Copenhagen are less phthisical than generally supposed, is stated by Hannover to be found in the more favourable conditions of their trade in that city, and the less amount of nightwork exacted of them than elsewhere.

More importance is assigned to the injurious effects of dust from animal tissues, such as skins, and the tables show that saddlers, furriers, glovers, and tailors are very consumptive. The dust of minerals is equally noxious. Of ten stonemasons admitted, five had phthisis; and of nine deaths among these mechanics, four perished from that disease. The manufacture of mother-of-pearl is very fatal to workmen, and the cutting of sandstone is more destructive than that of other sorts of stone. The evil effects of hewing and carving stone have been remarked by many of the older physicians; and Dr.

Alison, of Edinburgh, stated that scarcely a stonemason ever reached fifty years of age without becoming consumptive.

Lombard* pointed out the noxious influence of varnishes, turpentine, and drying oils, in developing phthisis, and it is curious to notice that this malady is more rife among workmen who use solder, such as tinmen, coppersmiths, goldsmiths, &c. On the other hand, those artisans who work in a humid atmosphere, or with moist materials, are among the least prone to phthisis; as, for instance, dyers, tanners, coopers, ropemakers and cigar-makers. In general, also, out-door occupations are more favourable to health. The high rate of frequency and mortality among tailors is undoubtedly owing mainly to their wretched, close, ill-ventilated workshops, to the heated stoves and the evaporation from the irons or "geese." Thachrah asserted that phthisis was unusual among butchers. This statement is not confirmed by the statistics of Copenhagen, for of 42 admitted into hospital, 5 died from phthisis; and of 30 deaths among butchers, 11 died from that malady. Sitting and leaning forward to work undoubtedly impedes the expansion of the chest and respiration; still, it operates only as a concurrent cause of phthisis, for on comparing trades subjected to this posture with others in which the work is performed standing, there is no such excess in the liability to phthisis among the former. Again, those occupations which require great bodily activity and strength are generally less productive of phthisis. To this rule joiners and coppersmiths seem to form an exception, caused, as Hannover surmises, by the constant movement of the arms on the side, which he regards as injurious, both in those trades as well as in weavers and shoemakers.

In forming a judgment of the influence of occupations in the production of phthisis, an important element might be obtained by noting the ages at which its victims were carried off in each trade. Hannover has published a table to show this, but its value in the case of most of the trades specified is very little, inasmuch as not more than from one to a dozen individuals of the same calling are enumerated—a proportion useless for statistical purposes. For instance, what is the value of the fact deduced from the occurrence of death from phthisis in the case of one basket-maker at thirty-five years old, in that of an umbrella-maker at thirty-two, of a plumber at fifty-five, or of three gilders at twenty? No average rate can be deduced from single or from so few cases. We shall not, therefore, follow Professor Hannover in the details respecting age he would deduce from the table in question; however, we may note that, on comparing the age reached by artisans and others suffering from phthisis, he discovers that the average age of labourers and of the serving classes is 41·7, and of artisans 35·9. More than one-half of the mechanics who had phthisis died before they reached thirty-five, and only one-ninth of their number lived to be fifty. Women were found to suffer from phthisis less than men; but at the same time phthisical women died two years earlier as to age than men.

* Annales d'Hygiène publique, tom. xi. p. 45. 1834.

Although many of the statistical facts arrived at by Professor Hannover need to be critically examined under the guidance of all the modifying circumstances mentioned, and much allowance need be accorded for imperfection in his statistical information, yet there is much to be learned from his essay, and the fact seems well made out that artisans contribute a much larger proportion to hospital inmates in comparison to their numbers in the community, and that consumption is eminently the disease to which they are most prone.

But further, the production of this paper will show what can be done with hospital statistics, and what direction should be taken in attempts to eliminate facts from them. In this country we have to confess to a reprehensible neglect in collecting and utilizing the immense amount of statistical information which our hospitals, especially those of the metropolis, can afford, and to this point we have repeatedly drawn the attention of our readers.

The other book whose title heads this article is especially devoted to a history of the diseases, accidents, and deformities of colliers or coal miners. It is written by a Belgian physician who has had many years' experience in the coal districts of Belgium, and can therefore treat his subject practically from his own observations. The book presents no information in a statistical form, but passes in review the principal diseases of the human frame, remarking, in passing, on the proclivity of miners to any of them, and on any modifications in their pathology and treatment connected with the employment and other external circumstances of those workmen.

In this country, females are very seldom employed in mining operations, but in Belgium, women and young girls appear to be largely engaged in the coal mines, particularly in pushing the coal in the small trucks through the galleries in the pits to the pit's mouth. This employment of women, especially of young girls, is much and most justly deplored by M. Boissau as productive of gross immorality and moral depravity, and as directly and indirectly causative of much bodily disease and physical degradation.

Those miners who belong to a mining-stock, in whose families the occupation has been pursued for several generations, have a distinguishable physical, and, it might be added, a peculiar mental conformation. The author describes them as of short stature, with large heads and short and thin hair, large flat faces without expression or mobility of features; thick-set in the body, with broad shoulders, short thick necks, and long arms; flattened haunches, short legs, so bent that the knees and feet turn inwards; and a slow, heavy gait.

M. Boissau carefully examines the external circumstances surrounding them in respect to their influence upon their health, and assigns a particularly prejudicial effect to the cramped position in which they frequently work, and the consequent impediment to the normal expansion of the chest. The moist foul air, poisoned not only by the dust and emanations from the coal, but also by the excreta of human beings and horses, and by the smoke from lamps, &c., likewise operates as a potent morbid agent. From these causes come diseases of the blood and various forms of chest complaint, of which some appear peculiar to miners. But though chest diseases are especially prevalent among these operatives, it is a remarkable circumstance that tubercular phthisis affects them in a less ratio than other workmen, and that when it does attack them, its course is slow, and of an asthenic, non-inflammatory character. This statement is more especially applicable to the adults, for M. Boissau notes the frequency of tubercular disease among miners' children, affecting the lungs, and still more often the brain and mesenteric glands.

Again, the strumous diathesis is nearly universal among miners, for, as the author remarks, their mode of life, their habits, and occupation concur to develope it, whilst their frequent intermarriages tend to perpetuate it. The cancerous diathesis is also more common among these workmen, and M. Boissau discovers in imperfect chylification and in defective aëration of blood, producing a purplish instead of the normal bright red blood, the remote though direct cause of the development of the diathesis named, as well as of the prevalence of dyspepsia, of lung disease, and generally of proclivity to asthenic disorders. The clothing they work in is usually very slight and insufficient, and from exposure to the humidity of the mine, and to rapid evaporation from the surface of the body, wherever the ventilation of the galleries is active, the cutaneous exhalations are suppressed, and serous diarrhœa and rheumatic and neuralgic attacks are the consequence.

Lung diseases, however, are the most prevalent among them, as might, *à priori*, be supposed from the conditions of their existence and labour. The coal dust inhaled penetrates to the vesicles, and gives rise to black sputa; but the presence of this foreign matter in the lung is not so prejudicial as might be presumed. Yet if it very largely enter into the lung tissue, it may eventually seriously curtail the process of respiration, and interfere with the due aëration of the blood, and then induce either capillary bronchitis or pulmonary emphysema, with cardiac lesion as a sequel, general debility and anæmia, and ultimately the consumption of miners.

Acute bronchitis is not a common malady among miners; but bronchorrhœa is rather so, and a slight bronchitic attack is actually beneficial to them by promoting the excretion of the black matter from the lungs. On the other hand, a chronic capillary bronchitis is almost a special disease in this class of artisans, and gives origin to the "consumption of miners," or "false melanosis," usually accompanied with black sputa. In symptoms, this disease has much in common with tubercular phthisis, but is not so generally located in the infra-clavicular region, and is less often attended by the formation of cavities or by hæmoptysis. M. Boissau says it is more analogous to chronic bronchial catarrh than to phthisis, pulmonary melanosis, or chronic pneumonia.

A form of asthma is another lung disease particularly rife among miners, due to incomplete aëration of blood, and consequent atony of the lung tissue; to imperfect respiration caused by confined position in working, to repeated bronchitic attacks, and to various general debilitating causes, excesses, and the like.

These observations, culled from the treatise under notice, are sufficient to indicate the distinctive and most common diseases of miners, and will serve to recommend its study to those especially who are occupied in their profession in mining districts. The peculiar lesion of the lung in miner's consumption well deserves a minute inquiry, for we have no doubt whatever that the majority of cases of this disease are looked upon and registered as cases of tubercular phthisis.

REVIEW XIII.

Consumption: its Early and Remediable Stages. By EDWARD SMITH, M.D., LL.B., F.R.S., Assistant-Physician to the Hospital for Consumption and Diseases of the Chest, Brompton, &c.—*London*, 1862. pp. 447.

THIS is a great theme, and as obscure as it is important. In the work the title of which is prefixed, it has been ably discussed by Dr. Edward Smith, who, with a large experience of the disease in its fully developed form, has brought to the study of it in its early insidious, and, as we think, its latent stage, such aids as an advanced physiology and pathology afford, and especially the results of his own physiological researches.

He states in his preface that in undertaking the work, he has had—

"Four principal objects in view—viz., to take advantage of the growing belief of the day that there is a stage of phthisis in which the disease is as remediable as it is irremediable at a later period; to write a practical work in which may be faithfully represented the actual conditions of those cases when regarded in the great numbers in which they have been brought before his observation; to treat the subject, as far as possible, on the inductive method, and on the improved physiology and pathology of the day; and to give practical effect to numerous series of special inquiries, which have been made by him during the preceding seven years."

In his prolegomena, he gives a brief historical sketch of the views which have been entertained of phthisis from the time of Hippocrates to the present period, including the opinions of his contemporaries, of those mainly who agree with him that phthisis has an early stage before the development of tubercle, and that tubercle is not its prime cause, but rather its epigenesis; and that in this its early stage it is recognisable and curable. Such is the pith of his great argument. Whilst, however, holding this doctrine, he liberally admits that there are still those, and distinguished pathologists, who advocate the doctrine to which he is opposed—viz., that tubercle, in the chain of causation, is the first that is admissible; that without it the disease does not exist; and, consequently, if curable, it is by the arresting of the production of tubercle, and the preventing of their renewal.

The authorities he quotes in favour of his own views are Sir James Clark, to whom the work is dedicated, Dr. Hughes Bennett, Dr. Hamilton Roe, Dr. Richard Quain, Dr. Cotton, Mr. Ancell, and especially Dr. Lawson. On the opinion of the last he lays much stress, as it so nearly accords with his own, which he states he had promulgated seven years before that of the American pathologist had been published. Dr. Lawson, treating of a *precursory* stage, describes it as "a morbid state existing intermediately between the mere diathesis on the one hand, and the deposit of solid tubercles in the lungs on the other." He defines *diathesis* to be "a constitutional predisposition to disease which, under favourable circumstances, may never become developed;" whilst "the *precursory stage*, on the contrary, is the beginning of a positive morbid action which, if not arrested, surely and steadily progresses to the deposit of tubercles."*

We dwell rather on this first portion of Dr. Smith's work, inasmuch as it contains the doctrinal principle on which all that follows may be said to depend. As to the manner in which he supports this doctrine of a precursory, recognisable, curable stage or "stages" of the disease, a degree of credit is due to him; and although we cannot say that he has made us entirely converts to his special views, we are ready to admit that he has rendered them specious, and put them in such a light as to be deserving of our attention.

The subject, we think, must be admitted to be, like the prodroma of so many other diseases, of great difficulty and obscurity, and hardly permitting of demonstration, which is indeed allowed by the author. What are the principal indications of this early stage? They are, as laid down by him, chiefly the following. We shall quote his words; and they are all given with emphasis in italics:

"The appetite seldom remains natural, but is somewhat lessened in respect of food in general, and of some foods in particular, and commonly wayward and uncertain."

"There is commonly some derangement of the function of digestion, but it is frequently small, and in such cases is not important."

"The amount of food taken is commonly somewhat lessened."

"The assimilation of food is commonly defective."

"The weight and bulk of the body are almost universally lessened."

"The fixation of fluid in the body is lessened, and the elimination of it increased."

"The action of the skin is commonly increased, either absolutely or relatively to the vital transformation."

"The amount of urine evolved is perhaps equal to that in health, but varies with the activity of the other outlets of the body."

"Perspirations are common in the early, as in the later stages of phthisis, and oftentimes have a sour odour."

"There is a general tendency to defects of temperature of the body."

"The muscular power is commonly lessened."

"The circulation is commonly enfeebled and somewhat quickened."

"The respiration is shorter, shallower, feebler, and perhaps quicker."

"The vital capacity of the lungs is diminished even when there are no evidences whatever of the presence of tubercular deposits."

"Innervation is commonly lessened."

"The menstrual function is frequently disturbed, but probably not in a greater degree than occurs in health. There is much liability to leucorrhœa."

"Muscular pains about the chest are very common."

* We hope shortly to present our readers with a notice of Dr. Lawson's work, entitled, 'A Practical Treatise on Phthisis Pulmonalis; embracing its Pathology, Causes, Symptoms, and Treatment.' Cincinnati, 1861.

"The form of the throat in phthisis is peculiar, and differs much from that seen in chronic bronchitis."

"There is commonly only a small or moderate amount of coughing."

"There is commonly a small amount of expectoration."

"In the majority of cases there has been hæmoptysis in some degree, but not necessarily proceeding from the lungs."

"A tendency to vomiting not unfrequently occurs."

These aphorismal propositions are minutely and ingeniously discussed. Considered individually, it seems to us that for most part little reliance can be placed on them as indicating, what they are supposed by Dr. Smith to show, the premonitory or precursory curable stage of phthisis, inasmuch as no one of them is pathognomonic, inasmuch as any one of them may be connected with an ephemeral derangement of health, or be the precursor of some pending malady totally distinct from tuberculosis. Taken as a whole, they may indeed tolerably designate an early stage of phthisis, but we cannot venture to say the premonitory curable stage. We write thus reservedly, from the belief that the symptoms expressed in the several aphorisms are of unequal value, and likewise from labouring under the uncertainty that they all belong to the same stage of the disease, especially the first and the last mentioned—the last, such as the hæmoptysis and vomiting, particularly the hæmoptysis—*that*, we think, being rather a sign of pre-existing tubercle.

Independently of the doubts we have of the possibility of recognising phthisis in its presumed earliest stage, from the vagueness of the symptoms laid down, we have another ground for hesitation in adopting our author's conclusions—viz., our belief that tubercles may exist in the lungs, and be even somewhat advanced, without materially deranging the general health—without at least occasioning any symptom to excite attention, either of the individual affected or of his friends, so as to call for medical advice and treatment.

The doctrine advocated by Dr. Smith we necessarily, after what we have said, hold to be, as regards ordinary practice, of less value than he attaches to it; and, indeed, not altogether free from the danger of abuse—that is, from the unscrupulous few, and from the little reflecting many, who exercise the medical profession. It may be said, and most truly, that the treatment propounded in accordance with the doctrine is such as is safe, even though there be an error of prognosis. The treatment proposed by Dr. Smith being of the tonic, invigorating kind, conducive every way, and on the soundest principles, to the improvement of the general health. Granted all this, yet how serious would be the alarm excited by the announcement of a threatening consumption, and how firm would be the hold on the patient by the practitioner, on his declaring, that if not timely taken, a fatal disease must be the issue.

As bearing on the subject of phthisis generally, irrespective of its early stage, the last chapter of the work, containing the analysis of 1000 cases of the confirmed disease, is specially deserving of attention; and to us the results arrived at are confirmatory of the opinion we have expressed of the comparatively little value of those indications which are assigned as premonitory.

The first result of Dr. Smith's analysis is, that phthisical patients are of no one condition, but are of a mixed class of the community, in accordance with all former experience. Also, in accordance with this experience, that the age of the largest proportion of the victims of the disease is between twenty and thirty years; only thirteen per cent. having been under twenty years, and only a few having attained sixty years. In relation to dress, that of the whole number only 25 per cent. had never worn flannel. In relation to marriage, that 43·5 per cent. were in that state, of whom 13 per cent. were childless. Lastly, in relation to bad habits and unwholesome occupations, that "11·6 per cent. had committed sexual abuse, 18·2 per cent. had masturbated, 22 per cent. had suffered from involuntary emissions, 16 per cent. had had syphilis, and 38·5 per cent. had had gonorrhœa; 29·6 per cent. had led a bad life at some period, 24·5 per cent. had drunk to excess, and 48 per cent. had smoked tobacco; 19·3 per cent. had submitted to late hours, and 22·2 per cent. had suffered much anxiety." And that in 70 per cent. there was some complaint as to the injurious influence of their occupations; and of these as causes, exposure, long hours, close and hot rooms, bending posture, and dust or fumes were complained of in 32·1, 28·6, 24·4, 20, and 15·8 per cent. in their order." Further, that "9 per cent. had taken mercury largely, and 54 per cent. had been bled at the arm from one to twelve times." Thus (he sums up) a large proportion of the patients had been born feeble, had had feeble and short-lived children, had suffered from the effects of injurious occupations, and had been injured by the anxieties and immoralities of life. These circumstances, he justly remarks, require to be taken into account, both in relation to the treatment to be employed, and the prognosis to be formed as to the probable curability of the disease, on the supposition that one or more of them have taken effect and have been concerned in the production of the disease *ab initio.*

Our restricted limits barely allow us to notice in the briefest manner the other portions of Dr. Smith's work, those parts of it which relate to the etiology, pathology, and treatment of the disease; in which he fully keeps his promise of following the inductive method of inquiry, and of giving practical effect to the latest results of scientific research in any way bearing on the correction of the presumed malady.

We must not conclude our too brief notice of this work without strongly recommending its perusal, satisfied as we are, that though it may not doctrinally carry conviction to the mind of the reader, yet it cannot fail to give a great variety of information, and of an important kind, both on healthy and diseased functions, as illustrated by the author's own researches; and on various remedial means and particular medicines, embodying his own experience; and on other cognate matters, including climate. The value of the book for reference is enhanced by an ample table of well-arranged contents, and by a copious and useful index.

REVIEW XIV.

Den medfödte Tillukning af Spiseröret samt Bidrag til Kundskab om den Medfödte Tyndtarmstillukning. Afhandling for den medicinske Doktorgrad af H. HIRSCHSPRUNG, Cand. Med. et Chir. Kjöbenhavn. —1861.

On Congenital Occlusion of the Œsophagus, with a Contribution to our Knowledge of Congenital Occlusion of the Small Intestine. A Thesis for the Degree of Doctor in Medicine, by H. HIRSCHSPRUNG.—*Copenhagen.* 8vo, pp. 132.

THE subject of the above-named work was first suggested to the author by a case of ileus in a newly-born child, which came under his notice in the summer of 1858. On post-mortem examination, he was surprised to find that while the large intestine was open, the small intestine was obliterated throughout a great extent of its course. Subsequently, by a strange coincidence, four cases of congenital occlusion of the œsophagus came in the course of a short space of time, in the year 1860, in the Lying-in Institution, under his observation, an affection of which many obstetric practitioners in extensive practice have never met with an example.

It is an interesting subject of inquiry, whether lesions such as the above are the result of an arrest of development, or of intra-uterine disease of the fœtus. The researches of Simpson, Velpeau, Rokitansky, Cruveilhier, Billard, and others, show clearly that the embryo is liable to an amount of inflammatory disease quite competent to produce such abnormities.

The author gives a *résumé* of the cases of congenital occlusion of the œsophagus recorded previously to his own observation of four cases in the year 1860, and remarks that—

"Writers evidently considered the simple, cord-like, or complete interruption of the œsophagus, as a rare, though occasionally-occurring phenomenon, and as the rule to which, in isolated instances, the open connexion with the trachea formed a still rarer exception. My cases, however, all belonged to the latter group, which was further increased by the addition of four similar examples which, in my search after fresh cases, I was so fortunate as to meet with, and no new one has come to my knowledge in opposition to this, referrible to the other forms. According to my experience, therefore, the proportion is the reverse of what was formerly received. If, in an institution like the Copenhagen Lying-in Hospital, where 1100 or 1200 parturient women are annually taken in, seven cases of a congenital malformation may be met with in the space of sixteen years, we are forced to deny its excessive rarity, and we are justified in establishing the exception as the rule, if in the same space of time—nay, in a much longer series of years—no case is met with of the affection, to which the malformation described was formerly referred.

"Hence by the comparative frequency of its occurrence, as well as by the fact that the abnormity has hitherto always been found in living and most frequently in otherwise well-formed children, and that it is consequently the subject of the investigation of the physician, and not merely of the knife of the anatomist, occlusion of the œsophagus, combined with opening into the trachea, has the greater claim upon our attention. If to this we add, that the affection may be regarded as so new to science that it is not very generally known, and that it is in many respects capable of giving important indications, not merely to the pathological anatomist, but also to the physiologist, a description of the symptoms which are developed during the life of the child, and as accurate as possible a sketch of the varieties presented by the appearances on dissection, together with a critical examination of the nature of the malformation, ought not to be out of place." (p. 24.)

Passing over the details of fourteen cases given by the author, drawn from various sources, as well as his remarks upon each case in particular, we shall proceed to lay before our readers, as briefly as we can, a few of the more important points contained in his general observations on the pathological anatomy of the affection.

In all the cases the upper part of the œsophagus was found expanded into a large sac, formed of its normal membranes, and terminating in a rounded shape at different distances from the mouth. The greatest depth was attained in the second and twelfth cases, where the sac was not half an inch from the bifurcation of the trachea.

"From the stomach the lower extremity of the œsophagus ascends and opens into the trachea or its branches. In eight cases the point of inosculation presents a regular, smooth-edged, oval opening in the back part of the trachea at different distances from its bifurcation. The greatest distance, $\frac{3}{4}''$, is observed in the tenth case. In four cases the œsophagus opens exactly into the bifurcation; in one it opens into the right, in one into the left bronchus. In Case 7 the cartilaginous rings peculiar to the trachea are found in the lower part of the œsophagus. Five times the latter is stated to be constricted upwards in its course from the stomach.

"The distance between the two portions of the œsophagus varies. In the tenth case it is only $1'''$, in the seventh the parts are removed $1\frac{1}{2}''$ from each other; between these extremes the distance ranges.

"In nine cases a connexion between the two parts is mentioned, twice by means of a broad muscular bundle, in the other instances by means of a slight fasciculus of muscular filaments along the back of the trachea. In one case it is expressly stated that there was no trace of connexion.

"The mucous membrane is, with a single exception, described as being perfectly healthy. In the fourth case there was a peculiar verrucose development in the fundus of the sac, and a superficial inflammatory condition of the mucous membrane in the lower part, with a slight cicatricial appearance in the cardia, together with numerous small ulcerations in the stomach."

In only eleven of the cases is there a complete report of the post-mortem appearances. The author, in his systematic summary, distinguishes the phenomena dependent on defective development, from those attributable to a pathological cause. Thus in three, atresia ani was present. In none of these cases was there any trace of anal opening, nor of the lower part of the rectum. In all three cases, the intestinal tube ended in close connexion with the posterior surface of the bladder, and in two the connexion was open, so that the two organs communicated. In two of the three cases the pelvis was imperfectly developed.

Of other organs, the lung was that which was most frequently found to be affected, no doubt in consequence of the fact that the pulmonary affection was an important causal element in the lesion of the œsophagus.

As to the *etiology* of the lesion, Pagenstecher, Levy, and Gernet consider the affection to be the result of an arrest of development. Levy suggests that embryology may hereafter show that the œsophagus is originally formed in two portions. Schoeller believes that the malformation is due to a mixed, partly pathological process. In opposition to the authors just named, Rokitansky in his manual describes atresia of the œsophagus and the coexisting communication between the trachea and œsophagus as the result of a destructive process in the fœtus. There are therefore two opinions to decide between.

"It has already been shown," observes the author, "that inflammation of the mucous membrane and follicular ulcerations have been found in the œsophagus of newly-born children, and the possibility that these processes might in certain cases produce partial destruction and adhesion of the canal, cannot therefore be altogether denied. . . . . But in that case we ought to be able to demonstrate the morbid process in its ordinary stages, and to trace it to its usual termination—a more or less irregular cicatricial formation; but in opposition to this we find in only a single case (the fourth) a trace of an ulcerative process, which is evidently of a secondary nature; in no other instance is there the least evidence of cicatricial formation, but in all a regularly rounded *cul de sac*, with healthy mucous membrane, and a perfectly regular inosculating opening into the trachea, which decidedly refutes the idea of any connexion with a destructive ulceration. Moreover, the affection is marked by different forms, which could not possibly depend upon an inflammatory process. How, for example, could it be supposed that the formation of cartilaginous rings in the inferior part of the œsophagus, mentioned in the sixth case, should be so produced? A perforating ulceration in the wall of partition might give rise to a communication between the two parallel canals, but can it be supposed that the opening into the right or left bronchus could be produced in a similar mode without the existence of more considerable destruction? This question must be answered decidedly in the negative, and we are therefore justified in raising a strong objection to Rokitansky's theory, which is not based upon his own experience, and which manifests only a superficial knowledge of the cases he quotes. At the last Scandinavian meeting of naturalists, it was only want of time which prevented Professor Levy, when exhibiting the preparations connected with this subject, from entering a decided protest against Rokitansky's theory in general, and especially against his use of Levy's name in support of the same." (p. 62.)

The facts which may thus be brought forward against the theory referring occlusion of the œsophagus to an inflammatory process, are strongly in favour of the view that a certain connexion exists between the affection and an arrested development. But in addition to these, the frequent coexistence of the lesion in question with other effects of arrested development, of the nature of which there can be no doubt, is strongly corroborative of the same opinion. Thus, in three of the fourteen cases already referred to, malformation of the œsophagus was combined with absence of the anal orifice, with opening of the inferior portion of the intestine into the bladder, or with adhesion of these two parts, with spina bifida—all malformations whose nature is evident, and which by analogy tend to strengthen the opinion that the affection of the œsophagus belongs to the same class. The author draws further arguments in support of the same view from comparative anatomy and from the history of development. For example, it is well known that in some fishes the swimming bladder, which is generally looked upon as the rudiment of an undeveloped lung, is in open connexion with the œsophagus, and Valentin and Ammon hold, in opposition to the old opinion, that the œsophagus is formed by the junction of two parts. Of these, it is always the lower which communicates with the air-tube.

As to the *symptomatology* of the lesion, the obstruction to swallowing is the first decided indication of the nature of the case. The first spoonful is taken without difficulty. When the third or fourth is given, the child begins to twist its mouth in a peculiar manner, and some of the fluid is regurgitated. Further attempts to give food produce still more distressing symptoms, respiration is interfered with, and the drink is rejected through the nose and mouth. An attempt to introduce a probang now reveals the true state of things. The fæces at first present the usual appearance of meconium, and subsequently assume a yellow colour (a proof that the change of colour is not due to the food ingested), but are rather scanty. The child emaciates, and dies of starvation on the third or fourth day.

The second part of the work is devoted to the consideration of congenital occlusion of the small intestine. Of this lesion, as distinguished from occlusion of the terminal portion of the intestinal canal, the literature is very scanty.

"Congenital occlusion of the small intestine," observes the author, after an able review of the opinions of various writers as to the mode of development of the intestinal tract, and of the theories which have been brought forward to account for its congenital obliteration, "present themselves under several forms, which most certainly are of different origin, and cannot be referred to the same group. Like Schaefer, I make the form of the atresia the basis of my division, without forgetting the great influence the seat of the occlusion has upon the type of the disease."

A. Congenital stricture of the small intestine.

B. Valve-like occlusion.

C. Complete interruption.

Having detailed, from his own experience and that of others, the history of several cases referrible to each of the above categories, the author gives a *résumé* of the whole. The number of recorded cases of occlusion of the small intestine is 31. As to the form of the lesion, these cases are thus divided: in 4 there was stricture, in 6 valve-like occlusion, in 21 complete interruption, with or without remains of the canal. As to locality, 16 cases (3 of stricture, 3 of valvular formation, and 10 of complete interruption) occurred in the duodenum; in 6 (1 of stricture, 2 of valvular formation, and 3 of complete interruption) the lesion was situated in the lowest part of the small intestine, close to the cæcum; in 9 (one of valvular formation, and 8 of complete interruption) it was scattered over the intervening portion of the canal. Hence the duodenum appears to be peculiarly liable to these lesions; whatever form the abnormity assumed in the duodenum, it was in no instance repeated in the course of the canal. In 6 cases there were unmistakeable signs of peritonitis having occurred at an early period of fœtal life, undoubtedly before the occlusion took place, and the latter in these instances always presented itself under the form of complete interruption. In 6 there was recent peritonitis, which probably had set in after birth, once after an attempt to form an artificial anus, once in connexion with perforation of the gangrenous intestine. Whether the occlusion had its seat above or below the opening of the ductus choledochus, there was usually excessive dilatation of the part lying above the lesion. In four cases of occlusion of the duodenum (3 of them stricture) there was more or less blood in what was thrown up, once in connexion with effusion of blood in other organs. In 5 cases of occlusion of the small intestine, the presence of jaundice is recorded, and in 4 of these the lesion existed in the duodenum.

The *symptoms* vary according as the occlusion is situated above or below the opening of the gall-duct. In the latter case, vomiting of meconium occurs sooner or later after birth, the strength diminishes, and death occurs in a few days without any considerable emaciation. In the former, the vomited matter consists chiefly of the food ingested.

In 24 instances the day of the child's death is recorded. The longest duration of life was thirteen days, the shortest twenty hours, the average five days (in occlusion of the œsophagus the child lived only to the third or fourth day). In the 12 cases of occlusion of the duodenum, in which the day of death is mentioned, the child lived on an average five days and a half. If we divide these cases into two classes, according as the lesion occurred (five times) above, or (seven times) below the diverticulum Vateri, we find that the duration of life in the first case, in which, consequently, the hepatic secretion had a free passage through the intestine, was almost seven days; while under the opposite circumstances it was only four days and a half. In the 12 cases in which the occlusion took place lower down, the children lived on an average five days and a half.

As to *treatment*, interference in those cases where the lesion is situated in the duodenum is, of course, out of the question; operation can be attempted only in cases attended with symptoms of ileus. Apart from those cases where there is a possibility of restoring the natural passage—that is, where the affection is situated in the lowest part of the rectum, there is only one mode by which life can be maintained—namely, the formation of an artificial anus. The objections to this operation are obvious, still, if the seat of the affection has been diagnosed to exist in the rectum, if attempts to restore the natural passage have failed, if circumstances are otherwise favourable, if the child is at the full period, and is in other respects well made, no one ought to hesitate to open the intestine higher up, so as by means of the formation of an artificial anus to procure the evacuation of the contents of the bowel. Rochard's report to the Académie de Médecine,* of five successful cases of the formation of artificial anus in newly born children with imperforate rectum, is sufficient to prove that the operation may succeed. The prospect is, of course, more unfavourable if we are not in a position to decide beforehand where the occlusion is situated, and

* L'Union Médicale, 1859, No. 11.

must therefore be prepared, perhaps, to find so large a portion of the intestinal tract unable to discharge its functions, that the nutrition of the child is impossible; but until experience has taught us to decide whether the occlusion is situated higher up or lower down, the indication for attempting the operation will not be weakened when we bear in mind the fate which inevitably awaits the child if the case be left to itself.

Such is a brief sketch of the leading points contained in Dr. Hirschsprung's volume, in which he has, in a clear and comprehensive manner, collated the facts already recorded in connexion with the lesions he describes, adding considerably to the literature of the subject from his own experience. The work is very neatly brought out, and the author's own cases are illustrated with excellent woodcuts, which give a better idea of the exact appearance on dissection than can be conveyed by any description, however full and accurate. The book must therefore be received as a valuable contribution to medical literature.*

Review XV.

Clinical Essays. By Benjamin W. Richardson, M.A., M.D., Senior Physician to the Royal Infirmary for Diseases of the Chest. ('Asclepiad,' Vol. I.)—*London*, 1862. pp. 272.

It has been often asserted that the events of most active-minded men, if carefully scrutinized, would furnish an ample store of material suitable for the biographer. Certain it is that the professional experience of every medical man who loves his work, if duly registered, would present most interesting and instructive facts and suggestions. Dr. Richardson evidently thinks so, and presents us with Volume I. of a series of essays which, if we may judge from the present instalment, promises well to fulfil the intention which he sets forth in his preface, wherein he observes:

> "While it will be my object in this series always to consult, as far as in me lies, the requirements of practical medicine, I shall endeavour to weave into the subjects discussed such matters bearing on the current theories of medicine as shall tend to open the way to new and more comprehensive views and to a sounder and more rational practice."

Of these essays we shall endeavour to present the reader with a brief account. They are seven in number, and are entitled as follows: I. Subclavian Murmur. II. On a Diseased Condition of the Nails. III. On Reduplication of the Second Sound of the Heart. IV. Contributions towards a more perfect Clinical History of Scarlet Fever. V. On Pulsatile Pulmonic Crepitation. VI. On Uræmic Coma. VII. On Cardiac Apnœa.

Essay I., concerned with the subject of subclavian murmur, a thoracic sound concerning the meaning of which every practical physician

* The reader may be referred to instances of congenital occlusion of the œsophagus recorded in the 'Transactions of the Pathological Society of London.' Thus a case is recorded at p. 91 of vol. iii., by Dr. P. B. Ayres; another at p. 52 of vol. vii., by Dr. John Ogle; and a third at p. 173 of vol. viii., by Dr. Ogier Ward.

has had his difficulties, deals with the question very conclusively, being founded on the stethoscopical examination of no less than two thousand patients, among whom he met with the murmur in 51 cases—i.e., 2·55 per cent. It is determined that the subclavian murmur is an arterial murmur, modified by respiration, the opinions of Drs. Kirke, Sibson, and Thorburn being canvassed as regards its causation; and that it arises from pressure brought to bear upon the artery either, (1), by *diseased lung*, from tubercular deposit or indurated and enlarged bronchial tubes, or from distant bronchial disease, the pressure being produced by undue distension of the lung acting for compensation; OR (2), *without any disease in the structure of the lung*, the pressure being exerted by the action of the subclavius muscle, and the result of occupations in which the "arms are being constantly thrown forwards and downwards, as occurs in wood-planing, hand-sawing, French-polishing, and the like." The two methods by which the sound is educed may of course be working in combination, and the sound in question may be INTENSIFIED by anæmia. The exact character of the subclavian murmur, and the diagnosis between it and that produced by aneurysm, valvular disease, pleural effusion, and bronchitis, are fully considered. Our space will not permit us to follow the author through all the details involved in such close differential diagnosis as he describes, but we will give the absolute diagnosis of this murmur which he offers. He observes:

"It is a murmur beneath one or both of the clavicles, confined to the subclavian regions, and synchronous with the pulse.

"It is coarse and loud—a rasp; or sharp and musical—a whistle; or soft and musical—a coo; or shot-like, coming down on the ear bluntly and dead.

"It is always to be arrested by pressure sufficient to check the pulse at the wrist, made by the stethoscope on the subclavian artery.

"It is susceptible of modification by the movements of respiration; a moderately full inspiration may develope it; a very deep inspiration may either intensify it or stop it; a prolonged expiration will often remove it.

"Movements of the arm modify the sound. The position of the arm when it is nearly down by the side of the patient gives mostly the minimum of intensity; the position of the arm when slightly raised above the right angle to the body is that in which the maximum intensity is usually gained, while movements between their extremes produce varying gradations of both the quality and the frequency of the murmur.

"In extreme cases there is to be felt, on application of the finger over the artery at the point where the murmur is heard, a marked fremitus, of which the patient may be conscious as well as the operator." (p. 25.)

Dr. Richardson has, within the last three years, had four cases referred to him as aneurysm within the thorax, which proved to be merely well-marked cases of subclavian murmur. According to his experience, when well marked in the early stages of phthisis, this murmur disappears in the later stages, those in which the deposit of tubercle has undergone the changes of softening and removal. We learn that out of the 51 instances of subclavian bruit which the 2000 cases presented, 48 were in males, and only 3 in females; also that the youngest subject in which it occurred was eighteen years of age, and that the murmur is always best developed when occurring in the right subclavian space, but that it is *most common on the left side.* It must be remembered, moreover, that the murmur *may* be produced by a light stethoscopic pressure on an artery thrown slightly towards the surface.

Essay II., "On a Diseased Condition of the Nails," is illustrated by a coloured lithograph. The condition in question is known in France under the name of "psoriasis of the nails," and is considered to be "allied to squamous disorders of the skin." There is at first numbness and tingling beneath the nail, the nail (of fingers or toes) then becomes brittle, dark on the surface, shining as if coated by varnish, pitted and indented, then lifted from the fingers, and finally destroyed. The disease apparently commences in the matrix, epithelial layers being interposed between the secreting membrane and the under surface of the nails, separating the structures. Three cases of the disease are adduced. Its causes are perfectly obscure; but as to the treatment, the evidence is strongly in favour of arsenic as a remedy.

Essay III., "On the Reduplication of the Second Sound of the Heart," a phenomenon which is occasionally met with, is engaged in the consideration of two cases exhibiting the peculiarity in question, and in reflections upon its causation, diagnosis, and bearing on practice and treatment. It is concluded that the phenomenon is produced by a want of simultaneous action in the aortic and pulmonary semilunar valves, and this view well accords with the nature of the general symptoms (the disturbance of the circulation, palpitation, oppressed breathing, irregular pulse, &c.) which accompany the physical phenomenon. A most interesting case is adduced, in which both the general symptoms and the physical phenomenon yielded to the use of the hot-bath, and consequent restoration of balance of the pulmonary and systemic circulation.

Essay IV. consists of a gathering together of observations which have been given to the profession through other channels, along with certain new remarks. For these the experience of several friends, and also the Registrar-General's Reports, constitute a basis, and we find the following questions discussed—viz., "Occurrence of Scarlet Fever at different periods of Life;" "the Influence of Sex on the occurrence of the Disease;" "the Influence of Meteorological Conditions in relation to Scarlet Fever;" "the Recurrence of Scarlet Fever in the same Person;" "the Mortality of the Disease;" and "the Types of Scarlet Fever." We have also a section on "Doubtful Scarlet Fever," in which the author states his belief in another poison "almost identical in its effects with scarlatinal poison, but which has not the physical properties rendering it capable of distribution and contagion;" one "On Scarlet Fever as complicated with Acute Rheumatic Fever;" also one on the "chemical pathology" of the disease, in which it is characterized as one in which there is increase of fibrine in the blood, and of uric acid and urea, along with reduction of water and the chlorides, in the urine. Finally, we have sections "On the Poison of Scarlet Fever in relation to its Propagation and its Mode of Action," and on the treatment. As regards these two considerations, the author observes:

"The primary poison of scarlet fever is probably solid in regard to its physical properties. It travels but a very little distance, except it be held in contact with some other body, such as an article of dress. It is destructible by heat at boiling-point. It is thrown off from the affected person by the skin or the lungs, and is received by the susceptible person by respiration.

"The symptoms of scarlet fever are probably due to the production in the economy of a secondary poison having the physiological properties of an acid, such as the lactic acid. This secondary poison is the product of a modified zymotic process in the blood, induced by the absorption of the primary poison.

"The curative treatment consists in producing free action of the skin, and in maintaining the fluidity of the blood by the administration of a solvent remedy (acetic acid or ammonia). The hygienic treatment consists in the admission of pure air to the patient, and in the establishment of a strict quarantine." (p. 114.)

The author remarks, *inter alia*, on the rarity of the urine being albuminous in adults after scarlet fever, and on the tendency to death even in mild cases of scarlet fever, owing to the formation of fibrinous clots in the right side of the heart; also that, according to his researches, there is no case known in which a second attack of scarlet fever has proved fatal. He deprecates any extensive application of caustics to the throat in this disease.

Essay V. is occupied in rendering the reader familiar with "Pulsatile Pulmonic Crepitation"—a crepitant sound connected with pulsation, and apart from any indication of pneumonia or tubercles. Four cases of this affection are instanced at length, three of which had been previously described in other places. A case related by Dr. Woillez in 'L'Union Médicale'* is also quoted. The differential diagnosis between this crepitation and that from pneumonia, tubercle, moist friction, as well as the "minute mucous râle," is minutely given, and the exact character of this crepitation is stated as follows:

"Pulsatile pulmonic crepitation is a loud, harsh, crackling sound, resembling that which may be produced by inflating a piece of lung-tissue, and during the inflation making forcible quick compression of the lung, so as to dislodge rapidly the air from the vesicles. The sound is heard only at the time when a pulsating structure, near to the point where it is met with, is presenting its impulse. Even then it is elicited only when the lungs are charged with air. Sustained inspiration gives a condition in which the crepitation is presented at every impulse of the pulsating organ. Sustained expiration reduces the crepitation, leaving to be heard the simple pulsatile sound of the vascular structure, and nothing more. The sound may be met with over the heart—i.e., between the heart and the thoracic wall; or over the aorta—i.e., between the aorta and the thoracic wall. Percussion over the points where the crepitation is detected yields a resonance more or less clear, according to the state of the respiration. During inspiration the resonance is very clear; during expiration it is reduced, but is less dull than is common on percussion over the body of the heart. Lastly, the sound under the stethoscope is superficial in character, seeming as though it proceeded from a limited point immediately below the thoracic parietes." (p. 129.)

In all the cases referred to there was marked pulmonic disease, which offered one analogous fact throughout — viz., unmistakeable signs of pre-existent pleurisy. Out of four cases, two had also suffered

* June 16th, 1860.

from long-standing bronchial disorder and emphysema, and the other two gave evidence of tubercle.

In Essay VI. the subject of Uræmic Coma is very interestingly handled; its diagnostic symptoms, complications, and pathology being discussed with precision. We have a highly instructive section on the *causation* of uræmic coma, in which much light is thrown on the subject by the results of experiment on the lower animals, and in which the views of Frerichs and Hammond are brought under notice. Dr. Richardson determines that the uræmic coma depends on suppression of the urine and on the presence of urea in the blood acting as a narcotic poison, and *not on its resolution into another poison*—viz., *carbonate of ammonia.*

In the closing sections of this essay, the relations of uræmia to forensic medicine, and the means of distinguishing between this form of poisoning and that from belladonna, opium, the narcotico-acrids, such as veratria, hellibore, colchicum, &c., and the cyanides, are considered. Finally, the treatment is divided under two headings—(1), the prevention of an acute attack; and (2), the application of direct measures on occasions when acute symptoms have been set up. We are told that, by way of prevention, the eliminative action of the skin is to be secured, active exercise and suitable diet ensured, purgatives occasionally given, and *diuretics to be avoided*, whilst in the treatment of an acute attack the skin and bowels should be brought into action, and bloodletting resorted to. The author does not fail to draw attention to the dangerous susceptibility on the part of uræmic patients to the action of certain medicines, specially mercury and opium, and our own experience with regard to this latter remedy quite justifies the following statements of Dr. Richardson:

"I cannot conceal my belief, indeed, that in many cases where opium has seemed to destroy life when given in very small doses, the 'idiosyncrasy' assumed to have been present has consisted in a condition of kidney, during the existence of which coma in its acute form might have stood prognosticated, had the earlier symptoms been known." (p. 152.)

In Essay the *Seventh* and last, the phenomena, diagnosis, causation, pathology, and treatment of cardiac apnœa are treated of. This affection appears to be much the same as angina pectoris, and is described as follows:

"It is an apnœa with open air-passages—not panting breathlessness, but suppressed breathing. The struggle for breath is due to spasmodic contraction of the muscles of respiration, and consequently is marked by no rapid effort of those muscles to overcome a difficulty in the respiratory tract. The apnœa is described by the patient, if he can express himself, as arising from without, as from external pressure, as though his chest were compressed and stiffened. There is darting pain through the chest—cramps. The other parts of the muscular system, if they are invaded, are cramped, not convulsed. The mind is usually unaffected. During the paroxysms there is either irregularity or prolonged absence of the pulse, and the same condition of necessity is presented by the heart.

"The surface of the body is cold and pale; the countenance sterned with anguish, but not incessant; if the spasm permit, there is constant movement on the part of the patient in the effort to obtain relief. There is always pain of the acute kind, more or less marked. The pain is most frequent and most lancinating between two well-defined points—the lower part of the sternum, through the chest towards the dorsal vertebræ; with this pain the breathing is locked up. I believe this pain to depend on spasmodic contraction of the diaphragm. There is generally more or less of muscular spasm and pain in other parts of the body. In some cases one limb is thus affected, as one arm, which during the whole seizure may be in a state of intense suffering, with more or less of rigidity; in other cases, where seizures are very severe, this spasmodic condition may extend throughout the whole of the muscular system, causing a general tetanic constriction. In prolonged cases of cardiac apnœa occurring in children, as in examples where the symptoms arise from fibrinous deposit on the right side of the heart, there is very frequently emphysema of the lungs, progressing to such an extent that the chest-wall is raised anteriorly; in the cases specified, the occurrences named are certain indications of cardiac obstruction in the right cavities, and of apnœa dependent upon that cause. In all cases of death from cardiac apnœa, the final act is one of persistent muscular contraction; the heart first failing, the muscles of the chest become fixed from tonic spasm; the voluntary muscles follow next; and the whole body, left more or less rigid, may pass into rigor mortis without any intervening relaxation of the muscular organs."(p. 243.)

Following the above account of an acute paroxysm of cardiac apnœa, we have the diagnosis between this form of apnœa and attacks of laryngeal, bronchial, and pneumonic apnœa, as also tetanus and the poisonous effects of strychnia. The author describes certain symptoms which are preliminary in chronic cases, and which "may be said to give the idea of a predisposition to an acute attack." It would occupy too much of our space to dwell further on this part of the subject.

As regards the pathology of the disease (which is the same as is often termed angina pectoris), the most common lesions in the circulatory system by which it is produced, are thinning and degeneration of the cardiac walls, with or without atheromatous degeneration of the valves, coronary vessels, or aorta. We have also, however, as causes, obstruction on the right side of the heart (from fibrinous concretion or disease of the pulmonary artery), mechanical embarrassment of the heart from external pressure, pericardial adhesion, effusion or ossification of the pericardium, spasm of the heart itself, and the influence of a poison, as tobacco, strychnia, ammonia, chloroform; and of some of these causes illustrative cases are supplied. The author dilates much and aptly upon the influence of fibrinous bands entangling the structures of the heart's valves, and of concretions of fibrin, in producing death in this disease.

As respects treatment, but little of a tangible character is offered to our notice; the author believes that the hot brandy-and-water which often is beneficially given by bystanders in paroxysms of cardiac apnœa, does good by virtue, not of the contained alcohol, but of the "diffusible caloric conveyed by the water," according to the law "that without a due measure of caloric a muscle must be spasmodically contracted."

Alkalies must be given if the disease be traceable to fibrin concretions in the circulatory system, and *in extremis*, artificial respiration is looked upon as the only source of relief.

Such is a concise description of the contents of Dr. Richardson's first volume of the 'Asclepiad.' We did not propose in any way to do more than to bestow a general attention on the questions broached therein; and the reader will find several points of much interest treated of by the author which we have not adduced or even hinted at. Many of these will evince much sound judgment, and proof that no opportunities for skilled observation have been lost; others, again, have merely a speculative interest—constituting a display rather of theoretic ingenuity on the part of the writer than of rigid deduction; but we refrain at present from their discussion.

Most of what is said in the book is well said. Here and there we could point to a piece of affected phraseology which we think unsuitable in such a work, as, for example, where, at p. 149, drunkenness, syphilis, and mercury are spoken of as a "Trinity of disease-makers." Nor, again, should we expect the plain fact of a certain gentleman having incurred syphilis to be described by the wonderful statement that "twenty years ago he had a slight attack of illness which taught him that Venus not unfrequently transfers her devotees from the sphere of her own charms to the gentle guardianship of Mercury."

If free from such superficial blemishes, we shall be glad to encounter Vol. II. of the 'Asclepiad.'

PART SECOND.

Bibliographical Record.

ART. I.—*Introduction to the Study of the Foraminifera.* By W. B. CARPENTER, M.D., F.R.S., &c.; assisted by W. K. PARKER and T. RUPERT JONES, F.G.S.—*London*, published for the Ray Society, 1862. pp. 320, with 22 plates.

THIS is one of the most important monographs issued by the Ray Society, and calculated to reflect the highest credit upon science in this country. Few of our readers, we apprehend, are at the present day unacquainted with the beautiful minute creatures generally known as Foraminifera, or sometimes as Polythalamia. They are peculiarly marine animals, are many of them found at great depths of the sea, and often constitute a considerable proportion of the sandy *débris* at the bottom of the ocean, and in the fossil state enter largely into the formation of geological strata. The forms best known to the public at large are those which resemble in miniature the beautiful shells of the Nautili—a resemblance, indeed, which betrayed certain former naturalists into the supposition that they were minute Cephalopoda. However, modern research has demolished the hypothesis of so high an affinity, and shown that, notwithstanding the varied complexity of the shells of many of them, they are, as organic beings, of the simplest possible character, consisting of animal matter in its primitive form, of a homogeneous, jelly-like substance, known as sarcode, devoid of organization or of parts or tissues endued with special functions, except some rudimentary elements connected with reproduction.

We cannot go into details respecting the character of these animals, although we might adduce many facts of high moment in their physiological bearings. Some of the teachings obtainable from the study of the lowest forms of animal and vegetable life we, however, would desire to place before our readers at some future time; but in the present place we have to congratulate the members of the Ray Society, as a body of naturalists, on the valuable volume which their subscriptions have evoked from the press, and which would, in all probability, have never seen the light without the fostering aid of such a society. It is in the publication of such works as the present one, which few authors could afford the expense of publishing on their own account, and which publishers, looking only, as they must, at the commercial aspect of the matter, would never think of bringing out, that a scientific publishing society can do so much good for the encouragement of scientific men and the promotion of science. Here, in the instance before us, the Ray Society has been the means of giving to the world a mass of original researches, and of illustrating them by twenty-two lithographed plates, and thereby brought honour on Dr. Carpenter and his coadjutors in the work, and added to the reputation of English naturalists at large among the scientific men of all countries.

ART. II.—*Della temperatura delle Orine in diverse ore del giorno e in diversi climi.* Ricerche sperimentali del Dottor PAOLO MONTEGAZZA.—*Milan.*

On the Temperature of the Urine in Different Hours of the Day and in Different Climates. By Dr. PAOLO MONTEGAZZA.—*Milan.* (Pamphlet.)

IN connexion with studies and investigations on animal heat, Dr. Montegazza in the present *brochure* presents the public with a *résumé* of observations made, in a space of two years, on the temperature of the urine, and he supplements them by a further series during a rapid voyage and return between Bordeaux and Buenos Ayres, including, in point of time with his stay at Buenos Ayres, a period from July 29th to October 10th of the same year.

The observations of Dr. Montegazza on the voyage lead him to the conclusion that during sudden and considerable variations, amounting to 25° Cent. of external temperature, the urine may be subject in the body to a change expressed by 3°·25 Cent. This is the extreme of variation noted by him.

The urine changes correspondingly with marked but not with slight affections of the external temperature; moreover, subjective sensations of heat and cold are accompanied by corresponding changes in the temperature of the secretion. A lengthened exposure to solar heat causes an elevation of its temperature amounting to 1°·1 Cent. Alcoholic drinks, muscular exertion, any degree of pyrexia, even its preceding malaise, raise the temperature of the urine; but not sea-sickness, as far as he could judge. On the other hand, a notable diminution is apparent after perspirations terminating a feverish attack. The observations on the voyage were 241 in number, and lead to the above conclusions. They are set forth in tables, which bear every appearance of care and accuracy. We may briefly notice the result of the previous observations made in Italy.

1. In a scale of atmospheric variation amounting to 28°·5 Cent. between the months of February and August, the variation in the urine was 1°·55 Cent.

2. The minimum temperature of the urine is during the night.

3. The temperature rises in the daytime from five A.M., attaining a maximum between ten and eleven in the morning. After eleven A.M. the temperature slowly falls, and again rises to a maximum at five P.M. From this point it falls on approach of night. The maximum of morning and that of evening are about equal.

4. Our author has ascertained that these changes are not modified by variation in the hours of meal.

5. The genital function in its exercise does not affect the temperature of the urine.

In accordance with those of our author, the experiments of Damrosch, made with the thermometer in the axillary cavity, are quoted. This observer also found the maximum of vital heat to occur at five P.M., and the minimum at seven P.M., and, moreover, a marked increase of temperature at ten in the morning, with a subsequent depression. The frequency of the pulse, according to the same observer, attains its maximum at five P.M., and declines from that hour to seven P.M. In the morning the pulse preserves a uniform frequency from seven A.M. to ten A.M.; it then increases till one P.M., an interval of time during which vital temperature is decreasing—this augmented frequency being, perhaps, a tardy physiological sequence of the previously augmented temperature. To the industry of other observers we confidently leave the corroboration and comparison of these facts.

ART. III.—*Lectures on the Germs and Vestiges of Disease, and on the Prevention of the Invasion and Fatality of Disease by Periodical Examinations.* By HORACE DOBELL, M.D.—*London*, 1861.

THESE lectures were delivered at the Royal Infirmary for Diseases of the Chest, to which institution the author is attached as physician. They are six in number, and, unlike many productions of the medical press, have some novelty in their composition—a quality which, *quoad valet*, is certainly a recommendation for them. A great portion of the novelty, however, consists in the terminology which the author has invented to express various pathological and etiological relations which other writers and readers have been content to recognise by other names. This terminology, in the construction of which there is much precision of definition, imparts to these lectures a fictitious originality, and at the same time gives their reader much trouble in translating the author's statements or ideas into ordinary medical language. We are willing to admit some of the terms and phrases to be well chosen and explicit, but whilst doing so we must remark that the author does not advance medical knowledge in a degree at all proportionate with the mass of verbiage with which he invests it.

We are first introduced to the "ultimatum" of animal life, which varies with race, species, age, sex, and conditions of life. Then we have to discover "the determination of force at each epoch of the animal's career, in that direction essential at the time to the attainment of the ultimatum," and presently have to make ourselves familiar with a "vitalized mode of force" and a "lifeless mode of force," respectively represented by three letters, V. M. F. and L. M. F. The V. M. F. obtains a share of physiological discussion and illustration, not without merit indeed, and the

"Scientific conclusions are as follows: that, 1. The V. M. F. may be altered in quantity and quality by numerous causes. 2. That these causes may affect either the existing individual, a succeeding generation, or both. 3. That these causes are, *principally*, the vestiges of disease, existing or coetaneous disease, and the conditions of life. . . . . The practical conclusions are these: 1. That the vestiges of disease, coetaneous disease, and the conditions of life may determine the efficiency or non-efficiency of the V. M. F., to prevent or arrest the invasion and progress of premature destructive changes in the organism, to secure its repair when damaged, to produce an offspring, to endow that offspring with V. M. F. of normal quantity and quality. 2. That abnormal conditions of the V. M. F., either congenital or acquired, may be changed by changes in the conditions of life and by means which exert an influence on the vestiges of disease; and that the influence of such changes may affect not only the individual but a succeeding generation."

Quitting the section devoted to the illustration of these deductions by an appeal to the teachings of human and comparative physiology, we arrive at the pathological part of these lectures, and are then introduced to the "essential antecedent of the disease," and to a consideration of its causes; to "the predisposing antecedents of the disease" and to their causes; to "the causes of fatality" in fatal cases; to "the causes of the causes of fatality;" and lastly, to "the vestiges which may remain from the disease under consideration when it does not terminate in death." These are the headings under which the phenomena, the causes, the progress, the complications and the results—or, in a word, the history of a disease—are arranged; and, for the sake of illustration, the author presents "an analysis of the natural history" of fever, apoplexy and paralysis, heart disease and pericarditis, and of some other diseases, represented according to the terms of the above formula.

Now, if we translate into ordinary medical language the "scientific" and "practical" conclusions at which Dr. Dobell arrives, what additional knowledge, or what clearer insight, we would ask, do we obtain? And again, what clearer apprehension of the history of a disease do we gain by the complicated system of analysis proposed? In answering these queries, we would refer the reader to the analyses of the history of the diseases given by the author, and to his tabular representation of the interdependence of disease.

The reader of the treatise under notice will, we are confident, hold us to be justified in thus speaking of it. At the same time, the author deserves undoubted credit for putting prominently forward, as essential to the correct appreciation and treatment of disease, the necessity of recognising the effects of hereditary taint, and of the lingering results of previous disease in the patient, or, to use his own phraseology, the effects or results of the "vestiges of disease."

The modifying effects of inherited and of previous disease on an existing malady are in some instances, as in the case of gout and of syphilis, generally recognised; but we are glad to be forcibly reminded by Dr. Dobell that similar modifying influences are much more widely in operation than we are apt, in the routine of practice, to recognise; and we have much, again, to admire in his illustrations of the coordination of diseases and their mutual interdependence. But could he tell us his experience, and append his illustrations, in the accepted diction of the time, he might produce a treatise of some value to the profession, and acquire such credit as a scientific and practical physician as the present book will hardly afford him.

The practice which Dr. Dobell advocates, of submitting the public to periodical minute medical examination and instruction in hygiene, &c., does not appear likely to find many supporters. Doctors are at a discount with individuals who suppose themselves, whether rightly or wrongly, to be in good health, and will not trouble or pay them for an opinion whether their V. M. F. be sufficient or deficient. Doubtless, prevention is better than cure, and it would be wise on the part of the public to demand medical skill before actual disease is set up; but that golden age is not yet arrived when individuals will fee their doctors for services in the staving off disease, of the possible proximity of which they are themselves unconscious. Happily, in some matters of hygiene, from experience of the evils of inattention to them, the public are willing to receive lessons from doctors when forced upon them and gratuitously given; but in others, as in the case of marriages contracted in spite of unfavourable conditions, the opinion of the profession is ignored, and no doctor is likely to be asked or heard in the matter.

ART. IV.—*Proceedings of the Pathological Society of Philadelphia*, 1860. Vol. I. pp. 307.

THIS volume constitutes a repertorium of facts which, from the nature of the Society, are of a very mixed nature as respects both their character and value. The cases related are sufficiently numerous, considering that the "resident members" do not exceed forty in number, and among them we find one or two papers of considerable proportions. The utility of the collection is, however, somewhat diminished by the fact that the various specimens described are not arranged in any order; and thus much time is lost in comparing those of the same category one with another. The following cases may be enumerated as of special interest: "Deposits of Carbonate of Lime in the Renal Tubes," exhibited by Dr. Mitchell; "Fistula of the Gall Bladder," by Dr. Morton; "Cystic Tumours of the Nates, obstructing Delivery," by Dr. Keller; "Bony Tumour of the Scrotum, taken from a Chinese;" also "Deformity of the Neck of the Thigh-bone, simulating Fracture, with Ossific Union;" and "Cysts from the Liver, coughed up through the Pulmonary Passages, attended by Numbness in the Foot and Arm of the Right Side," by Dr. Richardson; "Fibrous Tumour of the Uterus removed during Labour," by Dr. Keating; "Extensive Ossific Deposit, projecting into the Auricle," by Dr. Forbes; "Abscess of the Ovary, communicating with the Sigmoïd Flexure of the Colon;" "Astragalus discharged through an Opening in the Foot," by Dr. Hutchinson; "Dilatation of the Meningeal Artery, producing a Bony Protuberance in the Head," by Dr. Agnew; "Perforation of the Aortic Valves," by Dr. Mitchell; "Cystic Growths on the Intestines;" "Fatty Degeneration of a Nerve in Tetanus;" "Communication of the Appendix Vermiformis with the Ileum through a Mesenteric Gland;" "Double Pregnancy—one Tubal, the other Uterine," by Dr. Packard; "Severance of Cervix Uteri during Labour," by Dr. Keller. We have also several isolated cases of disease of the pancreas related, one being of a myeloïd nature. Two papers, remarkable for their length and for the amount of research involved therein, are, the one by Dr. Da Costa "On Cancer of the Pancreas," accompanied by a tabular list of thirty-seven cases brought together from various sources (p. 109); and the other, "On the Pyogenic or Suppurative Diathesis," by Dr. Packard. In many instances an epitome is given of the discussions which occurred on the cases related and specimens exhibited. A few woodcuts, conspicuous by their rarity, illustrate the volume.

ART. V.—*Prolapsus, Fistula in Ano, and Hæmorrhoïdal Affections: their Pathology and Treatment.* By T. J. ASHTON.—*London*, 1862. pp. 182.

MR. ASHTON'S large work 'On the Diseases, Injuries, and Malformations of the Rectum' is well known to the profession, and has been favourably noticed in the pages of this Review. The present small treatise on the most common diseases of the rectum owes its origin, as the author informs us, to the suggestion that his great book referred to has grown too large for "the wants of many practitioners," who can dispense, as seems implied, with dissertations on those affections of the lower bowel of more rare occurrence. Is this suggestion a laudable and correct one, or is it not rather a book-making expedient? When a medical practitioner purchases a work on some special class of diseases, is it not usually with the object of informing himself respecting certain of their number only briefly considered in their well-read system of general medicine or surgery, as the case may be; or, in other words, respecting the rarer forms? We consider this to be the case, and indeed the publication of a third edition of so large a volume as Mr. Ashton's on the diseases of one region indicates clearly enough, in our apprehension, that medical men have valued it as a comprehensive special work, conveying information to them over and above what is presented in any treatise on surgery at large.

We have no wish to decry the value of the information conveyed in the small book before us; indeed, we regard it as an excellent exposition of the nature and treatment of the diseases considered in its pages, but at the same time its production looks very much like book-making, and the works of the size and character of the one under notice must anticipate a sale more among the general public than among the members of the profession; indeed, these little books on one or a few particular diseases we are apt to view with suspicion, for they are too often discoverable on the consulting-room table, and are besides more readily transferable to the hands of the public than to the studies of medical men.

ART. VI.—*Sixteenth Report of the Commissioners in Lunacy of England to the Lord Chancellor.* Ordered by the House of Commons to be printed, July 16th, 1862.

THIS valuable Report, for the year ending January 1st, 1862, has just been published. It is much more extensive in its character than many preceding ones, and contains a large mass of information respecting the present state of lunatics and lunatic asylums. Fortunately, we are not this year alarmed by intelligence of a large increase in the number of insane persons in this country, which in most former reports was one of the first facts to strike the attention stated in their pages; for the Commissioners have not, as usual, commenced their Report with the general results of the summary, but refer the reader to the first appendix, where we find, that whilst on January 1st, 1861, there were 24,845 lunatics, there were on January 1st, 1862, 26,200 —an increase in the year of 1355 lunatics known to the Commissioners in Lunacy, and under their supervision in licensed houses, hospitals, and asylums. But it must be borne in mind that the total above given does not include several thousand imbecile and idiotic persons, and so-called harmless lunatics, confined in workhouses and resident in private houses, receiving parochial relief.

We cannot at present attempt an analysis of the Report, but commend it to every student of social science, and hope at some future time to return to its pages for information respecting the present condition and prospects of the insane.

ART. VII.—*Physiologie de la Pensée, Recherche Critique des Rapports du Corps à l'Esprit.* Par M. LEBERT. Two Vols.—*Paris*, 1862.
The Physiology of Thought: Critical Research into the Relation between the Body and the Mind. By M. LEBERT.

THE author of this work is well known to medical and scientific men by various valuable psychological contributions, and as a determined opponent of the phrenological system of Gall; and we are told in the preface that the production now published is to be regarded as the complementary treatise to the rest, the crowning work of the psychological labour of his lifetime. M. Lebert, however, modestly ranks it as only an essay, being sensible of the inability of any philosopher to elucidate and to compass in a treatise the whole region of mental philosophy, or to attempt to unravel the whole of the phenomena of mind and matter. The first of the two volumes is alone a new work; the second being a collection of memoirs or papers which have appeared in medical and other journals in France, or have been read before the Academy of Sciences, and are considered by the author supplementary to some of the articles in the first volume, and elucidatory of the questions discussed in them.

As this is not the place for a critical notice of this important work, we will content ourselves with enumerating the leading contents. The first volume contains thirteen chapters, of which the last is a brief *résumé*. In the first chapter the author deals with the subject of man in his two natures and their relations; the second sets forth a programme of the faculties of thought from a physiological point of view; the third treats of the sentiment of personality; the fourth, of the physiology of the appetites; the fifth to the eleventh, that severally of the instincts, passions, and external senses, of the memory, and imagination, of the intellectual powers, of the understanding and of the will; the twelfth takes into consideration the action of the nervous system in its relations with the electro-magnetic force. In the second volume are comprised a short synopsis of the philosophy of man; a sketch of the opinions of the ancients respecting the seat of the soul; a memoir on the phenomena and principle of life; one on determining ethnologically the medium stature in France; a disquisition on the intellectual capacity of woman and on her destination in society; one on the disposition of the white substance on the hippocampus in the hair of man and of some other vertebrata; a detail of anatomical examination of the brain of executed criminals, and particularly of that of Fieschi; a case of cerebral softening with lesion of emotion and loss of speech; remarks on apoplexy followed by sudden death, or by paralysis persistent until death, and without appreciable alteration of the encephalon; a case of cerebral softening in an epileptic bronzed by nitrate of silver; a commentary on diseases of the optic nerve, to illustrate its structure and function; one on the weight of the brain, and another on the development of the cranium in relation to development of the intelligence; a comparative examination of the dimensions of the cranium in homicides; a history of a maniacal attack in a melodramatic author; a formula of the relations between the brain and the faculty of thought; and a memoir on sleep, dreams, and somnambulism.

From this list it will be seen that the volumes under notice contain a rich collection of matter interesting alike to the physician and to the psychologist or student of intellectual philosophy; and, what is not without much merit in a work of this nature, the whole is conveyed in a pleasing style, and withal put forth by the publishers in a most commendable manner, both as regards typography and paper; therein favourably contrasting with many French books, particularly with those produced from the press a few years since.

ART. VIII.—*General Debility and Defective Nutrition; their Causes, Consequences, and Treatment.*—By ALFRED SMEE, F.R.S. Second Edition.—*London*, 1862. pp. 118.

THIS little treatise is in substance an oration delivered before the Hunterian Society of London, and conveys in a familiar manner the generally admitted truths relative to the elements of nutrition, to the changes in them involved in the processes of life, and to the circumstances under which they are so modified that debility is the consequence, and co-ordinately the causes of disease. The fifth chapter is devoted to general hygienic and therapeutical suggestions for the treatment of debility; and here the author shows himself an ardent adherent to the sustaining mode of treatment, and fails not to insist on the good effects of stimulants in a medicinal point of view. At the same time, every teetotaller will rejoice at the strong picture he draws of the dire effects of intemperance on the value of life and in the production of disease. In the debility of tubercular disease he prefers the administration of quinine to the preparations of iron, which he supposes are prone to provoke cough.

Mr. Smee puts prominently forward the wide-spread influence of debility, or rather of debilitating causes in the production of disease, and by way of special illustration adduces his experience in the etiology of eye-disease. One cause of debility which he presumes to prevail especially in towns, is the deficiency of iron in the blood, and his favourite tonics in almost every form of debility are ferruginous—not, however, the pet preparations of our inventive chemists and sanguine experimental practitioners, but the good old-fashioned salts, such as the sulphate and sesquichloride.

We do not discover any paragraphs in the book of sufficient originality or novelty to our medical readers to claim their reproduction here as extracts; indeed, the treatise addresses itself rather to the general reader, for whose perusal especially the appendix also must be compiled, consisting as it does of a collection of well-known tables from chemical works, and with many of which even the public have been for some time made familiar by the little handbook to the food collection of the South Kensington Museum, prepared by Dr. Lankester.

ART. IX.—*Lehrbuch der Chirurgie und Operationslehre, &c.* By AUG. VIDAL. 4 vols.—*Berlin*, 1852–59. 8vo.

Compendium of Surgery and Operations. By AUG. VIDAL. Translated into German and edited by Dr. A. BARDELEBEN, after the third edition, with special regard to the wants of students.

THIS is essentially a German translation of the well-known and esteemed 'Traité de Pathologie Externe,' by Vidal (de Cassis). A number of minor alterations have been introduced; thus the portion on the eye has been left out, the essay on tumours has been re-written, and numerous additions have been made to the text and the woodcuts. By these means Bardeleben has succeeded in providing a very satisfactory view both of French and German surgery; one which we can recommend to English surgeons. They will find in it much valuable information, presented in a simple and readable manner.

ART. X.—*Handbook of Surgical Operations.* By STEPHEN SMITH, M.D. Surgeon to Bellevue Hospital.—*New York*, 1862. pp. 261.

THIS book is not very accurately named. It is intended as a convenient and portable handbook for the numerous civil surgeons whom the present war in America has driven into the army, and is meant to contain such hints on minor surgery, operations, and the treatment of gunshot-wounds, as shall be sufficient for all the emergencies of military practice. We are sorry to be obliged to say, of an author from whom we have seen some meritorious contributions to surgical literature, that in this attempt he has entirely failed. The work before us is encumbered with descriptions of operations which are seldom if ever used in any practice, and which certainly have no special bearing on military surgery. What can be said of a portable manual for a military surgeon which contains formal directions for tying the internal mammary, vertebral, inferior thyroïd, innominate, superior thyroïd, lingual, facial, temporal, occipital, peroneal, popliteal, epigastric, and dorsalis-penis artery; an account of every plan of operation on varicose veins; and a *manual opératoire* of all the excisions which can be practised on the human body, including those of the clavicle, scapula, sacrum, coccyx, vertebræ, ribs, and even that of the pubes, as a substitute for the Cæsarean operation? While this mass of matter is pitchforked into the book out of old treatises on surgery, to the great embarrassment and annoyance of any reader who is in search of useful information, no operation on the urinary or genital organs is mentioned (except the aforesaid ligature of the dorsalis-penis artery), nothing is said about the reduction of dislocation, the treatment of fractures, the construction and management of ambulances, field-hospitals, &c., and many other matters of vital importance to the military surgeon. Nor can we say much for the way in which the operations are described. The style is frequently an outrageous violation of grammar, the matter is directly extracted out of well-known books, and might have been compiled by any tyro as well as by an operator of Dr. S. Smith's experience; and to the main description of each operation is appended a list of other (and those generally disused or inferior) methods, which can only perplex the student, since no attempt is made to point out under what circumstances modifications of the usual plan may be advisable. The illustrations also are so exceedingly bad as to be in most cases quite unintelligible without a careful study of the text, and in some instances not to be understood even then. For instance, there is on p. 225 a representation of three hands engaged in digging a great hole into some part of the body, and working about a sort of centrebit in it; but there is nothing to show what is meant by this wretched caricature of a surgical operation, further than that it is placed in the description (extracted from South's 'Chelius') of excision of the arch of a vertebra. We can, however, seriously affirm, that after a long and patient attempt we have failed to discover even what part of the body is supposed to be the seat of operation much less what the proceeding itself is meant to be. Finally, to complete the objections to this book which we think necessary to produce, we may end by observing that the author appears to have no personal knowledge of military surgery, since when he comes to treat of gunshot-wounds he is reduced to say, "Without presuming to discuss these subjects from the stand-point of civil practice, we present the following condensation of an article on gunshot-wounds, communicated to 'A System of Surgery,' recently published in London, by Professor Longmore." (p. 242.) It would surely have been better for Dr. S. Smith, if he has no special qualifications for the work, to leave the field open for some of the numerous military surgeons with whom America must now abound, and who must be the best judges of the nature of the want felt, and the best way to supply it. Such compilations as the one before us can add nothing to the professional reputation of a man occupying Dr. Smith's position.

ART. XI.—*A Manual of Minor Surgery and Bandaging, for the Use of House-Surgeons, Dressers, and Junior Practitioners.* By CHRISTOPHER HEATH, F.R.C.S., &c. &c. Second Edition.—*London,* 1862. pp. 228.

WE had the opportunity of noticing this little manual favourably on its first appearance,* and are glad to find that the sale for it has been so rapid that a second edition has been called for within a year. The present edition has been somewhat enlarged, chapters on the immediate treatment of cases of poisoning, and on the method of taking cases, having been added. Both are useful and accurate, as far as they go, although that on the treatment of cases of poisoning is extremely meagre, probably for the reason hinted at by Mr. Heath, that his book is principally written for *surgical* students, while such cases are more usually treated as *medical.*

If Mr. Heath will pardon us for a single suggestion, we would venture to submit to him the propriety in a future edition of expunging certain references on pp. 21, 24 to two cases of hæmorrhage, reported in the journals, in both of which Mr. Heath casts blame on the treatment adopted, but, as it seems to us, without the least ground for doing so. In the former case (one of wound of the palmar arch), which we had an opportunity of watching for ourselves, we happen to know that the cause to which Mr. Heath attributes the failure of the treatment—viz., the too frequent removal of dressings—was not the real one; nor on perusing the report to which he refers his readers, can we discover anything which could have led him to that conclusion. In the second passage he contrasts, in the most invidious manner, two cases of hæmorrhage from the neighbourhood of the tonsil, in one of which styptics succeeded in arresting the bleeding, while in the other the common carotid was tied with a fatal result; but he omits to state that the circumstances of the two cases were totally different, and that

* Vol. xxviii. p. 179.

the accidents had taken place at quite different periods. Nothing is more dangerous than to found general conclusions on isolated cases, and certainly no judgment unfavourable to the reputation of the surgeon in charge of a difficult case should be expressed, except on much clearer evidence than Mr. Heath has possessed in either of these instances.

We could point out several minor matters in which we should have written somewhat differently from Mr. Heath, but on the whole we think his book an excellent one, and cordially congratulate him on the success it has obtained.

ART. XII.—*Short Abstract of the Diagnosis, Prognosis, and Treatment of the Diseases of the Ear.* By WILLIAM KRAMER, M.D.

THIS pamphlet is designed to familiarize the profession in England with the author's views and methods of treatment in respect to aural disease. A brief description is given of the various affections he recognises, and of the plan of treatment he recommends for each. Prefixed is a statement of his methods of diagnosis; the whole being compressed in twenty-seven short pages. Dr. Kramer, as is well known to those who are familiar with his writings, repudiates post-mortem investigations into the condition of the internal parts of the ear, and bases his diagnosis of their diseases entirely on the symptoms he elicits by the use of the Eustachean catheter and of catgut bougies, &c., during life. As a natural consequence, we find in his classification of aural disease, certain conditions enumerated (such as submucous exudation within the tympanum) which rest purely on his own assertion, unsupported by any evidence, and certainly not to be detected after death; while, on the other hand, some of the most frequent and important causes of deafness—ankylosis of the stapes, for example—receive no mention from him.

ART. XIII.—*The Assurance Magazine and Journal of the Institute of Actuaries.* Vol. X. Parts II., III., IV. January, April, and July, 1862.

WE have much pleasure in directing attention to the above quarterly journal of the Institute of Actuaries (edited, we understand, by the accomplished President, Mr. Charles Jellicoe), inasmuch as on looking over past numbers we have met with several papers which cannot fail to be of interest and material service to any of our profession who may be engaged in vital statistics, mortality and life assurance questions, and so forth. In Part II., above mentioned, we would specially notice a paper by W. Spens, Esq., "On the Mortality Experience of the Scottish Amicable Society;" and in Part III., one by Archibald Day, Esq., "On the Statistics of First and Subsequent Marriages among the Families of the Peerage," &c.

PART THIRD.

Original Communications.

ART. I.

On Irregularities of the Pulmonary Artery, Arch of the Aorta, and the Primary Branches of the Arch, with an attempt to illustrate their Mode of Origin by a reference to Development. By WM. TURNER, M.B. Lond., F.R.C.S.E., F.R.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

(Concluded from our last.)

IN the ordinary course of development of the aorta in man and other mammals, the fourth left vascular arch is converted into the arch of the aorta, whilst the corresponding arch on the right side almost entirely disappears, a part of it only remaining as the innominate and right subclavian arteries. In birds, on the contrary, an exactly opposite arrangement prevails, for in them the greater part of the fourth left vascular arch is merely temporary, whilst the fourth on the right side remains as the arch of the aorta. So far, then, as regards the course and position of the arch in these and similar cases, the ornithic, and not the mammalian, type is followed.

Owing to the absence of an innominate artery, each of the two great trunks, right carotid and right subclavian, normally derived from it, arose directly from the arch of the aorta. To explain this, it must be remembered that these vessels are in the first instance single trunks, with independent origins from the fourth pair of vascular arches, and that the formation of an innominate is a secondary change due to a shortening and approximation of the vessels to a greater or less extent at their origin. In this case, then, since the fourth right vascular arch was converted into the arch of the aorta, the right carotid and subclavian arteries simply continued to arise from it, after the original manner of their development.

To trace out the peculiar mode of origin of the left subclavian artery is a somewhat more difficult task, for it involves the consideration not only of the normal mode of origin of this artery, but also of the manner of formation of the trunk of the aorta itself. It will be remembered that the left subclavian arises as a lateral twig from the side of the anastomosis between the fourth and fifth left vascular arches, and in close proximity to that part of the aortic root which extends from this anastomosis to the secondary trunk of the aorta. The trunk of the aorta itself is formed by the junction of the two aortic roots, which are continuous superiorly with the posterior anastomoses between the different pairs of vascular arches on their respective sides.

In these cases it is probable that at a very early period of embryonic life, an obstruction, followed by complete atrophy, had taken place in the fourth left arch, on the cardiac side of the left subclavian, between it and the left carotid, which obstruction, by preventing the flow of blood beyond this point, had caused the right fourth arch to remain pervious, in order that along it the blood might be conveyed to the lower part of the body. From the same obstruction also, the passage of blood into the left subclavian by its proper channel was prevented. To reach this vessel it had to pass either along the fifth left arch (ductus arteriosus), which joins the aortic root immediately below the origin of the left subclavian, or it had to pursue a recurrent course along the left aortic root which intervenes between the origin of the left subclavian and the place of junction of the two aortic roots to form the secondary trunk of the aorta. It is probable that in the first and second set of cases, the blood had flowed along both these channels into the left subclavian. In the first set this had evidently been the case up to the time of death, for both channels were pervious. In the second set, probably, both sources of supply had existed during intra-uterine life, but the closure of the arterial duct after birth had cut off this channel and limited the flow of blood to the left aortic root. In the third set of cases, owing to the absence of the duct, the subclavian artery could only receive its supply of blood from the aorta, through the left aortic root.

Fig. 4.*

The vessel which, in the description of my case, I have named the short pouch-like trunk, and the corresponding vessel in all the other cases I have cited, I regard as the persistent aortic root of the left side. Previous writers on this subject have, however, adopted a different conception of the morphology of this vessel. Sandifort speaks of it simply as a purse or protuberance into which the aorta was expanded below the arch. Quain regards it as the end of the arterial canal, unobliterated, because of the transmission of blood through it from the aorta; but most writers content themselves with a description of its connexions and relations. The present state of our knowledge of the development of the great vessels warrants me in coming to the above conclusion. The reasons which have led me to adopt it are the following: 1st. Its position behind the trachea and

* Scheme of the mode of origin of a right aorta, and of the origin of the left subclavian from a "pouch-like trunk," by the persistence of the left aortic root.

œsophagus, which is in accordance with that of the aortic root. 2nd. Its connexion, on the one hand, with the aorta, and, on the other, with the subclavian artery. 3rd. Its place of junction with the aorta itself. 4th. The point of attachment, in the first and second set of cases, of the ductus arteriosus to it, close to the spot from which the subclavian artery arises. That it is not to be regarded as an unobliterated portion of the ductus arteriosus, the above reasons, together with the fact that in some cases—e.g., those of the third set—the duct is completely atrophied, appear to me to be sufficiently conclusive.

In the cases falling under the first and second heads of this subdivision, and in all those classed under Subdivision A, the trachea and œsophagus are surrounded by a vascular ring. The rings are not, however, morphologically identical in the two subdivisions. In A it is composed of the fourth pair of vascular arches and the aortic roots. In B, on the other hand, it consists on the one side of the fourth right arch and the right aortic root, and on the other of the fifth left arch and left aortic root.

Subdivision C.—The cases which I have arranged under this subdivision agree in possessing a right instead of a left aorta, and in this respect they correspond with those which we considered under Subdivision B. They differ from them, however, in the mode in which the great branches arise from the arch, the difference being so great that we are warranted in placing them in a class by themselves. The character which especially distinguishes them is the presence of a left innominate artery, although in one case which I have placed in this subdivision this vessel did not exist, but the blood reached the left carotid and subclavian arteries through a remarkable enlargement of some anastomosing arteries. In a considerable number of these cases the transposition of the aorta from the left to the right side was accompanied by a corresponding transposition, more or less marked, of the viscera. Examples of such an arrangement have been recorded in the earlier volumes of the 'Philosophical Transactions,' by Sampson,* Baillie,† and Abernethy.‡ In the Thomsonian collection of drawings, in the University of Edinburgh, is the figure of a preparation from the museum of the College of Surgeons in Dublin. Mr. Quain (Pl. V., fig. 3 and 4) represents two cases from the museums of St. Bartholomew's Hospital and University College. Dr. Allen Thomson also records a case in the 'Glasgow Medical Journal.'§ In these cases the branches from the arch arose as follows: left innominate, right carotid, and right subclavian. The formation of a left innominate artery is one of the results which may follow the development of a right aorta. It is produced by the same series of changes which occasion the formation of a right innominate in a normally developed fœtus, the sides simply being reversed. The pulmonary artery ascended to the right and not to the left of the aorta. The position of the ductus arteriosus is not noticed in the description of any of

* Vol. iii. of abridgement, p. 112. † 1788, vol. lxxviii. ‡ 1793, p. 59.

§ July, 1853. Virchow, in his Archiv, 1861, vol. xxii. p. 426, describes a case of transposition of the viscera occurring in a new-born infant.

these cases, except in those of Abernethy* and Allen Thomson, in which the duct joined the descending aorta in the same manner as when the vessels have their accustomed course. In Allen Thomson's case it is especially noted that the duct was connected with the right instead of the left branch of the pulmonary artery. This is what, *à priori*, might have been expected; for, owing to the lateral inversion of development, the fifth right, instead of the fifth left arch, had formed the pulmonary artery, and from it the ductus arteriosus was continued into the descending part of the arch of the aorta. A case, which affords a very good illustration of a persistent right ductus arteriosus, in conjunction with a right aorta and a left innominate artery, has been recorded by Breschet.† It occurred in a male infant. Here the duct persisted as a pervious canal; and on account of the occlusion of the orifice of the trunk of the pulmonary artery, which prevented all communication between it and the ventricle, the blood passed into the right and left pulmonary branches through this duct from the aorta itself. The viscera were not transposed. The case, owing to irregularities in the heart itself, has been alluded to in Group 1, Subdivision A. It does not necessarily follow, however, that when a right aorta and left innominate artery co-exist in the same subject that the ductus arteriosus should be on the right side. When the pulmonary artery, instead of passing to the right, ascends to the left, i.e., is a development of the fifth left and not the fifth right arch, then the ductus arteriosus proceeds from it. In such a case the duct joins the left subclavian artery close to its origin from the innominate. Bernhard‡ has described a case of this kind, and others are referred to by Quain. Cases of this description possess considerable morphological value, as they show that the subclavian and innominate arteries are developed in the fourth vascular arch of the embryo, and not in the third arch, as was supposed by the early embryologists. The left innominate is a vessel homologous with the right aorta, and it is joined by the ductus arteriosus at the place where it gives off the subclavian artery.

The last case to which I shall allude under this head is one which has only recently been described by Panas in an adult male.§ The aorta in it arched to the right, but there was no corresponding transposition of the viscera, except that the œsophagus lay to the right of the trachea. The right carotid and subclavian arteries arose directly from the arch. The corresponding vessels on the left side had no direct communication with it, neither were they derived from an innominate trunk. Arising from the aorta, on a level with the sixth dorsal vertebra, was an artery the size of a goose-quill, which extended in a corkscrew-like manner, at first leaving the thorax, and then re-entering it at the second intercostal space. It passed upwards in front of the second rib, first intercostal space, and first rib to the root of the neck, where

* Noted by Quain, p. 159, of Text of Anatomy of Arteries.

† Mémoire sur l'Ectopie du Cœur : Répert. Gén., tom. ii. p. 7.

‡ Quain's Arteries, p. 162.

§ Bulletin de la Société Anatomique, 1857, No. 6; Canstatt, 1858; Phys., p. 23.

it joined an artery, possessing a transverse direction. The part of this transverse artery external to the place of junction had the course of the left subclavian, whilst the internal part extended as far as the margin of the sterno-thyroïd muscle, and then turned upwards to form the left common carotid. To explain this case, we must suppose that a complete atrophy of the fourth left arch had occurred, so that the fourth right remained pervious throughout its entire length; that from this circumstance no left innominate could be formed, and that the left carotid and subclavian arteries were completely severed from all direct connexion with the aorta. To convey blood to them, an enlargement had taken place of one of the aortic intercostal arteries, which formed an important anastomosis with the superior intercostal branch of the subclavian, and through this circuitous channel the blood reached the great trunks for the left side of the head and left upper limb.

Subdivision D.—The cases which we have now to consider present, as in those recorded under the two preceding subdivisions, an atrophy of the fourth left vascular arch; but on account of the persistence of the ductus arteriosus and the left aortic root, the descending aorta lies on the left side, forming indeed an arch, although this arch is not to be looked upon as homologous with the true arch of the aorta. We have no longer, therefore, to regard the anomaly of a right aorta. The best example of this rare abnormality is the case which has been recorded by Dr. Greig.* It was met with in a fœtus. Two arteries arose from the base of the heart—one, the aorta, ascended to the right, and after a short course divided into the right subclavian and right and left common carotid arteries; the other, the pulmonary, gave off a branch to each lung, and then continued onwards into the descending aorta, which received its supply of blood solely from this source. The left subclavian arose from the latter trunk after it had given off the branches to the two lungs. The spot from which it sprang evidently corresponded to the place where the fifth left arch joined the aortic root, close to the anastomosis between the fourth and fifth arches, which is the normal place of origin of the left subclavian. The part of the pulmonary artery between the origin of the branches to the lungs and the left subclavian—i.e., the ductus arteriosus—remained pervious in order to convey the blood both into the subclavian and descending aorta. There were also deficiencies in the structure

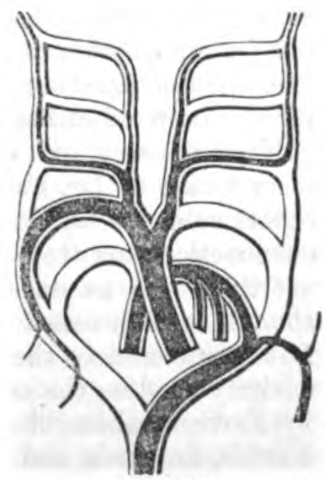

Fig. 5.†

* Edinburgh Medical Monthly Journal, 1852, vol. xv. p. 29; also in Struthers' Anatomical Observations.

† Scheme of the mode of origin of the descending aorta and left subclavian artery from the ductus arteriosus and pulmonary artery. Atrophy of the fourth left arch between the left carotid and subclavian arteries. Termination of the ascending aorta in the right subclavian and right and left carotid arteries.

of the heart of this fœtus, the septum ventriculorum being imperfect at its upper part. Cases of this kind, where the ascending and descending aortæ are completely detached, and where the left subclavian arises from the lower trunk, are evidently extremely rare. In the Library of Practical Medicine, vol. iii. p. 384, allusion is made to a case of Breschet's, where the left subclavian arose directly from the pulmonary artery, but as no account is given of the condition of the aorta, it is impossible to say whether in this very important particular the two cases were identical.

Subdivision E.—As in the last subdivision, we have to consider an atrophy of the fourth left vascular arch, but it is of a more limited extent, and it occurs a little lower down, beyond the origin of the left subclavian artery, so that this vessel remains in connexion with the proper aorta. Numerous cases exhibiting this form of irregularity have now been recorded. In an elaborate article in the 'Medico-Chirurgical Review,'* Dr. Peacock has given an account of 40 cases. The general arrangement of the aorta in these cases was as follows: It ascended in the usual manner, and then turned to the left, so as to form its arch, from which the right innominate, left carotid and subclavian arteries, arose. Below the origin of the left subclavian the aorta was contracted, but the exact position of the contraction varied slightly in different instances. In a case recorded by Dr. Farre,† from a preparation of Sir A. Cooper's, it took place immediately above the point of junction of the ductus arteriosus, but it much more commonly occurred either at that point or immediately below it.

The amount of contraction, also, varied considerably. In a second case by Dr. Farre,‡ also from Sir A. Cooper's collection, and in others recorded by Paris,§ Otto,|| Nixon,¶ and Rees,** although the constriction was well marked, and the aortic canal greatly narrowed, yet there was an opening sufficient to permit the passage of blood, though in diminished quantity, from the arch to the descending aorta. In other instances the contraction had gone on to such an extent as entirely to close the canal of the vessel, examples of which are afforded by Farre, Graham,†† Jordan,‡‡ S. Jones,§§ Wood,|||| and others. In Farre's, Jordan's, and Graham's cases, the contraction was so marked as almost to separate the arch with its branches from the descending aorta. In an interesting case recorded by Steidele,¶¶ the arch, with its three primary branches, was completely separated from the descending aorta, which was continuous with the pulmonary artery through the pervious ductus arteriosus.

The ductus arteriosus was present in all these cases, but its condition varied. In Farre's first case, and in those of Steidele, Rees, and

* 1860, vol. xxv. p. 467. † Pathological Researches, p. 14.
‡ Idem, p. 15. § Dessault's Journal de Chirurg., vol. ii. p. 108.
|| Seltene Beobachtungen, p. 66. ¶ Dublin Medical Journal, vol. v. p. 386.
** Trans. Path. Soc., vol. i. p. 203. †† Med.-Chir. Trans., vol. v.
‡‡ North of England Medical Journal, vol. i. p. 101.
§§ Trans. Path. Soc., vol. viii. p. 159. |||| Idem, vol. x. p. 97.
¶¶ Quoted in Meckel's Handbuch der Path. Anat., vol. i. p. 468; and in Kilian, Ueber den Kreislauf des Blutes. Karlsruhe, 1826.

Chevers,* the duct was widely pervious, and evidently formed the chief channel for the conveyance of blood to the descending aorta. In Farre's second case, and in those of Graham and Nixon, the duct was very much contracted, so that but a small quantity of blood could pass through it at a time. In the cases recorded by Otto and Jordan, the duct was contracted to a fibrous cord, so that its canal was completely obliterated. The pervious or non-pervious state of the ductus arteriosus appears to have been determined by the age of the patient at the time at which death had taken place. In those instances in which it was largely patent, death had occurred within a short period after birth, the dilated state of the duct in them representing a condition which is at that time normally met with. When the canal of the duct was greatly narrowed, or completely obliterated, the adult period of life, or at least that of adolescence, had been reached; in most of which cases it is reported that a collateral circulation had been established through an enlargement of various anastomosing vessels, more especially the aortic and superior intercostals, internal mammary, scapular, superficial and deep epigastric arteries, the increased size of these arteries being necessitated by the closure of the arterial duct and the contraction of the aorta.

Various theories have been advanced to account for the irregular formation of the aorta which we have now been discussing. The one which appears to me the most correct was originally propounded by M. Reynaud,† and has received the support of Rokitansky‡ and Peacock. It ascribes the defect to a faulty development of the fourth vascular arch, below the origin of the left subclavian, and at or about the point of attachment of the ductus arteriosus.

Subdivision F.—This subdivision comprises cases which exhibit a very peculiar displacement of the origin of the left subclavian artery. In them that vessel no longer arises from the fourth vascular arch, but appears to be derived from the ductus arteriosus. The first recorded case of this kind which I have met with is reported by Holst.§ He states that when the left brachial artery was followed to the thorax, it ended as the subclavian, almost an inch from the aorta, and that at that point it received a long canal of two inches, which was furnished by the pulmonary artery, at the position of the ductus arteriosus. At the place of junction the vertebral artery was given off. The three vessels, left subclavian, vertebral, and the long canal united in a considerable dilatation of a triangular form, to the angles of which the above-named vessels were connected.. The aorta gave off the right subclavian, right carotid, and left carotid, and then diminished much in volume. A case of a closely similar description has been recorded by Hildebrand.|| In it the left branch of the pulmonary artery gave off a canal, two inches long, at the situation of the

* Trans. Path. Soc., vol. i. p. 55.

† Jour. Heb. de Méd., tom. i.

‡ Ueber einige den wichtigsten krankheiten der Arterien. Denk. der Kaiser. Acad. Wien, 1852.

§ Arch. Gén. de Méd., 1836, tom. xi. p. 91.

|| Idem, 1842, tom. xiv. p. 87.

ductus arteriosus, which was directed to the left, and joined the left subclavian almost at a right angle. The last-named artery had no connexion with the arch of the aorta. The vertebral artery arose from the left common carotid, from which it especially received its supply of blood. It made an angle to the left, and communicated with the left subclavian.

In both these cases the left subclavian appeared to be a direct prolongation of the fifth left vascular arch. It has already been pointed out that the subclavian on the left side arises as a lateral twig from the side of the anastomosis, between the fourth and fifth left vascular arches. We may suppose that in the above cases a slight change in the origin of the artery had taken place; that it had become more especially attached to the fifth left arch, so that its connexion with the fourth arch was severed, and thus in its completed development it became directly continuous with the ductus arteriosus, which vessel terminated in it, and not in the arch of the aorta, as is customary.

It may not be amiss at this stage of our investigation to cast a retrospective glance at the different methods by which the left subclavian artery may arise in cases where irregularities in the development of the fourth and fifth vascular arches exist. In each of the subdivisions in which this was the case, a different plan was adopted, the variation depending upon the nature of the irregularity in the arches themselves. In B, where there had been extensive atrophy of the fourth left arch, but where the left aortic root was preserved, the subclavian arose from that root. In the majority of the cases alluded to under C, where the fourth left arch had been considerably atrophied, and where the left aortic root had entirely disappeared, the part of the fourth left arch which remained formed a left innominate artery, and from it the left subclavian arose. In Panas' case, however, owing to the complete atrophy of the fourth left arch, no left innominate was formed, so that not only the left subclavian, but also the left carotid, received their vascular supply through an enlargement of various collateral anastomoses. In D, where the atrophy was limited to the fourth arch, between the origins of the left carotid and subclavian, but where the latter vessel continued to arise from the side of the anastomosis, between the fourth and fifth left arches, it received its blood through the pervious ductus arteriosus. Finally, in F, where both the fourth left arch and left aortic root were present, the subclavian artery appeared to have entirely disconnected itself from either of these vessels, and to have become incorporated with the fifth left arch, with which it was directly continuous. The various methods which have been described afford admirable illustrations of the compensating influences which are at work, to provide for the proper conveyance of blood to the left upper extremity, in the different kinds of vascular irregularity which may arise, and which are, as it were, especially provided for in the intimate anastomoses which exist between the vascular arches on each side of the body.

Subdivision G.—We pass, in the next place, from the consideration of irregularities affecting the left subclavian artery, to a class of cases in which the right subclavian possessed an unusual mode of origin from the aorta. In these cases the aorta arched in the normal way to the left side, but the branches exhibited certain abnormal arrangements. There was no innominate artery, the great vessels arising as separate trunks, as follows—right carotid, left carotid, left subclavian, right subclavian. Numerous examples of this irregularity have now been recorded, to which reference may be made, more especially in the systematic works of Meckel and Quain. In them the exact place of origin of the right subclavian varied slightly: sometimes it proceeded from the upper part of the arch, at others from its posterior aspect; most commonly from about the junction of the transverse and descending parts of the arch. The artery then passed obliquely upwards and to the right, in front of the bodies of the upper dorsal vertebræ and behind the trachea and œsophagus (very rarely between those tubes), to the root of the neck on the right side.

I have dissected three subjects in which this irregularity existed, and I am quite disposed to agree with the statement of Mr. Quain (p. 47), "that when the four primary branches arise separately from the arch of the aorta, the arrangement which apparently deviates most from the usual disposition—viz., that in which the right subclavian arises from the left side of the aorta, is of the most frequent occurrence." He considers that it is found on an average once in 250 cases, a result which agrees pretty closely with my own observations. In a recent article,* Mr. John Wood, of King's College, has propounded a theory explanatory of the mode of origin of this abnormality, with which I am disposed to concur. At an early period of development, atrophy of the fourth right arch had occurred, on the cardiac side of the origin of the right subclavian. As a consequence of this atrophy, the flow of the blood into this artery by its proper channel was prevented. To reach this vessel it had to pass along that portion of the right aortic root which intervened between the origin of the right subclavian and the place where the two aortic roots join to form the secondary trunk of the aorta: this root consequently remained pervious and persistent, instead of disappearing, as is usually the case. Now, as the right aortic root lies behind the trachea and œsophagus, and pursues the direction which was followed by the commencement of the right subclavian in these cases, we must regard the first part of the artery in them—i.e., the part behind the trachea and œsophagus—as really formed of the persistent pervious

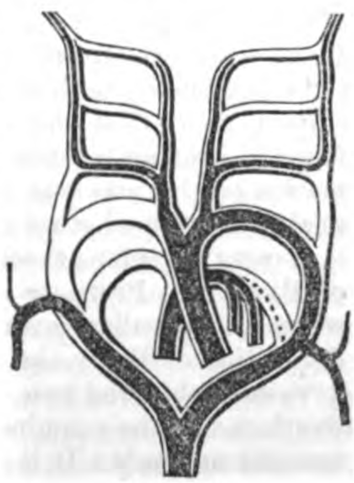

Fig. 6.†

* Trans. Path. Soc., 1859, vol. x.

† Scheme of the mode of origin of the right subclavian as the last branch of the arch of the aorta, through atrophy of the fourth right arch and persistence of the right aortic root.

right aortic root; for the place of junction of the two aortic roots usually occurs at or about the spot from which the irregular right subclavian was found to proceed.

In the cases which I dissected, it was observed that, whilst the left vagus and recurrent possessed their normal course, the inferior laryngeal branch of the right vagus deviated considerably from its usual arrangement. It arose from the trunk of the pneumogastric, as that nerve crossed in front of the right subclavian by two or three distinct bundles of fibres; but instead of turning round behind that vessel, it passed almost directly inwards in its course to the larynx.* In one of the three cases, a peculiar disposition of the gangliated cord of the sympathetic was noted. The internuncial fibres between the lowest cervical and first dorsal ganglion were separated into two bundles, one of which went in front of the right subclavian, the other behind it; so that they embraced not only the subclavian trunk, but also its vertebral branch. This close relation between the artery and the nervous cord was evidently due to the subclavian being situated on a plane posterior—i.e., in closer relation to the spine, and consequently to the sympathetic cord—in this form of irregular origin, than is the case when it arises in the normal manner. The altered position of the right inferior laryngeal nerve, lying in front of, instead of behind, the right subclavian artery, tends to confirm the view which has been adopted of the mode of origin of the artery in these cases. The complete atrophy of the fourth and fifth right vascular arches of necessity precluded this nerve from possessing its normal recurrent direction.

It has been thought by some pathologists, that, in certain cases where this arterial abnormality existed, the close relation of the subclavian to the œsophagus occasioned a difficulty in swallowing. Dr. Bayford† was the first to direct attention to this particular form of dysphagia, which he described under the name of dysphagia lusoria. In his case the artery passed between the trachea and œsophagus. Otto and Quain, both of whom have given especial attention to this subject, think that the evidence on which this supposition has been founded is of an insufficient nature. As I do not possess any information of the previous history of the cases which I dissected, I am unable to state whether in them any difficulty of swallowing existed.

A very ingenious theory has recently been propounded by Dr. Oehl, of Pavia, and Professor Hyrtl,‡ of Vienna; that those individuals in whom this peculiar arrangement exists are left-handed, and that this disposition of the vessel is the cause of such left-handedness. They have each observed two cases in which a special use of the left hand was found at the examination after death to be accompanied by this vascular anomaly. It is not very difficult to find an explanation of the

* In a case of this form of irregular subclavian which I saw in Dr. Allen Thomson's dissecting-room, in the University of Glasgow, the right inferior laryngeal nerve turned round the inferior thyroid artery.

† Memoirs of Medical Society, London, 1793, vol. ii. p. 275.

‡ Canstatt, 1859, p. 26.

co-existence of these two conditions: for if the place of origin of the right subclavian be carefully examined, it will generally be found to be in a direct line with the attachment of the ductus arteriosus to the aorta ; so that the imperfectly arterialized blood, which, in the fœtus, flows along the duct from the pulmonary artery, would to a great extent pass into the right upper limb. Hence the nutrition of this extremity would not be so perfect as that of the left arm and hand, at least during the period of fœtal life. In a case which has been described by Murray,* it is especially noted that the entire right arm was smaller, and its muscles feebler than the left. There will thus be a greater tendency to use the left arm and hand than the right—a tendency which may persist through life, unless it be counteracted by close attention on the part of the individual. In order to accommodate his theory to the numerous instances of left-handedness which undoubtedly occur, Hyrtl considers this vascular irregularity to be much more common than is generally supposed. He gives as high an average as twice in 100 cases. This is unquestionably a much higher average than we are justified in assuming to exist from the experience afforded by the dissecting-rooms in this country; the proportion of cases in which it has been seen both by Quain and myself not being more than once in 250 cases. Although it is very probable that this peculiar mode of origin of the left subclavian is one of the causes capable of inducing left-handedness, yet it most certainly ought not to be regarded as the only one: the inquiry into the origin of such a condition ought to embrace not only the disposition of the vessels, but also that of the nerves and muscles, and the peculiar influence which may have been exercised by education.

Subdivision H.—I have met with but one recorded case in which there was a persistence of both fifth vascular arches: it is reported by Breschet.† It occurred in a male infant. The heart possessed only a single auricular and ventricular cavity. The aorta arose from the base of the ventricle and arched to the left side. From it the innominate, left carotid, and subclavian branches proceeded. The pulmonary artery also arose from the ventricle, and shortly divided into two branches, one for each lung. From the left branch proceeded the ductus arteriosus, which opened into the arch of the aorta in the usual position. From the right branch another arterial canal, more slender and longer than the preceding, passed to join the right subclavian artery close to its origin from the right brachio-cephalic trunk. It is unfortunate that no mention is made of the position of the recurrent laryngeal nerves. In this case the original symmetrical development of the fifth pair of vascular arches is preserved. It would appear from this case, that instances may occur in which the two pulmonary branches, instead of being developed from the fifth left arch alone, as has been described by Rathke to be the rule in the mammalia, may be developed from both fifth arches—the one on the left side from the left, the one on the right side from the right arch.

* Meckel's Handbuch, vol. ii. p. 107. † Répert. Gén., tom. ii. p. 9.

The development, therefore, in it would correspond with that which has been observed by Rathke to take place in birds. Dr. Allen Thomson has allowed me to examine some drawings in his possession, taken from preparations which illustrate the mode of development of the pulmonary arteries in the mammal, which drawings he has permitted me here to make reference to. Judging from them, it would certainly appear that the pulmonary arteries are developed from both the fifth arches, and not from the fifth left arch alone, as Rathke maintains. It is evident, from this discrepancy of opinion between such competent observers, that the subject of the development of the pulmonary arteries is one which affords room for additional investigation.

The case affords an important illustration of the accuracy of Rathke's view of the development of the subclavian arteries from the fourth pair of vascular arches, and not from the third pair, as was supposed by Von Baer, for the subclavian arteries on each side arise from their trunks, close to the point of junction of the arterial ducts with them; the right brachio-cephalic artery is therefore a vessel homologous with the arch of the aorta.

Subdivision I.—A class of cases has been recorded which presents a striking contrast to that considered under the last subdivision. Instead of both fifth vascular arches being preserved, not only has the right disappeared, as is customary with it, but the left has also become atrophied to a greater extent than is usually the case. The extent of this atrophy of the fifth left arch varies in different cases. It is a well-known fact that at the time of birth the ductus arteriosus is an open canal, and that when aërial respiration is established, this canal gradually closes, and in time becomes converted into a fibrous cord, the exact period of obliteration presenting many differences. Dr. Chevers* has recorded the case of a fœtus seven months and a half old, in which the ductus arteriosus was almost closed, and capable only of admitting the shank of a large pin. The atrophy of the canal may take place to such an extent as not merely to occasion a complete obliteration of the tube, but even of the fibrous cord which generally results, so that the pulmonary artery becomes entirely disconnected from the aorta. Dr. Peacock† has reported a case of this kind in a child one year and three days old. The most frequent form of atrophy of the fifth left arch consists in a diminution in calibre, amounting in some cases to absolute closure of that part of the arch which constitutes the trunk of the pulmonary artery. This condition is generally accompanied by contraction or even closure of the pulmonary orifice. In the majority of these cases, the ductus arteriosus remained pervious, communicating on the one hand with the aorta, and on the other with the right and left pulmonary branches. It was by a backward passage from the aorta through the pervious duct that the blood entered into the pulmonary branches. An illustrative case of this kind is recorded by Dr. Farre.‡ In it and most of the other cases of

* Trans. Path. Soc., vol. i. p. 60. † Idem, vol. vii.
‡ Pathological Researches, p. 27.

a like nature, the imperfection in the pulmonary artery was generally accompanied by deficiencies in the auricular and ventricular septa. A very curious case has been reported by Cruveilhier,* in which, along with a right aorta and left innominate artery, the pulmonary artery existed as a mere rudiment to the left of the aorta. The mode in which the blood passed to the lungs was very peculiar. Each lung received three arteries, which arose from different parts of the aorta. These are probably to be regarded as enlarged bronchial arteries, which to some extent performed the duties of the defective pulmonary branches.†

Subdivision K.—Embryologists have for some time been familiar with the fact that the thoracic and abdominal parts of the aorta are originally two distinct vessels, which extend parallel to each other along the under surface of the spine as far as the posterior end of the body. These vessels have been called by Remak‡ the primitive aortæ, and Bischoff§ has described them under the name of the inferior vertebral arteries. In the chick it has been seen that on the third day these primitive aortæ coalesce anteriorly, and that the coalescence gradually extends backwards throughout their entire length, until they ultimately form the single vessel with which we are familiar.

In the human subject, even in the adult condition, indications of the original duplicity of the trunk of the aorta have been observed. Vrolik‖ has recorded a very interesting case of an adult male, communicated to him by Professor Schrœder van der Kolk, in which an oblique septum existed in the lower part of the thoracic and the whole length of the abdominal aorta. Through this septum the artery was subdivided into two tubes, one anterior and to the right, the other posterior and to the left. It is probable that in this case the two primitive aortæ had coalesced along the line of their apposed walls, but that this coalescence had not been accompanied by an absorption of the intervening septum, so that two distinct tubes were preserved. Kölliker, in his recent work on development,¶ refers to two preparations in the museum of Dr. Allen Thomson, in which a septum divided the aorta along its entire length into two tubes, and which he considers to be illustrative of the origin of the aorta from the two primitive vessels. From a communication which I have had with Dr. Thomson on the subject, it would appear that although at first sight the preparations seem to admit of such an explanation, yet that a further and more careful examination would require to be made before a positive opinion could be given. Both cases are complicated with a morbid condition of the arterial wall, which renders a determination of the signification of the appearances very difficult. I believe it is

* Anat. Path., liv. 1, pl. 6.

† Reference to other cases, and a discussion of the causes which occasion this form of irregularity, may be met with in Dr. Peacock's work on Malformations of the Heart.

‡ Kölliker's Entwickelungs-geschichte, p. 410.

§ Entwickelungs-geschichte des Kaninchen Eies, Tab. 14, fig. 60, p. 152.

‖ Tab. ad illustrand. Embryogen, 1849, Tab. 88.

¶ Entwickelungs-geschichte, 1861, p. 412.

the intention of Dr. Thomson to give a full account of the peculiarities of these cases.

Third Group.—When the great vessels arise from the arch of the aorta in the normal manner, they spring from it from right to left, in the following order: arteria innominata, left carotid, left subclavian; the left carotid at its origin lying nearer to the innominate than to the subclavian. Cases occasionally occur in which this arrangement is departed from. In some instances the branches do not preserve their usual distance from each other, but become approximated. This may take place either to the right or left side, as the case may be. The approximation may be so great, that a junction may actually be effected between two, or even all three of the branches, so as to occasion a smaller number of arteries to arise from the arch than is customary. In others the converse of this may take place—the branches may be separated from each other by a larger interval. The separation may be so great, that the innominate artery may be resolved into its two constituent vessels,—right carotid and right subclavian—so that in such a case a greater than the normal number of arteries arise from the arch. The increase in number of the branches of the arch may be also due to some of the smaller arteries, which usually spring from the great branches of the arch, taking their origin from the arch itself. Finally, cases occur in which the branches do not arise in their regular order, but are to a greater or less extent transposed. The change in the number and relative distance of the branches does not appear to be necessarily accompanied by such important alterations in the fourth and fifth vascular arches as have been considered in the last group. It rather appears to be due, for the most part, to a greater or less amount of approximation or separation of the vessels at their origin during the early developmental changes which accompany and occasion the formation of the great arteries.

Owing to the diverse arrangements which these irregularities possess, it will be advisable, in this as in the two preceding groups, to make a separation of the entire group into subdivisions. The following appears to me to embrace the various forms of irregularity which have been recorded:

A. Approximation and coalescence of innominate and left carotid arteries.

B. Approximation and coalescence of innominate, left carotid, and subclavian arteries.

C. Approximation and coalescence of left carotid and subclavian arteries.

D. Separation of innominate into right carotid and subclavian arteries.

E. Separation of innominate into right carotid and subclavian: approximation and coalescence of right and left common carotid arteries.

F. Separation of innominate into right subclavian, and, through atrophy of the right common carotid, into right external and internal carotid arteries.

G. Irregularities in the mode of origin of the vertebral arteries.

Subdivision A.—The left common carotid artery, at its origin, in a normally-formed subject, is situated nearer to the innominate than to the subclavian of its own side. Cases not unfrequently occur in which it approaches more closely to it than is usual. This approximation may be so great, that the left carotid may touch the innominate, or even arise from it, in which latter case the aorta, at its arch, gives origin to but two branches, the innominate and the left subclavian. Mr. Quain concludes, from an examination of a very large number of cases, that this is by much the most frequent change which occurs in the number of the branches of the arch. The approximation and junction of these arteries is evidently due to the vessel which connects the third with the fourth left vascular arch, and which is ultimately developed into the left common carotid artery, arising so near to the commencement of the fourth left arch, that by a slight shortening of the same it becomes continuous with the fourth right arch—i.e., with the vessel which is subsequently developed in the mammalia to the innominate artery.

This irregular arrangement of the arteries in man is the regular mode of origin in many of the lower mammalia. In the apes, the left carotid arises from the innominate almost immediately after that vessel has sprung from the arch. In many rodents, as the rabbit, the same arrangement is presented. In many feline animals, again, the innominate artery gives origin to both common carotid and the right subclavian arteries at the same spot, so that it serves in a more marked degree as a common trunk for all the arteries proceeding from it.

Subdivision B.—This subdivision includes cases which exhibit a much greater degree of coalescence of the great branches than is presented by the last. In it, not only the left common carotid, but also the left subclavian, is blended with the innominate, so that all the branches of the arch arise by a single trunk, which may be called the anterior aorta. So great a departure from the normal human arrangement occurs very rarely. The most remarkable example of this form of irregularity was seen in a case originally recorded by Klinz,* and subsequently described and figured by Meckel, Tiedemann, and Quain. In it the aorta divided into an anterior and posterior branch. The former ascended for four inches, and then gave off branches in such a manner as to form a cross. From the right horizontal branch, the innominate, arose the right carotid and subclavian arteries; the left horizontal branch was the left subclavian, whilst the vertical artery became the left carotid. The posterior branch continued as the descending aorta to the lower parts of the body. Before the date of Klinz's case, M. Troussieres† had described a case in which it is probable that the aorta divided into an anterior and a posterior branch (ascending and descending), the former afterwards separating into two large branches for the head and upper limbs. This case is referred to

* Abhand. der Joseph. Med. Chirurg. Acad. zu Wien, Band i. S. 271. 1787.
† Journal des Savans, 1668.

by Haller.* Meckel† also states that he has seen a single case of this kind in a very long, narrow-chested man. He describes the common basis of all the arteries as elliptical, although all were connected together at their inner circumference. In the case recorded by Dr. Vernon,‡ (which has already been alluded to in Group 1, Subdivision B,) a vessel is described as arising from the summit of the aortic arch, and then dividing into the innominate, left common carotid, and left subclavian arteries. It is next stated that the aorta then continued its course downwards as usual.

To produce the peculiar arrangement met with in the above cases, we must suppose that the fourth left vascular arch had undergone considerable shortening, so that the left common carotid and subclavian arteries were so closely approximated as not only to effect a junction with each other, but also with the innominate artery of the right side; by which means a single trunk, the anterior or ascending aorta, was formed. As confirmatory of this view, it may be mentioned that it is especially noted in Klinz's case that the aorta did not form any proper arch, but divided at once into an ascending and descending aorta. This division of the aorta into two parts, without the formation of a definite arch, occurs as the normal disposition of the vessel in the ruminants and solipeds. As a rule, in these families the left subclavian (axillary) arises from the ascending aorta at some point or other, varying slightly in the different genera, before the origin of the carotids and right subclavian (axillary), which latter vessels may either arise together or in succession. In Klinz's and Vernon's cases the ascending aorta appears to have divided into its various branches at a common centre.

Subdivision C.—In the kind of case we are now about to consider, the left common carotid is the vessel which has assumed a position other than regular. But instead of passing over to the right side, as was the case in the two former subdivisions, it has now coalesced with the subclavian artery of its own side, so as to form along with that vessel a left innominate artery. These cases are of a different character, and must therefore be distinguished from those described in Group 2, Subdivision C, in which the left innominate artery was accompanied by the formation of a right aorta. A case of this kind, in which the arch of the aorta gave origin to two innominate arteries, has been figured by Tiedemann,§ from a preparation in the Berlin Museum. Cases of a similar nature have also been described by Malacarne|| and Biumi.¶ Cases sometimes occur in which a tendency to the formation of a left innominate takes place without actual union of the left carotid and subclavian being effected. This tendency is exhibited when the left carotid arises from the arch midway between the innominate and left subclavian, or even nearer to the latter vessel. The left innominate cannot be regarded as a vessel homologous with

* Element. Physiologæ, vol. ii. p. 162. † Handbuch. Band ii. S. 131.
‡ Medico-Chirurgical Transactions, vol. xxxix. 1856.
§ Tabulæ Arteriarum, ii. fig. 4.
|| Meckel's Handbuch, vol. ii. p. 132. ¶ Ibid.

the right innominate when they occur together in the same subject, for the latter vessel is, as already stated, the commencement of the right vascular arch, whilst the left innominate is formed by the junction of the subclavian branch of the fourth left arch with the left common carotid.

Meckel and Tiedemann have pointed out that in the bats (*Vespertilio*) an arrangement of a similar character exists, for in them two innominate arteries arise from the arch of the aorta. Daubenton* also has described a right and left innominate in *Pteropus rufus*, so that it probably constitutes the arrangement in the *Cheiroptera* generally. Meckel† and Cuvier‡ have also recognised in the common porpoise (*D. phocæna*) a right and left innominate proceeding from the arch of the aorta. I have seen this to be the case, not only in the common porpoise, but in the bottle-nosed dolphin (*D. tursio*), a fœtal specimen of which I examined for this purpose. As to the arteries which arise from these innominate vessels in the genus *Delphinus*, there is some difference of opinion. The consideration of this question will be deferred to Subdivision F.

Subdivision D.—This subdivision differs materially from those which have preceded it in the same group; for the branches, proceeding from the arch, instead of exhibiting any tendency to coalesce, rather show a disposition to separate from each other, so that a greater number of vessels arises from the arch than is usual. The cases which I have arranged under this subdivision are those in which the arteria innominata is separated into its two constituent vessels, so that the right subclavian and carotid arteries are given off in their proper order independently from the arch. Meckel§ has collected a number of cases of this kind, which have been observed by Winslow, Ballay, Heister, Petsche, Nevin, and himself. Tiedemann‖ has added other cases to this list. Dr. Walshe¶ has since described a case in which the same arrangement existed. It must be kept in mind that these cases differ materially from those described in Group 2, Subdivision G, in which no right innominate existed, and in which the right subclavian and carotid arose as separate branches. In that subdivision the right subclavian had completely altered its position, and arose from the left side of the arch, this change being due to very important developmental irregularities in the fourth vascular arches and aortic roots. In this subdivision the arteries arise from the arch in the regular order from right to left, the absence of an innominate, and the separate origin of its constituent vessels, being due apparently to a simple shortening of the fourth right arch, through which both the right carotid and subclavian are brought in contact with the fourth left vascular arch so as to arise from it.

An approximation to this irregularity may be seen in those cases in

* Buffon: Histoire Nat., tom. x. p. 70.

† System der vergleichenden Anatomie, vol. v. 1831.

‡ Leçons, tom. vi. p. 111.

§ Handbuch, vol. ii. p. 107.

‖ Tabulæ, Tab. 3.

¶ Medico-Chirurgical Transactions, vol. xxv. p. 1.

which the innominate artery is shorter than usual. Daubenton* has described in the walrus an arrangement which may perhaps correspond with that described above. He states that in that animal four branches arise from the arch of the aorta. It is unfortunate that the names of these branches are not given.

Subdivision E.—In the cases falling under this subdivision, as in those of the last, there is no innominate artery. The four vessels do not, however, arise independently from the arch, but the two common carotid arteries are united together at their origin so as to form a common trunk. A very remarkable case of this description has been figured by Tiedemann.† It occurred in the body of a young man, aged twenty-two years, which came under his own observation. The right subclavian arose as the first branch from the arch, then succeeded a short trunk, which bifurcated into the two common carotid arteries; lastly arose the left subclavian. Portal‡ has described a case probably of a like nature. Tiedemann§ and Quain|| have both figured cases—the former from a preparation of Walter's, the latter from one in his own possession, in which a trunk common to both carotid arteries existed, and in which there was at the same time a displacement of the right subclavian from its proper position, that vessel arising beyond the left subclavian as the last branch of the arch.

The changes which would have been necessary to lead to the formation of a trunk common to the two carotid arteries, could not have been very great in such cases as those of Portal and Tiedemann. A very slight amount of shortening of the fourth vascular arches at their commencement would lead to the coalescence of the two vessels with each other. In Walter's and Quain's cases, however, a much more extensive developmental change must have occurred, for not only was there complete atrophy of the fourth right vascular arch, but the right aortic root had remained permanent, so as to cause the right subclavian to arise from the left side of the arch. On the authority of Cuvier (vol. iv. p. 249) it may be stated that, in the elephant, the two carotid arteries arise by a common trunk from the arch, the right and left subclavians proceeding separately from each side of the conjoined vessels.

Subdivision F.—This subdivision comprises a very rare and remarkable class of cases. In it the common carotid artery is atrophied either on one side, or on both, so that the external and internal carotid vessels arise directly from the arch of the aorta.

One of the best authenticated cases of this kind was observed by Dr. Power, of Dublin, by whom a drawing and description were communicated to Mr. Quain, who has figured it in his plates on the arteries. (Plate XII., fig. 3.) In this case there was no innominate artery, and the branches arose from the arch as follows: right subclavian, right external carotid, right internal carotid, left common carotid, left subclavian. The irregularity was therefore confined to the branches from

* Buffon: Hist. Nat., vol. xiii. p. 420.

† Tab. 3, fig. 2.

‡ Cours d'Anat. Médicale, tom. iii. p. 155.

§ Tab. 2. fig. 8.

|| Plate 7, fig. 5.

the right side of the arch. In the case of double aorta, by Malacarne, already alluded to in Group 2, Subdivision A, it is stated that each aorta gave off directly the external and internal carotid of its own side, without the intervention of a common carotid. Cases not unfrequently occur in which the common carotid artery bifurcates into its two terminal branches at a point much lower down than is usually the case. One very remarkable case has been recorded by Morgagni* in which the trunk of the left common carotid was only one inch and a half long. These cases of early bifurcation must be regarded as presenting an approximation to complete absence of the common carotid.

The great length of the common carotid artery in a normally formed neck is due to the descent of the heart and aortic arch, from their original position in the neck, into the cavity of the thorax during the early stages of their development. This descent necessarily occasions great elongation of the carotid arteries, so that they may become accommodated to the new arrangements. In these cases no such elongation could have taken place, but instead, a shortening, or even a complete atrophy, and as a compensation the internal and external carotid vessels were elongated and brought into immediate relation with the fourth pair of vascular arches.

In one genus at least of the mammalia we find an arrangement existing as the normal one, which in some respects corresponds with this very rare form of irregularity in the human subject. It has already been pointed out that, in the genus Delphinus, two innominate arteries arise from the arch of the aorta. Cuvier† and Meckel‡ both described, in the D. phocæna, these arteries as dividing into the carotid, subclavian, and vertebral branches. Stannius§ has, however, objected to this description. He states that he has convinced himself, by repeated observations, that the common carotid trunk is entirely wanting, so that both internal and external carotid arteries arise immediately from the arteria innominata. This statement of Stannius has been confirmed both by Barkow‖ and Rathke.¶ I have myself dissected the great vessels of the neck both in D. phocæna and D. tursio, and am of opinion, with the above observers, that in the genus Delphinus the common carotid arteries are altogether wanting, and that the external and internal carotids arise directly from the innominate arteries. The absence of the common carotid vessels appears to have reference to the stunted condition of the neck in these animals.

So far, then, as regards the absence of the common carotid trunks, the genus Delphinus presents certain marks of resemblance with the above-mentioned cases in man. In other respects, however, they differ; for whilst in the instances observed in the human subject, the internal and external carotid arose from the arch of the aorta, in delphinus they sprang from the arteriæ innominatæ.

* Epist. 29, Art. 20. † Leçons, tom. vi. p. 111.
‡ Vergleich. Anat., Band v.
§ Müller's Archiv, 1841, p. 379; Lehrbuch, 1846, p. 437.
‖ Anatomische Abhandlungen, S. 95. Breslau, 1851.
¶ Untersuchungen über die Aortenwurzeln den Saurier, S. 60.

Subdivision G.—Cases occasionally occur, in which the vertebral artery no longer arises from its proper subclavian trunk, but derives its origin from other sources. This irregularity generally affects only one side, most commonly the left, but in a few very rare cases both vertebrals have been displaced from their proper position. The usual form of irregularity met with is that in which the left vertebral arises from the arch between the left subclavian and left common carotid arteries. This occurs not unfrequently, so that it has attracted the attention of most anatomists. Tiedemann and Meckel have each seen a case in which the left vertebral arose from the arch, beyond the origin of the left subclavian. Tiedemann also figures a case by Huber, in which the left vertebral arose by means of two roots, one derived from the aorta, between the left carotid and subclavian, the other from the left subclavian. The right vertebral has been seen to arise from the following parts: by Meckel, from the arch, between the right innominate and left common carotid; by Cassebohn,* from the arch, in front of the right innominate; by Otto† and Tiedemann, in cases where there was no innominate artery, from the arch, between the right subclavian and common carotid arteries; by Quain,‡ Wood,§ and others, in cases in which the right subclavian was derived from the left side of the arch, from the right common carotid; by Hyrtl,|| from the left side of the arch, behind the origin of the left subclavian.

The greater tendency which the left vertebral artery, more than the right, exhibits to take its origin from the arch of the aorta, is due to a difference in the mode of development of the two subclavian arteries from the vascular arches. The left subclavian is a lateral branch of the fourth vascular arch, whilst the right subclavian, or at least that part of it which lies internal to and behind the scalenus anticus, is a part of the fourth right arch itself. Thus it may be said that the normal place of origin of the right vertebral is from the fourth right arch, and that when the left vertebral springs from the arch of the aorta, instead of from its subclavian branch, it is merely a return on its part to a symmetrical arrangement.

The cases of abnormal origin of the right vertebral artery figured by Quain and Wood, in which the vessel proceeded from the right common carotid, are especially interesting, because they were accompanied by a great irregularity of the right subclavian, which arose from the left side of the arch. The developmental changes which had led to the transplantation of the subclavian, had also caused the vertebral to assume a new position. It does not necessarily follow, however, that in every case in which the subclavian is thrown to the left side of the arch, that the vertebral springs from the common carotid. The cases are capable of the following explanation:—The atrophy of the fourth right arch, which occasioned the transplantation of the right subclavian, took place beyond—i.e., on the distal side of the origin of the

* Quain, Pl. 7, Fig. 12. † Frenzel's Thesis, op. cit.
‡ Pl. 6, Fig. 13. § Trans. Path. Soc., vol. x. || Canstatt, 1859, p. 26.

right vertebral from the arch; so that whilst the subclavian remained in permanent communication with the right aortic root, the vertebral was separated from it; and by the shortening of the arch, between its origin and that of the right common carotid, it assumed a direct connexion with the carotid vessel; the vertebral and common carotid in these cases being all that remained of the fourth right vascular arch. In those cases in which, along with a transplanted right subclavian, the vertebral artery continues to arise from its proper trunk, the atrophy of the fourth right arch, which has led to this transplantation, has occurred between the origins of the vertebral and common carotid vessels.

The very curious arrangement recorded by Hyrtl is probably unique in its characters, so that it is worthy of especial notice. It occurred in a new-born child. The three large trunks appear to have proceeded from the arch in the usual manner, the right subclavian being derived from the innominate. In addition, the right vertebral arose from the arch, taking its origin behind the left subclavian. It then bent itself downwards to the anterior surface of the second thoracic vertebra, passed between it and the œsophagus to the right side, and coursed upwards to the superior aperture of the thorax, and finally entered the foramen of the sixth vertebral transverse process. Its origin from the aorta was in close relation to the insertion of the ductus arteriosus into the arch. At the first glance it would almost appear as if there were something so anomalous in a right vertebral artery arising from the left side of the arch, that it would be impossible to render a rational exposition of its mode of origin. I think it quite practicable, however, by applying those principles which have guided us throughout this exposition, to give a satisfactory explanation of this irregularity. Let us suppose that the right vertebral, instead of being developed as a branch of the fourth right arch, had proceeded from the right aortic root beyond the origin of the right subclavian, and that in the course of time atrophy of the extremity of the arch had occurred, so as to separate the two vessels from each other. In such an event the only connexion of the vertebral artery with the great trunks of the vascular system would be through the right aortic root, which would remain pervious for this purpose. I consider, therefore, in this case of Hyrtl's, that the portion of the right vertebral which extended from its origin to the superior aperture of the thorax was the persistent pervious right aortic root; its position between the œsophagus and the spine, and its origin from the arch close to the attachment of the ductus arteriosus, confirm this supposition.

I have not included in the above description of the irregularities of the great vessels certain forms which have been represented by Tiedemann in his plates, as there appear to be some doubts as to their correctness. In Plate IV., fig. 8, a representation of a case recorded by Zagorsky is given, in which a left innominate is shown to be giving origin to the right carotid as well as to the left subclavian and carotid arteries. The aorta is drawn as if it arched to the left side, an arrangement which is hardly recognisable with the existence of such a form of left innominate. Quain has, however, pointed out (p. 46), that no mention is made by Zagorsky of the course of the aorta in his description. Tiedemann, also, in Plate III., figs. 5 and 6, has figured two cases taken from Walter's memoir, 'Sur les Maladies du Cœur,'* in which the great vessels arise in a very irregular manner; but the value, not only of these, but of some others figured by Walter, is greatly diminished, owing to the direction taken by the arch of the aorta not being stated. The descriptions are therefore imperfect, so that they do not permit any chain of reasoning to be constructed from them. In Meckel's 'Handbuch,' vol. i., p. 467, a brief note is made of a case recorded by Wrisberg, in the 'Götting. Gelehrt. Anzeigen,' 1778, No. 50. It is stated that in a girl, seven years old, a second smaller peculiar vessel, said to be a ductus arteriosus, proceeded from the right ventricle close below the true pulmonary artery, and terminated in the aorta. The same objection, imperfection of description, applies also to this case.

Art. II.

Observations on Yellow Fever. By Robert Lawson, Deputy-Inspector-General of Hospitals.

(*Continued from No. 58, p. 485.*)

Division II.—Morbid Appearances.

Kidneys.—The prominence given to the urinary secretion in the preceding part of this paper naturally suggests that the condition of the kidneys after death should come under consideration first. These, with the liver and intestinal canal, are the organs most implicated in yellow fever, and it will be as advantageous to treat them in this order as in any other.

After death the kidneys are always found congested, and almost always somewhat larger than usual; those that were weighed having been from $5\frac{1}{2}$ to 8 oz., though the subjects from which they were taken were none of them above medium size. In the cortical substance, though the bloodvessels were full, the intermediate portions were light greyish yellow, more prominent when cut into, and the thickness from the surface to the base of the pyramids rather greater than usual. The substance was rather more friable than natural, the capsules not more adherent than common. The pyramids showed the congestion better than the cortical part; they were not enlarged; on pressure, a little milky-looking matter exuded from the papillæ, and traces of the same were found in the calices; this has been noticed by Blair and others, and consists of tube-casts. The pelvis, or course of the ureters, seldom exhibited much congestion. In one case the whole organ seemed quite œdematous from interstitial effusion.

The bladder was always contracted, sometimes quite empty, at others containing urine, varying from two drachms up to five or six

* Mém. de l'Acad. Roy. Berlin, 1785.

ounces. When death occurred before the eighth day this was always albuminous, and contained tube-casts. The lining membrane of the bladder itself was pale in every case which came under my notice, and nothing anomalous was detected in it.

On placing a thin section of the kidney under the microscope, the convoluted tubes were found with their epithelium very granular, the outlines of the cells being for the most part indistinct, and the nuclei difficult to detect among the multitude of granules, or if they were apparent, their contents were granular as well. In most instances the convoluted tubes were filled with this granular epithelium, and frequently distended by it to a considerable diameter; I have met with them as large as $4^{t}\cdot0$; their canal of course was quite obstructed, and in such cases there was little or no urine in the bladder. In others, though the epithelium was equally granular, it did not close the canal altogether in any situation, so that some fluid could pass; and in these the substance of the epithelium was generally tinged brownish, and some urine found in the bladder. In the case of Goodwin, detailed above, the epithelium almost entirely filled the convoluted tubes whenever examined; in many places it was deeply tinged with hæmatin, which gave a brownish-red colour to the whole thickness of the cells, as in the casts that were obtained from the bladder. In another case there was a deposition of pigment at intervals along the convoluted tubes, covering the contained epithelium thickly in the form of minute granules for a distance equal to from one to two diameters of the tube, the intermediate portions of the canal being quite clear of it; this peculiarity was found not only in the convoluted tubes, but in the capsules of the kidneys and in the tubes of the cell-containing network of the liver.

The capillary vessels ramifying among the convoluted tubes always had their surfaces covered with nuclei. These, which in the usual condition of the vessels are scattered and not very prominent, in the congested kidneys of this disease were so numerous as to cover the whole surface continuously. Under the action of acetic acid they seemed to contract somewhat, so as to expose the parietes of the vessel between them and the whole to become clear; but the nuclei still remained distinct, studding the surface at much less intervals than in healthy vessels, or in those in other parts of the same subject. Under a power of 450 these nuclei appeared granular. The loops of the Malpighian bodies were similarly affected, and the appearance was quite distinct on the larger vessels in their interior. In this condition the capillaries appear tumid and opaline or dark grey, as they are just within or beyond the focus of the object-glass. The connective tissue around them generally seems swelled, and contains a little clear or opaline exudation, and often granular matter. In the case alluded to above, in which the kidneys seemed œdematous, which had passed the ordinary period of the disease, and proved fatal on the twenty-third day with suppression of urine, there was a large amount of opaline exudation found among the straight and convoluted tubes, with numerous granules, oil, and some pus globules, in addition to the condition of the capillaries just described, and the tubes had lost their sharpness of outline, so that the whole was clouded and indistinct.

These conditions of the kidneys were well marked in cases which proved fatal from the fourth to the seventh or eighth day of the disease. They indicate an active exudation into the parenchyma of the organ, and a catarrhal affection of its mucous surfaces. It can be easily understood that the latter, at least, though salutary in itself, may prove excessive; and by closing the tubes prevent the passage of urine, and so become one of the most fatal complications of the disease. This seems to be the reason of the frequent suppression of urine in yellow fever about the fifth day, and one of the causes of its so often terminating in death after the urgency of the febrile symptoms had passed away. The case particularly alluded to above, however, shows that there may be a suppression at a subsequent period, after the flow had become copious, and danger from that source had apparently ceased, by interstitial exudation destroying the secreting power of the gland. This might occur earlier in the disease, but the choking of the tubes by the enlarged epithelium seems the more common cause at that period, though the congested state of the kidneys, and the active exudation into them even then, requires the closest attention.

Liver.—This viscus was generally somewhat enlarged, its weight, when ascertained, was from three pounds twelve ounces to a little above four pounds, the subjects being small, or not above medium size. There was generally a flaccidity about it, giving the impression that the firmness and cohesion of the parenchyma was diminished. On cutting into it, the colour was found very different in different cases. In several it was of a uniform light yellow, with tinges of grey or brown in different individuals; in others this uniformity of colour was varied by congestion of the portal or hepatic capillaries, or of both, producing varieties of the rhubarb or nutmeg liver; in others, little or no yellowness was visible, and to the naked eye the parenchyma did not differ in appearance from that of health. There are other cases, again, in which the colour was olive or greenish; in one such the outlines of the lobules could be perceived on the surface of a section quite distinctly from the interlobular tissue, as if that formed a separate capsule; the latter was congested, while the lobule itself was comparatively bloodless. These varieties of colour were mainly owing to the varying quantities of oil, bile, and blood in the organ. When there was a large quantity of oil, as in a young, robust, healthy white man, the colour was most uniform, and there were fewest traces of blood in the capillaries, or the redness of that was so covered by the bile and fat, that there was merely a brownish tinge throughout the yellow. When the oil was less copious, the capillary congestion came out better, and was more intense; but, on the other hand, more or less of the nutmeg appearance was often visible in livers in which no oil was detected by the microscope. The olive or green colour was found only when there had been an early and more decided affection of the liver, and deeper bilious impregnation of all the white tissues in the body, than in ordinary cases, and in which there was more interstitial exudation in the liver itself than was common, with little or no oil.

In the class of cases last mentioned, there was tenderness over the liver, and yellowness as early as the third day of the disease; the peritoneal covering in these was opaline in places, thickened, and with a quantity of exudation, containing numerous granules under it, and on tearing it off, more or less of the parenchyma adhered to it. In one case, about two drachms of pus was found between the surface of the liver and the diaphragm, inside the coronary ligament, where the liver was not covered by peritoneum.

The gall-bladder was often distended by bile, and never was quite empty. The contained fluid varied from a mucus tinged yellow to a dark-green or almost black bile, which in a thin stratum was of a yellowish-brown colour, and the quantity of fluid in it did not seem to depend on the ducts being unobstructed, for it was occasionally found quite full, though these were pervious. Late in the disease, stools were occasionally seen quite black to the unassisted eye, but which with the microscope were found to consist of mucus coloured with a deep-green bile, and similar matter has been detected in the small intestine in some of the fatal cases, though the whole of the white tissues in the body were deeply tinged with bile, so that even when there is sufficient retention of bile to produce jaundice, still a notable quantity may find its way into the intestine.

On putting a thin section of liver from a case of yellow-fever under the microscope, the following appearances could generally be made out at some part or other. The smaller branches of the portal vein were found to be imbedded in a layer of connective tissue, more or less abundant in different cases, of an opaline appearance, separating the vein from the lobules on either side, interposing between the vein and accompanying artery and duct, and separating these from each other to a greater extent than in a sound liver. When this connective tissue was neither very abundant nor very opaline, the condition of the artery and duct could be perceived distinctly, and smaller branches proceeding from the vein or artery, or to join the duct, could often be distinguished. The condition of the smaller vessels, whether arterial or venous, resembled that already described in connexion with the kidneys,—viz., they were covered with closely-set nuclei and granules, and the connective tissue in their neighbourhood contained more or less exudation and granular matter. The bile-ducts in the smallest portal canals sometimes had their epithelium distinct and tinged brown, at other times this was less distinct, of an opaline appearance, without the least trace of brown; in the former case bile seemed to have been passing through them to the last, and in the latter its passage seemed to have ceased; the former was found when there had been no jaundice during life, the latter in the more frequent cases where it was well-marked.

These appearances were continued from the lesser portal canals through the interlobular spaces, and into the lobules themselves, the smallest vessels in all cases being covered with nuclei, and surrounded with more or less connective tissue, which seemed to contain exudative material, clear in some instances and opaline in others. The connective tissue was very copious in the lobules occasionally, lying between the tubes of the cell-containing network, and towards the margin of the lobules occupying as much space as the tubes themselves, and the connexion between the two was so slight, that in making a thin section, the one was torn from the other, and the tubes of the cell-containing network left bare at various points.

In every case in which death occurred about the usual period in the disease, the hepatic cells were found of a pretty deep brown colour from biliary matter. The formation of bile, therefore, had gone on, though, owing to the condition of the smaller ducts already described, it may have been detained in the lobule. The cells were usually somewhat granular, the nuclei not always distinct. In one case only have I seen the cells dissolving away, leaving a little granular matter in the tubes in their place; this was confined to the immediate vicinity of the smaller portal canals, and was in a liver with much interstitial effusion. The case was that alluded to above as having proved fatal on the twenty-third day. The cells were frequently altogether without oil-globules, or displayed a few very small ones only; at other times the liver was decidedly fatty, and large clear globules were seen, equal in diameter with the tubes, or even swelling them out to twice that of the portion occupied by the cells without oil near them. On examining a very thin section of a portion of fatty liver, the large oil-globules appeared as if between the tubes of the cell-containing network, but on separating these as much as possible, the oil-globules were always found attached to and partially imbedded in the hepatic cells, the smaller oil-globules were often quite imbedded in them, while none could be seen outside the tubes among the capillaries, unless at a later period, when considerable interstitial exudation had taken place. The deposition of oil in the liver corresponded with that in the subcutaneous connective tissue in amount, and must be regarded rather as a physiological phenomenon than as a pathological one, and seems quite unconnected with the disease, all the characters of which were found equally well developed when no oil was detected in the liver at all.

Some of the smaller branches of the portal vein, and more frequently those of the hepatic vein, contained quantities of clear granular matter without distinctly-formed fibres or cells; at other places, or frequently close to the former, there were distinct fibrillæ, and granular cells of the size and appearance of the white globules of the blood, the two presenting exactly the character of the fibrinous concretions generally found in such cases in the heart.

There is thus an active exudation into the parenchyma of the liver in yellow fever, and the symptom from which it has derived its name depends on the exudation process having embraced the minute bile-ducts, and closed them against the passage of the bile, of which there are abundant indications in the lobules. The affection of the liver, as indicated by the occurrence of jaundice and uneasiness in the hepatic region, does not always appear at the same period of yellow fever; sometimes there is no yellowness until after the urine has become albuminous and tube-casts have appeared in it; at others, there may be tenderness over the liver, and bilious vomiting, with yellowness, as early as the third day, and with the kidneys as yet not affected perceptibly, though in the next two days the urinary symptoms follow the usual course. In such cases the yellowness of the conjunctivæ and surface becomes much deeper than in those in which the liver is implicated at a later period, the urine contains more bile, and blistered surfaces give out a deep yellow serum profusely; if there be a pulmonary complication (and it is not infrequent), the expectoration is green. So far as I have seen, there was much less urrhodine in the urine when the liver was thus early affected than when it became so later in the disease; the cases were more prolonged, too, and the convalescence much more tedious, than when this organ was less seriously involved.

Spleen and Pancreas.—These viscera have not received as much attention as the liver or kidneys. The spleen varied in size from what was natural to one weighing above a pound; its texture was normal in some, and friable or even pulpy in others. It had a greenish tint occasionally, when there had been much bilious impregnation of the other tissues in the body. The pancreas was examined in a general way only; nothing anormal was detected in it.

Stomach and Intestinal Canal.—The stomach was occasionally much contracted, so as not to exceed the ordinary size of the duodenum, but more commonly it did not present anything remarkable in this respect. The intestines, so far as I have seen, were much as usual after death. When the patient had taken little food for some days, or had had great irritability of stomach, it was most likely to be greatly contracted, and its mucous membrane was then found in longitudinal folds, with their free edges congested, and most probably with some acid mucus more or less tinged with blood in it. When the organ contained black vomit, its size was seldom diminished.

The contents of the stomach and intestines varied considerably in different parts of their course, and in different cases. The stomach sometimes contained nothing but a little mucus, or fluid the patient had taken shortly before death; at others, a little clear acid mucus—the white vomit of Blair; this was seen tinged reddish-brown by blood, or contained brown specks, the transition to black vomit, or it had gone on to black vomit, and a variable quantity of this was found. The duodenum and intestines presented mucus more or less opaque from the quantity of epithelium it contained, and variously tinted with blood or bile, or occasionally both together. In the colon the mucus had sometimes become consistent, and occasionally presented a yellow tint from bile; here, as higher up in the intestine, the other appearances were occasionally obscured by bloody matter.

The mucous membrane of the stomach was somewhat thickened and tender, giving way readily; it was sometimes much congested, and this was generally confined to either the cardiac or pyloric half.

When the organ contained black vomit there might be patches of congestion here and there. When little black vomit had been formed, a large portion of the mucous membrane was often of a deep brown colour; when the quantity was greater, there was merely a number of streaks or patches of a brown colour; when that was copious, the whole inner surface of the organ was free from colour. This appearance is owing to the impregnation of the epithelium of the tubular glands of the mucous membrane with the matter of black vomit, as already explained.* The congestion sometimes extended to the duodenum, and I have seen it there to a greater extent than in the stomach of the same body. There was seldom much fulness of the vessels of the jejunum met with, but in the lower part of the ilium and colon it was occasionally seen, though there were at the same time congestion or black vomit in the upper part of the canal.

One feature of the disease—the desquamation of the mucous membrane of the duodenum and jejunum—was very remarkable in several instances, the columnar epithelium not only being found copiously among the mucus in the tube, in a separate form, but large flakes of it were numerous in which the cells still adhered to each other side by side, and such flakes could be seen also still loosely attached to the membrane. This condition seemed most developed when there had been little black vomit or hæmorrhage, but a good deal of irritation of the mucous membranes.

In the former part of this paper it has been shown that black vomit is a secretion from the tubular glands of the stomach, and a case was given in which a similar one had taken place from the kidneys. I have met with some instances in which there was black vomit in the stomach without any having passed down the intestine, yet in the ilium the mucus, though clear at other parts, was of a dark-brown or reddish hue opposite the patches of Peyer's glands, and these were impregnated with a similar colour. The colon, too, occasionally throws off a bloody-looking fluid, and after death it is found confined to it, not extending beyond the ilio-cæcal valve. From this we may conclude that in yellow fever many other glandular organs beside the stomach may give origin to a matter resembling black vomit.

Small ulcers were occasionally seen in the ilium, cæcum, and descending part of the colon. These had raised surfaces, appearing as if covered with a patch of adherent feculent matter. Their position and size rendered it probable that they were connected with the solitary glands, which were more prominent elsewhere than usual. Cases of typhoïd fever were by no means uncommon at one time, with the enlarged tumid patches of Peyer well developed, and leading to ulceration,

* British and Foreign Medico-Chirurgical Review, vol. xxix. p. 483 (April, 1862). In two cases of typhoïd fever in which there had been frequent vomiting of mucus tinged green, the lining membrane of the stomach presented large patches of a greenish colour. The analogy of the black vomit suggests that in these cases the mucus, tinged apparently with bile, came from the stomach itself, and was not a regurgitation from the duodenum. This will apply to the vomitings in malarious fevers pretty generally, I expect.

hæmorrhage from the bowels, and in some to perforation of the intestine; these were quite different from the others as regards the enteric lesion, but most of them occurred in the same locality. At the same place an intermediate kind of case was not infrequent, in which the enteric lesion of typhoïd fever was quite distinct, whole patches of Peyer's glands being tumid, and many of them ulcerated, while death was preceded by more or less bloody discharge from the bowels, albuminous urine with granular tube-casts, going on to suppression, yellowness of the conjunctivæ (most of these cases were in black men), and after death congestion of the mucous membrane of the stomach or duodenum, and a condition of the liver and kidneys closely resembling that described above. There was thus a gradual transition from uncomplicated yellow fever through one with small ulcers in the ilium, cæcum, and colon, to another with well-marked typhoïd ulceration in the ilium presenting many symptoms and post-mortem appearances of yellow fever, and finishing the series by one characterized by the fully-developed enteric lesion of typhoïd fever, terminating in perforation and death. Though the appearances peculiar to each form of the disease were distinct enough in the extreme cases, yet in the intermediate ones their development and combination were such that it was impossible to decide to which the case should be referred.

Brain and Membranes.—The frequency of head symptoms in the course of yellow fever renders an acquaintance with the condition of the nervous centres of much importance; but hitherto comparatively little has been done, with the improved means for investigation of the present day, to increase the amount of information which has long been available regarding them in this disease.

When there had been much heaviness, stupor, or coma before death, the membranes of the brain were always found with their vessels full, and the pia mater looked opaline from interstitial effusion, and there was a variable quantity of yellow serum at the base of the brain and in the ventricles. On removing the substance of the hemispheres slice by slice, red points seemed more numerous than in a healthy brain, and the consistence was in some cases less firm than usual, but in others there was scarcely a perceptible difference, though the head symptoms had been well marked. On subjecting portions of the brain to microscopic examination in these cases, more precise information was obtained, those which to the unassisted eye presented nothing unusual, then manifesting unequivocal indications of disease. In every case I examined in which there had been a well-marked affection of the head before death, the large exudation corpuscles were numerous in the white matter near the surface of the hemispheres; and the smaller bloodvessels and capillaries, which are difficult to detect in a healthy brain, were often seen extending beyond a cut surface, or even ramifying in the substance of the section when it was compressed, with their sides covered with closely-set nuclei, the same as already described in connexion with the kidneys and liver; it was more difficult to determine whether there was exudation surrounding these to any extent, but their appearance rendered this probable, while the exudation corpuscles showed that active exudation had been going on. Amyloïd bodies were seen in some instances, but the exudation corpuscles just noticed had neither the firmness nor fracture of these, but under pressure merely became flattened. Softening of the upper surface of the cerebellum was found in the case of Goodwin, which was detailed in the first part of this paper (vol. xxix. p. 475), and a soft state of its whole surface in another case.

Head symptoms are always a source of anxiety in yellow fever, and they will sometimes take even experienced practitioners unawares by the suddenness of their onset, and their unmanageable nature, when it was thought all danger from them had passed. The following case will illustrate this point: Corporal Brownlow, a white artilleryman, aged thirty-seven, had been four years in Jamaica, and had given way to intemperate habits latterly. He was stationed at Fort Augusta, two miles across the harbour from Port Royal. On the 7th August, 1859, he had a paroxysm of intermittent, which came on in the forenoon, and went off in the afternoon. On the 8th, the second day of his complaint, he walked about ten miles in the sun, returning to the fort in the evening much fatigued, and with severe headache. On the third day fever came on about seven A.M., with severe frontal headache, and he went to Port Royal, which he reached at four P.M., and was taken into hospital, where the fever went off at six P.M., with profuse perspiration, and he then expressed himself quite free from pain. Ten grains of calomel, with as much quinine, were given, and he had a good night and slept well. On the fourth day he was quite apyrectic; the bowels were moved thrice during the night. Quinine in doses of five grains was given every six hours. I saw him about noon, when he expressed himself quite free from pain; there was a complete absence of fever; pulse 86; surface cool and soft; and he seemed in a favourable condition, save only a slight want of animation about the eyes, scarcely amounting to heaviness. Half an hour afterwards fever came on, with great heat of surface and severe headache; pulse 120. Notwithstanding the application of ice to the head, and a blister to the back of the neck, delirium ensued quickly; at three P.M. he was comatose, with slow respiration, and he died at a quarter to six P.M. There was no yellowness in this case. Three ounces of urine were found in the bladder, which was highly albuminous, and contained granular casts; the mucous membrane of the stomach and duodenum was much congested. The inner surface of the dura mater and pia mater were much congested; the substance of the cerebrum soft, but not otherwise changed in appearance; the cerebellum apparently healthy. Under the microscope the cerebrum showed numerous large exudation globules; the small arteries and capillaries were covered in every direction with closely-set nuclei, and a quantity of granular matter.

It is scarcely to be supposed that the morbid appearances in the brain in this case were produced during the last six hours of life, the history of the case is altogether opposed to it; yet if it be admitted that considerable active exudation had taken place into this organ previously, the complete subsidence of fever and the freedom from pain from the evening of the third day until the accession of fever on the fourth, though there was active disease in the brain, is a remarkable circumstance, and the rapidity with which this led to coma and death on the fourth day no less so; not that they are rarely met with, but because the force of the local affection was so much governed by the phases of the fever. It is possible that on the evening of the third day the head symptoms might have run on to a fatal termination but for the profuse critical evacuation by the skin; but on the fourth day, ere this could afford relief the brain was so deeply implicated that death was inevitable. Every one who has had experience in the malarial fevers of warm climates must have encountered cases of this description now and then, and those best acquainted with their character will be most desirous of getting even the smallest information as to their nature, or the slightest hint for averting their danger.

There is another form in which a cerebral complication is found in fever, but the accompanying fever is pure remittent or intermittent, and the urine remains copious to the last, and presents neither albumen nor tube casts. In this there is not more uneasiness about the head than usual in remittent fever, and there is neither increased fulness of the vessels about the head, nor any alteration in the senses, or mental manifestations to indicate that the brain is particularly implicated. The only peculiarity is the persistence of the regular accessions of fever, often slight in themselves, long after they should have given way in ordinary cases to the remedies employed. The patient, perhaps, cannot be persuaded there is anything serious the matter with him, and the medical attendant, though uneasy, cannot satisfy himself as to the cause of the fever going on in spite of his treatment, until at last, at the regular period of exacerbation, the patient experiences some unusual or unpleasant sensation about the head, congestion takes place rapidly, and within an hour or two he is comatose, and in a few more dead; or, the first indication of the implication of the head may be a convulsion, from which the patient never recovers, but which leads to a fatal termination even more quickly. Such cases are always a source of great anxiety to the medical attendant, even when fully alive to their nature; if he do not stop the fever, every paroxysm increases the mischief in the brain; and if he be not cautious in the measures he adopts to overcome the fever, he is very likely to increase the cerebral affection by his remedies, and so accelerate the result he is so desirous of averting. Every practitioner who has been long in the tropics, who will look back inquiringly on his past experience, cannot fail to recognise instances illustrative of both these difficulties.

Heart and Blood.—On opening the pericardium, serum, varying in quantity from half an ounce to ten times as much, and generally yellow, was found; the heart itself was rather pale, soft, and its cavities frequently distended. The cause of distension was found to be blood, or coagula of lymph, in variable quantities in different cases.

Sometimes there was little lymph, the cavities being filled with dark fluid blood; sometimes there were large coagula extending continuously from the hepatic veins through the auricle and ventricle into the pulmonary artery, and sometimes even into the ramifications of this vessel; in one case, a coagulum was withdrawn from it, which showed many distinct branches corresponding to the fifth subdivisions of the artery. These coagula were mostly met with in the right cavities, but sometimes smaller ones were found in the left, and there was a quantity of blood with them varying inversely as the size of the lymphy concretion. They were always lying free in the cavities of the heart and vessels, and nothing unusual was detected in the lining membrane of either, unless in one case, in which a portion of firm lymphy exudation was found adherent to the surface of one of the mitral valves, and the valve itself thickened. These coagula appeared to be larger and firmer in cases in which the liver was more severely affected than common; when the kidneys, intestinal canal, or brain were more implicated, they were less, or even absent. Their presence was perceptible during life by the distinct murmur over the origin of the pulmonary artery with the first sound of the heart.

Some of the American writers have stated that the heart in yellow fever was fatty; it is quite possible an individual with this organ so degenerated might contract fever, but fatty degeneration of it is neither a common nor necessary condition of the disease. I have never seen an instance of it. The soft undulating pulse of the latter stages of yellow fever, it is true, suggests an alteration in the condition of the muscular fibres, and on examination that proved to be the case, for on submitting a portion to the microscope they were found pale, rather opaline in appearance, not well defined, their transverse striæ indistinct, and with a very remarkable disposition to separate longitudinally into their component fibrillæ. The vessels among the fibres were all covered with closely set nuclei, and had a hazy outline, from exudation into and around them, and here and there a communicating capillary could be seen running across a fibre from one longitudinal capillary to another, in the same state. Portions of voluntary muscle from the same bodies presented none of those appearances, but had the fibres sharply defined, the transverse striæ well marked, and there was no disposition in the fibrillæ to separate from each other longitudinally. The capillaries were found at various points with clear membranous sides, with scattered nuclei, and without any trace of exudation around them. The heart thus appears to be subject to acute exudation in yellow fever, as well as the kidneys, liver, or brain, and the state of its actions in the latter days of the disease is obviously connected with this condition.

It seems still a common belief that the blood in yellow fever is in a dissolved state, and that exudation of this from the vessels into the surrounding tissues is the cause of the yellowness. The blood, however, is not dissolved, but the globules remain distinct and well formed, and the yellowness of the serum depends on the colouring matter of bile, as indicated by its changing to green on the addition of nitric acid.

I have examined portions of serum from the brain, pericardium, or from the blood itself, for urea, and have always obtained indications of it. For this purpose the albumen was coagulated by heat, and removed by filtration, the filtrate evaporated to dryness over a water-bath, and the residue exhausted by alcohol. On concentrating the latter, and adding solution of oxalic acid, crystals of oxalate of urea were obtained. In several instances the process was continued by adding an excess of solution of oxalic acid to the residue of the alcoholic solution, filtering, and agitating the filtrate with ether; on removing this, and allowing it to evaporate spontaneously, there were traces of hippuric acid; the fluid remaining after the removal of the ether being neutralized by carbonate of lime, evaporated to dryness, and extracted with alcohol, gave unequivocal indications of urea.

In several instances, a few drops of nitric acid were added to serum, and the whole boiled to remove the albumen. After separating the latter, the fluid remaining gelatinized on cooling. If treated with acetic acid the serum did not give this result.

Lungs.—The condition of the lungs varied much. Sometimes they were remarkably free from anormal appearances, even from the hypostatic congestion of fatal cases, and contained very little blood, though both sides of the heart were distended by it. At other times there were the traces of acute active disease in them. This was occasionally in the form of congestion of the mucous membrane of the trachea and bronchi, with bloody mucus in the tubes, and accompanied by more or less pneumonic condensation. Occasionally there was pneumonic condensation alone of a portion of the lung, and in one there were numerous small portions throughout the lungs, in front when there was no general congestion, as well as behind when there was much, from the size of a millet-seed to that of a filbert, which were firm, of greyish-yellow colour, and surrounded by a narrow line of congestion. Under the microscope these showed the air-cells filled with epithelial cells in a granular condition. Exudation into the pleura was not met with in any decided case of yellow fever.

Division III.—Form and Causes.

There is still much difference of opinion among authors on yellow fever, whether it be a disease of one paroxysm followed by a lull, in which the hæmorrhages or other fatal symptoms occur, or of a remittent character. Many authors whose opportunities of observing it were great, claim for yellow fever a distinctly remittent, or in certain cases an intermittent form; while others maintain that the remissions, if these occur at all, are nothing more than slight diminutions of the symptoms or abatements in the morbid feelings, and not remissions in the true medical acceptation of the term. Some of this divergence may be attributed to the varieties in the form of the disease in different climates or localities; but at the same place, and even in the same case, some medical practitioners will designate that a remission, which others regard a mere diurnal oscillation of symptoms quite unworthy of the name.

It may be asked, then, what constitutes *a remission?* The answer, I apprehend, can only be, that diminution in the febrile symptoms observed in cases of remittent fever, in localities where that disease is common. I have had considerable experience of remittent fever, personally, during rather a prolonged service on the coast of Africa, and when no other form than remittent was prevailing. In numerous attacks I found my pulse, during the exacerbation, was very rarely above 96, and in the remission not lower than 92. The exacerbations came on in the afternoon, and continued until about four the following morning, after which the headache, heat of skin, and feverish restlessness gave way, and there was a slight moisture inside the wrists and across the forehead. On the accession in the afternoon, these were replaced by the original symptoms, and much to the same extent. The comfort and relief during this abatement of the symptoms was very marked as compared with the feelings during the preceding or following paroxysm; and it would have been difficult to convince me that the one was not a decided remission, in fact, whatever it might have been according to theory, while the other was an exacerbation, not attended with great acceleration of pulse perhaps, but still with such an accession of fever, restlessness, wakefulness, and other disagreeable symptoms, as to leave no doubt of its nature, and create a strong desire for the return of the remission. I have had numerous opportunities of observing the same in others. The pulse, it is true, will generally alter a little more than in my own case, but very often does not differ above eight beats between the exacerbation and remission, though I have met with some in whom the change was only four. The amelioration is perceptible in such cases rather in the diminution of the restlessness and feverish oppression, than in the pulse or heat of surface. Many expect a remission to present an abatement of fever almost equal to a complete intermission, and will not apply the term to anything less marked. I can only say that in my experience this was not very common, and in far the greater number of cases it was as described above.

Authors seem pretty well agreed that the following peculiarities are characteristic of yellow fever:

1st. A febrile disease, usually terminating in convalescence or death from the fourth to the seventh day; but it may be as early as the second, or as late as the tenth or twelfth, or even later.

2nd. There is generally yellowness of the surface, and, in case of death, the same of all the white tissues in the body, commencing at various periods in different individuals or epidemics.

3rd. In the course of the disease the urine becomes much diminished in quantity, and often is nearly or altogether suppressed. These are more particularly seen from the third day onwards.

4th. The alvine discharges are devoid of the natural feculent appearance, especially from the third day onwards, until the disease gives way.

5th. As the alvine and urinary secretions assume these peculiarities, there is a great tendency to black vomit, or discharges of similar matter from the bowels, or to hæmorrhages from the various mucous passages, or even the skin, and on death such may often be found in the intestinal canal when not manifested during life.

Though a case present most of these characters, those who attribute contagious properties to yellow fever declare, when remissions are mentioned in connexion with it, that he who mentions them must have mistaken an endemic remittent for true yellow fever. The remittent, they allow, may occasionally present yellowness, and even something approaching the appearance of black vomit, but state that it rarely proves fatal before the eighth, ninth, or tenth day, or causes suppression of urine, or the peculiarity of the alvine evacuations noticed in yellow fever. Instances are not rare, however, in which a fever distinctly remittent proves fatal by the fifth day with these characteristic symptoms, and black vomit as well. Nevertheless, as there may be a doubt as to the form of the disease when all these are not fully developed, it is of importance to obtain some symptom, or combination of them, diagnostic of yellow fever, and which will serve to distinguish it, whether fatal or not, from the other forms resembling it that may occur at the same time and place.

The diminution or suppression of urine is a constant feature in yellow fever, which the investigations of Collins, Blair, and others have shown to be accompanied by albumen and tube-casts, and farther conditions connected with which have been more fully developed in the previous part of this paper. Blair, at an early period, found that in ordinary intermittents the urine did not contain albumen, and immediately saw the value of the distinction in diagnosis. In Jamaica I found the same peculiarity: there were certain cases with all the appearance of ordinary remittents and intermittents, in which, though watched from day to day, there was no trace of albumen, or the other albuminous substances particularized above, found at any time; and there were neither desquamation of the bladder or uriniferous tubes of the kidneys, nor material diminution of the chlorides in the urine. There were other cases of fever, again, sometimes under treatment at the same moment in contiguous beds, in which the urine contained albumen, &c., presented the desquamation of the bladder and kidneys, and the absence of the chlorides, occurring in the order and at the period of the disease described above. These cases all presented other characters of yellow fever, and some terminated fatally, with black vomit or hæmorrhages, from the fifth to the seventh day. Therefore, without asserting dogmatically that the urine must present these in every case of this disease wherever it may arise, it seems quite justifiable to designate any in which they do occur, at the period, and in the succession previously detailed, as undoubtedly yellow fever. With the means of distinguishing them admitting of such precision, and so easily put in practice, the diagnosis between the yellow and other forms of fever will acquire much more certainty, and the relations between them be made out in a clearer manner than has hitherto been done.

With the advantage of this means of diagnosis, I have no hesitation in confirming the observations of many of the older observers, that yellow fever, in Jamaica, is frequently remittent, or sometimes even an open intermittent; and that, in cases of this nature that prove fatal, death takes place for the most part on the fifth, sixth, or seventh days of the disease, usually with yellowness of the surface, black vomit, or other hæmorrhages. The disease, therefore, answers the character of true yellow fever, and is not merely a yellow bilious remittent, which many who endeavour to prove the contagiousness of yellow fever assert.

The following case shows the disease in its remittent form, and illustrates many of the other points dwelt on in the previous part of this paper. Sergeant P. Sheehan, a European, aged twenty-nine, had been eight months in Jamaica—five at the mountain station, Newcastle, and the last three in Kingston, as a clerk in a public office, but had not had fever. After some days' indisposition he had a paroxysm of fever on the afternoon of the 31st August, 1859; but the following morning, feeling better, returned to his office. On the 1st September, or the second day of the disease, fever ensued again about one P.M., with severe frontal headache, suffusion of the eyes, and slight vomiting. Happening to be at his office at this time, I sent him to hospital. On the third day there was a remission in the morning; at two P.M., the exacerbation took place, with irritability of stomach and bilious vomiting, but the headache was less than the previous day. The cutaneous capillaries were injected, giving a dusky-reddish colour to the surface; and when any part was pressed, the marks of the fingers were left bloodless for a little, while the intermediate spaces retained their colour. Stools consisted of a fetid dirty mucous-looking matter, tinged with bile. Urine scanty; on acidulating some passed in the afternoon with nitric acid, it displayed a little albumen, and with nitrate of silver very little chlorides. On the morning of the fourth day there was a remission again, which at two P.M. was followed by the exacerbation. At five P.M., there was moderate fever, the capillaries of the surface much injected, and a copious eruption, resembling lichen tropicus. The marks of the fingers remained after pressure, as on the previous day. The tongue was furred in the centre, red at tip and edges; the stomach much less irritable. Stools more feculent in appearance, and less fetid than the previous day. There was a sense of weight across the loins, but no tenderness on pressure or percussion. A portion of urine passed this morning, after standing a little, presented a sediment of one-fourth its bulk. This consisted almost entirely of scaly epithelium from the bladder, with a few slightly granular tube-casts from the kidneys. The urine, after being filtered, was of the colour No. 6 by Vogel's scale;* specific gravity, 1023 (at 60°), and strongly acid. It contained a moderate quantity of albumen, also of casein, very little urea, scarce a trace of chlorides, but a good deal of hippuric acid, creatin, and apparently creatinine. On the fifth day there was the usual remission in the morning, only

* Thudichum's Pathology of the Urine, Plate 8.

more complete, the skin cool and moist; pulse 80; and there was no exacerbation in the afternoon. A portion of urine passed this morning presented a sediment nearly as copious as that of the previous day, but which consisted of tube-casts, mostly very granular, with traces only of epithelium from the bladder. The contents of some of the casts were tinged brown. The colour and constituents of the urine, otherwise, were the same as on the fourth day of the disease. From the fifth day there was no return of fever, the skin acted well, the papular eruption disappeared, the urine became copious, and the convalescence was uninterrupted. On the eighth day the tube-casts were much less numerous, and the chlorides decidedly increased in quantity. On the ninth there was no indication of albumen with nitric acid, and the chlorides were more plentiful. He returned to duty quite well on the thirteenth day.

This case, though slight, is interesting in many respects; it was the first attack in an unacclimated European; it presented the frontal headache, the injection of the conjunctivæ and skin, alteration in the alvine discharge, diminution of urine, and the duration of cases of yellow fever; the appearance of albumen in the urine on the third day, of scaly epithelium from the bladder on the fourth, and of tube-casts from the kidneys on the fifth, with great diminution of the chlorides and increase of the albumen from the third to the fifth days, are all well marked, and leave no doubt as to its having been true yellow fever. The form was distinctly remittent throughout, the patient himself remarking on his freedom from fever in the forenoon. The appearance of the eruption on the fourth day, together with the desquamation of the bladder and more feculent character of the stools, accompanied as they were by less irritability of stomach and a more moderate exacerbation in the afternoon, show these to have been critical efforts, insufficient to prevent the febrile paroxysm at a later hour that day, but enough to moderate it, and to obviate its renewal the next. Had the improvement in the stools not taken place, secretion of hæmatin from the kidneys would likely have occurred, ot which some of the casts on the fifth day gave indications by their brown colour; and had the kidneys also ceased to act, black vomit might have been looked for. The rapid improvement of the case under the opposite conditions is well marked, and is of value in suggesting the plan of treatment.

In the following case, as well as that of Brownlow, given above, the fever seems to have been intermittent in form, though the account is not quite so precise on that point as desirable. Thomas Coker, aged twenty-five, a black soldier, employed as policeman in barracks at Up Park Camp, in Jamaica, had a paroxysm of fever on the afternoon of Monday, the 17th May, 1858, which induced him to go to hospital the following morning, where he received some medicine, and was excused duty for the day. On Wednesday, the third day, he felt better, and went to his duty in the morning; it could not be ascertained subsequently whether he had had fever that afternoon. On the fourth day he was pretty well in the morning, but a paroxysm of fever came on in the afternoon, with vomiting. Next morning, the fifth day, he walked to the hospital at 7 A.M., being then feverish, inclined to drowsiness, with soreness in pharynx, and pain in swallowing. Conjunctivæ yellow; tongue red at tip and edges, white in centre; pulse small and quick; condition of the stomach, bowels, and urinary organs not noted. After admission he vomited once, and passed some urine, which were thrown out without having been examined. At half-past one P.M. he brought up a small quantity of black vomit, and died immediately. On examining the body three hours after, the white tissues were all of a pretty deep yellow colour. The stomach contained a pint of acid black vomit, its mucous membrane was somewhat congested in parts; that of the duodenum and jejunum was thickened, as if about to desquamate, and the solitary glands were enlarged like those in cholera cases. The mucous membrane in the lower part of the ilium was somewhat congested. The liver was rather large, flaccid, and presented congestion of the hepatic and portal vessels, with a circle of yellowish-grey colour between. Gall-bladder distended with dark fluid bile. Spleen rather large and pulpy. Kidneys enlarged and congested; bladder contained a quarter of an ounce of urine only. This case was well marked yellow fever appearing in a black man, a circumstance I have found of more frequent occurrence than many authors lead us to suppose. The congestion of the lower part of the ilium accompanying that of the stomach and duodenum, and the enlargement of the solitary glands, deserve attention in connexion with what has been already stated regarding typhoïd complications.

These cases are corroborative of the opinion entertained by the older writers throughout the West Indies and Southern States of America, that yellow fever is often a remittent, or intermittent, and consequently, that whatever may be the form it presents in other localities where the ordinary causes of periodic fevers may be less common, yet when they are rife, the yellow fever so closely resembles their more common products in many of its features, and so coincides as to time of appearance and locality with ordinary remittents, as to suggest a community of origin, the resulting disease taking the guise of yellow, or pure remittent fever, for reasons all of which we are not yet well acquainted with. The following table of the mortality among the troops at the chief military stations in Jamaica, between 1804 and 1820, will show its regularity according to season. The deaths from all diseases are included, but as about five-sixths of them were from fever, and when numerous generally of the yellow form, the table will give a pretty fair idea of the periods of activity of the causes of fever. To facilitate comparison, the numbers in the table have been calculated on the assumption that 1000 men died at each station, but the actual number of deaths is given also.

Mortality in each month in 1000 deaths from disease, among the white troops at the undermentioned stations in Jamaica, between 1804 and 1820:—

Month ending	Port Royal, 1807 to 1820 (14 years).	Spanish Town, 1804 to 1820 (17 years).	Up Park Camp, 1804 to 1820, excluding 1819 (16 years).	Up Park Camp, 1804 to 1820, including 1819 (17 years).	Stoney Hill, 1804 to 1820 (17 years).
January 20th	65	94	75	64	110
February ,,	37	45	37	30	79
March ,,	75	43	28	22	42
April ,,	101	45	26	21	21
May ,,	91	33	32	28	17
June ,,	90	40	34	30	25
July ,,	50	71	43	52	32
August ,,	65	83	80	125	66
September,,	120	66	95	127*	91
October ,,	89	106	156	145	231
November,,	112	152	263	227	183
December ,,	105	222	131	129	103
Year	1000	1000	1000	1000	1000
Total deaths recorded in period	536	995	1929	2398	967

These stations are all on the south side of Jamaica, and within sight of each other; the distance from Port Royal to Spanish Town being about nine miles in a direct line, to Up Park Camp five miles, and to Stoney Hill ten miles. They differ in the time of occurrence of the least and greatest mortality, and while Spanish Town, Up Park Camp, and Stoney Hill have each a single minimum and maximum in the course of the year, Port Royal has an earlier minimum than either, and a maximum in April, when the others are about their minimum; and maxima occur again in September and November as well. The table has been brought down to 1820 only, as up to that time the troops seem to have been retained at the different stations whether they were healthy or not; the severe fever epidemic of 1819 induced a deviation from this system, and the remains of the 50th and 92nd Regiments, which had suffered so terribly from it at Up Park Camp, were removed in August and September. Similar movements took place in subsequent epidemics, so that the records for the periods embracing these give a less correct view of the activity of the morbific influences at each than those previous to 1820.

These facts show that fever in Jamaica is a disease of season, the period for which is well-defined at each station, and that these periods differ materially at different stations, though in the immediate neighbourhood of each other. Each retains its distinctive features to this day, though now they are never permitted to develope their powers to any extent, the troops being removed as soon as fever threatens to be serious. The experience of other countries is to the same effect, the yellow fever in all observing regular periods of increase, culmination, and decline, which are partly dependent on the climate, partly on cir-

* In consequence of the severe epidemic of 1819, most of the troops were removed from Up Park Camp in August and September, and did not return until December, which gives a considerably different result if 1819 be included. Previous to this year the troops do not seem to have been removed, however sickly the season.

cumstances more immediately connected with particular localities, as at Port Royal.

Many illustrations of the power of local causes to generate yellow fever, can be obtained from the countries where the disease is common; but some, taken from a place where fever of this form is rarely met with, and when there was no question of importation to complicate it, may be more convincing. The subjoined cases are particularly interesting in this respect; the subjects of them were men of the 36th Regiment, stationed with their company at Vido, a small island in the harbour at Corfu, where they were employed removing a considerable depth of moist alluvial clay for the foundation of a water-tank; both seem to have been drinking freely before their attack. It is to be remarked, that at Vido there is some peculiarity leading to fever, as it is stated in the 'Statistical Report on the Sickness and Mortality among the Troops in the Mediterranean' (p. 41), that between 1817 and 1836 the majority of the cases of remittent fever in the returns for Corfu were derived from Vido.

Private J. Culleton, aged twenty-six, while employed as above-mentioned, was attacked with fever on the 22nd of August, 1850. The following day, the second of the disease, he was admitted into the Regimental Hospital at Corfu, in a state of considerable prostration; the heat of surface but slightly increased; pulse small and frequent; tongue coated, with a brown, dry streak in centre, but moist and clean at edge. On the third day the prostration continued, and there was oppression at the præcordia, nausea, and vomiting. Lemon colour of the surface, with little elevation of its temperature; pulse 136, small and unsteady. On the fourth day the surface was moist; pulse 110; tongue moist and cleaner; yellowness increased; stomach irritable. During the night he had passed three ounces only of a high coloured, turbid urine. On the fifth, the yellowness was deeper, temperature of surface natural; pulse ninety-four; the nausea and vomiting rather less; alvine evacuations offensive, and wanting in bile; passed six ounces of urine only since previous morning; an eruption of bullæ filled with a yellowish serum, with some purple discoloration about their bases, had appeared on the arms and chest. On the sixth day, the yellowness was deeper; pulse continued the same; no urine had been passed; and there was a fixed pain in the region of the right kidney, for which he was cupped to six ounces. On the seventh day, the suppression of urine continued. In the evening, a purgative enema brought away an offensive motion without bile. The eruption did not present an increased number of bullæ, but they were larger, and their contents had become a dirty, sanious pus, or were mixed with dark-coloured blood. On the eighth day, after having been wandering all night, there was epistaxis; the functions of the liver and kidneys were still suspended; the stomach irritable, the matters ejected of a dark, grumous character from admixture of blood. Death occurred at midnight.

On examining the body, the mucous membrane of the stomach was found softer than natural, and congested in parts, more especially about the cardiac orifice. There was fulness of the vessels, with effusion of blood beneath the mucous membrane in the ilium and rectum, but no trace of ulceration anywhere. The liver weighed five pounds two ounces, its colour was between olive green and dark brown; structure friable, and capillaries congested. Gall-bladder full of dark-coloured bile. Spleen soft, easily broken up. Kidneys almost double their usual weight; much congested, with some muco-puriform matter* on the lining membrane of the calyces and uriniferous tubes. Bladder quite empty, but healthy. The low fever, its abatement from the third day, the diminished secretion of urine on the fourth, and want of bile in, or ordinary feculent appearance of, the stools from the fifth, are all characteristic of yellow fever, and serve to distinguish the case from any of the other forms of fever. But for the eruption on the fifth day, black vomit, or hæmorrhage from some mucous surface, would probably have appeared on that day, and even this did not obviate the epistaxis and black vomit on the eighth day.

The other case was that of Private G. Hargrave, who was attacked on the 17th September, at Vido; but I have no detail of symptoms before the 19th, the third day of the disease, when he was admitted into the General Hospital at Corfu. There was then much debility present, with headache, soreness of chest (part not specified) and abdomen, and pains in limbs; heat of surface slightly above natural; pulse small, weak, and frequent; bowels acting freely from medicine given at Vido; tongue thickly coated with a brown fur. On the fourth day the headache and uneasiness in chest and abdomen continued; the skin and conjunctivæ were yellowish; pulse less frequent, rather firmer; was purged during the night; urine rather scanty, high coloured, depositing lithates freely; tongue as yesterday. In the evening the fever was less, but was much troubled with flatus. On the fifth day the surface was cool, the yellowness increased; stomach irritable, and vomited after taking anything; tongue coated, but moist and clammy at the edges; bowels freely moved during the night, when he passed a few drops of urine; pulse 96. On the sixth day the fulness at the epigastrium and irritability of stomach had increased, and become distressing; the yellowness was deeper; no alvine evacuation or secretion of urine; pulse 100. In the evening an enema brought away some hardened lumps of offensive slate-coloured fæces. On the seventh day there was considerable drowsiness, stomach rather less irritable; pulse 100, smaller; no urine secreted. In the evening there was one liquid stool, containing some clots of blood, and only one ounce of turbid, high-coloured urine passed during the day. On the eighth day there was again increased irritability of stomach, and he was passing dark-coloured offensive liquid evacuations from the bowels, containing much dark liquid blood; tongue brown and dry; pulse thready; temperature of surface failing. These symptoms increased, and he died at eight P.M.

On examination after death, the surface of the lungs was found

* Most likely tube-casts, but the matter does not seem to have been examined with the microscope.

mottled with dark red spots of active congestion; they contained much fluid, with considerable hypostatic congestion. The mucous membrane of the stomach, towards the pyloric orifice, and the upper part of the duodenum were intensely congested. The intestinal glands were free from ulceration, but throughout the jejunum, ileum, and colon there was exfoliation of the mucous membrane, the epithelium lying in ragged patches on the subjacent membrane, beneath which there was extravasated blood. The liver weighed four pounds; its texture was softened and portal capillaries congested; gall-bladder full of dark-coloured bile, ducts pervious; spleen of usual size, very friable. The kidneys weighed seven ounces each, and were much congested; bladder contracted, and did not contain a drop of urine. This case, like the former, presented a commencing diminution of the pulse and fever on the third day. The yellowness was first perceived the following one, when also the scantiness of urine attracted attention, which subsequently was almost completely suppressed. The irritability of the stomach on the fifth day, the light-coloured alvine evacuations noted the following one, and hæmorrhage from the bowels at last, are all characteristic; and the congestion of the pyloric portion of the mucous membrane of the stomach and of the duodenum, the desquamation of the intestinal mucous membrane, congested state of liver and kidneys, and empty bladder, leave no doubt whatever that the case was yellow fever of a malignant description. The supposed deposit of lithates on the fourth day, from its immediately preceding suppression, was most likely a sediment of epithelium from the bladder or tube-casts, or both. It does not seem to have been examined by the microscope, nor with the view of seeing whether it really were composed of lithates. The treatment in both cases was judicious; it consisted of quinine, with cupping over the loins and epigastrium, and external warmth. Mild saline diuretics and diaphoretics also were given, to promote the flow of urine and determine to the surface, and wine and nourishment as required.

These cases were the only ones presenting the characters of yellow fever seen at Corfu in 1850, and there was no question of importation to suggest a doubt as to their origin. The men were stationed on a small island which had previously produced much remittent; they were engaged excavating the soil, an operation which has often proved hazardous to those engaged in it; and their susceptibility was heightened by free indulgence. Under these circumstances they contracted fever, which proved to be the yellow fever; and it can be attributed to no other cause than the emanations from the soil to which they were exposed in the island, if not actually in the work on which they were engaged. The cases are particularly valuable. They were recorded by officers who had no theory to support. The notes, of which the above is an abstract, were communicated by Dr. Jopp, surgeon of the regiment.

I have encountered yellow fever in Barbadoes, Sierra Leone, and Jamaica; and after the best consideration I could give the facts which presented themselves, am firmly impressed with the belief that the evidence they afford was quite in accordance with the origin of the disease from local causes, and irreconcileable with the opinion that it either arose from, or was propagated by, contagion. General statements were often submitted to me which would have borne a different construction, but on a full examination of the circumstances they were said to embrace, their insufficiency became apparent, and the deductions from them fell to the ground. It would occupy too much space to give details for all these, but I may here refer to the outbreak at Newcastle, in Jamaica, in 1856, an account of which appeared in the number of this Review for October, 1859, p. 445.

Newcastle is placed on the crest of a sharp ridge, so narrow that in several places there is space for a single barrack-room only. The cantonment occupies something under 800 yards of the ridge, within which distance it falls from 4120 feet to about 3500 feet above the sea. Here, in August and September, 1856, cases of yellow fever showed themselves in persons recently from the low ground, and on the 17th and 18th September, two men in the same ward of the hospital with the last case, and who had not been away from Newcastle for months, were attacked. After this the disease appeared in two houses near the hospital, and subsequently in other parts of the cantonment, and before the end of December had proved fatal to forty-five persons. This general statement is correct in every particular mentioned, and would be received by many as incontestable evidence of the contagiousness of yellow fever. Most of those advanced for this purpose are of the same description. Its insufficiency for this purpose will be obvious, however, and these facts appear in a very different light, when it is explained that, on tracing every person to his place of residence at the time of attack, it was found that there were four zones in the cantonment, embracing all the rooms and tents across the ridge, which enjoyed immunity almost complete from the disease. These included, and alternated with, three others, in which the disease appeared; and the hospital and rooms where the first cases occurred were in one of these unhealthy zones. But the proofs in favour of local causes did not end here. Other patients and attendants on the sick having been attacked in the hospital, it was cleared, and another opened on one of the healthy zones; and though the unhealthy situations continued to afford fresh cases, which were removed to this as they occurred, the disease could not extend itself from them to the attendants or others at a point not 200 yards from where they originated.

In every instance I have met with yellow fever among the troops, the limits within which it appeared were well defined, though the positions of danger and immunity did not alternate so remarkably as at Newcastle. Facts of this description, of which there are many, show the unsoundness of the evidence in favour of contagion sought to be derived from the spread of yellow fever among persons in an unhealthy locality, or from its non-appearance among others in quarantine who may happen to be placed in a healthy one; they prove, also, that sickly spots may be very limited in extent, and may adjoin others that are healthy, and that these may even be mixed up together in a manner wholly irreconcileable with the notion of the extension of the disease by personal communication. It would be well, in any attempt to prove that crowding or other personal influences were instrumental in extending yellow fever, to admit these facts, as unless this be done, no sound advance can be made in our information on the subject.

If yellow fever arise from the same local causes which at other times produce remittents or intermittents, it may be asked whether the emanations from these are merely more concentrated, as some suppose, or are somewhat changed—whether, in short, there be merely a more concentrated poison, or another with somewhat different properties produced. The latter view represents the facts better than the former, for were the poison more concentrated only, all cases of yellow fever should be more severe than remittents, whereas there are many cases of the former well-marked, even in unacclimated Europeans, which are as slight as the slightest remittent, and sometimes scarcely sufficient to detain the patient in bed. On the other hand, pure remittents may be so severe as to prove fatal in four or five days. Again, yellow fever is sometimes produced by the emanations from the holds of ships,* which are not known to give origin to intermittents or remittents. It has been observed, too, that heavy rains either suspend the production of the poison causing yellow fever, as seemed to be the case at Newcastle, in November, 1856,† or alter its nature so far that most of the fevers which appear during their continuance are mild remittents only, as occurred at Sierra Leone in the epidemic years 1823, 1829, 1837–8, and 1847, though when the rain was succeeded by fair weather, whether at the end of the rainy season or during a break in its middle, yellow fever presented itself, in the latter case to be supplanted again by the remittent on the recurrence of heavy rain.

A further proof that the emanations causing yellow fever are in some respect different from those giving rise to remittents, is derived from the symptoms of the former itself. Its peculiarity, as compared with remittents, consists in the desquamation of the bladder and uriniferous tubes of the kidneys about the fourth day, the cessation of the secretion of colouring matter by the colon about the same time or earlier, and the early active exudation into the substance of the liver and kidneys, leading respectively to jaundice and suppression of urine; the black vomit and hæmorrhages, and irritation and exfoliation of the mucous membrane of the small intestine, when they occur, seem to be consequent on the suppressed function of the colon, kidneys,

* This does not refer to the odour of "bilge water," as it is called, which is well known from the sulphuretted hydrogen it contains, but to an emanation arising from a foul hold in a sailing vessel, and more frequently met with of late years in steamers and vessels which have carried coals to the tropics. Of the first, Bancroft gives many instances in his essay and sequel; many also are mentioned by La Roche. Others are noticed in the Reports on the Health of the Navy; and a very striking one in the Address of the President of the Epidemiological Society, Transactions, vol. i. p. 139.

† British and Foreign Medico-Chirurgical Review, Oct. 1859, p. 479.

and liver. In ordinary remittent, the functions of these organs are less frequently suspended, and very rarely so early as in yellow fever; the system can, therefore, go on relieving itself from time to time by partial critical evacuations, and, provided the brain do not become seriously affected, the patient has a fair chance of recovery. The occurrence of desquamation of the uriniferous tubes, and the other symptoms connected with them, on the fourth and fifth days in the yellow form of the disease, coincide in time with the natural remissions of the second tertian period, and are therefore to be regarded as critical efforts, and their appearance is undoubtedly connected with some peculiarity in the poison from which the disease originally sprang. Being of a critical nature, it is quite possible they might appear during the first tertian period in some cases, or not before the third or fourth in others, but for the most part they are met with in the second period, or between the third and fifth days of the disease.

Can a fever originally of a continued form present, at an early period, an affection of the kidneys and other organs such as those which have been shown to characterize the remittent forms of yellow fever, and, on their appearance, remit and assume the other characters of the latter disease? The reply, it seems to me, must be in the affirmative, as most who have seen yellow fever must have met with cases in which there was no apparent remission until the fourth day, or later, when the urinary secretion became diminished, and albumen was found in it. There is nothing extraordinary in this, if the affection of the kidneys be regarded as a critical effort, for a sufficient crisis will at all times resolve a fever, of whatever form it be, and though in our experience in this country such seldom occur, and the continued fevers are usually extended over a long period, yet in warm climates, where pure synocha is met with, it is otherwise, for a sharp attack of that is frequently terminated by a profuse sweat on the fourth or fifth day, and the patient may be able to move about within a week. It must be admitted, therefore, that yellow fever may present a continued as well as a remittent or intermittent form, the course of all these being assimilated only after the lesions peculiar to the disease come to be developed.

Fevers in tropical climates are by no means so simple or well-defined in their forms on all occasions as many suppose; on the contrary, they are often very complicated. I have known cases commence as remittent, and continue as such to from the sixth to the tenth day, having an intermission on the morning of these days, yet the same afternoon fever recurred, which soon took on the character of yellow fever, and proved fatal on the fourth or fifth day of that form, with black vomit and other unmistakeable symptoms of this disease. I have seen in other cases which commenced as intermittent, diarrhœa ensue, and after three or more tertian periods, the fever became continued, and assumed the character of typhoïd fever, and ultimately presented the affection of the kidneys and urine seen in yellow fever, and then terminated in death. On examining the bodies, the intermixture of the morbid appearances peculiar to yellow and typhoïd fevers were detected in variable proportions, as already detailed. The cause of the typhoïd complication in these cases was a privy immediately in rear of the building from which they came, but to windward of it at night; this had a deep cesspit, which had been emptied, and thus exposed the additional surface of the sides, as well as the bottom, to give off emanations. On clearing the building of its inmates, the typhoïd complication disappeared. These facts show that yellow fever is not a complaint separate and distinct from all others, but that it becomes mixed up with them in various ways, according to circumstances.

Division IV.—Treatment.

There are cases of yellow fever so slight that they get well with little or no treatment; there are others, again, and unfortunately they are much more numerous, that seem doomed from the first, and in which treatment is of no avail. The number of the latter varies much in different epidemics, but under all circumstances they may be expected to cause a high mortality, quite beyond the power of medicine to prevent. There are many, however, between these extremes, in which the disease endangers life, and in which an enlightened and discriminating treatment may avert a fatal result: such a plan of treatment is still a desideratum in this disease.

Treatment is naturally divided into means for resolving the disease during its course, and those for meeting the various morbid actions which arise when that is unchecked, and the evil consequences resulting from them. A few remarks on each in connexion with what has gone before, though they have no pretension to be considered as more than an outline, may serve to clear away some of the obscurity in which the whole subject is involved.

Blair, at Demerara, employed large doses of calomel and quinine as early as possible in the disease, with the view of checking it; the quantities he employed were twenty grains of the former and twenty-four of the latter, which he repeated every six hours, for several times, if necessary. This medicine acted freely on the bowels and skin, and, according to his experience, was often sufficient to cut it short. The same practice has frequently proved successful in Jamaica; but, according to Dr. Davy, it was not attended with beneficial results at Barbadoes; and the American practitioners at New Orleans have not found it answer their expectations in stopping the fever, while the large and frequently repeated doses of quinine were often highly injurious. This discrepancy may be explained in part by the fact, that fever at Demerara generally, and in Jamaica very often, is of the periodic form, and yellow fever when it arises will retain more or less of the same peculiarity; in such, if the fever can be checked, and the system brought under the influence of quinine, that medicine may prevent another paroxysm, and thus obviate the affection of the liver or kidneys when these have not actually commenced. At Barbadoes, on the contrary, though periodic fevers are not unknown, they are not common, and when yellow fever arises, it is apt to have a continued form, and not be amenable to the anti-periodic influence of quinine; hence its exhibition there was not likely to be equally beneficial. This explanation, I apprehend, will apply to a considerable extent to New Orleans; for although there were abundant sources of remittent fever around that place, still in the city itself, when many of the cases of yellow fever originate, the causes will be rather those peculiar to large towns, with the surface more or less covered in, than to the more exposed places where remittents are common. The calomel is a very essential part of the treatment; it acts powerfully on the colon, causing a profuse dark pultaceous stool, and seems to anticipate that condition of the intestine when its secretion ceases. I saw a case of yellow fever at Barbadoes cut short by an emetic, followed by strong purging with calomel and sulphate of magnesia, but without quinine, while another, attacked at the same time, and in the contiguous bed, in which these measures were not adopted, went on to black vomit and death. These were men in hospital, and were brought under treatment within a few hours after the fever had declared itself. The practice deserves notice, though I have not many cases to adduce in support of its efficacy.

Blair, who recommended these large and frequently repeated doses of quinine in yellow fever, at first thought he had never seen them act injuriously; subsequently, however, he altered this opinion; but many still believe they do no harm. In 1847, at Sierra Leone, I saw coma induced in a robust adult female labouring under remittent fever by a dose of quinine of fifteen grains only, at a time when much larger were commonly exhibited; and though she had no more fever there was for a considerable period afterwards so much fulness and uneasiness in the head as to induce caution in the employment of this medicine with others. I have seen or heard of several other cases of coma from over-doses of quinine, and have known a good many men in whom even moderate quantities, not exceeding twenty grains in twenty-four hours, have brought on at least congestion of the membranes of the brain, which could not be distinguished from meningitis, unless perhaps by the readiness with which it gave way to a large blister on the back of the neck. The experience of the physicians at New Orleans is to the same effect; the frequency of deafness, blindness, partial paralysis, and other consequences of injury to the nervous centres, by the too free exhibition of quinine, has rendered them more cautious in their use of it. The fact seems to be, that many persons can take large quantities of this medicine without further inconvenience than a temporary singing in the ears, while others are much more sensitive to it, and smaller quantities are sufficient to induce serious disease of the brain. So far as I have seen, there is no certain means of distinguishing these. It is satisfactory, however, to know that most of the beneficial effects of quinine can be obtained with smaller doses, if not so quickly on all occasions, at least with less risk. During the latter years of my service at Sierra Leone and the Gambia, I never gave more than fifteen grains of this medicine in the day, and though living in the midst of severe remittents, found the results not less satisfactory that when large doses were employed, while the bad effects were rarely encountered.

Should the effort to cut the disease short have failed, or should the patient not have been seen until it was too late to attempt it, the object of the medical attendant will be to moderate excessive action in any organ, and endeavour to bring about as complete a crisis as he can about the fifth day, the natural period of resolution in the disease. For this purpose nothing is of more importance than to re-establish the secreting function of the colon and to obtain feculent evacuations, not mere bilious discharges—for a patient may have these and yet die—but proper dark-brown feculent stools. But how to bring about this desirable result is the difficulty. Violent measures of any kind are not likely to be successful at this period of the complaint; and the best devised will but too often prove ineffectual. Gentle excitement of an extensive portion of the lining membrane of the colon, with frequent copious enemata of tepid water, or any bland fluid, or these rendered more stimulating with any of the ordinary substances employed for this purpose, deserve a full trial; powerfully stimulating enemata of any description, or drastic purgatives, will only serve to increase the mischief. Should the case present violent purging, with or without blood, which is sometimes the case, this of course would be inapplicable.

Should a natural alvine secretion be obtained, it is not likely there will be much further trouble with the case; but if, as will too often happen, it is not, the next condition demanding attention will be the state of the kidneys. The congestion of these on the fourth and fifth days will require watching, so as to diminish the chances of suppression of urine, and to bring about a more natural secretion, so far as that can be done by reducing the congestion, and preventing the closure of the uriniferous tubes by accumulated epithelium. This point has not received that attention it deserves, though it is obvious that serious diminution of the urinary secretion will inevitably aggravate the other symptoms, if it do not induce a fatal result. Cupping, either dry or with the abstraction of blood, and frictions with stimulating liniments over the loins, have proved useful; these, with warm baths, or still better, hot-air baths, deserve a full trial; not that they will cure every case, or even a large portion of those in which they may be required, but they afford the best chance of relieving the kidneys, and may save life under circumstances otherwise all but hopeless. Small doses of acetate of ammonia with potash or soda, or their salts in common use, with diaphoretics, so as to act gently on both kidneys and skin, may prove beneficial, in addition to the other measures.

Local abstraction of blood, counter-irritation, and a variety of medicines calculated to relieve urgent symptoms, or check excessive discharges, can be had recourse to as the judgment of the practitioner may suggest. The patient will often derive much comfort, or troublesome complications be kept in check for a little, by a discriminating employment of these; but inasmuch as all the conditions calling for them seem to be but efforts of the system, whether by secretion from a mucous surface, or exudation into the parenchyma of an organ, to rid itself of what under ordinary circumstances should have passed by the bowels, kidneys, or skin, unless the functions of those parts be restored, remedies directed specially to other organs are but palliatives, useful in their way, no doubt, but still not striking at the root of the evil.

The administration of alcoholic stimulants in yellow fever requires notice, as they are often pushed at a time when, to say the least, their use is very questionable. When a patient is very low, and the heart's action weak and languid, it is necessary to employ wine or brandy to maintain life; and provided the kidneys be unaffected, large quantities of either can be given, it may be with benefit, at all events without serious injury; but when the kidneys are congested, and still more if secretion has ceased, the alcohol will accumulate in the system if given freely, until it either cause irritation of the bowels and diarrhœa, or pass off through the lungs. I have known the air in a large room in which there was a window and door always open, to be so impregnated with the vapour of brandy given off with the breath of a patient in yellow fever, that it was disagreeable; and I have seen the lungs of another who had suppression of urine, and died of the disease, so impregnated with brandy that they gave off the odour of it from every part as strongly as if they had been steeped in it for some time. A patient whose life is already endangered by the embarrassment of the kidneys and intestinal canal, is not likely to have his chances of surviving increased by so drenching him with alcohol; the congestion of the kidneys will be most certainly increased, if it have not as yet gone beyond what is compatible with a restoration of function, and the irritation of the bowels will only present a further obstacle to a healthier action on their part.

These remarks are necessarily very incomplete. All practice in yellow fever must at present be more or less empiric, and we can expect to substitute more rational plans only as our knowledge of the diseased actions and their results becomes extended. It is much to be desired that the alvine evacuation should be as fully investigated both in health and disease, as the urinary has been; but even then, until the transformations of matter in the system during the secondary assimilation are more fully understood, we shall not be in a position to develop a really scientific practice in this disease.

I was indebted to the various medical officers who served with me in Jamaica for their assistance in carrying out the investigations on which the above remarks are based. The details of cases were obtained from their records, though I saw many of them frequently myself, and was present at nearly every post-mortem examination. All the remarks on the urine and morbid appearances of the various organs are from my own observations.

ART. III.

Observations on the Pathology of Emphysema of the Lungs. By A. T. H. WATERS, M.D., M.R.C.P., Physician to the Liverpool Northern Hospital.

BEFORE proceeding to offer a few observations on the pathology of emphysema of the lungs, it is necessary that I should premise one or two remarks with reference to its morbid anatomy.

Pulmonary vesicular emphysema—for it is to this that I wish to direct attention—exists in three forms, not differing in their minute anatomical arrangement, but only in the extent to which they involve the lung. We have—First, that form which is most partial, and confined to a few air-sacs (air-cells), or a single lobulette of the lung—*partial lobular emphysema;* secondly, that in which the whole of a lobule is affected—*lobular emphysema;* and thirdly, that in which the whole of one lobe of a lung, or more frequently the whole of the lung itself, is involved in the disease—*lobar emphysema.*

1. *Partial lobular emphysema* is not often seen as an independent affection; but in lungs in which the second form exists, we occasionally meet with patches of dilated air-sacs, especially along the margins of the lobes—patches which clearly involve only a few air-sacs, or, at most, a single lobulette. These portions resemble enlarged vesicles; they push out the pleura so as to raise it above the level of the surrounding lung-tissue. Sometimes these partial emphysematous patches are seen extending for some distance along the margin of the base of the lung, and they have then an appearance very like that of a row of beads.

2. *Lobular emphysema* is the form most frequently met with; in fact, in the majority of *post-mortem* examinations I have seen since my attention has been directed to this subject, I have found this form of emphysema more or less prevailing. It involves one or more lobules in different parts of the lung, and is more especially found along the margins of the base, at the anterior border, and at the apex of the lung. This is the kind of emphysema so frequently met with in phthisis.

In lobular emphysema it is easy to trace the different divisions of the lung, for the boundary walls of the lobules have not given way, and there is no interlobular emphysema, except in some cases, and to a partial extent. The diseased lobules are seen on the surface of the lung projecting beyond the level of the surrounding tissue, and along the margins of the lobes they often form projections of considerable size.

3. The third form of the disease is the most important of all. I have denominated it *lobar emphysema.* As far as my observations go, it more frequently attacks both lungs than one, and the lower as well as the upper lobes. It constitutes a most formidable affection, and often destroys life at an early period. The occasions on which I have had

an opportunity of seeing the disease after death, have been in persons of adult age. Its features are very characteristic, and it is easily recognised during life. It consists of an emphysema of the whole pulmonary tissue of a lobe or lung. The lung-substance has a peculiar doughy feel, and a colour very closely resembling that of a calf's lung. The whole organ is increased in bulk, does not collapse when the chest is opened, nor yet when removed from that cavity, and if inflated reaches a very large size.

If we examine the emphysematous lung-tissue, we find that in the early stages of the disease there is a simple dilatation of the air-sacs (air-cells), an increase in the size of the alveoli, and a diminution in the height of the alveolar walls, which yielding with the distending cavities, become partially obliterated. As the disease progresses, the air-sacs become still more distended, and the alveolar walls in some instances completely obliterated, so as to give a regular and smooth appearance to the inner surface of the air-sacs. This distension of the air-sacs is followed by a perforation of their walls. This at first is but slight; here and there a circular or oval opening may be seen; as the disease advances, however, these openings become more numerous and larger; in some instances the whole of the walls of the air-sacs being perfectly riddled with small openings, so that a horizontal section of the lung-substance has a general cribriform appearance. The subsequent steps in the progress of the disease consist in a further distension of the air-sacs, an enlargement of their perforations, and rupture of their walls. As these results take place, the walls become more and more imperfect, and the openings in them coalesce; a further breaking-down then takes place, so as to leave but very partial partitions between the cavities; and in the most advanced stages of the disease these partitions undergo other changes, and are reduced to mere membranous shreds or thin fibrous cords, passing in various directions—traversing, in fact, the distended sacs, two or more of which, by the destruction of their walls, have united to form a single cavity. These cavities occasionally assume a large size, and form what are called appendages.

I have observed an anatomical difference in preparations of different lungs, which I think is important in its pathological bearing. In some lungs in which the emphysema has been of the lobular kind, I have found the air-sacs distended to a very considerable extent, but I have observed few or no perforations; whilst in other lungs, and especially in those where the disease has been of the lobar character, I have found extensive perforation of the walls of the air-sacs, with certainly not more, and in some instances less, dilatation than in those alluded to above. In all the cases of *lobar* emphysema that have come under my notice, I have found the pulmonary tissue extensively perforated, whilst in some cases of *lobular* emphysema this condition has not existed. These facts would seem to indicate that in the cases where rupture takes place under a dilatation, which in others does not produce such rupture, there must be some degeneration of the tissue which renders it more liable to give way.

Having premised these observations on the morbid anatomy of emphysema, I proceed briefly to consider some points in its pathology. An examination of the subject is of the greatest possible importance, for it lies at the very root of the therapeutics of the disease.

The great question for consideration in connexion with the pathology of emphysema is, whether there is any degeneration of tissue either preceding or attending the affection—whether, in fact, there is any local or general condition which so interferes with the normal nutrition of the walls of the air-sacs, as to cause their perforation, rupture, and even total destruction. If it could be shown that either in the elastic fibres which are so numerous in the walls of the air-sacs, in the basement membrane, or in the capillary bloodvessels, certain changes take place of a degenerative character, a very important step would have been made in reference to our knowledge of this disease.

The first appreciable anatomical change which occurs in the lung-tissue is an increase in the size of the air-sacs. This must necessarily be attended with a loss of elasticity on the part of the elastic fibres, which on being stretched by mechanical power, are unable to recover themselves. When we find emphysema existing only as a partial affection, and in conjunction with some old-standing disease of the lung, we can readily imagine that it may have been produced by mechanical violence whilst the tissue affected was in a healthy state; but when we see the disease, even in moderately young persons, creeping insidiously on, and attacking the whole of one or both lungs, without the previous existence of long-continued or violent cough, we can scarcely imagine that changes of so extensive a character can result from mechanical violence without the previous existence of some morbid condition of the lung-tissue.

My own observations have led me to conclude that all cases of emphysema cannot be included, either with reference to their pathology or determining cause, under the same head. Where the disease is partial, and situated, as is then usually the case, along the margins and at the apex of the lung, and is associated with or has followed some other pulmonary affection—as, for instance, chronic bronchitis, or, in fact, any disease which has been attended with long-standing or violent cough, I believe the morbid changes may have been brought about mainly by mechanical violence, without there having been any pre-existing affection of the lung-tissue. The general appearance of the lung-substance in these cases is very different from that which characterizes the disease to which I have given the name of lobar emphysema; the tissue has more the appearance of that of the healthy lung, but it is paler and more anæmic than the latter. It is quite true that in these cases the same anatomical changes—dilatation, perforation, &c.,—take place in the progress of the disease, as in the larger and more formidable affection; but these changes necessarily ensue, not only from a rupture of the elastic fibres and basement membrane, but also from the giving way and absorption of the capillary bloodvessels; circumstances which lead to imperfect nutrition and consequent atrophy.

Lobar emphysema has been but imperfectly treated of by systematic writers, and has not received that attention which its importance deserves. It is true that some of our great pathologists have recognised it as a substantive affection, but practically it has not been sufficiently distinguished from the more partial and less formidable kind of the disease. As I have before remarked, it is occasionally seen in early life, but the most numerous and most marked instances of it that have fallen under my notice have occurred in adults.

That this form of the disease differs very materially in its pathology from the partial emphysema which I have spoken of in a former paragraph, will, I think, be admitted by all who will give a careful attention to the subject. The insidious manner in which the disease sometimes comes on; the almost entire absence of cough frequently observed, as well as of all other symptoms except a gradually increasing dyspnœa, and, as the patients constantly describe it, a "smothering in the chest;" the occasional rapidity with which the affection progresses; the secondary consequences which ensue, and the general atrophy which often supervenes—all point to the grave character of the malady, and I believe to its constitutional origin in some degeneration of the pulmonary tissue.

But if emphysema be the result of some degeneration of the lung tissue, it behoves us to inquire what the nature of that degeneration is. On this point a good deal of obscurity still exists; it does not appear that much attention has been given to this part of the subject, and its important bearings on the treatment of the disease have been but little dwelt on.

In a paper which was presented to the Royal Medical and Chirurgical Society of London, and subsequently published in their 'Transactions,' Mr. Rainey has described the condition of an emphysematous lung, which he seems to have examined with great care. This lung was taken from a subject forty years of age, and the emphysema seems to have been only partial, for the general aspect of the lung, especially in the vicinity of the emphysematous parts, is described as being healthy. A few tubercles, however, existed in some spots. Mr. Rainey found the pulmonary membrane in the emphysematous portions more or less studded with fatty matter, and he has expressed an opinion that this deposit of fat is the precursor of the perforations and subsequent changes which take place in the disease.* As it does not appear that Mr. Rainey has observed this condition of the lung-substance as a general accompaniment of emphysema, but only in the specimen from which he has drawn the chief conclusions referred to in his paper, and as his observations have not been confirmed by subsequent inquiries, the pathological view he has sought to establish—viz., that the disease is the result of fatty degeneration—cannot be considered as settled.

In his recent lectures, delivered before the Royal College of Physicians of London, Dr. C. J. B. Williams, in speaking of emphysema of the lungs, says: "It is fatty degeneration of the lung-tissue which aids in bringing about the atrophy and rupture of the cells."†

* Med.-Chir. Transactions, vol. xxxi. † Lumleian Lectures, 1862: Lancet.

With the exception of the two authors quoted above, no pathologists, as far as I am aware, have supported the opinion that emphysema is produced by, or attended with, fatty degeneration. On the contrary, one of our most accurate observers in connexion with this disease, Dr. Jenner, to whom we are indebted for an able paper on it, has expressed his opinion that "the most frequent anatomical change in the lung," producing loss of its elasticity, "is fibrous degeneration, the consequence of the exudation of that variety of lymph which escapes from the capillaries when they are the seat of slight but long-continued congestion."*

With the view of ascertaining whether emphysema is preceded by, or has associated with it, fatty degeneration of the pulmonary tissue, I have made a careful examination of a large number of specimens of lungs which were the seat of the disease. These specimens were taken from lungs which presented the affection in all its varieties, whether partial or general. I have not only submitted to examination the diseased portions, but (where the disease was partial) pieces taken from contiguous parts, and where the lung-tissue was apparently healthy. The general results of my investigations may be briefly stated as follows: In the large majority of cases I have found no indications whatever of fatty matter. In some few instances, however, I have seen deposits of fat in the walls of the air-sacs.

My examinations have been conducted with the microscope on recent, dried, and injected specimens, and also by heating the lung-tissue between pieces of glass, so as to dissolve out the fat, if present, and thus get indications of its existence.

In examining this question I have viewed it in several lights, and considering that the disease might possibly be due to some affection originally commencing in the capillary bloodvessels and producing mal-nutrition in the pulmonary tissue, I have made a careful examination of the branches of the pulmonary artery from their commencement to their termination in the pulmonary plexus, in order to ascertain if they were the seat of anything like fatty or atheromatous degeneration. The results are that I have found in some cases atheroma existing in the branches of the pulmonary arteries and in their capillaries, whilst in others I have found no indications of it whatever. In the cases where the atheroma existed in the pulmonary arteries, I have always found it in the aorta as well, so that the affection of the former vessels must be considered as simply the result of the general tendency to arterial degeneration, and not as possessing any specific bearings on the emphysematous condition of the lung-tissue.

I have further endeavoured to ascertain by careful microscopical examination whether any appreciable difference could be traced between the elastic fibres of the emphysematous lung and those of the healthy organ. Here, again, I am not able to say that any marked distinction existed between the diseased and healthy fibres, except that the former sometimes appeared less regular in their outline, and had

* Medico-Chirurgical Transactions, vol. xl.

less tendency to curl up at their ends; but as to any structural change in the fibres, I have not been able to satisfy myself that any such had taken place.

Considering, therefore, the facts and statements I have adduced in connexion with this question, I cannot agree with the view that has been expressed as to the dependence of emphysema on fatty degeneration; for when we find that this condition is only an occasional and not a constant accompaniment of the disease, we cannot look upon it as its essential and predisposing cause. Nor can I, on the other hand, agree with Dr. Jenner in his view of the nature of the degeneration—at least, as regards the lobar form of emphysema. It is quite possible that in cases of chronic bronchitis there may be congestion of the pulmonary plexus, which may give rise to a weakening of the walls of the air-sacs, and that under this condition the sacs may become distended and ruptured by the mechanical act of coughing; but this will not apply to those cases where the emphysema is of the primary kind, coming on without any pre-existent affection of the bronchial tubes. In such cases the degenerative process is the first step in the disease, and any congestion which may occur is but a secondary consequence.

Although my investigations do not enable me to say what is the exact nature of the degeneration which leads to the production of emphysema, nor yet whether it commences as an affection of the capillary bloodvessels, or of the elastic fibres and basement membrane, I do not entertain the slightest doubt that *the disease in its severer forms is of a constitutional nature; that one of its most important features, and perhaps the primary step in it, is a mal-nutrition of the pulmonary tissue, causing its degeneration, and giving rise to all the structural changes I have previously described.* That fatty deposit occasionally exists, I have already stated; but the question arises whether this is a primary cause of the anatomical changes which take place, or whether it may not be the result of the imperfect nutrition which necessarily ensues in the progress of the disease.

The view I have taken of the constitutional nature of emphysema receives support from the facts which have been brought forward of its hereditary character. On this point I quote the observations of Jackson, which furnish us with very important results. He found that of twenty-eight persons affected with emphysema of the lungs, eighteen were the offspring of parents (father or mother) affected with the same disease, and that several of these had died in its course. In some instances the brothers and sisters of these persons were also emphysematous. On the other hand, of fifty persons not affected with emphysema of the lungs, three only were the offspring of emphysematous parents. M. Louis also states that the parents of more than one-half of the patients which he treated for this disease had been subject to the same affection.

Facts of this kind tend to throw great doubt on the generally received opinion of emphysema being produced by mechanical dilatation of the healthy air-sacs, and to favour the view that some deep-seated pathological cause for it exists in connexion with the lung-tissue.

In considering the pathology of a disease, we often derive material aid from observing the manner in which it is influenced by certain remedial agents; and with reference to emphysema, we may gather from this source very important indications. From a close study of the disease for some years past, and from having had an opportunity of treating a large number of cases, I am convinced that, setting aside the bronchial and asthmatic symptoms which are so constantly associated with the affection, the main principles on which emphysema should be treated are precisely those which guide us in the treatment of diseases attended with degeneration—such, for instance, as Bright's disease of the kidneys, and fatty degeneration of the heart. The principles must of course be modified in reference to certain peculiarities of emphysema; but as I hope on a future occasion to lay my views on this subject before the Profession, I shall content myself here with remarking, that in this disease I have seen so much benefit derived from a steady course of constitutional treatment, and especially the administration of some form of iron, that it has much strengthened the opinion I entertain of the pathological nature of the affection.

In conclusion, I will state briefly the circumstances connected with emphysema which induce me to believe that it is the result of some degenerative process; and although it would be more satisfactory to be able to state positively the exact nature of the degeneration, yet for all practical purposes it is sufficient to point out the general pathology of the malady, and to indicate the principles on which it should be treated.

1st. The high degree of development which the disease often reaches, without any previous history of violent or long-standing cough, either in connexion with bronchitis, whooping-cough, or any similar affection.

2nd. The frequency with which the disease attacks the whole of both lungs, and the uniformly equal character of the morbid changes often observed throughout all parts of the lungs.

3rd. The hereditary nature of the disease, as shown by the observations I have alluded to.

4th. The manner in which the disease is influenced by certain remedial measures which are known to act beneficially on other diseases attended with degeneration of tissue.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

By J. F. STREATFEILD, F.R.C.S.
Assistant-Surgeon to the Royal London Ophthalmic Hospital, and to the Eye Infirmary of University College Hospital, &c.

PART I.—PHYSIOLOGICAL MICROLOGY.

EPITHELIUM.

On the Dispute concerning the Epithelium of the Air Cells.—The author of this paper,* Dr. C. J. Eberth, of Würzburg, is opposed to the general view of the absence of epithelium in the air cells. In the first place he notices the explanations of those who think that the epithelium which has been described is an error of the observers. Thus, according to Mandl and Henle, epithelial cells which have passed from the bronchial passages into the air cells, as well as pathological granules and pus cells, and also the nuclei of the walls of the air cells, which lie with great regularity in the spaces of the capillary network, are said to have been taken for epithelium of the air cells, and sections of fine bronchial passages for air cells, and their epithelium for that of the latter. The capillaries projecting into the cavity of the air cells have been said to cause other deceptions.

He examined the lung of the calf and of the pig, inflated and dried, or partly hardened in alcohol, the vessels having been injected with size and carmine. He made sections to contain pleura and the terminal air cells, so as to give a greater number of superficial sections of the air cells, and to obviate the chance of confounding the latter with the fine air-passages. Upon the inner surface of the terminal air cells, he says, a tolerably complete delicate epithelium is to be seen, whose cells, of 0·012—0·015 mm. in size, delicate, finely granular, slightly polygonal, and having a nucleus, are separated from each other only by narrow fissure-shaped spaces. On the sections of the air-cells these epithelial cells appear flatly arched, at their bases separated only by small spaces. A confounding of the cells with projections of the vessels, or with nuclei of the wall of the air-cells and of the vessels, is not possible, because the former are but very little pronounced, and like the nuclei of the capillaries and wall of the air cells, are smaller than epithelial cells.

A fresh pig's lung, the vessels injected with size and carmine, hardened in alcohol and dried, shows, in all sections, the following characteristics. After the addition of water and alcohol, there are seen in the meshes of the vessels very pale, roundish, and polygonal cells, 0·012—0·018 mm. in size, according to the size of the capillary meshes, in a number of 1 to 3. The cells are

* Virchow's Archiv, Band xxiv. Hefte 5 und 6, p. 503.

separated from each other, as well as from the extreme limit of the walls of the vessels, by fine fissures; many partly cover the vessels, but most of them have the greater convexity of the loops of the vessels free. By employing a very strong magnifying power, the epithelial cells appear to lie half upon the vessels. In the sections of the air cells one sees the epithelial cells, especially between the projections of the capillaries, in some parts as flat arched, at others as rather spindle-shaped cells, which are easily distinguished by their delicacy and size from the smaller darkly-contoured nuclei of the walls of the vessels. Their external form here is different to that which they present in the non-injected lung, in which they appear only as flat elevations, towards within, they are like unequal-sided triangles, the larger sides of which are turned towards the cavity of the air-cell. Thus they lie wedged in between the projections of the capillaries, generally leaving their greatest convexity free.

There is generally no nucleus to be seen in the epithelial cells after the addition of water. One per cent. of acetic acid makes them visible as a dark body in each, and in the periphery of the cell a very delicate membrane.

However, very many larger air cells are also seen, which are distinguished within by a sharper uniform limitation, and in which there is to be seen nothing of either an epithelium or of more numerous projecting vessels. The tortuous course of the vessels lies beneath the surface: very few of them projecting at all. These places must be but the openings of individual air cells, whose partition walls, at their extreme sharp limitation towards the infundibula, and the very few existing short projections of the vessels, do not afford a sufficiently broad basis for the epithelial cells. According to the state of fulness of the vessels and the dilatation of the air cells, the width of the meshes and the compactness of the epithelial layer change also. From this, the facts stated concerning the epithelium of the air cells so often contradicting each other seem to be explained In lungs little expanded, non-injected, the fissures separating the individual cells are small, the cell-layer therefore appears more complete. Such preparations may have been lying before those persons who maintain the existence of a pavement formed of cells joining each other closely. Then perhaps the spaces remaining between the cells were occupied, with regard to surface views, by projections of the capillaries, which might contribute to make the epithelial covering appear yet more complete. To this may perhaps have been added, that the less projecting cells were somewhat pressed by the pressure of the glass cover, by which they flattened and spread, and made the fissures still less perceptible. In the sectional views, a complete epithelial layer might very easily be supposed from the causes before discussed.

The author then proceeds to review the opinions of some other authors. He agrees with Donders, except that he allows the capillaries to run closely beneath the cells in the fibrous layer, which is the case only in a limited degree. His paper concludes with the following sentences:—

"1. The air cells of the lung at their bases and side-walls possess a delicate interrupted epithelium, which particularly occupies the meshes of the vessels. Only the narrow free edges of the divisions of the air cells are without epithelium.

"2. The compactness of the epithelial layer is dependent on the extent of the air cells and the degree of fulness of the vessels.

"3. The projections of the vessels which are found between the epithelial cells have hitherto led to the assumption of a complete epithelium.

"4. The nuclei lying in the capillary meshes, conceived sometimes to be those of the epithelium, at others to be those of the stroma, are the nuclei of the epithelium."

GLANDULAR SYSTEM.

On the Histology of the Spleen.—Dr. L. Stieda, of Riga,* in the first place refers to the contributions of Billroth and other recent writers on the anatomy of the spleen. Concerning the parenchymatous structure, he says, that in the meshes of the framework a reticulation of fine fibres, with nuclei in the knot-points, has been represented—the "alveolar network of star-like cells" first mentioned by Förster, and compared to the framework of the lymphatic glands—more fully described and differently named by Billroth as the intervascular or cavernous network. The author says he has long sought for this structure according to Billroth's method, and in another way that shows very well the framework of the lymphatic glands—soaking fine sections in carmine, and afterwards washing them with a brush. He has, however, not been able to find with certainty *any fine connective-tissue fibrous reticulation either in the spleen of the mammals* which he has examined, *or in the human spleen.* He believes the network is a fallacy.

Of the Malpighian corpuscles he says: If we cut through a perfectly injected spleen of a mammal—e.g., of a cat—we shall be at once struck by white roundish spots of different sizes, which offer a strong contrast to the remaining coloured part. These white parts are sections of Malpighian bodies. In finer sections, with a low magnifying power one sees generally in each of these white roundish spots, in or near their centre, a bloodvessel cut across, which on a closer inspection is recognised as an artery. With a dog's spleen we sometimes succeed in obtaining sections in which these roundish spots are not completely isolated from each other, but are connected together by a more or less broad border. A more minute examination shows that in this white part, showing swellings at the sides, an artery can be followed clearly for a distance as in a sheath. In the spleen of the rabbit there appears, after a complete injection, the white mass, rarely, upon sections, with round contours, but one generally finds a not very broad white substance sending striæ in an irregular tree-shaped form from all its sides into the coloured mass. In many cases one can show, through the microscope, that this white mass encloses within itself a larger artery. In the spleens of children also one frequently sees that the white mass, in fine ramifying figures, accompanies the arteries, as Billroth also states. According to this, I believe I may maintain that the Malpighian corpuscles are only produced by mass-like depositing of cells in the loosened middle layers of the artery sheath. Vessels which I could consider to be veins, were *never* met with in the Malpighian bodies. Key lately is said to have observed a lymphatic going from a Malpighian body, but the author agrees with Billroth, who never found an afferent or efferent lymphatic. In all the different spleens which I have examined, I have never noticed anything in the Malpighian bodies that I could take to be a lymphatic. Not only according to this, but also upon the ground of the considerable difference in the structure of the lymphatic glands and of the Malpighian bodies, it appears that we may deny *without ceremony every relation of the Malpighian bodies to the lymphatics. The network seems to be the intercellular spaces of the spleen-pulp filled with injection. Between the arterial net (capillary arteries) and the beginnings of the veins (capillary veins), the connexion is maintained by the intercellular spaces of the intercellular passages of the spleen-pulp, which in injected spleens appear as an intercellular net.* The author again agrees with Billroth as to the fact of the injection passing from the capillary arteries into the capillary veins, but not that they are therefore continuous. He points out that it is after a complete, or at least partially effected, filling of the intercellular passages of the spleen-pulp that we have the transition into the open beginnings of the veins, and that Billroth is opposed to the presence of that

* Virchow's Archiv, Band xxiv. Hefte 5 und 6, p. 540.

intercellular net is most likely owing to the fact that he perhaps was examining with a very low magnifying power and too strong a light, but that net only appears clearly with a higher power—with a lower it may easily be confounded with an extravasation.

Investigations on the nature of the Spleen.—Dr. F. Schweigger-Seidel* begins his paper by referring to his essay on the subject published last year (July, 1861). He proposes to review the whole matter, and add new materials confirmatory of his former opinions. He chooses now the subject of the Malpighian bodies. His results have led him to examine the structure of the other glands and gland-like formations of the lymphatic system, because of the doubt that has been expressed of their connexion. For making preparations, to get hardening without shrinking of the tissues, he praises chromic acid. He says it has been used too strong, that it should be only of half a grain to an ounce, that some fresh acid must be added from time to time, and a little alcohol. The spleen should be fresh, or the infiltrated cells cannot be washed out. A brush may be used very carefully. Sections may be well coloured with carmine.

All the observations in this paper are comprised in the following summary. The light grey spots in the parenchyma of the spleen of some animals correspond with the filled lymph-sheaths of the arteries, in which, just as much as in the other conglobate glands, follicles may be formed. The real Malpighian corpuscles, so called, are either simply more or less globular enlargements of this very sheath, or are formed by actual follicles having been developed within the areolar tissue of the same. In a general way, therefore, the formations in relation to it might, as Joh. Müller has it, be defined as excrescences of the sheath of the arteries, or by a modern term, which is somewhat more exact, as a local *hyperplasien*, inasmuch as the follicles also are integral parts of the lymph-sheaths.

The view given here certainly does not contradict what is known of the changes in the Malpighian bodies. They are founded on the alternating conditions of the sheaths of the vessels and of the follicles. The greater or lesser number of lymph corpuscles, the more or less active transudation out of the very numerous capillaries in the follicles, the more difficult or easier carrying off of the formed cells; all these must bring about a swelling or a shrinking of the Malpighian bodies. In the other organs, also, which must be numbered with these, as in the lymphatic glands, Peyer's glands, &c., we find indeed the follicles do not always appear equally distinct, without our being able to state for this a more definite reason. It does not now appear necessary to raise the question whether the Malpighian bodies possess any special efferent lymphatics. Above all things, the Malpighian bodies must not be considered as completely closed formations, but as standing in connexion with each other. The separate globular cell accumulations are parts, connected with each other, of the conglobate gland-substance, arriving at their development in the spleen. But that this stands in connexion with the lymphatics, that the cells formed in it are carried off by the lymphatics, perhaps admits of no doubt. More precise information on the subject is certainly not yet obtained, and in favour of a direct transition of the matters out of the Malpighian bodies into the lymphatics, there can be quoted but one observation of Gerlach, who, on the occasion of the bursting of the vessel, saw the injection mass in the Malpighian bodies flowing through the deep lymphatics. Above all, the experiment must be repeated, to fill the lymphatics of the spleen in a direct way.

* Virchow's Archiv, Second Series, Band. xxiii. Hefte 5 und 6, p. 526.

Two Sorts of Tubuli Uriniferi in the Kidney.—Henle describes* in the papillæ of the kidneys two sorts of tubules: one kind which discharges on the apices, and another which curves round; the former are larger, and covered with cylindrical epithelium—the latter are smaller, and lined with small pavement epithelium cells. The loops of the numerous smaller canals are found at every distance in the height of the pyramid. At the basis of the pyramids, where the diameter of the tubules lined with cylinder epithelium is decreased, the distinctive marks become less sharp, and there remains the question whether the little cortical tubules are continuations of the small or large tubules of the medullary substance, or of both.

On the different Conditions of the Hepatic Cells in relation to the Activity of Glucogeny. By M. Colin.†—The author shows, 1st, that in herbivorous animals, as the horse, ox, sheep, &c., the fatty matters exist in large quantities in the cells and in the form of large drops. 2. That in carnivorous animals, as the dog, hedgehog, the fat of the hepatic cells is always much more divided than in the herbivoræ, and that, consequently, it is not so easily distinguished from the corpuscles with which it is mixed. 3. That in birds, whose hepatic cells are very small, and especially in fishes, the fat is to a great degree extra-cellular, and entirely free in the tissue of the organ.

On the Anatomy and Pathology of the Secreting Apparatus of the Female External Genital Organs. By C. A. Martin and H. Leger, Internes à St. Lazare.‡—The anatomical researches here detailed were undertaken with the object of obtaining a rational explanation of certain pathological facts difficult of interpretation, and in carrying them out, thirty-three vulvæ, from women of every age, from fœtal life upwards, were examined. The general conclusions arrived at were as follows: 1. That the secreting apparatus is constituted solely (with the exception of the vulvo-vaginal gland), of clustered sebaceous and some sudoriforous glands, which are to be met with at the outer or cutaneous surface of the labia majora. The sebaceous glands increase in number and diminish in size from the outer surface of the labia majora to the inner surface of the labia minora. At the edge of the latter they suddenly cease; no traces of them being found at the vestibule. 3. The muciparous glands of the vestibule of the meatus and of the urethra often described, do not seem really to exist; they have been probably confounded with mucous crypts. 4. The sebaceous glands of the labia minora do not exist in the fœtus, and only come to their full development at puberty; after the cessation of the catamenia they undergo atrophy, as also do those on the inner surface of the labia majora. During pregnancy they attain larger dimensions than at any other period of life.

The second part of this paper§ is devoted to the consideration of the effects which the different morbid affections of which the vulva may be the seat, may have upon its secreting apparatus, and is concerned mainly with vulvitis, abscesses, and cysts of the labia majora, hypertrophy of the sebaceous glands, chronic urethritis, &c.

MISCELLANEOUS.

On a New Method of Pulmonary Assaying by Means of a Magnifying-glass and of the Microscope, for the Medico-Legal Investigation of Infanticide.—Dr. E. Bouchut, of Paris, ends his paper on this subject|| with some conclusions (thirteen) to which he has been led by the numerous cases and experiments

* Medizinische Jahrbücher, Wien, Heft iii. 1862; Berichte, p. 180.

† Memoir addressed to the French Academy of Sciences; see Archives Générale de Méd., Janvier, 1862, p. 113.

‡ Archives Gén. de Méd., Jan. 1862, p. 69. § Ibid., Fév. 1862, p. 174.

|| L'Union Médicale, No. 91, p. 233.

just published by him. He says that the microscope and magnifying-glasses have not hitherto been used for determining if a lung or a portion of a lung has respired; but that a strong magnifying-glass will certainly determine the presence of pulmonary vesicles, and their comparative number when few of them have been filled with air. A magnifying-glass will show, on the exterior surface of the lungs of a new-born child, that which the naked eye cannot distinguish—the presence of air in the pulmonary vesicles, the giving way of these vesicles in disease, and their congenital impermeability if the child has made no effort to inspire. With the microscope and a sufficient magnifying power, the examination of a portion of the central part of a lung that has respired shows bubbles of air imprisoned in the tissue. The author describes the great characteristics of lungs which have not respired and which have completely and incompletely respired. As regards a minuter examination, he says, of the lungs which have completely respired, that not without a good magnifying-glass can one see the mass of air vesicles, brilliant, rounded, and transparent; in each lobule of those which have respired incompletely, he says, no air vesicle is to be made out with the magnifying-glass. In such cases, he continues, one sees sometimes the greater part of the lungs compact and impermeable, but here and there infinitely small globules, the size of a few millimetres, in which are air vesicles filled with air. After a few days putrefaction in air or water, the magnifying-glass will still show the air-vesicles of a lung which has respired. Putrefaction, which produces gas in the cellular tissue, or in the serous cavities, only produces in the lungs interlobular emphysema, never gas in the pulmonary vesicles. With a magnifying-glass one can distinguish some cases of general interlobular emphysema produced by insufflation, from the partial interlobular emphysema which the first efforts of inspiration often produce.

A New Method of Investigation of the Vitreous Body.—At the close of a communication* on this subject, Dr. E. Neumann, of Königsberg, observes:—"I chose a solution of albumen (thinned white of egg); allowed the vitreous, freed from the coats of the eye, together with the lens, to lie in it twenty-four to forty-eight hours; and then, the albumen being well absorbed, coagulated the albumen by placing it in hot water for a few minutes. By this means the soft mass, which was melting away, shapes itself into a tolerably compact, milk-white ball, which, by being placed in spirit, acquires a consistence quite sufficient for making fine sections. The granular dimmed preparations can be cleared by acetic acid and soaked in an ammoniacal solution of carmine.

"My investigations are not yet concluded, but I am convinced of the presence of cellular elements in the vitreous of grown-up animals and men."

On the Composition of the Human Urachus. By Professor Luschka.†—After reviewing the various and contrary statements regarding this structure, the author speaks of it as follows: A number of bundles of muscular tissue (*musc. detrusc. urinæ*) are described as passing from the upper part of the bladder upwards along this cord, and gradually losing themselves in a paleish yellow tissue, which for the most part composes the upper part of the structure, and which consists chiefly of elastic tissue, serving as a kind of tendon to the muscular fibre, and gradually getting thinner as it approaches the umbilicus. Much more frequently the urachus does not extend as far as the navel, but about five or six centimetres from the bladder passes into a number of tendinous threads, which unite with the left and right lateral ligaments of the bladder; but, in addition to the above structure, the original fundamental

* Virchow's Archiv, Band xxiii. Hefte 5 und 6, p. 594.

† Ibid., Band xxiii. Hefte 1 und 2, p. 1.

elements of the embryonal formation may be traced. In very many cases a tubular elongation of the upper part of the bladder, an immediate projection of the mucous membrane of the bladder may be found, the commencement of which is indicated by a minute opening passing from the cavity of the bladder; but, generally, instead of this opening we find only a depression, and this is often not to be found, so that all trace of the communication is lost. In these cases it will be found that the urachus becomes hollow a little distance from the bladder.

In the adult this hollow structure is beset by many, larger or smaller, roundish out-bulgings, which give it a knotty appearance, and a configuration much resembling an acinose gland, and which affect sometimes the entire circumference of the tubule, but more often are only lateral, having sometimes a broad base and sometimes being pedunculated. The author surmises that these out-bulgings may be separated and become cysts, which may attain considerable dimensions, and even demand surgical interference.

On making sections of the above-described hollow formations within the central ligament of the bladder (urachus), and treating it with acetic acid, and freeing it from the spongy cellular tissue, the following structures may be traced as entering into its formation:

1. A structureless, transparent basis-membrane, unaffected by acetic acid, which can only be isolated in fragments, which are apt to assume a folded appearance.

2. A fibrous layer sharply deposited from the former, and containing numerous elongated, darkly-contoured nuclei, which are dispersed irregularly in an intervening substance, partly striated and partly fibrillated. This structure is unaffected both by caustic potash and acetic acid. It is doubtful whether these oblong nuclei are elements of contractile fibre-cells; most probably they are merely the nuclei of fibrillated connective-tissue.

3. An epithelial layer. This cell-layer is of considerable thickness, the cells being of various forms—rounded, polygonal, flattened, branched, and cylindrical. The smaller termination of the latter showed traces of having been united with neighbouring cells. Many cells possessed bud-like projections.

The contents of this urachus-canal was of variable character, being chiefly pale yellow in colour and transparent, but sometimes cloudy, and of a brown or reddish colour. They have a large number of cells, such as those above described mixed up with them, also fat molecules, also accumulations of fat and corpora amylacea. Where cysts have been formed, the contents are of a pasty consistence, and contain structures very like the prostatic concretions. Cell-degenerations of various kinds—pigmentary, fatty, and amyloïd—are to be met with also.

In connexion with this subject, we may here appropriately allude to an interesting paper "On the Operation for Pervious Urachus with Stillicidium Urinæ," by Mr. Paget, of Leicester, lately published in the 'Medico-Chirurgical Transactions,' vol. xxvi. p. 13, an account of which is given at p. 86 of our previous number.

On the Polar Globules of the Ovule, and on the Method of their Formation.—By M. C. Robin.*—Under the term mucous or hyalin globule, most embryologists since Dumostier have described the appearance of a translucid globule at the sides of the embryo. When once formed it remains beneath the vitelline membrane, unaffected by the phenomena which occur around it, and it falls away along with this membrane when the ovule is expelled. The inverse spot of the surface of the yolk where these globules arise, indicates, some hours before, the "pole" of the yolk which is about to become depressed, and after-

* Memoir presented to the French Academy of Sciences: see Archives Générales de Méd., Fév. 1862, p. 241.

wards to be indented by a divisional furrow which gradually becomes equatorial; hence the term "polar globules." It is, moreover, the point where at a later period the cephalic extremity appears, and, in fact, points out the place where segmentation will begin.

The method of development of these elements is far from being explained in the same way by those who have studied it. M. Robin states that in animals in whom the yolk subdivides after the ova are deposited, the polar globules begin to form from four to six hours after this deposition—that is, from twelve to twenty-four hours after the disappearance of the germinal vesicle. The time occupied by their production is from two and a half to three and a half hours, and about two hours subsequent to their completion the segmentation commences.

The method of the origin of these globules is as follows: It is essentially marked by a veritable gemmation of the limpid material of the yolk, followed by a constriction and then a transverse division of the base of this prolongation. The phenomenon commences by the withdrawal of the granules of the vitellus to a circular portion of the surface, so as to leave the hyaline substance quite translucid. At the end of some minutes this transparent part forms a hemispherical and then a conical projection. The base becomes constricted; the constriction soon proceeds so as to cause actual separation of the projection. These globules, like the limpid projections from which they arise, are full, without any distinct parietes, and the small number of vitelline granules within show no trace of movement.

On the Identity of Hæmatoïdin and Bilifulvin. By MAX JAFFE, Cand. Med., Berlin.*—After alluding to the researches of Zencker, Brücke, Valentiner, Kühne, &c., on the same subject, this observer states that he has obtained crystals in a chloroform solution of bile, which in form and all known reactions closely agree with hæmatoïdin. He then proceeds to describe an examination of a cerebral apoplectic cicatrix, which proved that hæmatoïdin and bilifulvin were identical.

This cicatrix, which was of a yellowish-brown colour, and showed under the microscope a large number of crystals of hæmatoïdin, was dried in a water-bath and cut into small pieces. A chloroform extract was made, and was then moistened with a few drops of absolute alcohol, by which the action of the chloroform was apparently facilitated. The extract which contained the cerebral fat was of a deep yellow colour. The chloroform extract was then gently evaporated down in a watch-glass, and for twenty-four hours was placed in a darkened place (for fear of oxidation changes, which, in the case of bilifulvin solutions, take place in the sunlight, and quickly produce changes of colour), and then examined microscopically. It was found to consist of transparent, golden-yellow, beautifully formed crystals, corresponding accurately to hæmatoïdin in form.

On freeing the crystals by ether from fat, a portion of them became dissolved therein (pure ether as well as pure alcohol partially dissolves bilifulvin), the remaining crystals being soluble with tolerable facility in solution of carbonate of soda. The yellow solution became green during filtration. A small remaining portion was treated with sulphuric acid and examined, and the crystals then showed the well known colour-play of biliary colouring matter. Similar changes were observed in the chloroform solution on the addition of sulphuric acid.

From these observations on the microscopical and chemical character of the crystals, the author concludes that hæmatoïdin and bilifulvin are identical, and he points out the bearing which this statement has upon our views regarding the changes of blood-cells in the liver.

* Virchow's Archiv, 1861, Band xxiii. Hefte 1 und 2, p. 192.

Examinations of other apoplectic changes in portions of brain which had been long immersed in spirit did not disclose crystals of bilifulvin.

The fat which remained after distillation of the yellow chloroform extract assumed the shape of margaric acid crystals, and probably mechanically hindered the formation of bilifulvin crystals.

Part II.—Pathological Micrology.

Nervous System.

Cancerous Infiltration of the Mental Nerves.—Dr. E. Neumann, of Königsberg,* reports this occurrence in a case of cancer of the lip. He is led to do so by the publication of a similar case by Dr. Busch, in which the disease had involved *both* mental nerves symmetrically; they were to be felt through the lip as hard, roundish cords. In Dr. Neumann's case the tumour was at the middle of the lower lip, of the size of a walnut, and excavated. It was little sensitive to pressure; as a thin, hardish cord, it extended an inch and a half in length, from the lower edge of the tumour to the left side, where it could be followed as far as the bone at the fold of mucous membrane of the lip. It was uncertain whether or not it extended into the bone, but the tumour readily separated from the soft parts when it was removed by operation, and did not show any connexion with the bone.

The investigation of the tumour is thus described: the cancroïd infiltration had penetrated deeply into the substance of the orbicularis, its boundary being marked by a faint wavy, definite line. Microscopically, the well-known concentric collections of epidermis formed a principal component part; between them was found some remains of narrow lines of connective tissue, with exuberant connective-tissue corpuscles interspersed. The cord that has been mentioned, was found to be rather more than a line thick, solid, with irregular knotty swellings of a white colour. It was loosely embedded in quite normal muscular tissue. It could not be traced into the tumour. It was easily shown to be formed by a degenerated nerve, which, from its position, could be no other than the left mental nerve. Its bundles of fibres were spread out through clusters of epidermis-cells like those found in the lip itself; their development from the connective-tissue corpuscles of the sheath of the nerves and neurilemma was easily to be traced. The nerve-fibres themselves appeared to be normal.

Glandular System.

Misformation—Liver.†—E. Wagner has twice found in children, one two months, and the other nine days old, in the suspensory ligament, near the navel, ten very small, $\frac{1}{4}'''$ in size, roundish, or irregular, sharply-defined, greyish-white or brown-red granules, which, being microscopically investigated, were found to consist of liver cells. Microscopic gall-ducts were not found to exist, but isolated sheath-like canals were met with. The liver was normal. He considers this congenital new formation of liver-substance in the suspensory ligament of the liver similar to the cases of secondary spleens, secondary pancreas, &c.

Concerning the change of the liver cells in fatty liver, Wagner differs from all authors on the subject, in maintaining that the liver cells are not themselves affected with amyloïd degeneration, but the diminution in size and the disappearance of the cells are dependent on the pressure of the fatty degenerate capillaries, the great granulation or dimming of the liver cells; their deficiency in gall pigment is occasioned by the diminished afflux of blood from the venæ

* Virchow's Archiv, Second Series, Band xiv. Hefte 1 and 2, p. 201.
† Medizinische Jahrbücher, Wien, Heft i. 1862.

portæ and the arteries; the large fat contents is explained by the destruction of numerous cells, and the vicariation of the remainder. In the kidneys, the degeneration is very distinct, in the vessels of the Malpighian bodies, in the afferent arteries, later in the remaining vessels of the cortical and medullary substance. The epithelial cells are atrophied and fatty degenerated, the uriniferous tubules, and the proper membranes of the Malpighian bodies thickened. In the spleen, either only the Malpighian bodies (sago-spleen), or only the pulp, or both together, are degenerate; and so are, according to several authors, even the trabeculæ. The lymph glands have an analogous bearing.

CARTILAGE.

Soft Cancer involving Articular Cartilage.—Dr. Watson,* at the meeting of the Edinburgh Medico-Chirurgical Society on the 16th of April, this year, showed a "specimen of a medullary tumour of the leg, developed from the head of the tibia, for which he had successfully amputated the thigh. The tumour had commenced about two years ago in the substance of the bone, and gradually developed itself outwards, involving the head of the fibula, the interosseous ligament, and the tibialis anticus muscle. It had also latterly implicated the cartilage, encrusting the inner articulating surface of the head of the tibia. On making a section of the tumour through the cartilage, while the central part was of its natural thickness, towards the margins it was found to have undergone a transformation by which its cartilaginous character was destroyed, and fibrous tissue, containing cells exactly like those contained in other parts of the tumour, was observed to have become developed. Tracing this development from the altered into the sound cartilage by means of the microscope, the changes analogous to those which occur in the ulceration of cartilage were seen to occur—viz., the enlargement of the corpuscles, the multiplication of cell-contents and their diffusion in the bone, granular, and fibrillating hyaline substances. There was, however, this difference in the present instance, that the cells and fibrous tissues retained their cohesion and constituted a part of the medullary mass. Here and there the cartilage cut grittily under the knife, and in these parts calcification of the cartilage was found to be in progress."

MISCELLANEOUS.

On the Formation of Pus in Meningitis. By Dr. J. Klob.†—After consideration of the anatomical connexions of the arachnoïd and pia-mater of the brain and the well-known spaces in which various so-called exudations take place, Dr. Klob proceeds to describe the fluid which he obtained from the sub-arachnoïd spaces in cases of delirium tremens and injury to the brain—cases in which chronic transudations are most frequent. The serum obtained is either quite translucid or slightly turbid. On microscopical examination it is found to contain a great number of large cells, the largest being round, the smaller chiefly oblong and polygonal. Within there existed cells of the size and form of pus-cells (young tissue-elements), chiefly two or three, but sometimes as many as eight or ten. Along with these endogenous cell-formations, here and there were free nuclei of the size of the endogenous cells, with nucleus-corpuscles, and of the size and form of pavement-epithelium cells. In some of these, about the third part of the periphery corresponding to the cell-membrane was elevated by granular cell-contents, having the appearance of being surrounded by a crescentic-shaped border. On the addition of diluted acetic acid, more small round, glittering nuclei were seen in most of the endogenous cell-structures. The fluid from the sub-arachnoïd spaces, in cases of

* Edinburgh Medical Journal, June, 1862, vol. vii. p. 1162.
† Wochenblatt d. Zeitschrift d. k. k. Gesellsch. d. Aerzte zu Wien, No. 28, p. 225.

meningitis, the same formations, only to a greater extent, were met with. This is also the case as regards the exudation generally to be found in such instances in the slight sub-arachnoïdean spaces which exist around the veins traversing the surface of the brain.

The author was led, by reason of the above pathological researches, to examine carefully the cerebral arachnoïd as to the existence of epithelium on its inner surface, bearing in mind the recent observations attributing the formation of pus-corpuscles to pre-existing cells (epithelium, &c.). He has come to the determination that the arachnoïd in all those places where it is stretched over the brain-substance in a bridge-like manner (especially over deeper depressions), possesses a completely-connected epithelial investment on its inner surface as well as on the outer surface, and that this investment only fails in those parts where the arachnoïd is in contact with the pia-mater. Also in those places where spaces are formed by the protrusion of a vessel between the laminæ of the serous investment, smaller pavement-epithelium is met with. The outer surface of the pia-mater also, in the places described, is furnished with an obvious pavement-epithelial covering. In those places where the cellular elements of the connective tissue exist, which are only few and far between, the pus-formation cannot be met with.

On Miliary Tubercle.—Dr. E. Rindfleisch, of Zurich, gives the following account* of his investigation of the brain of a boy seven years old, who had died of acute hydrocephalus. A yellowish, transparent, jelly-like exudation, which had occupied the middle region of the base of the brain, had been already removed. The pia mater there was loosened, hyperæmic, and interspersed with a considerable quantity of miliary tubercle. The well-known appearance of the grey nodules which are, by preference, found in the finer ramifications of the arteries, whether placed on their coats or imbedded in them, was met with. Were it not for the thick connective-tissue of the pia mater, this would have been a suitable opportunity for studying the development of the miliary tubercle and its apparently very close relation to the coats of the small arteries, which apparently leave the connective-tissue layer of the pia mater to dip into the substance of the brain. If the tubercle formation do not really proceed from the connective-tissue as well as from the arteries of the pia mater, it is a question whether these brain vessels passing off uncovered are not also tuberculously degenerate. On cutting out in the region of the fossa of Sylvius a piece of the middle lobe of one hemisphere with the pia mater belonging to it, laying it inverted upon the point of the forefinger, and bringing it under a moderately strong jet of water, the brain substance could be washed away, so that only the vessels on the inner surface of the pia mater were left attached to it. It became evident that all the ramifications of the arteries were covered with numerous miliary nodules. They appeared as projections, singly or united into little groups on one side of the thicker little stems, whilst on the finer branches and the almost capillary vessels they presented themselves as spindle-shaped varicosities which occupied the whole circumference of the vessel. The great similarity which these formations offered to the arteries of the spleen set with Malpighian bodies was apparent; a similarity to be again reverted to later on. The nodules were mostly grey and transparent; the larger ones showed a whitish dimness, which, beginning at the centre, extended towards the periphery.

That no disturbance in the natural co-ordination of things, which is so important for the study of the process of development, might be caused, any kind of preparation was avoided, and the tuberculous vessels, just as they were, brought under the microscope. Moreover, the smaller among them,

* Virchow's Archiv, Second Series, Band xxiv. Hefte 5 and 6, p. 571.

indeed, afforded a very instructive survey for preliminary information. The changes evidently proceeded from the outer coat. That is to say, this membrane was uniformly thickened for long distances; the thickness, which could be reduced to a deposition of cells, attained in certain circumscribed places a particularly high degree, and then presented itself to the naked eye as tubercle. There was also an unmistakeable difference among the cells, and definite arrangement of these various cell-forms within the nodule. However, in order to understand the detail of their development, and, retrospectively, the necessity of that arrangement, a superficial tearing of the specimen with a cataract needle was necessary. In this way the following component parts of the miliary tubercle were isolated.

Rather large cells of a round, or often round-cornered shape, in their chief bulk; the cell contents formed of a very finely granulated, strongly refracting, probably very dense substance. The sharp contour, that is, the smooth surface, with which the cell is bounded externally, allows us to assume a cell membrane, although it cannot be demonstrated by the means usually adopted for the purpose—as by raising it through the addition of water, by crushing, or by the evidence of a double contour. There is present in most of them a round nucleus, relatively not a large one, single, very brilliant, which is generally placed eccentrically, but rarely so as to cause a protuberance on the side of the cell surface. In some two, less frequently three or more, such nuclei are found, which have, no doubt, proceeded from a division of the originally single nucleus, inasmuch as the intermediate stages of division of the nucleus—viz., the longish biscuit-shaped and yet narrowed nuclei—exist. This nucleus division is to be considered as a preparation for an endogenous development of smaller cells, extremely unlike those just described. [The paper is accompanied by illustrations of the different microscopic appearances.] They have nothing in common with the before-mentioned cells, except the brilliant darkly contoured nucleus. In every other respect they are distinguished from them, and at once, indeed, by their much smaller size, by which, in their endogenous origin, there is room in them for three or more; moreover also by this, that often a very scanty quantity of liquid contents is found between the periphery of the relatively large nucleus and that of the simply contoured cell-membrane, as clear as water, homogeneous, and of a very low refracting index. In this respect, therefore, we could not fail to notice a very characteristic bearing of the larger cells. This difference makes it possible for us to recognise the young cell within the mother cell. The appearance of bright halos round the nuclear formations of the latter is characteristic of the commencement of the endogenous development, which we may consider to be completed when the bright halo has settled down, as a sharp line, the new formed cell-membrane, from the surrounding, finely granulated, and strongly refracting substance of the mother cells.

The author alludes to the fact, that the existence of two sorts of cells in miliary tubercle has been long known, and on this point refers to Virchow; but for himself, he says he has carried somewhat farther the anatomical distinction of the two cell forms, and has shown between them a particular genetic relation. The larger he considers to be the integral elements of tubercle, to which the smaller unimportant forms bear the same relations as do the pus-corpuscles to the epithelial cells in catarrhal pus-formation. He thinks this because the elements called characteristic represent an intermediate link in the development of the miliary tubercle, and all simply lymphatic cells to be found in the miliary tubercle proceed from them; an evidence which may be adduced for, at least, the tuberculosis of the vessels of the brain, with a near approach to certainty of the facts. To proceed—the smaller observable tumours, and therefore the tubercle at the commencement, are seen to be made up only of those larger cells, and with only the more advanced growth, the smaller lymphatic-like cell forms, appear. If they do appear, this occurs in the middle of the tubercle, where we should probably be inclined to assume would be the place of their first origin; the larger kind of cells is removed to the periphery, and indicates the boundary of the periodical growth of the nodule. Under these circumstances it is a near assumption that, by virtue of the endogenous formative process above described, the smaller have proceeded from the larger ones; and that this, which may be best compared to the endogenous pus formation, and may perhaps be called a "dry suppuration," represents a regular second stage in the development of the miliary tubercle. What next follows is well known to be a fatty and granular degeneration, which is called cheesy. This change also first appears in the centre, progressing from this point outwards, so that the temporary succession of changes which the miliary tubercle suffers is locally represented by concentric zones, the outermost of which is formed by the tubercle cells, the second by the lymphatic elements, the third by the cheesy transformation-product of the latter.

In answer to the question, How do the tubercle-cells themselves arise? the author observes that the outer coat of the arteries is to be considered as their place of formation. Now the outer coat of the vessels passing into the brain is a glassy, homogeneous membrane. In this he agrees with Kölliker; but to his view he makes an addition, which for his object is of the greatest interest—viz., that there are found on its inner surfaces, in an irregular arrangement, very pale, flat, round nuclei with nucleoli. They can be perceived in detached shreds of the outer coat, as well as in intact vessels. In the latter case they appear as wand-like formations imbedded between the exterior of the vessels and the muscular layer. These nuclei are not naked, but lie in a small quantity of finely-granulated substance, which is particularly aggregated in the direction of the length of the vessel. A second contour which should surround the nucleus in a narrower or wider extent, and which might be considered cell-membrane, could not be discovered even with the best instruments; on which account that finely-granulated substance is to be considered as an accumulation of protoplasma less distinctly closed towards without. A considerable increase of this protoplasma is the first act of the process in question. This is dependent on the uniform swelling of the outer coat, or rather the elevation of its glassy membrane, by swelling of an interior layer hitherto almost unobserved. Contemporary with this is a propagation of the nuclei by division, which one may follow up through all its stages. The nuclei separate from one another, and whilst in some the process of division may be repeated, others pass through peculiar changes. Instead of a disc-like shape, they assume rather a globular form, becoming smaller in consequence of it; having been very pale and finely granular before they become very brilliant and homogeneous,—in short, they assume that exterior appearance which we have dwelt upon as being characteristic of the nuclei of the tubercle cells; at the same time changes take place in the immediate neighbourhood of the nucleus. The protoplasma becomes again more strongly refracting, therefore perhaps denser, so that the nucleus appears to be surrounded by a faintly shining ball; on the boundary of this ball a line, at first indistinct and afterwards sharply defined, appears, and thus the whole formation is complete towards without; the tubercle-cell is, in its essential parts, finished. In one of the preparations representing shreds torn off the outer coat of the limit of growth of a small tubercle, in the thickened protoplasma layer one observes, on one side, ready formed elements, or elements developing themselves; on the other side, spaces which correspond with them in form and size; the cells are fallen out during the preparation. In another is shown the edge of a small vessel, in the outer coat of which are embedded several tubercle cells and nuclei also.

The formation of these cells is so scanty at first, that broad bridges of protoplasma exist between them, as one of the preparations shows. In proportion as it becomes more abundant these bridges are consumed, the newly-formed elements touch each other, and flatten themselves somewhat against each other. Then there is left but small remains of the protoplasma, which fills up that system of communicating spaces which always must remain between globular formations in apposition—a delicate cordage, in the meshes of which the tubercle-cells are deposited. One of the examples shows this cordage, out of which the cells are partly removed by washing with a brush. It consists of fine round threads, which here and there spread out into small, three-cornered membranes. Wherever this is the case, there are always two neighbouring cells, not so near to each other as to be in contact, so that the thin flattish layer of protoplasma has remained between them. Nuclei are also still found in single points of junction of the cordage. The whole, as may be seen, resembles, up to a certain point, the finer framework which is found in the lymphatic glands, so that one cannot help drawing a parallel between the tubercle tissue and the tissue of the lymphatic glands.

As to comprising in few words the anatomical signification of the lymphatic-gland apparatus, the author pronounces himself in accordance with the most recent investigations made on the subject, in that they all must be referred to the principle of the lymph sheath formations around the vessels. The outer coat of the vessels, especially of the smaller arteries, here plays an important part, inasmuch as the lymph-sheath formation may be considered directly as a fibrillation of its tissue, with deposition of lymph cells. According to the previous investigation, the history of miliary tubercle leads us back, the author observes, to the same principle. Therefore, with Virchow, he designates the miliary tubercle as a lymphoïd formation; in the same way, isolated data of its history of development, especially the origin of the fine meshwork in the interior, might serve as useful indications during the study of the development of normal lymph-sheaths; but as to that, what is peculiar to the tubercle must not be forgotten. As a chief peculiarity, the cells alluded to must be considered as a product of the lymphatic apparatus not physiologically performed.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.A., M.D.
Senior Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology at the Grosvenor-place Medical School.

I. Toxicology.

Intoxication by Sulphate of Quinine. By Dr. Gelineau.—It is well known that by virtue of certain idiosyncrasies, medicines usually harmless, produce actually poisonous effects. It is to a susceptibility of this nature that Dr. Gelineau attributes the well-marked effects to which he was a witness in May last.

A lady, thirty-two years of age, of an extremely nervous temperament and very delicate organization, wishing to be relieved from an erratic fever with which she had been inconvenienced for some days, took, on her own judgment, about seven grains of sulphate of quinine in one dose. This was about half-past five in the morning. At seven o'clock she was awakened by a violent colic and sensations of anxiety, desire of having her bowels relieved, feeling of chilliness all over her, and cold sweats. She became worse, and was put into bed again. Her face was pale, her eyes heavy and restless, the pupils dilated, her teeth shut tightly together, her limbs benumbed and inert: she did not reply to the questions addressed to her.

When hastily called to the invalid, Dr. Gelineau found a slight diminution in the symptoms as compared with the account given to him. She replied sputteringly to his questions, but she heard them; her respiration was calm, her pulse at sixty. He prescribed cold affusions of vinegar and water to the head, and warm applications to the extremities; a lemonade purgative and an injection of assafœtida and valerian when warmth returned. In an hour a decrease in the symptoms was perceptible, and she continued to improve. The next day all these symptoms had disappeared, and she only felt a slight weariness in her limbs and heaviness in her head.

The points to be remarked in this case are the colics preceding the loss of hearing, the slow appearance of these symptoms, the dilatation of the pupils, and the appearance of the illness a fortnight before it was anticipated.—*Journal de Médecine et de Chirurgie Pratiques*, Juillet, 1862.

Poisoning by Bitter Almonds, with Iodide of Iron.—M. Toscani administered, as a tonic, the syrup of iodide of iron to a child about six years old, with great success. While under this treatment, in opposition to repeated warnings, a person gave to this child two bon-bons containing essence of bitter almonds. One hour afterwards the mother gave a teaspoonful of the syrup of the iodide. At the end of another hour, intense cardialgia and faintness succeeded each other at short intervals for three hours. In a moment of rest a little soup was given which brought on vomiting, with relief. Recovery took place in five hours. The writer of the above case remarks, that all preparations containing hydrocyanic acid should be taken with great care whenever salts of iron or mercury are being administered, as they form with these poisonous compounds of extreme activity. Hence mixtures containing calomel and bitter almonds are peculiarly dangerous.—*Bullet. delle Sc. Med. di Bologna*, Mars, 1862.

Poisoning by the Root of the Common Amaranth.—Zambelle gives the following particulars in the 'Rivista Friulana.' On the 25th of February, four healthy young girls employed in the dyeing works of the Brothers Angeli, ate various quantities of the root of the amaranth, which they believed to be a carrot. In a short time they were attacked with vomiting, diarrhœa, cramp in the stomach, colic, with an intensity proportionate to the quantity they had eaten. In one case fears were entertained of the success of the treatment employed. Rum, cinnamon water, laudanum, and ether were administered, and ammoniacal and dry frictions were used externally. These remedies proved successful, and all the patients recovered.—*Révue de Thérapeutique Medico-Chirurgicale*, Juin, 1862.

Absorption of Strychnine by the Bladder.—M. Leroy describes twenty-one experiments made on rabbits, in which strychnia was introduced into the bladder in various quantities. Of these animals, twelve died and nine survived, although the dose was in some instances the same. Comparative experiments were made in which the poison was introduced by the stomach, and from these the author opines that the absorption by the bladder is as effective as by the stomach, while it might be assumed that the absorption was quicker by the vesical surface, if the diverse conditions in which the stomach is found after death, in respect to food found in it, were not taken into consideration.—*Répertoire de Pharmacie*, Avril, 1862, No. 10.

Tannin as an Antidote to Strychnine.—Professor Kurzak gives the following results of experiments made by him upon rabbits and dogs, in which tannin was administered as the antidote to strychnine. He says that when administered in proper time, tannin is an excellent antidote, and the favourable results produced by the experiments on rabbits and dogs give every hope of analogous results upon man.

It is necessary that the dose of tannin be from twenty to twenty-five times that of the strychnine which has been taken; and in cases of poisoning, it would even be prudent to make the dose of tannin much stronger still.

The powder can be immediately employed and obtained by pulverizing nut-galls, and then making an infusion of this latter substance.

An infusion of black tea may be useful when the dose of strychnine is not very great; coffee also possesses the same properties, but in a less degree than tea.

The bark of the oak, which contains 8·5 in 100 of tannic acid, may be rendered serviceable by the facility with which it can be procured; it is also employed pulverized or in decoctions similar to that of nut-galls.

The author mentions besides, acorns, the bark of the chestnut-tree and the willow, the skins of nuts, the root of tormentil, pinks, and snakeweed as rich in tannin.

It is important to avoid the use of vegetable acids whilst administering tannin, on account of the solubility in these acids of the precipitate produced by the tannin in the solutions of strychnine. It is the same with alcoholic drinks. In fact, it is important to avoid every kind of voluntary movement, and all excitement, of whatever nature it may be, these influences being capable of producing spasmodic contractions.—*Zeitschrift der k. k. Gesellschaft. der Aerzte zu Wien*, 1862.

Colour-tests for Strychnia in Presence of Morphia.—A statement has recently been making the round of the daily press, that the presence of morphia in organic mixtures would prevent the detection of strychnia if that also were present. This statement, very satisfactory to bad analysts, never obtained much credence amongst men experienced in science, but it was of sufficient importance to demand investigation. The inquiry has been taken up very ably by Dr. R. P. Thomas, who shows definitely that with moderately careful manipulation, no error should occur in detecting strychnia in the presence of morphia. He found the colour-tests for strychnia were rendered easily manifest from simple solutions of morphia and strychnia, and also when the poisons were mixed with organic materials. In the process of testing in the latter case he selected as agents three fluids: 1. Acetic acid, of specific gravity 1·011; 2. A solution of one drachm of caustic potash in a fluid ounce of water; and 3. Chloroform.

Acetic acid was chosen, because when in excess it has the property of dissolving all the ordinary salts, both of morphia and strychnia, as well as their tannates, which are generally described as being insoluble; and therefore by treating an organic mass containing these alkaloïds with this acid, we would obtain a solution of the acetates of morphia and strychnia.

The solution of caustic potassa was selected for several reasons. For instance, in neutralizing the acetic acid, it forms a soluble salt of potassa, thereby getting rid of the acid when we have done with it. It saponifies the fats of the organic materials; it decomposes their sugars, and it dissolves morphia, but does not dissolve strychnia, thus enabling us to separate one alkaloïd from the other by its agency.

Chloroform was resorted to for its solvent and volatile properties. Thus 100 parts of it, according to M. Pettinkoffer, at ordinary temperatures, dissolve 20·16 parts of strychnia, and only 0·57 parts of morphia. A fluid drachm of it, holding the strychnia in solution, will evaporate spontaneously in a few minutes if placed upon a saucer or plate.

As the solution of potassa dissolves morphia and rejects strychnia, while chloroform has the reverse property of taking up the strychnia and rejecting the morphia, it must be evident that the conjoint use of these fluids would effect an entire separation of the two alkaloïds, the morphia being held by the potassa, and the strychnia by the chloroform.

Another important practical advantage in the use of the fluids is found in their different specific gravities. The chloroform, being the heavier, sinks to the bottom of the vessel containing them, and thus a separation is easily accomplished.

The eliminating properties of the three fluids were determined in the following way.

One grain of strychnia and three grains of opium were macerated for three days in a mixture of equal measures of acetic acid and water; then filtered, and to the clear liquid equal bulks of the caustic solution and chloroform were added, and the whole well shaken together. Upon subsidence the chloroform was separated, and a part of it evaporated on a plate. The deposit thus obtained was treated with sulphuric acid and the bichromate of potassa, in the usual manner, when a fine play of test-colours resulted.—*American Journal of the Medical Sciences*, April, 1862.

Physiological Test for Strychnia in the Presence of Morphia.—Dr. Reese, of Philadelphia, arrives at a conclusion somewhat different from that of Dr. Thomas in respect to the effect of morphia on the colour-test for strychnia. He concludes "that the influence of morphia in preventing the detection of minute quantities of strychnia in the presence of an organic fluid, depends upon the relative quantity of the two alkaloïds, the strychnia not being discoverable when the morphia is in excess, and barely discoverable when in equal quantity." It seems to us that the differences existing between the conclusions of Reese and Thomas depend entirely on the different methods of testing employed by them individually; and Reese himself admits that the physiological test for strychnine is in no way impaired by the presence of morphia.

He supplies in proof of this statement the following experiments:—

Experiment 2.—A frog weighing forty grains was immersed in a solution containing one grain of strychnia and eight grains of morphia in twenty-four fluid ounces of water; it exhibited tetanic spasms in five minutes.

Experiment 3.—A frog weighing one hundred grains was immersed in a solution containing one grain of strychnia and twelve grains of morphia in forty-eight fluid ounces of water. It exhibited the usual tetanic spasms in fifteen minutes.

Experiment 4.—A frog weighing thirty-five grains was immersed in a solution containing one grain of strychnia and thirty-two grains of morphia in six pints of water. It was convulsed in twenty minutes. Another animal rather smaller was affected in five minutes.

Experiment 5.—A cat was poisoned by taking $\frac{1}{20}$th of a grain of strychnia and $\frac{1}{10}$th of a grain of morphia. The stomach, on being analysed by Staäs' process, failed to yield the colour-test, but the watery solution of the extract produced most decided tetanic convulsions in eight frogs, generally resulting in death.—*Ibid.*

Detection of Nicotine in the Viscera of a Snuff-taker.—M. Morn has examined the liver and lungs of a determined snuff-taker, who died aged seventy. The organs, cut up into little pieces, and triturated in a mortar with powdered glass, were brought into contact with distilled water acidulated with some drops of sulphuric or of oxalic acid. After several days, the liquid was filtered through paper devoid of carbonate of lime and reduced to one-third by ebullition. As it became thus concentrated, flocculi were produced and deposited. Thus reduced, it was filtered again, and pure alcohol was poured on to it and gave rise to other flocculi, which were separated by filtration. When the alcohol had been driven off by evaporation, a slight excess of pure potassa was added to the residue. After cooling, this mixture was agitated with sulphuric ether, which was decanted after some hours and evaporated in a pneumatic machine. The residue presented the irritating and acrid flavour of nicotine, and comported itself like that substance with various reagents.—*Med. Times and Gazette*, Jan. 11, 1862, *from the Presse Belge*, No. 51.

Detection of Morphia.—M. Lefort, in studying the method of testing for minute quantities of morphia in organic substances, has established, he believes, the two following facts. 1st. That when organic matters decompose iodic acid, the iodine set free is decomposed by caustic ammonia, and the mixture is entirely decolorized. 2nd. That on the contrary, morphia, which is decomposed by iodic acid with the production of a red or brown colour, acquires a much deeper coloration on the addition of ammonia. These reactions are so delicate that they indicate in a solution the presence of the $\frac{1}{10000}$th of a grain of the alkaloïd.—*Gazette Hebdomadaire*, Nov. 1, 1861.

Poisoning by Aniline, and by Nitro-Benzol.—George L——, aged sixteen, was brought to the London Hospital, on June 8th, 1861, in a state of insensibility. The general surface of the body was pallid and cold; the lips, buccal membrane, face and nails, of a deep purple colour. The pulse was slow and scarcely perceptible, and the apex beat of the heart very feeble. He had vomited several times before admission, and was then just sufficiently conscious to complain of pain and swimming in the head. He smelt strongly of coal tar. He had been found in a state of insensibility in the interior of a vat used in the manufacture of aniline, which he was engaged in scrubbing. All his clothes, which were strongly impregnated with the peculiar odour, were removed: he was placed in a warm bed, and some hot brandy and water and a dose of camphor and ether were administered. When the patient had rallied a little, his body was well washed with soap and water, to prevent any further absorption from impurities adherent to the skin. On the following day the patient was still rather blue, and complained of weakness, and his breath still smelt strongly of aniline. These symptoms gradually passed off, and in a few days he became quite well and left the hospital.

This is the first case of poisoning by aniline placed on record, and it differs in many respects from the recent case of poisoning by nitro-benzol, which occurred at the same chemical works. Through the courtesy of Mr. Fletcher, the following facts have been ascertained concerning the death of the lad from nitro-benzol. He was employed in the laboratory, and finding that a siphon did not act properly, he thoughtlessly sucked through it some of the fluid (nitro-benzol) which he wished to transfer from one vessel to another. He did not, however, attach any importance to this act until some time afterwards, a circumstance which shows that gastric uneasiness was not immediately produced.

Mr. Fletcher noticed in the morning that the lad did not look well, but, on being questioned, the boy said that he felt "quite well, only sleepy." When he went home he told his mother that he felt "as though he were drunk." It was then that he first mentioned his imprudence with the siphon. He ate very little for dinner; the stupor became gradually more profound, till it was impossible to rouse him, and he died at ten o'clock P.M., about twelve hours after swallowing the nitro-benzol.—*Med. Times and Gazette*, March 8, 1862.

Death from Venomous Caterpillars.—A curious case is recorded in 'Galignani's Messenger,' in which it is stated that death occurred from caterpillars. The report is that a boy, eight years old, of the commune of Dardilly, in taking a bird's nest from a tree, shook down upon himself a number of caterpillars, many of which adhered to his shirt, some covered his breast, and some penetrated to his arms and shoulders. After a time there was itching of the skin, followed by an eruption of red spots, swelling, fever, coma, and delirium, ending in death in a few hours. The kind of caterpillar was the *Bombyx processionea* of Réaumur. Dr. Calmell offers an experiment which shows that the emanations from these animals produce a papulous eruption on the skin when it is exposed to them. The subject is one of great interest in a physiological point of view.

Siguatera, or Fish-poison Disease.—There are six varieties of poisonous fishes already known and described—viz.: the perches, the gurnards, the flounders, the spares, the gobies, the sardines, and the globe fishes, the last including two forms—the *Diodon* and the *Tetrodon.*

Confining our attention exclusively to these poisonous fishes, we find that they are most common in the following localities—at all events, that they have been discovered in these localities more frequently than elsewhere: in the Caribbean Sea, off Brazil, New Caledonia, the Seychelles, the Chinese Sea, the Malabar coast and other parts of India.

It should be remarked, that in these poisonous fishes the digestive organs, the spawn, and the liver, are invariably most dangerous; and that there are many fishes that may be eaten with the greatest safety when those parts are avoided. Another fact worthy of notice is the age of the fishes: some are dangerous when they have arrived at maturity. The *Lethrinus mambo,* for example, can be safely eaten when very young, but afterwards is exceedingly dangerous. Some naturalists attribute the poisonous qualities to the food found in the seas frequented by certain classes of these fishes. This is true under some circumstances, as in the case of the *Meletta venenosa,* which at certain seasons of the year feeds upon a green monad which covers the sea in large quantities. Wherever this green monad is seen the *Meletta* is poisonous, but wherever it has not appeared the same fishes are eaten with the greatest safety. MM. Fonssagrives and Méricourt agree with M. de Rochas in his opinion respecting the spawn, and with him consider it as the most poisonous part. If such be the case, it could soon be determined by ascertaining whether the injurious properties of the fishes are permanent, whether in the same species adults only are poisonous in their effects, and whether there is poison in those fishes only which contain spawn. To decide these questions, comparative experiments might be made with the male and female fishes of the same species inhabiting the same streams or waters. If it were found that the latter only were injurious, the difficulty would be satisfactorily solved.

The Spanish colonists gave the name of *Siguatera* to that union of symptoms which results from the eating of poisonous fishes indigenous to hot countries. The symptoms which arise are of two kinds. Severe attacks of indigestion or gastro-enteritic poisoning; or an icy coldness and depression, accompanied with great nervous disturbance. The symptoms are the same, whether severe enough to cause death, or only to excite inconvenience or temporary derangement; they differ only, i.e., in intensity. Gastro-enteritic *Siguatera* has all the appearance of a severe attack of indigestion—viz., nausea, vomiting—first of the food, then of mucus—coldness, depression of the pulse, cramp, and diarrhœa. The nervous type of symptoms—viz., convulsion and paralysis, which characterize the process of poisoning by fishes, are not to be found in any case of metallic poisoning. They seem to arise from a combination of accidents, as if they had been produced by different vegetable poisons of narcotic and acrid character. When the *Siguatera* assumes a gastro-enteritic form, the sufferer is, in general, quickly restored to health; while the nervous symptoms leave behind them the most serious traces of debility and irregularity. These have been known to continue for eight or nine days.

As illustrating the way in which the members of crews of vessels are poisoned by the eating of poisonous fishes, the following facts from the 'Linnæan Transactions' for November, 1860, are valuable. The history of the circumstance was communicated by Mr. H. Jameson, of her Majesty's ship *Winchester*, to Sir William Burnett. The accident occurred on board the Dutch ship *Postillion*, lying in Simon's Bay, Cape of Good Hope. The *Winchester* being near, Mr. Jameson was called to render his services to the sufferers. On arrival he found that the boatswain's mate and purser's steward had been suddenly taken ill after eating a part of a well-known deleterious fish, common in Simon's Bay, and called the toad or bladder fish—the *Diodon*. They had been warned that the fish was poisonous, but were resolved to try the experiment, the boatswain declaring that the liver was not poisonous, but a great delicacy. They had partaken of dinner at twelve o'clock; immediately afterwards they partook of the fish, and scarcely ten minutes had elapsed when the boatswain became so ill that he was unable to raise himself without the greatest difficulty; his face was somewhat flushed; his eyes glistened, the pupils were rather contracted; his mouth was open; the lips were tumid and somewhat blue; the forehead covered with perspiration; the pulse weak, quick, and intermittent. The patient was extremely uneasy and in great distress, but still conscious; he complained of pain from constriction of the throat, and appeared inclined to vomit. It was with difficulty he could swallow a powder with some warm water. His state quickly assumed a paralytic form; his eyes became fixed in one direction; his breathing was difficult, and accompanied with dilatation of the nostrils; his face was pale, and covered with cold perspiration, his lips livid; his consciousness and pulse failed, and in scarcely seventeen minutes after partaking of the fish he was dead. The symptoms exhibited by the purser's steward were of a similar kind. He also died within twenty minutes of the time after he had partaken of the fish.

The quantity consumed between the two men was only the liver of one fish; the liver might have weighed about four drachms. The entire fish measured only from six to eight inches in length.

Other examples similar to the above have been recorded by Præger; in all death was rapid, but we do not stop to chronicle these, as the effects were the same as in the instances above cited.

It is worthy of note that some of the poisonous fishes are as hurtful to inferior animals as to man. Several illustrations of this fact have been collected. Dr. Collas, chief of the marine department of health at Pondicherry, had occasion to inquire into the poisonous nature of the goby, as he had been informed by the director of police there that several accidents had occurred in a native Mussulman's family of three persons, who had partaken of a dish made of some small fishes called in Talmic *Calou-oulouvé*. The head of the family also told Dr. Collas that three fowls had died soon after eating some of the same dish. A native doctor or "*mestris*" repeated this experiment of feeding fowls on the fish, and with the same result.

At eight o'clock in the morning, Dr. Collas gave to one chicken three heads, and to another four heads of these fishes; at half-past nine the symptoms began, at eleven o'clock they increased, between one and two the poisoned animals died, without convulsions, in a state of extreme prostration. In a second experiment, the bodies of these same fishes were used which the heads had been taken off. The animals suffered from the same symptoms, but less severely, and were quite well the next morning.

The livers of ten gobies were administered to one chicken, and killed it in two hours. The intestines of ten of these fishes, separated from the livers, produced the same results. The entire fishes, deprived of their livers and intestines, caused death in four hours and a half, in other experiments.—*Social Science Review*, July 19th, 1862.

Tobacco considered as the Cause of Angina Pectoris. By M. BEAU.—"There is in pathology a serious disease called angina pectoris. It comes on suddenly, with attacks which last from some minutes to an hour, and are characterized by a feeling of insupportable pain in the region of the heart, also pains extending thence all over the thorax and even to the superior members. The heart is the organ affected in angina pectoris. The painful sensation of which it is the seat sometimes completely suspends its movements of contraction, and sudden death is the result of this grave functional lesion. The causes of angina pectoris are numerous. I am going to mention one which has not yet been discussed; it is the use, or rather the abuse, of tobacco. The following demonstrate this etiological point:—

1. A gentleman of small means, sixty years of age, passed the greater part of his day in smoking. For about a month, during the night, he frequently had attacks of palpitation, with oppression, and pains about his shoulders. He left off smoking; the nightly attacks entirely disappeared, at the same time the digestive functions improved. At the end of three months he resumed the use of tobacco, and the attacks returned. He at last completely discarded tobacco, and his attacks of angina disappeared, never more to return.

2. A doctor, about fifty years old, weak and dyspeptic, spite of his fine healthy appearance, smoked cigarettes whenever his occupations allowed him to do so. For some time he suffered from palpitations, with pain and constriction of the chest, which came on under the form of an attack either in the evening or the night. One day he was with a smoking party without doing so himself, but he could not help breathing an air impregnated with tobacco-smoke; the following night an attack came on.

3. A doctor, thirty-five years of age, practising in the provinces, continually used to smoke cigarettes while making his visits and rounds. During some time he ate little and without appetite. One morning, not having eaten anything and smoking on his way to his patients, he was suddenly seized with pain in the region of the heart, with transversal constriction in the upper part of the chest. He could neither walk nor speak; the pulse was insensible, the hands cold. The attack continued half-an-hour. The patient came to Paris. He left off smoking by the advice of M. Beau, and returned into the country, promising to inform him if he had renewed attacks. M. Beau, however, never heard from him.

4. A young Spaniard, about thirty, used to smoke cigarettes continually. He had no appetite, and his digestion was out of order. One evening, while smoking, he was suddenly seized with violent pain in the chest, as if he had been squeezed by a vice; his pulse was insensible. The attack lasted ten minutes. Terrified, he consented to smoke less, and the symptoms of angina have not reappeared.

The conclusions derived from these facts, admitting that the abuse of tobacco causes in some persons symptoms of angina pectoris, are confirmed by the experience of M. Bernard, who, by introducing pure nicotine into the bodies of certain animals, has produced mortal phenomena which he considers similar to the symptoms of angina pectoris in man.

In order that angina pectoris may reveal itself in persons who use tobacco, a reunion of circumstances is necessary which is but rarely met with. 1st. The excessive use of tobacco; 2nd, a peculiar susceptibility in the individual; 3rdly, debilitating circumstances—such as grief, fatigue, a weakness in the digestive organs, &c.—which, preventing the organism from expelling the absorbed tobacco matter, permits the accumulation of these matters to such a degree that nicotine is found in sufficient quantity to produce its poisonous action on the heart."—*La Presse Médicale Belge*, Juillet, 1862.

[We have ourselves described a form of cardiac apnœa — the angina of

M. Beau—as produced by tobacco. Vide 'The Asclepiad,' Vol. I. Essay 5, 1861.—REP.]

Notes of a Case of Hydrophobia in a Soldier of H.M. 4th Bengal Infantry.—Dr. R. W. Cunningham, Assistant-Surgeon to the 4th Bengal Infantry, reports the following case.

A private, aged thirty-seven, who had two and a half years' service in India, and had previously been in the Baltic fleet in 1854, under Sir Charles Napier, of sanguine temperament and intemperate habits, had suffered several times from delirium tremens. His constitution was a good deal shattered, and he suffered frequently from dyspepsia and intermittent fever. For the last three months he had been more regular in his habits, and had enjoyed better health than during the previous nine months. On the 6th of June he was bit on the back of the hand by a small dog in the barracks; he went immediately to the hospital, and nitrate of silver was freely applied by the apothecary. The wound healed very quickly, and he thought no more of it. The same dog bit four other men about the same time, but was then under no suspicion of being affected with rabies. A few days afterwards, however, the dog became quite mad and was killed.

July 20th, at 3 P.M., the soldier came to the hospital, complaining of a choking sensation in the throat, difficulty in swallowing, and a feeling of constriction about the chest. The apothecary gave him a draught containing ammonia and camphor, and this relieved him considerably. On going to the hospital about 6 P.M., Dr. Cunningham found him lying quietly on his cot, but on entrance, he started up and stared wildly. He said that on the 18th July, he felt a sensation of numbness creeping up the right arm (it was the right hand that was bitten). On the 19th, this sensation continued, but with no symptoms of inflammation, nor was there any perceptible enlargement in the axillary glands. On the morning of the 20th, he tried to drink a bottle of gingerade, but turned from it with a feeling of loathing. At intervals during the day, he was unable to quench his thirst, from the same cause, but as yet he had no marked paroxysm. The most prominent symptoms were a *thickness* in the voice, like that of a person with enlarged tonsils, and an appearance of anxiety and excitement. The vascular system was perfectly quiet. He took a draught containing sulphuric ether, opium, ammonia, and camphor, then and also at bed-time. On the 21st at half-past 6 A.M., he felt much relieved, and had slept a little during the night, but still he was anxious and oppressed. He could not look at water without a spasmodic paroxysm being induced. He also said that he "felt like a dog." He was ordered to continue the draught every third hour, and to have a blister applied to the neck. The right hand was very tremulous, the left one much less so. At 2 P.M., he was much worse, the paroxysms being more frequent and severe. At 4 P.M., he was seen by Dr. Carden, Deputy-Inspector-General of Hospitals, who coincided in the opinion as to the nature of the case, and ordered in addition to the former draught, chloroform, ten minims to each dose. At 6 P.M., the patient said he was better, but this would seem to be merely the anæsthetic effect of the draught, for his paroxysms were quite as severe and more extended; the respiratory functions were involved, the paroxysms commencing with something like sobbing. At 9 P.M., he was much worse, the whole chest, the diaphragm, and abdominal muscles were involved. In the intervals, the breathing was tranquil, and he said he had no pain. His pulse was now scarcely perceptible at the wrist, and his heart beat quick and weak. Vomiting succeeded the paroxysm on several occasions. About 12 P.M., fits of contortions of the features appeared, resembling intense maniacal laughter. This was accompanied by profuse frothing at the mouth.

About 1 A.M. on the 22nd he died quietly, as if completely exhausted. His faculties were perfect from beginning to end, even during the fits of laughter, which were not the result of any aberration of intellect, but perfectly involuntary.

Post-mortem examination.—The surface of the body was pale, except in the neck, where there was discoloration from venous congestion, no doubt caused by the obstruction to the return of blood to the heart by the spasmodic action of the muscles of the neck. The parotid glands were perfectly healthy, not the slightest appearance of congestion being found in them. The superficial veins of the neck and those of the scalp were all distended, and the blood was quite fluid. On removing the calvarium, the dura mater was found congested; all the veins being distended with dark-coloured liquid blood. On opening the dura mater, a large quantity of muddy serum flowed out, having the appearance of containing much fibrine. In many places, the two layers of the arachnoïd were adherent, especially along both sides of the falx. The adhesions were quite soft and recent, and many flakes of coagulated fibrine floated in the lymph-like fluid in the cavity of the arachnoïd. There was also considerable effusion between the arachnoïd and pia mater, which adhered firmly to each other; but there was nothing abnormal between the pia mater and surface of the convolutions. The substance of the brain appeared perfectly healthy, with the exception of a reddish tawny spot in the substance of the pons Varolii, having somewhat of the appearance of inflammatory softening. The ventricles contained fluid similar to that contained in the arachnoïd, and the various parts forming the floor of the cavity all appeared healthy. On the lower surface of the medulla oblongata, at the origin of the seventh, eighth, and ninth pairs of nerves, the membranes were highly vascular, thickened, softened, and matted together, but the substance of the nerves themselves and of the medulla oblongata, at their exit, seemed perfectly normal. The examination was not pursued further.

Résumé.—On looking over the symptoms, there is exhibited first, a numbness in the limb stretching towards the sensorium, with a tremulousness of the limb, but with no evidence of lymphatic absorption. There is much reason, therefore, to suppose that the poison introduced by the tooth of the dog, is not at once absorbed into the circulation, but, like the syphilitic virus, lies for a time inert, all the while magnifying itself zymotically, until at length the dread disease is produced. On this hypothesis, we might with good reason expect to obviate the disease in all cases by a thorough excision of the cicatrix; more especially if we assume at the same time that the morbific influence is conveyed to the nervous centres by the nerves, as I think we have much reason for supposing, and not through the medium of the lymphatics or general circulation. Again, all the muscles partaking of the spasms, receive their nervous influence from the seventh, eighth, ninth, and phrenic nerves, or those at whose exit from the nervous centre inflammatory lesions were observed. Now the question arises, whence these phenomena? how are the spasms produced? The point resolves itself into three heads, as follow:

1. Are the musculary phenomena produced by a reflex or excito-motory process? The virus in the seat of the wound being the excitant, and the nerves of respiration and deglutition being the efferent nerves through which the motor influence is conveyed to the muscles, after passing by an afferent or sensory nerve to the spinal column. 2. Does the influence originate in the nervous centres in obedience to the stimulus of a special poison, which, at the same time, causes a psychical change in the sensorium whereby the mere mention of liquids, or, so to speak, the contact of liquids with the mind, is as effectual in producing the spasmodic phenomena, as the contact of water with the mucous membrane of the mouth? 3. In how far are the local lesions connected with the production of the phenomena? Do they occur as the consequence of the action of the poison, and prior to and causatory of the development of the convulsive symptoms? Or are they to be considered as

purely secondary, arising from the disturbed function of the parts, and determination of blood thereto by increased action?

Till these questions are answered, we cannot hope to cure the disease by any rational method, with whatever success empirical attempts (for in the present state of our knowledge on the subject all our efforts at cure are empirical) may be followed. These are the proper points to investigate, and we may fairly expect that ultimately the difficulty will reach a solution.—*Indian Annals of Medical Science*, September, 1861.

[We direct the attention of the reader specially to these observations of Dr. Cunningham; they are full of excellent suggestion.]

II. Miscellaneous.

Responsibilities of Somnambulists.—Two opinions are before us relative to the responsibility of the somnambulist. The first is supported by Hoffbaüer, Fodéré, and Muyart de Vouglans; it consists in regarding every person as guilty who commits a criminal act during his sleep of somnambulism. "The actions of somnambulists are probably the result of the ideas and meditations of the evening." Fodéré has even passed the following severe judgment:—"He (the somnambulist) whose conscience is always in conformity with his social duties, never belies himself when he is alone with his own soul; he, on the contrary, who only thinks of crime, falsehood, and vengeance, displays during sleep the bent of his depraved inclination, which the presence of exterior objects had kept enchained during the evening. *Far from considering these acts as delirium, I regard them as the most independent which can exist in the human life.* I look upon somnambulism as a crucible in which thoughts and intentions disconnect themselves absolutely from the mass of matter." Undoubtedly, therefore, the impenetrable secret of the labours of the mind during sleep would not find favour with these rigid appreciators. Their inhuman theory, in truth, seems to be inspired by the behaviour of one of the Cæsars under a circumstance worthy of narration. A Roman citizen dreamt he was killing the emperor. "If thou hadst not thought of assassinating me during the day," said the implacable monarch to him, "thou wouldst not have dreamed it during the night;" and he delivered up to punishment the inoffensive victim of the mysteries of sleep.

The second opinion, that which is most generally received, tends to consider the somnambulist as being in possession of a will too uncertain, too fragile, to hold him subject to penal laws; in fact, *dormiens furioso æquiparetur*.

Upon what foundation may criminality reasonably be based? Upon a dream, regarded rightly or wrongly, as the reflective mirror of the evening's preoccupations. But has a guilty thought never crossed the brain of an honest man? How go back to a vague project which we are assured of having nourished, when sleep recals these intimate impressions of the soul, and bares them to your tardy examination? As MM. Chauveau (Adolphe) and Faustin-Hélie have also so justly remarked:—"By what ladder of presumption arrive at the punishment of a presumed intention?"

Somnambulism may be simulated for the purpose—

1st. Of accomplishing an act which would be difficult or impossible to execute during the evening.

2ndly. To relieve oneself from the just punishment of a reprehensible or injurious action.

3rdly. To excite commiseration and obtain assistance by fraudulent means.

Falsehood and cunning are not long in being unmasked; imitators generally take to it very badly, and scarcely know the first elements of the game they vainly endeavour to play. Besides, the possibility of simulation must always be present to the mind of the expert. The fear of fraud will always guard him from a hasty judgment, and from falling into a snare. These kinds of misadventures are to be regretted for the honour of the profession, as they compromise the knowledge, the character, and the dignity of the medical man, whose good faith has been startled and whose religion has been misguided.—*M. le Dr. Legrand du Saulle: Annales d'Hygiène Publique*, July, 1862.

To the following Works and Papers on Toxicology and Hygiene we have not space for more than allusion.

Traité Pratique de Médecine Légale. Par J. L. Casper. Translated from the German by Gustave Germer Baillière.

Influence of Railways on Public Health. By M. Gallard, M.D.—*Gazette Médicale de Paris*, June 7th, 1862. No. 23.

On the Injurious Influences of Marriages of Consanguinity. By Professor Duvay.—Duvay quotes many instances which came under his own observation, where various physical defects in the offspring followed on marriages of consanguinity. He adds statistics relative to the numbers of the deaf and dumb in France.

On the same subject. By M. Boudin.—*Annales d'Hygiène Publique*, 1862, and *Recueil de Mém. de Méd. Militaire*, March, 1862.

On the same subject. By Dr. Mitchell.—*Edinburgh Medical Journal* for March, 1862.

Hydrophobia, Case of, Two Months after Incubation of the Poison. By E. E. Lloyd.—*Madras Quarterly Journal of Medical Science*, July, 1862. No. IX.

Snakes of the Madras Presidency. By Captain Beddome.—*Ibid.*

Homicide in the Civil Medical Practice of India. By J. Stewart, M.D.—*Ibid.*, April, 1862. No. VIII.

Medico-Legal Studies on Hallucinations and Illusions. By A. Brierre de Boismont.—*Annales d'Hygiène Publique*, July, 1861.

On the Poisonous Effects of Coal-Gas on the Animal System. By Dr. Aldis.—*Medical Times and Gazette*, Feb. 1st, 1862, and March 29th, 1862.

Suggestions for Disinfecting Sewage. By G. Norton, Assistant-Professor of Chemistry, Madras.—*Madras Quarterly Journal of Medical Science*, April, 1862.

Suggestion of a New Treatment for Hydrophobia. By M. J. Nicole, M.D.—Dr. Nicole's new treatment consists in inserting the point of a syringe into a wound caused by a bite, and of exhausting by this means, in lieu of using the cupping-glass or other form of suction.—*Revue de Thérapeutique*, Aug. 1862. No. 16.

Case of Poisoning by Opium, in which Belladonna was successfully used as an Antidote. By W. S. Duncan, M.D.—*American Journal of the Medical Sciences*, July, 1862.

Case of Poisoning by Opium, in which Belladonna was used as an Antidote. By Dr. James Blake.—In this case the result was fatal; but Dr. Blake believes that the belladonna exerted its specific influence over the symptoms; that it caused return of sensibility at each dose, and would, he believes, have produced a cure, had there not been a copious secretion in the bronchial tubes, arising from a pre-existent pneumonia.—*Ibid.*

Studies on Infanticide. By Dr. A. Toulmouche.—This paper is devoted to the consideration of cases of infanticide caused by asphyxia.—*Annales d'Hygiène Publique*, Juillet, 1862.

Infanticide: its Law, Prevalence, Prevention, and History. By William Burke Ryan, M.D. *Separate Volume.*—This work by Dr. Ryan includes the Fothergillian Prize Essay for 1856. It will repay perusal, and will be noticed in our January number.

On the Poisonous Plants of the Fiji Islands. By Dr. Seeman. *A Parliamentary Report on the Fiji Islands.*—The report of Dr. Seeman was drawn up at the instance of Colonel Smythe, who inspected the islands for the Government. The article cited above is full of interest to the toxicologist, but is too long for us to print and too important for us to compress.

On the Crystalline Principles of Opium. By R. Haines, M.D.—*Transactions of the Medical and Physical Society of Bombay for* 1861.

Action of Aconite on the Animal Economy. By MM. Liégeois and Ernest Hottot.—*Journal de Physiologie*, No. 16, 1862.

Notice upon the Sanitary Condition of Military Hospitals. By M. Hte. Baron Larrey. *Monograph.*—*Paris*, 1862.

Homicidal Mania. By D. Yellowlees, M.D.—*Edinburgh Medical Journal*, August, 1862.

On Suicide. By J. N. Radcliffe.—*Social Science Review*, Nos. 13, 14, 15, September, 1862.

Statistical Studies in Pathological Geography. By Dr. Bertillon.—*Annales d'Hygiène Publique*, Juillet, 1862.

Fourth Report of the Medical Officer of the Privy Council.—*Blue Book*, 1862.

Gheel. A Study upon the Best Mode of Treating Mental Diseases. By M. Jules Duval. *Separate Volume.*—*Paris*, 1860.—A most interesting account of Gheel, tending to show the advantages which have accrued from the humane method of treating the insane adopted in that colony.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On the Treatment of Burns.* By Dr. Ashhurst. (American Journal of Medical Science, July, p. 81.)

Dr. Ashhurst, speaking from a large experience at the Pennsylvania Hospital, observes that we should not be in too great haste to dress bad burns, the constitutional treatment of the shock being the most pressing indication. The patient should be well covered up in bed, a stimulus and anodyne, as an ounce of brandy and sixty drops of laudanum, being at once given him. The patient will rapidly perish if reaction be not secured, while, when this is obtained, he will bear the exposure necessary for dressing the wound. Brandy and opium are the means most to be relied on, and the former is best given in the form of milk punch. If the patient be already drunk when brought in, we should employ external stimuli, and give five grains of carbonate of ammonia, made into an emulsion with gum and sugar, every half-hour. The amount of stimuli borne after burns is surprising, for Dr. Ashhurst states that he has given for weeks together a pint of brandy in the twenty-four hours to delicate women and sober men. He also generally gives as much as half a grain of sulphate of morphia every six hours. The prognosis much depends upon the size of the burn. Thus, if half of the surface be involved, no matter how superficial the burn may be, or how good the patient's condition, he will almost surely die; and even if only a third of the surface be burned, if this be the trunk, the same result almost necessarily follows. In no case should recovery be predicted with certainty, for burns are not only the most mortal, but the most deceptive injuries the surgeon is called upon to treat.

Only a portion of the surface should be dressed at a time; and Dr. Ashhurst prefers proceeding methodically, commencing with the arms, proceeding to the trunk, and finishing with the face. The dressings cannot be applied so closely to this last, and are apt to fall off while turning the body to attend to other parts, if they have been put on at first. Linseed-oil and lime-water Dr. Ashhurst regards as by far the most convenient and soothing primary dressing. If linseed-oil cannot be obtained, good lard-oil will answer nearly as well. The oil forms a bland, soothing, not easily evaporating coating, while the lime-water is unirritating, and yet furnishes enough stimulus to save what can be saved and hasten the removal of parts already dead. The best mode of applying it is to soak in the mixture pieces (not more than eight inches square) of patent lint, flannel, or even old rags, and after adjusting them, to cover over with oiled silk and bandage. Plentiful diet should be allowed, consisting of soups, eggs, chicken, &c. The bowels, constipated at first, may require enemata; but at a later period diarrhœa, as well as retention of urine, have to be guarded against. Extreme thirst is an universal accompaniment of burns and scalds, and its injudicious gratification leads to sickness. It is best met by allowing small pieces of ice to be held in the mouth, or giving small quantities of carbonic acid and water, not more than a mouthful of water to be drunk at a time. The burns should be dressed as seldom as possible, and not until the discharges have penetrated through; simple cerate when the sloughs have separated. In washing burns, the water should be of a higher temperature than the room, which itself should be rather warm. The raw surfaces must not be touched with the sponge; while the new skin, that it should exercise its functions, should be cleanly wiped and not covered over with the dressings.

The following are Dr. Ashhurst's general conclusions:—1. We must look, *first*, to the general condition of the patient, and only secondarily to the local mischief done; and while many topical applications answer equally well, the constitutional treatment is all-important. Many a life has been lost by enough food not having been given, and by brandy and opium being withheld, the stomach being deluged with floods of cold water. 2. A burn is essentially a disease of depression, not of excitement; even the violent delirium is but simulative of excitation—the remedies being food, brandy, and opium. 3. There is nothing to be eliminated, no "fire to be drawn out." All that we have to do is to strengthen nature, protect her from the assaults of cold air and other enemies from without, keep the functions regular, and meet complications as they arise. 4. Although we know that a large proportion of our cases will certainly die, we must not give up any one until *in articulo mortis*. But we should never give a positively favourable prognosis in any case, however slight it may appear, until at least the first and second stages have passed. 5. In no cases is decision more called for than in these; the patient may die while the vacillating surgeon is making up his mind what to do.

II. *On the Employment of the Metallic Suture in the Operation for Hare-Lip.* By M. Oscar Ansiaux. (Presse Médicale Belge, No. 25.)

In this paper the author relates some cases of operation performed by his father, Professor Ansiaux, of Liege, in order to exhibit the superiority of the silver suture over the twisted suture in the treatment of hare-lip. He usually unites the pared edges by means of two or three points of suture, adding another at the lowest extremity of the wound, in order to obviate the notch which often persists after the operation. The sutures, in the cases cited, were removed at the end of from the eighth to the thirteenth day, leaving the line of union complete. The higher the lip, the greater the number of sutures is to be employed; and by their aid a complete approximation of the entire surfaces may be effected, avoiding the partial tractions and consequent sections consequent upon the twisted suture. It is essential, however, that the sutures traverse the entire substance of the lip. There is a great difference in the effect upon the tissues produced by the pins of the twisted suture and the silver sutures. The former are rigid and voluminous, while the latter are flexible and thin; and the ulceration, and even gangrene, sometimes consequent on the use of the one, are not met with after the employment of the other. The advantages of the silver suture are, in fact, 1, its prompt and easy application; 2, the exact and continuous approximation of the surfaces is attainable; 3, ulceration or section of the tissues is not to be feared; 4, it may be retained as long a time as may be required to secure complete union; 5, it allows of the performance of the operation on infants after the first fortnight.

III. *On the Use of Iodine Injections in Large Acute Abscesses.* By M. Cosmao-Dumenez. (Bulletin de Thérapeutique, tome lxii., p. 545.)

The author of this paper having had the opportunity of observing, under M. Demarquay, the great utility of iodine in the treatment of acute inflammations and of large abscesses, which ordinarily are so tedious in their course, is desirous of calling attention to the subject. As examples, he relates two cases of deep-seated abscesses—the one in the region of the groin, and the other in that of the buttock—in which a few iodine injections proved rapidly curative, notwithstanding that the amount of pus discharged on opening the abscesses had been very large. Most of the cases observed by the author under M. Demarquay were abscesses, accompanied by much detachment of skin, in the groin, axilla, popliteal space, &c., and examples of phlegmonous erysipelas. The tediousness of the ordinary modes of treatment, even aided by compression, and the frequency with which formidable accidents arise, are but too well known; while the iodine expedites the cure, and obviates these various inconveniences. By its aid, according to M. Monod, suppurative is converted into adhesive inflammation, plastic lymph taking the place of pus. Moreover, a portion of the iodine injected becomes absorbed into the system, as manifested by its appearance in the secretions, and may influence the deteriorated constitution beneficially. The formula employed by MM. Monod and Demarquay is: water, 100; alcohol, 50; iodine, 5; and iodide of potassium, 5 parts, all by weight. When there is great sensitiveness, this may be diluted by one-fourth or one-half of water. As the injection should be made to penetrate into all the sinuosities of the purulent cavity, a syringe strong enough to throw it with force must be employed; and M. Demarquay annexes to the mouth of the syringe a gutta percha catheter. The cavity should be first cleansed out by means of tepid water, the iodine being injected as soon as this has been gently pressed out. The catheter is then to be removed, and any of the iodine allowed to run out which may do so unaided by pressure. If there are several apertures, they should all be injected—unless, indeed, the iodine entering by one runs out by the others. At the end of forty-eight hours, unless the pus has changed in nature and quantity, the injection must be repeated; and M. Monod states that he has several times seen this mode of treatment cut short those troublesome suppurations which gradually invade almost an entire breast—a disease thus becoming terminated in ten or fifteen days which otherwise might have continued for months.

IV. *On Lithotripsy in Children.* By M. JOBERT. (Comptes Rendus, July, p. 158.)

M. Jobert, in a paper read at the Académie des Sciences, observes that the controversy with respect to the application of this operation to children has not yet terminated; but, at all events, the possibility of its employment in children from five to eight years of age, formerly contested, has been necessarily admitted. For his own part, for several years he has been in favour of the procedure in these subjects, in whom stricture, enlarged prostate, diseased conditions of the bladder, &c., which complicate stone in the adult, are not present. Serious obstacles exist in the irritability and indocility of these young subjects, but these may be generally overcome. M. Jobert has never had to dilate the urethra, and the urine has never been sanguinolent—a result due to the absence of great vascularity of the neck of the bladder and the prostate; and he has been much struck with the facility with which the manœuvres are operated, when the head and trunk are lowered and the bladder injected. The operations, too, are not followed by inflammation. Although the voluntary and involuntary muscular contractions, movements of the pelvis, and closure of the thighs, which cause much difficulty in the operation, may usually be triumphed over by the aid of calmness and prudence, yet M. Jobert is of opinion that chloroform becomes a valuable agent under these circumstances, as it does also when severe suffering is produced by the presence of fragments in the urethra. It enables us also to sooner repeat the operation, and to prolong it sufficiently to reduce the calculus into powder. When, in spite of all precaution, a large fragment becomes entangled in the urethra, the urethral lithotrite is found a very useful instrument.

V. *On the Utility and Superiority of Metallic Sutures.* By M. OLLIER. (Gazette Hebdomadaire, Nos. 9, 12, 17, and 23.)

M. Ollier, Surgeon to the Hôtel Dieu, Lyon, terminates a series of papers with the following conclusions:—1. Metallic sutures are less irritating than those of vegetable or animal origin; they divide the tissues less rapidly, are sooner and longer tolerated, occasion less suppuration in their track, and leave less apparent cicatrices. 2. It is not only on the results of a great number of operations in which we have employed these ligatures that we base our statement of their superiority, but also upon comparative experiments rendered as rigorously exact as possible; the advantages possessed by the metallic sutures of the same size over the organic become still more striking when the very delicate metallic threads, which we call capillary, are employed. 3. The more delicate the thread is, the less it irritates and divides the tissues, this division being the result of ulceration, and not a mechanical action; in order that this advantage be realized, the flaps must not be submitted to too violent traction, for pressure being then exerted on an excessively narrow line, the thread acts in some sense as a cutting instrument: to prevent this inconvenience, the number of sutures must be multiplied, in order to distribute the resistance over a great number of points. 4. The superiority of the metallic threads consists in the following circumstances:—(1), their delicacy, for we may give them the fineness of a hair, and yet preserve sufficient resistance; (2), the constancy of their volume, while organic threads notably increase in this through imbibition of the discharges; (3), the polish of their surfaces and their impenetrability by putrefiable fluids; and (4), the fixity with which they maintain the edges of the wound in contact, while the organic sutures become relaxed and float in their track when ulceration has commenced. 5. Of the various metals from which sutures may be made, iron is the most suitable, by reason of its greater tenacity and the facility with which it may be procured by covering it with an unoxidizable metal, all the advantages are conferred on iron which appertain to other metals, which it might seem desirable to substitute for it by reason of their resistance to the reaction of organic liquids. 6. For autoplastic operations, iron threads of a greater fineness than have hitherto been employed are very suitable; of the delicacy of a hair, they still possess sufficient resistance to allow of their being manipulated with safety and convenience, while so slight is the irritation which they give rise to (being, so to say, forgotten by the tissues), that they are often tolerated without giving rise to suppuration; they may be multiplied without inconvenience, and they may generally be employed without covering them with gold or tin; when they are intended to remain long within the tissues the iron should be galvanized, but in no case has their oxidation hitherto given rise to any serious inconvenience. 7. Metallic should, then, replace organic threads in all kinds of sutures; when they are fine, they are very easily passed through the tissues, and can be fixed by a greater number of procedures than the organic threads; their removal from amidst the tissues in deep-seated regions (as the vagina, velum of the palate, &c.), is the sole difficulty contingent on their employment, but this inconvenience cannot be considered as counterbalancing their advantages; the capillary threads are the only ones which are supple enough to admit of being easily removed. 8. It is a useful practice to use sutures of different sizes for different parts of the same wound; capillary threads are of great utility as "perfectioning sutures" in autoplasty, when it is our object to obtain a perfectly exact union; for some operations large threads are required, as "sustaining sutures," to bring and keep together the base of the flaps, the edges of which are maintained in contact by capillary threads. 9. Metallic sutures may be left longer within the tissues, and they thus become a precious resource in wounds which, uniting slowly, require that their edges should be kept a long time in contact. 10. They may be advantageously employed as setons in small abscesses of the neck and face, when we wish to avoid producing visible cicatrices. We may also make use of them for the ligature of bloodvessels, and they are especially adapted for the operation for varicocele, allowing of the gradual division of the venous agglomeration by a very simple procedure.

VI. *On the Operation for Ranula.* By Professor Deroubaix. (Presse Médicale Belge, No. 25.)

Professor Deroubaix, having found that the various operations for ranula hitherto practised are objectionable from one cause or another, recommends the following procedure. He first makes an incision into the ranula, near the posterior surface of the jaw, and divides it through a considerable portion of its extent. He next seizes the posterior lip of the wound with a tooth-forceps, and excises, by means of concave scissors, a portion about two centimetres in length and three in breadth. The fluid now having become discharged, the loss of substance consequent on the incisions appears as an elongated oval orifice, having towards the tongue and the jaw an edge of the cyst, which, though of small extent, suffices for the introduction of the suture needle. A silver ring (possessed of sufficient resistance not to yield to the pressure of the tongue, but flexible enough to allow of its being fashioned into the form of the orifice and to lodge conveniently behind the jaw) is then introduced into the wound, its circumference being retained in immediate contact with the edges of the orifice by means of metallic sutures. To prevent the possibility of the ring being swallowed during sleep towards the end of the treatment, it is attached to a tooth by means of a metallic thread. After the operation, the interior of the sac is painted with tincture of iodine—a procedure which is repeated every few days. In this way the lips of the wound are effectually held apart while the cavity of the sac is filled up with granulations. In the case cited in illustration, M. Deroubaix removed the ring at the end of the eleventh day, the granulations having by that time passed beyond its level.

VII. *On the Substitution of Atrophy of the Eye for Extirpation.* By Dr. Larghi. (Gazetta Medica Italiana, 1862, Nos. 3 and 4.)

Dr. Larghi calls attention to the desirableness of substituting a less serious operation for extirpation of the eye in those cases in which the organ after failure of depression becomes small, softened, and incapable of fulfilling its functions. To this becomes added pain of intolerable severity, so that the patient, unable to obtain any relief from his sufferings, demands the removal of the organ with importunity. However advantageously the operation of removal of the eye may have been modified of late years, it is still to be deprecated as unnecessary. The tendency of nature's efforts in such cases is the production of atrophy of the organ, the repeated recurrence of choroida-retinitis leading to its slow and complete wasting. The process of atrophy once accomplished, all pain ceases; and what nature thus tends to accomplish slowly and amidst great suffering, may by artificial means be much more rapidly accomplished. An artificial eye is much more effectually adapted —all the motor muscles remaining intact—than after extraction of the diseased organ. The plan which Dr. Larghi proposes for effecting this object, and in one case has carried out with advantage, is to make an incision into the cornea or sclerotica, and cauterize the interior of the eye by means of a stick of nitrate of silver.

VIII. *On Accidents after Catheterism.* By MM. Sédillot and Mercier. (Comptes Rendus, tome liii., Nos. 19 and 21.)

Many surgeons have described serious or even fatal accidents as occasionally supervening upon catheterism, or other slight operations practised upon the urethra. In M. Sédillot's opinion, such accidents only occur when there has been some erosion or slight laceration of the canal; and, at all events, in the course of his extensive practice, he has never known the easy and painless introduction of an instrument to be followed by morbid manifestations of a general or constitutional character. He believes, indeed, that the true cause of such accidents is urinary absorption, their gravity being proportionate to the quantity and more or less virulent quality of the urine. He has arrived at this opinion by observing the results of the experimental injection of urine into the blood, and of urinary infiltrations consequent upon operations. In these last, the symptoms produced may surpass in intensity and rapidity of course those due to purulent infection, or they may be milder, and result in recovery; the most common complication being more or less violent paroxysms of fever following speedily upon catheterism, when this has been forced, or intra-urethral incisions. The best means of preventing these accidents, is to leave a large catheter in the bladder, having its extremity unobstructed, so that the urine may not accumulate in the bladder, and efforts at micturition, which might force it between the instrument and the walls of the canal, be prevented. After a day or two the little traumatic surfaces no longer remain susceptible of absorption, and the catheter may be removed.

M. Mercier observes, that formerly he was of opinion that urinary resorption was a necessary result of lesion of tissues when the venous system was implicated. Subsequent experience has, however, obliged him to recognise that such accidents may occur independently of any mechanical lesion, and when not a single drop of blood has flowed. He refers to a case in which urinary poisoning was the result not of resorption, but of suppression of urine, through defective elimination of excrementitial elements due to nephritis. Facts of this kind show that in certain cases M. Sédillot's plan of leaving a catheter in the bladder would be of little avail. Nor does M. Mercier deem it an useful precaution, inasmuch, although he has never adopted it, he has not, any more than M. Sédillot, met with accidents after his operations of urethrotomy.

IX. *Of the Curability of Wounds of the Brain.* By M. FLOURENS. (Comptes Rendus, July, p. 70.)

M. Flourens observes that works of surgery are filled with important cases of wounds of the brain, the symptoms varying with the portion of the organ injured, and sometimes none at all being present. Lapeyronie relates one of the most remarkable of these cases, in which the loss of brain was such that the *corpus callosum* was exposed. The extent to which loss of brain is compatible with the retention of life in animals was shown by M. Flourens' experiments made in 1822, by which he found that an animal would survive the removal of one lobe, or even of both lobes, of the cerebellum, certain functions alone becoming abolished. He has of late been continuing these researches by introducing bullets, varying in weight from one to four drachms, into various parts of the brains of dogs and rabbits—the ball, after the trepan had been applied and the dura mater and cerebrum beneath divided, being allowed to sink by its own weight until after a few days it reached the portion of the dura mater lining the base of the skull. The kind of fistula thus made by its track remained open for some time, and then healed and cicatrized. And what is most curious, is that if the ball was not very large, it would thus traverse the entire substance of the cerebrum or cerebellum without producing any symptom, accident, or functional disturbance. When the ball was too large, or several were introduced, abscesses were induced. These experiments, furnish additional proofs of the curability of wounds of the brain, and the singular facility with which this is accomplished.

X. *On the Employment of Forced Rotation outwards in the Reduction of Intra-coracoïd Dislocation of the Humerus.* By Dr. SCHINZINGER. (Prag Vierteljahrschrift, 1862, No. 2.)

In this paper Dr. Schinzinger, of Freiburg, refers to five cases in which he has found the above procedure of very easy execution, the obstacle to reduction, in fact, lying not in the tension of the muscles, but in the narrowness of the rent in the capsule. This method has been resorted to by Syme, Lacour, Dummreicher, and other surgeons, and the following is M. Schinzinger's mode of putting it into execution: An assistant stands behind the patient, who is seated on a stool, and fixes the shoulder-blade by placing his hands flat and crossed over the point of the shoulder. The surgeon, seated opposite his patient, seizes his wrist with his right hand (if the luxation is on the left side), and with his left takes hold of the elbow of the arm, which is almost bent at a right angle, and presses the shaft of the humerus as near the thorax as possible, without exerting any traction. The patient's hand is next directed outwards and backwards as far as possible, so as to bring the inner side of the upper arm opposite the surgeon. During this manœuvre the head of the bone may be plainly felt and seen leaving its abnormal position, and proceeding towards the glenoïd cavity—the surgeon, towards the end of the rotation, forcing the humerus somewhat upwards. The rotation outwards having reached its highest point (when a sound may be heard, probably due to a farther tearing of the capsule), the assistant places two fingers of his right hand against the head of the bone so as to prevent its sliding from the cavity, the surgeon at the same time completing the reduction by slowly rotating the arm inwards.

The following points may be stated in favour of this mode of procedure: 1. No preparatory means are required, and the use of chloroform, with the risk of the consequences, is avoided. 2. While by the various procedures usually put into force great exertions have to be employed to overcome muscular resistance, and assistants are required to make extension and counter-extension, in this a single assistant is required, and no traction is exerted. 3. The vessels and nerves, which during violent extension or by reason of the insertion of the knee, heel, &c., in the axilla, become contused or torn, remain during rotation quite uninjured. 4. The head moves rapidly and easily from its abnormal position towards the glenoïd cavity, and by no other means can the edges of the lacerated capsule be kept so gaping and suited to the re-entry of the head. 5. The author's five successive cases exhibited the ease and rapidity with which this procedure can be executed. Four of them had already been submitted to unavailing attempts at reduction, only one coming under his care immediately after the injury.

The following are References to Articles in the Foreign Periodicals which we have not space to digest:—

Amaurosis.—Lente on Amaurosis from Injury to Branches of the Fifth Pair. (American Journ. Med. Science, July, p. 70.)

Artificial Limbs.—Martini's Report on Artificial Limbs. (Schmidt's Jahrbücher, Band cxv. S. 105.)

Bladder.—Mercier on a Mode of Entrance of Foreign Bodies into the Bladder. (Gazette Hebdomadaire, No. 34.)

Bones.—Lorinser on Acute Inflammation of Bone. (Wien Medicin. Wochenschrift, Nos. 26 and 27.) Volkmann on Surgical Observations on the Bending and Growth of Bones. (Virchow's Archiv, Band xxiv. s. 512.)

Breast.—Neumann on Tumours of the Breast. (Virchow's Archiv, Band xxiv. S. 316.)

Carbuncle.—Feldmann on the Treatment of Furuncle and Anthrax. (Gazette Hebdomadaire, No. 29.)

Castration.—Reali on Strangulation of the Spermatic Cord as a Substitute for Castration. (Omodei's Annali, July, p. 74.)

Cleft Palate.—Passavant on Operation for Cleft Palate and for Hare Lip complicating it. (Arch. der Heilkunde, Band iii. S. 304.)

Coloboma.—Bœcumler on Coloboma Oculi. (Würzburg Medicin. Zeitschrift, Band iii. S. 72.)

Ectropion.—Alphonse Guerin on a New Plastic Operation in Ectropion. (Bulletin de Thérapeutique, tome lxiii. No. 3.)

Exostosis.—Lowe on the Pathology and Treatment of Exostosis. (Madras Quarterly Journal, April, p. 248.)

Fungus.—Bidie on Morbus Pedis Entophyticus. (Madras Quarterly Journal, April, p. 223.)

Hare-lip.—Depont on a Rare Form of Hare-lip. (Bull. de Thérapeutique, tome lxiii. Nos. 1, 2, 3.)

Hernia.—Gruber on Hernia Interna Mesogastrica. (Petersburg Med. Zeitsch., Band ii. S. 161.) Dittel on Formation of an Internal Sac in Inguinal Hernia. (Wien Wochenblatt, 1862, No. 7.)

Hydrocele.—Duval on Transparency in Hydrocele. (Gazette des Hôpitaux, No. 74.)

Intestinal Canal.—Erichsen on a Rare Form of Intestinal Obstruction. (Petersburg Med. Zeitsch., Band iii. S. 305.) Roth on Cases of Enterostenosis. Würzburg Med. Zeitsch., Band iii. S. 113.) Frohbeen on Invagination of the Large Intestine. (Petersb. Med. Zeit., Band ii. S. 193.)

Laryngotomy.—Matejovsky on Cases illustrating the Operation of Laryngotomy. (Prag. Vierteljahrschrift, Band lxxiv. S. 1.)

Periosteum.—Malachia on the Importance of Periosteum in relation to Pathology and Surgery. (Omodei's Annali di Medicina, June and July.)

Prostate.—Demarquay on Peri-prostatic Abscess. (L'Union Médicale, No. 75.)

Pupil.—Galezowski's Report on Operations for Artificial Pupil practised at Desmarres' Clinique. (Annales d'Oculistique, May.)

Syphilis.—Diday on Syphilitic Reinfection. (Archives Générales, July, p. 26.) Sigmund on Inoculation as a Means of Diagnosis in Syphilis. (Wien Med. Wochensch., No. 25.) Lindwurm on Different Forms of Venereal Disease. (Würzburg Zeits., Band iii. p. 143.)

Urethrotomy.—Reybard on Superficial Urethrotomy. (Gazette Médicale, Nos. 30, 32.)

Varicose Veins.—Nivert on Spontaneous Inflammation of Varicose Veins. (Archives Générales, August, p. 153.)

Vesico-Vaginal Fistula.—Verneuil on the Treatment of Difficult Vesico-Vaginal Fistulæ. (Bulletin Thérapeut., May and June.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.

Lecturer on Midwifery at St. Thomas's Hospital, Physician to the Royal Maternity Charity, &c.

I. The Unimpregnated Condition.

1. *A Case of Air in the Blood, associated with Uterine Carcinoma.* By Professor Oppolzer. (Zeitschr. d. k. k. Ges. d. A. zu Wien, May, 1862.)
2. *On the Vegetable Parasites of the Female Genital Organs in their Relations to Practice.* By Dr. L. Mayer. (Monatsschr. f. Geburtsk., July, 1862.)

1. Dr. Oppolzer relates a case of uterine carcinoma, in the course of which air entered the circulation. A woman, aged forty-two, was admitted into the Professor's clinical ward in April, 1862. She had borne eleven children, the last a year before. Symptoms of carcinoma appeared after the last labour; amongst these was frequent hæmorrhage. There was a complication with chronic tuberculosis of the lungs. Eleven days after admission, the patient complained of acute pains in the hypogastrium. Next morning, suddenly, acute pains attacked the skin of the right thorax. In the evening, the skin of the right side was intensely red, covered with vesicles of various sizes—some being as large as a dollar—filled with a reddish serum. The red colouring of the skin proved to be due to hæmorrhage. A subcutaneous emphysema was also formed in the epigastric region. The appearances of the skin quickly extended to the whole side as low as the crista ilii. Collapse set in, and death occurred at midnight. The autopsy was performed thirty-three hours after death. Over the breast, abdomen, and as far as the middle of the thigh, and the arms as far as the wrists, the integuments crepitated on pressure. In the pericardium were about two drachms of red fluid. The heart was flaccid; underneath the visceral layer of the pericardium were several air-bubbles; the substance of the heart was dull, easily tearable; the endocardium red from imbibition; in the left ventricle was dark blood; the right contained blood resembling raspberry jelly mixed with air-bubbles. In the pulmonary arteries were whitish clots. In the vessels of the liver was a fluid pale-red blood, mixed

with air. The spleen was enlarged, and crepitated on pressure. The kidneys were flaccid, their vessels contained frothy blood. The uterus was enlarged; the walls at the fundus flaccid, pale-red; the cervix, including the vaginal portion, and especially the posterior lip, was replaced by a soft pale-red substance, giving issue to a turbid juice, and floating out in villi in water. In the veins of the vesical plexus were dark occluding clots. The ovaries were flaccid, the stroma œdematous; a collapsed corpus luteum was found in the left. The internal spermatic vein, as well as the inferior cava, were strongly distended; the latter gave a tympanitic resonance on percussion; both were bright red, and contained air, copiously mixed with blood. Microscopic examination showed the disease to be a villous cancer. A piece of skin from the right thorax showed that the capillary vessels contained in part air alone, in part air with fluid blood. Other capillaries were torn, and exhibited extravasated blood around them. The blood showed numerous cancer-cells, so that the whitish streaks which the blood displayed to the naked eye consisted only of agglomerations of cancer-cells. Besides this, the blood contained many air-bubbles and much fat, and lastly, colourless corpuscles in great numbers.

2. Dr. L. Mayer, in his memoir, considers the subject of vegetable parasites in connexion with uterine and vaginal diseases. He describes a species of broad fungus, which he has found in six instances in the secretions of the female genitals. These fungi spread over the inner surface of the labia, the nymphæ, clitoris, vagina, and also over the vaginal portion of the uterus. He has never seen them extend beyond these limits. There are usually observed spots in size from a pin's head or smaller, bright yellow, roundish, or irregular, generally loosely attached to the mucous membrane. More rarely, diphtheritic-like membranes are formed, which leave on removal shallow ulcers. The basis on which the fungi grow is always hyperæmic and gives increased secretion. The secretion has a mucous, opalescent, milky, or creamy character, but is sometimes of greater consistency, not unlike potato-paste. The six individuals affected with vaginal mycosis were of various ages and of different conditions of health. Five suffered from diseases of the sexual system, and one was pregnant. In all, excepting this last, the fungi were associated with more or less severe inflammation of the genital mucous membrane, and all complained as soon as the mouldiness had taken root of intense burning, itching, and pricking in the vulva and vagina, occurring paroxysmally, and destroying rest and sleep. In the sixth case there was no inflammation, and the peculiar burning and itching were absent. The troublesome symptoms disappeared with the removal of the fungi. Dr. Mayer recommends frequent injections with water for the purpose of washing away the fungi.

II. Pregnancy.

1. *The Organic Connexion of the Fallopian Tube with the Ovary.* By Dr. Panck. (St. Petersb. Med. Zeitschr., 1862; and Mon. f. Geburtsk, June, 1862.)
2. *On the Changes of Body-weight in Pregnant, Parturient, and Puerperal Women.* By Dr. Gassner. (Mon. f. Geburtsk., January, 1862.)
3. *On the Non-Shortening of the Supra- and Infra-Vaginal Portion of the Cervix Uteri.* By Dr. J. E. Taylor. (American Med. Times, June, 1862; and Amer. Journ. of Med. Science, July, 1862.)
4. *On the Extraction of the Fœtus by the Natural Passages in the Moribund or Dead Woman.* By Dr. Devilliers. (L'Union Méd., June, 1862.)

1. Dr. Panck, relying upon comparative anatomy and observations on the human subject, submits that the adhesion of the orifice of the Fallopian tube with the ovary at the time of conception, takes place through a newly-formed

membrane, which at a later period is absorbed. He examined the body of a girl who had died shortly after conception, and found a delicate new membrane, which fastened the fringes of the tube to the ovary. In subsequent researches Dr. Panck found a similar membrane so frequently as 34 times out of 58 cases, but only in women who had borne children, and only on the posterior wall of the uterus and of the ala vespertilionis. At times rudiments only could be observed; in other cases numerous strings and expansions covered the whole posterior surface of the uterus and alæ. These have extended over the anterior aspect, on the bladder or rectum. The most various degrees of adhesion, from partial attachment to complete capsular investment of the ovary, were found. The membranes were thin, delicate, translucent, and could be easily separated from the serous membrane, which always exhibited its normal appearance. Similar appearances are found in animals whose ovaries lie free, as in man.

2. Dr. Gassner availed himself of his residence in the Lying-in Hospital of Munich to institute an extended series of observations on the variations in body-weight of pregnant and lying-in women. His observations appear to have been made with every care to avoid fallacy. His memoir contains a number of tables, in which the results are detailed and classified. We can only here give some of the conclusions:

Pregnancy.—During the last three months the body increases in substance, and so remarkably that the gain cannot be explained by the growth of the ovum alone. The maternal organism shares in the increase. A diminution of weight is pathological. The increase of the body-mass during the period named is a thirteenth part of the body.

Primiparæ do not gain so much in proportion as multiparæ.

The intra-uterine retention of a dead fœtus is constantly attended by a considerable loss of the maternal body-mass. This circumstance may come in aid to diagnosis, when auscultation no longer detects sounds of fœtal heart.

Labour.—The loss of weight following labour is on an average nearly the ninth part of the body-weight of a pregnant woman who has reached the end of the tenth month. It is constituted of the expelled ovum, blood excrements voided during labour, and the lung and skin exhalations.

The weight of the entire ripe ovum, fœtus, liquor amnii, and placenta is about the 10·8 part of the body-weight of the woman in labour.

The mass of the several parts of the ovum at the end of pregnancy may be stated as follows:

The weight of the child	: to weight of the ovum	=	1 : 1·755
" " liquor amnii	: " "	=	1 : 3·070
" " placenta	: " "	=	1 : 9·600

The weight of the ovum and of its components—that is, of the child, liquor amnii, and placenta, is in proportion to the body-weight of the mother.

The mature ovum of the primipara is smaller than that of the multipara.

The quantity of the amniotic fluid increases during the latter three months of pregnancy.

In all cases where a change of position, or a *culbute* of the child, took place in utero, a disproportional increase of liquor amnii was present.

The size of the periphery of the abdomen at the end of pregnancy is in direct proportion to the sum of the body-weight of the pregnant woman. This should be borne in mind in estimating the question of twins.

Puerpery.—The loss of weight during the first eight days of the labour, occasioned by excretions, secretions, especially of the lochia and milk, increased excretion of urine, and the involution of the genitals, is on an average the twelfth part of the mother's body. This loss is the greater in proportion to the proximity of the labour to the normal term of gestation.

In primiparæ and in women who do not suckle, this loss is somewhat less than in pluriparæ and suckling women.

The loss is in direct proportion to the quantity of the body-mass of the lying-in woman.

The surprising loss of weight on the first day of childbed is due to the increased secretion of urine resulting from the resorption of the serous infiltration which took place during pregnancy, to the more copious lochial discharge, and to the scarcely ever failing sweat.

The loss of weight due to labour and childbed amounts on an average to the fifth part of the body-weight of the pregnant woman.

3. Dr. J. E. Taylor, Professor of Obstetrics in the Bellevue Hospital Medical College, submitted upwards of 150 women, at various stages of pregnancy, to examination, for the purpose of determining the condition of the cervix uteri. He concludes that the cervix does not unfold or lose itself during gestation; that it remains of its normal length, and is sometimes even elongated; that the whole cervix uteri remains intact up to the full time of pregnancy, and sometimes during the first stage of labour.

4. Dr. Devilliers, referring to the recent discussions in the Academy of Medicine upon the conditions calling for the Cæsarian section, discusses the question as to the propriety of delivering pregnant or parturient women, either dying or recently dead, by the natural passages. He considers the matter rather from a moral or ethical than from a scientific or surgical point of view. He inclines to this mode of attempting to rescue the child in preference to the Cæsarian section.

III. The Puerperal State.

1. *On Spontaneous Inflammation of Varicose Veins of the Legs in recently-delivered Women.* By M. Nivert. (Arch. Gén. de Méd., August, 1862.)
2. *A Case of Puerperal Tetanus.* By Dr. Maclaren. (Edinburgh Medical Journal, August, 1862.)
3. *Cauterization of the Uterus as a Preventive means against Puerperal Fever.* By Dr. Larghi. (Gaz. Méd. Italiana prov. Sarde, March, 1862.)
4. *Two Cases of Puerperal Pyæmia; Recovery under use of Tannin.* By Dr. Woillez. (L'Union Med., July, 1862.)

1. M. Nivert records the histories of, and gives the deductions from, several cases of inflammation of varicose veins in puerperal women, which occurred in the Maternité at Paris under Dr. Hervieux.

His general conclusions are:—that suppurative phlebitis may become encysted, and that thus the entrance of pus into the circulation may be prevented; or the suppurative inflammation may be free, and lead to purulent infection. The inflammation of the coats of the veins did not arise primarily, but was preceded by symptoms suggesting the influence of puerperal epidemic fever. Quickly following these general symptoms came lancinating pains in the legs in the course of the veins; a red line marked out the course of the varicose vein, forming a hard cord with knots at intervals. In no case was œdema observed. The cellular tissue around the vein shared in the inflammation. Severe general symptoms set in with great rapidity. Suppuration quickly took place in the venous trunks, and pus was drawn into the circulation. Once only cure resulted through the sequestration of the phlebitis. Autopsy displayed in several points in the course of the veins a red clot resembling those formed above an arterial ligature. In other points the clot, which had undergone a certain amount of transformation, was composed of concentric fibrinous layers; in the centre was a blackish mass, easily broken up by the fingers. In other points, again, the centre of the clots was composed of matter like pus. Throughout nearly the entire course of the diseased vessels was a liquid matter, seeming to be pus, in different stages of formation. Ulcerations had taken place at various points of the inner walls of the veins. The lining membrane was in places red, rough, dull, and at times covered with a pultaceous matter. The middle tunic was much thickened, and its density augmented by intimate union with the external tunic, which was thickened and infiltrated with serosity or plastic lymph. The vessel, cut transversely, gaped like an artery. There was no general œdema, as in phlegmasia alba dolens.

2. Dr. Maclaren relates an interesting case of puerperal tetanus. The patient, aged thirty-five, a spare, poorly-nourished woman, mother of five children, after bleeding for about eight days, sent for Dr. Maclaren. The vagina was filled with clots, a three-months' fœtus, and a half-detached chorion. These were removed, the passage stuffed with sponge, and a bandage applied round the abdomen. The loss of blood had been very considerable. After twenty-four hours the plugs were taken out. She took a drachm of liquor ergotæ. She did well for ten days, when she was seized with an uneasy sensation in the muscles of the neck, and some difficulty in swallowing. Trismus, and attacks of opisthotonos, followed. She took laudanum and Indian hemp, and was blistered along the upper part of the spine, a solution of morphia being applied to the blistered surface. Injections of beef-tea were given by rectum. Death occurred on the fourth day after the onset of the affection. The uterus, brain, and upper part of the spinal column were examined, but no lesion was detected.

3. Dr. Larghi, reasoning from the experience of hydrophobia and other diseases which seem capable of being prevented by cauterization, proposes to cauterize the internal surface of the uterus after labour, by a solution of nitrate of silver carried on a sponge. [This suggestion is based on the assumption that puerperal fever takes its origin from the absorption of poisonous matter from the internal surface of the uterus.—REP.]

4. Dr. Woillez relates two cases of severe puerperal pyæmia, attended with formation of abscesses, which recovered under the employment of tannin. The patients took sixty centigrammes daily in the form of pills.

IV. THE NEW-BORN CHILD.

Researches on the Conditions of Weight in a Hundred New-born Infants during the First Days after Labour. By Dr. WINCKEL. (Monatsschr. f. Geburtsk., June 1862.)

Dr. Winckel has made careful observations on the weight of infants during the first days after birth. One hundred children were subjected to accurate weighings. The following are his results:—

1. The boys weighed on the average more than the girls.
2. The umbilical cord fell off in three-fourths of the cases on the third or fourth day.
3. All the children lost weight soon after birth.
4. The maximum of this loss of weight reaches, in healthy children, an average of six ounces.
5. The duration of the loss is mostly from two to three days.
6. In mature, healthy children, nourished at the mother's breast, a gain of

weight begins immediately after the cessation of the loss, generally on the third or fourth day after birth.

7. This epoch coincides, as a rule, with the fall of the remains of the umbilical cord.

8. The maximum of the gain in weight to the tenth day included is, on an average, seven ounces and a half.

9. Most children have recovered their original weight on the tenth day.

10. The exceptions are children nourished on cow's milk, and the immature.

11. The children nourished on cow's milk mostly go on losing weight for a time, after the fall of the cord; immature children fluctuate between loss and gain.

12. Diseases of the mother and of the child entail a longer period of loss of weight, and a lesser and fluctuating increase.

The following list of papers, which are not analysed for want of space, may be referred to as of interest :—

Amputation of the Cervix Uteri. By Dr. A. K. Gardner. (Bulletin of the New York Academy of Medicine, 1862.)

Notes of Cases (three) of Puerperal Convulsions. By Walter Channing, M.D. (Boston Medical and Surgical Journal, July, 1862.)

Contribution to the Knowledge of Osteomalacia. By Dr. Gusserow. (Monatsschr. f. Geburtsk., July, 1862.)

Fistula Vesico-Vaginalis: great deficiency in the base of the bladder; protrusion of the right ureter into the fissure; operation; death through infiltration of urine, in consequence of the opening of the free cellular-tissue. By Dr. Hegar. (Op. supra cit.)

On Corpse-deliveries: Observations of several Members of the Berlin Obstetrical Society, collected by Dr. E. A. Meissner. (Ibid.)

Cæsarian Section after Death; a Living Child. By Professor Breslau. (Ibid.)

On Puerperal Diffuse Metritis and Parametritis. By Rud. Virchow. (Virchow's Archiv, 1862.)

A Contribution to the Statistics of the Cæsarian Section, with an Appendix on Osteomalacia. By Dr. Pagenstecher. (Monatsschr. f. Geburtsk., Jan., 1862.)

Obliquely-contracted Pelvis with Sacro-iliac Ankylosis, with Remarks on Simon Thomas's View of this Deformity. By Dr. R. Olshausen. (M. f. G., March, 1862.

On the New Method of inducing Premature Labour at a Pre-determined Hour. By Robert Barnes, M.D. (Edin. Med. Journ., July, 1862.)

A Case of Extra-Uterine Pregnancy. (In this case the bones of the fœtus were brought away through the rectum. The patient made a good recovery. By Dr. Day. (Australian Medical Journal, Jan., 1862.)

MEDICAL INTELLIGENCE.

The late Dr. M'William.

THE disappearance of a man like the late Dr. M'William from among us is too serious a loss to the profession to be passed over without notice in a journal which had often been graced by contributions from his pen. We have seldom, if ever, known of the death of a medical man which has been more widely and generally regretted than that of him who, a few months ago, was so unexpectedly called away in the midst of apparently robust health, and in the busy discharge of most useful and honourable duties. Nor can we wonder at this when his character and the tenor of his professional career are considered. A thorough conscientiousness of purpose, and a resolute, untiring energy of action, were the moving springs of his conduct through life. What he felt to be right he took up heartily, and carried through with unfaltering spirit. As surgeon of a ship, he soon distinguished himself by zeal and assiduity in the care of the sick, and won the Blane medal for the fulness and accuracy of his reports to the Admiralty. Scrupulously obedient to all existing regulations of the service, he yet from an early period set himself to the correction of a great injustice to which he, as all other assistant-surgeons in the navy, had to submit—that of having no separate cabin, and of being associated with the midshipmen instead of, as ought always to have been the case, with the officers of the gun-room. This old, but not the less absurd, custom had been too long established in the service to be got rid of without a hard and wearisome struggle against official indifference to all such matters, and the stolid prejudices of too many crotchety admirals and post-captains, who too often stand up for things as they are. It required the persevering efforts of the friends of the much-needed change to be continued for a good many years before their object was attained; and that no one worked harder or more efficiently in the cause than Dr. M'William, was shown by the handsome testimonial which his brother officers presented to him in acknowledgment of his services.

The sailing of the ill-fated expedition to the Niger in 1843—from which so much was anticipated in promoting the civilization of the interior of Africa—may be regarded as the occasion of the tide in the affairs of his life which, taken at the full, led on to eminence and renown. Unsuccessful in the object proposed, and unfortunate for most of those embarked in this mission of philanthropy, it was not so for the medical officer, as it gave him the opportunity of displaying that heroic constancy and unflinching devotedness to duty in circumstances of great difficulty, which won for him a name among the worthies of the medical profession. The steering of the disabled ship some hundreds of miles down the river to the coast, when almost all the crew were down with fever, will always be associated with the name of M'William.

A year or two subsequently, the sad story of H.M.S. *Eclair*, returning to our shores from the coast of Africa, with the loss of nearly one-half of her crew from yellow fever, again afforded him a fitting opportunity for the display of those qualities of mind and character which had so markedly distinguished his career hitherto, and which were now as conspicuously shown in his investigation of the pestilential fever at Boa Vista, one of the Cape de Verd islands, where the *Eclair* had so terribly suffered.

In 1847, he was appointed by Lord John Russell, then Prime Minister, in reward of his many services, Medical Inspector of the Customs, and this office he continued to hold up to the period of his decease. How ably and usefully he performed its duties has been, on more than one occasion, attested by the public expression of grateful approval on the part of those with whom he was more immediately associated; and the admirable statistical reports which he prepared from year to year of the health of the men committed to his care must be well known to all who take an interest in such inquiries. Within the last two years his attention had been specially drawn to a subject of public hygiene of the highest importance, but which has hitherto engaged far too little notice—the health of our mercantile marine. His first paper on the subject, printed in the last volume of the 'Transactions of the Association for the Promotion of Social Science,' excited great interest both in and out of the profession, and it is earnestly to be hoped that this most useful inquiry, which he initiated, will not be allowed to drop until some salutary reforms have been effected for the better preserving the health of our merchant seamen, and for preventing much of the sickness and death now prevalent among them.

As the indefatigable Secretary of the Epidemiological Society since its institution twelve years ago, he probably did more to promote the study of epidemics than any other man in this country, save only its distinguished President. He was one of the few who early recognised the importance to science of having a regular periodic registration of this class of diseases over the entire world, as far as this can be effected from the imperfect data at our command; and he was certainly the first to give effect to the idea by his annual report of all the leading facts bearing on the subject, which had come to his knowledge during each previous twelvemonth. Two of these reports have already been published in the Transactions of the Society.

Such are the leading points in the professional career of our late dear and valued friend. Nor were the virtues of his heart less than those of his head. One who knew him well has remarked, that you could never please M'William better than by asking him to do something for you. Need more be said for his good and kindly disposition? May his example have its due effect upon us all, and in an especial manner upon our brethren in that branch of the public service of which he was so great an ornament!

Registration of Births, Deaths, and Diseases in Ireland.

THE following series of resolutions was unanimously adopted by the British Medical Association, at its recent meeting in London.

"1. That in any measure of legislation for the registration of births and deaths in Ireland, this Association deems it highly important that the local machinery for such registration should be altogether distinct from that for the registration of marriages; and is happy to perceive that this principle has been recognised in the Bills which have been introduced into the House of Commons during this and the preceding sessions of Parliament.

"2. That it is most desirable to introduce into any such measure the principle of local scientific supervision of the returns of births and deaths.

"3. That the office of *Superintendent Registrar of Births and Deaths* ought to be held by persons well acquainted with the physical and biological sciences, versed in sanitary and vital statistics, and accustomed to make medico-legal investigations.

"4. That it is desirable to combine with the superintendence of the registration of births and deaths the registration of all sickness attended in public institutions or at the public expense.

"5. That each superintendent registrar should be required to publish, for the information of the local administrative authorities and the instruction of the inhabitants of his district, an *Annual Report* of the results of registration, as also a *quarterly* summary of the deaths and diseases, with their causes, according to forms to be determined by the Registrar-General for Ireland.

"6. That in the local reports of mortality and sickness, it is important to specify age and occupation, to record meteorological observations, and to note local events and circumstances affecting the public health.

"7. That it is desirable to require the authentication of the cause or mode of death by a certificate from a legally qualified medical practitioner; and that, where no such certificate is delivered, the sub-registrar be required to inform the superintendent, who should forthwith make inquiry into the case.

"8. That the registration of births should be compulsory; and that still-births (after the sixth month of utero-gestation), when not certified by a legally qualified medical practitioner, should be subject to the regulation stated in the last resolution.

"9. That the boundaries of registration districts and sub-districts ought, as nearly as possible, to conform to the limits of existing districts for the relief of the poor, and for the administration of medical aid (union and dispensary districts), having due regard to the jurisdictions of local sanitary authorities.

"10. That the proposed scientific superintendents, as statistical inquirers and reporters for national purposes, should be made independent of local and party influences, debarred from private medical practice, and paid out of national funds.

"11. That the Council of this Association be requested to open communications with the Government, and with the Poor-law Commission of Ireland, for the purpose of laying before them the suggestions of the Association, and of conferring with them as to the best mode of embodying them in a legislative enactment."

This important movement is in continuation of proceedings taken by the Social Science Association,* and these again originated in a discussion raised by Mr. Rumsey's papers† read at the Bradford Congress of that Association. These papers were reprinted in 1860, and prefaced by an account of subsequent proceedings, with much additional information on the subject. The author has made an interesting comparison of our own with continental systems, and replied to sundry criticisms and official objections.‡

Men of acknowledged authority in such matters support the proposed reform. Among its advocates, though holding themselves free to differ upon details, may be seen the names of Lord Brougham, Hon. W. F. Cowper, Sir J. K. Shuttleworth, Dr. Farr, Mr. Simon, Dr. Headlam Greenhow, Rev. C. H. Hartshorne, Mr. Chadwick, Sir Charles Hastings, Mr. G. W. Hastings, Mr. Aspland,§ and Mr. Charles Hawkins.

The immediate point at issue is the nature of the forthcoming measure for the registration of births and deaths in Ireland. Legislation of some sort is inevitable. Bills have been introduced and withdrawn or dropped for the last three sessions. The two associations already mentioned concur in an effort to apply the main principles of Mr. Rumsey's plan to Ireland; and Dr. Harkin,

* See Transactions of the National Association, 1860, Introd. p. xxvii.; 1861, Introd. p. xxxviii.

† The first paper, "On Certain Deficiencies in our Public Records of Mortality and Sickness, with Suggestions for an Improved and Extended National System of Registration;" the second, "On Certain Departments of Medico-Sanitary Police and Medico-Legal Inquiry, in connexion with the Scientific Superintendence of Mortuary Registration." 1859.

‡ Public Health: the right use of Records founded on Local Facts. London, 1860. This elaborate exposition of the whole question, in its various aspects and relations, deserves a thoughtful perusal by all those—whether medical practitioners, or statisticians, or publicists—who are concerned in promoting an efficient machinery for sanitary inquiry.

§ See his paper, "On Certain Fallacies in our National Mortuary Returns," 1857; also, "On the Statistics of Paris, and the Mode of obtaining Facts for Mortuary Tables," 1861 (Transactions of the Manchester Statistical Society).

of Belfast, has come forward most opportunely with an able pamphlet in support of a scientific machinery for registration.* He puts the case thus forcibly:

"All experience points to a division of offices into sub-registrar, superintendent-registrar, and registrar-general. The duties of the sub-registrar bring him often face to face with death; he is to receive and forward to the superintendent registrar a statement of the cause thereof; he should be able, from medical and scientific knowledge, to test the value of the cause assigned, to recognise the effects produced on the human frame by mortal disease, and to understand the nature of cadaveric change. It is, however, upon his superior officer, the superintending-registrar, that the more important duties would devolve. His office should be to receive, to verify, to tabulate in a scientific manner the returns transmitted; to act as a check upon careless sub-registrars; to investigate the causes of death when none are assigned; to be capable, from medical knowledge, of advising non-medical coroners, when post-mortem examinations are necessary for the discovery of truth; and when making up his quarterly returns to the registrar-general, to give them such form and shape as shall afford information as to the vital statistics of his district, accessible to all within it. In the higher office of registrar-general, the Government has always recognised the necessity of medical supervision, by attaching to the office, both in this country and in England, most accomplished and able medical statisticians."

While asserting the importance of medical knowledge in each of these offices, Dr. Harkin very properly admits its supreme necessity in that of superintendent-registrar. It is to this point that the English reformers particularly address themselves. They consider it the keystone of a normal sanitary organization.

None but a medically-educated officer would be competent to revise the returns of sub-registrars, to verify, digest, and tabulate the facts recorded, to transmit the returns in a serviceable form to the registrar-general, to compile the statistical reports from the various dispensaries and hospitals of his district, and to prepare all these and other cognate observations for publication in the locality as a means of instruction to the people and of guidance to those who are empowered to carry into effect sanitary regulations.

Whatever might be the public advantages—and doubtless there would be many—of a liberal education and special acquirements for every grade of registration officers, it would be no easy matter to induce Parliament to make so high a standard of qualification obligatory upon all. If scientific attainments and statistical experience are by law to be required of one grade only, it ought certainly to be secured for the higher local appointment. If the choice of sub-registrars by the local boards or the county magistrates can be limited by certain general instructions as to competency, &c., and by subjecting each appointment to the approval of the registrar-general, it is perhaps as much as we can reasonably expect.

It is the superintendent who (if a thoroughly competent person) will be the referee in medico-legal difficulties, who will be required to utilize the returns in the cause of sanitary improvement, and who would in time be the real health officer of the district. The advocates of this measure will have to define more clearly what they mean by a scientific qualification.

The third resolution does not show what is to be the precise *ensemble* of acquirements, how these are to be tested, and where the certificate or diploma is to be obtained.

It is doubtful whether any university or medical college confers a *testamur* declaring the possession of the special knowledge required. This want, however, may be supplied.

* "On the Registration of Births, Deaths, and Diseases," read before the National Association at Dublin, with Notes and Addenda, by Alexander Harkin, M.D., Belfast, 1862.

Something must be said about the various projects of registration in Ireland recently laid before the House of Commons. In 1861 there were the rival schemes of Mr. Cardwell and Lord Naas. Their respective merits and demerits are well sifted in the Report of the Select Committee on Irish Registration in that session. This contains some amusing counter-evidence, and is not of alarming length. Mr. Cardwell would have given a statutory preference to dispensary medical officers in the appointment of sub-registrar, a proposal which Dr. Harkin has vigorously defended, and which seems to be acceptable to the majority of dispensary surgeons.

Are we to suppose that the small pittance to be paid to a sub-registrar would be a desirable addition to their inadequate salaries? that their duties as registrars would not lead them into unpleasant collision with their brethren in private practice? that their time would not be too fully occupied with higher professional work for due attention to the pen-and-ink details of an inferior office? Possibly; but they would hardly consent to serve under the agents and attorney, who as union clerks would have been, according to Mr. Cardwell's Bill, the superintendent-registrars. If the Poor-law machinery should be selected, the great influence with boards of guardians and dispensary committees which the medical officers have so deservedly acquired, may be left to work its natural result without legislative interference.

On the other hand, the Irish Secretary, Sir Robert Peel, has lately taken up Lord Naas's plan, and in his little Bill of last session—happily strangled in its birth—he proposed to work a registration of vital statistics by means of constabulary. This has not diminished the worthy baronet's unpopularity in Ireland. His measure would, as Dr. Harkin says, establish a system incapable of improvement, stunted by the limited capacity of the agents, and most repulsive to the peasantry. In all probability it would prove a miserable failure, unless the registering policemen were placed under the direction of a far greater number of medical superintendents than would be necessary on the other plan.

How far better than either would be the project of the Social Science and Medical Associations, worked by educated and intelligent men. Its scientific results and the accuracy of its statistical details, could hardly fail to reflect high credit on the Irish code. Like the excellent dispensary system of that country, it would become a model for Europe, and would react beneficially upon English defects and shortcomings.

The field in Ireland is still clear for the establishment of a system correct in principle, suited to the people, and approved by competent judges. If the opportunity be neglected, and the imperfect English arrangements forced upon Ireland, we shall have thrown away our dearly-bought experience, and virtually retrograded in sanitary legislation. The question is not one of place and salary, but of science and public health.

Dr. Kebbell's 'Climate of Brighton.'

THE Editor is sorry to find that the writer of the notice of this *brochure* (an abridgment of a former work) which was inserted in our last number (p. 80), was not aware that the original work had been previously commented upon, and so favourably, in our pages.

Dr. Kebbell has written to us expressing his surprise that the pamphlet should have been noticed with the reprobation which, by reason of certain passages pointed out, the reviewer thought right to express, and also his belief that the writer was an enemy of his. We have thought it fitting, for Dr. Kebbell's satisfaction, to observe that the reviewer in question was totally free from any personal hostility whatever, and is only desirous that the value of his judgment should be determined by a perusal of the pamphlet itself.

BOOKS, &c., RECEIVED FOR REVIEW.

The Spas of Europe. By Julius Althaus, M.D., M.R.C.P. London, Trübner and Co. 1862. pp. 494.

Amputation of the Cervix Uteri. By A. K. Gardner, M.D. (Reprint from the Bulletin of the New York Academy of Medicine.) (Pamphlet.)

On the Growth of the Recruit and Young Soldier, with a view to a Judicious Selection of "Growing Lads," &c. &c. By W. Aitken, M.D., Professor of Pathology in the Army Medical School. London, Griffin and Co. 1862. pp. 72.

Illustrations of Puerperal Diseases. By R. Uvedale West, M.D. Second Edition. London, Churchill. pp. 201.

Diet of European Soldiers in India; with the Effects of "Tobacco Smoking" upon the Animal Economy. By A. E. T. Longhurst, M. R. C. S. Calcutta, 1862. (Pamphlet.)

Experiments on the Formation of Infusoria in Boiled Solutions of Organic Matter, &c. &c. By J. Wyman, M.D. (Pamphlet.) From American Journal of Science.

Is Tracheotomy in True Croup a Justifiable Operation? By J. O'Reilly, M.D. New York. (Pamphlet.)

Synopsis of a Course of Lectures on Midwifery and the Diseases of Women and Children. By John Christie, M.D. Aberdeen, 1861. (Pamphlet.)

Lecture Introductory to a Course of Lectures on Midwifery, &c. &c. By John Christie, M.D. 1861.

A System of Surgery, Theoretical and Practical, in Treatises by various Authors. Edited by T. Holmes, M.A. Cantab., &c. Volume the Third. London, Parker, Son, and Bourn. 1862. pp. 916.

Self-supporting Dispensaries: their Adaptation to the Relief of the Poor and Working Classes, &c. By J. Jones, M.R.C.S., &c. Churchill. (Pamphlet.)

The Anglo-Turkish Bath; or, the Modern Application of the Ancient Roman Therma as a Hygienic, Prophylactic, and Therapeutic Agent, &c. By York James Moore, M.R.C.S. London and Torquay. pp. 58.

Homicidal Mania. A Biography. By D. Yellowlees, M.D. Edinburgh. (Pamphlet.)

A Manual of Military Surgery, for the Use of Surgeons in the Confederate States Army; with an Appendix of the Rules and Regulations of the Medical Department of the Confederate States Army. By J. J. Chisholm, M.D. Second Edition. Richmond, Va., West and Johnston. 1862. pp. 514.

A Practical Treatise on Phthisis Pulmonalis, embracing its Pathology, Causes, Symptoms, and Treatment. By L. M. Lawson, M.D. Cincinnati and New York, 1861. pp. 557.

Mémoires de Médecine et de Chirurgie Pratique. Par le Dr. Prosper Hullin, Médecin-en-Chef de l'Hôpital Saint-Alexandre de Mestagne. (Vendée.) Baillière. 1862. pp. 527.

Addresses delivered by Dr. Burrows, Dr. Walshe, Mr. Paget, and Dr. Sharpey, at the Thirtieth Annual Meeting of the British Medical Association. London, 1862. London, Richards. (Pamphlet.)

On the Strength and Beauty of Mineral Teeth. By E. Truman, M.R.C.S. Paper read before the Odontological Society.

Infanticide: its Law, Prevalence, Prevention, and History. By W. B. Ryan, M.D. London, Churchill. 1862. pp. 266.

The Mensuration of the Human Skull. By J. A. Meigs, M.D. Philadelphia. (Pamphlet.) (Reprint.)

On the Anatomy of Muscular Fibre. By S. Martyn, M.D., &c. (Reprint from Beale's Archives.)

On Connective Tissue. By the same. (Reprint from the same.)

On the Proposal to Introduce a New Grain Weight. By G. E. Paget, M.D. (Pamphlet.)

On the Establishment and Management of Cottage Improvement Societies. By W. A. Greenhill, M.D. Paper read at the Social Science Association. London, 1862.

Researches and Observations on Pelvic Hæmatocele. By J. Byrne, M.D., &c. New York, 1862. (Pamphlet.)

The Half-yearly Abstract of the Medical Sciences. By Drs. Ranking and Radcliffe. January to June, 1862. Churchill. pp. 370.

The Retrospect of Medicine. Edited by Drs. W. and J. Braithwaite. London, Simpkin and Marshall. 1862. pp. 424.

On Ovarian Dropsy, its Nature, Diagnosis, and Treatment. By J. Baker Brown, F.R.C.S. London, Davies. 1862. pp. 283.

General Debility and Defective Nutrition: their Causes, Consequences, and Treatment. By Alfred Smee, F.R.S., &c. Second Edition. Churchill. 1862. pp. 117.

Transactions of the Medical and Physical Society of Bombay. No. VII. New Series. For 1861.

A Handbook of Volumetrical Analysis. By R. H. Scott, M.A. Dublin. London, Longman and Co. 1862. pp. 104.

Vichy and its Mineral Springs, &c. By E. Lee, M.D. London, Churchill. 1862. (Pamphlet.)

A Notice of Menton. By the same. London, Adams. 1862. (Pamphlet.)

The Winter Climate of Menton. By H. C. Price, F.R.C.S.E., Surgeon to the Great Northern Hospital, King's Cross. London, Churchill. pp. 79.

Klinische Mittheilungen von der Medicinischen Abtheilung des Allgemeinen Krankenhauses in Hamburg, 1858, 1859, 1860. Von Dr. C. Tüngel.

The Progress of Ophthalmology. A Sketch. By D. A. Robertson, M.D. Edinburgh. (Pamphlet.)

Clinical Medicine: Observations Recorded at the Bedside, with Commentaries. By W. T. Gairdner, M.D. Edinburgh, Edmonston and Douglas. 1862. pp. 741.

Illusions et Réalités de la Thérapeutique. Par G. Pécholier, Prof. Agrégé à la Faculté de Médecine de Montpellier. 1862.

Della Temperatura delle Orine in diverse ore del Giorno e in Diversi Climi. Ricerche Sperimentali del Dottor Paolo Mantegazza. Milano, 1862. (Pamphlet.)

On the Rain-fall in the Lake District in 1861; with some Observations on the Composition of Rain-water. By John Davy, M.D., F.R.S. (Reprint.) Edinburgh, 1862.

On some of the more important Diseases of the Army. With Contributions to Pathology. By John Davy, M.D., F.R.S., &c. London and Edinburgh. Williams and Norgate. 1862. pp. 438.

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