14

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL

REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL. XII.

JULY—OCTOBER, 1853.

LONDON:

JOHN CHURCHILL, 46, PRINCES STREET, SOHO;

AND

SAMUEL HIGHLEY, JUN., 32, FLEET STREET.

MDCCCLIII.

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

CONTENTS OF No. XXIII.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1853.

Analytical and Critical Reviews.

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

CONTENTS OF No. XXIV.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1853.

Analytical and Critical Reviews.

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1853.

PART FIRST.

Analytical and Critical Reviews.

Review I.

Principles of Human Physiology. By William B. Carpenter, M.D., F.R.S., &c. &c. Fourth Edition, 1853. *Chapter XIV. "Of the Functions of the Nervous System."*

The unremitting attention which Dr. Carpenter has devoted to the Physiology of the Nervous System would, under any circumstances, render the latest views he entertains in this branch of medical philosophy of much interest; but when we find it expressly stated in the preface to the current edition of his Principles of Human Physiology, that the chapter devoted to it is the result of special and laborious research, and that it contains "the more matured fruits of his inquiries and reflections," we feel that little in the way of apology is due to our readers for bringing these views under their notice, and subjecting them to critical inquiry. It is not possible to overrate the value of this department of Physiology; for whether the study of the functions of the nervous system be considered simply as a branch of medical science in relation to the pathology and treatment of diseases of the nervous system, or in its relations to the study of human nature, and therefore to Mental Philosophy and Morals, the extent of its social and professional importance seems incalculable. We shall first state Dr. Carpenter's views as much in detail as our narrow limits will permit, and then analyse more particularly his views as to the functions of the cerebrum and the great ganglionic centres within the encephalon—the acknowledged seat of mental operations.

Dr. Carpenter considers the functions of the nervous system, with reference to three great anatomical divisions—namely, the excito-motor or true spinal, the sensori-motor or ganglia of sensation, and the cerebral. The acts dependent on the first are automatic, and occur without sensation; those dependent on the second are automatic also, but occur with sensation; those of the third are in connexion with ideas, but may also be purely automatic, like the two first. The whole nervous system of the invertebrate animals may be regarded as ministering entirely to automatic action. The ganglia in connexion with the nerves of special sense are the seat of sensation and the source of the "sensori-motor" movements; the remaining ganglia of the chain not ministering to sensation, but inducing none other than "excito-motor" movements. Although the actions of these animals are exquisitely adapted (as is specially seen in the economy of the common bee-hive), yet they are not *designed* by the animal; the adaptiveness being inherent "in the original construction of the nervous system, which causes particular movements to be executed in direct respondence to certain impressions and sensations." These movements are, in short, those usually termed instinctive, and that *mechanism* (including both the dynamical and material conditions under the term) which causes them is none other than *instinct*. Dr. Carpenter is not prepared to maintain that there is *nothing whatever* in the invertebrata analogous to the reasoning powers and will of higher animals; they may exist, but in a rudimentary state.

In the invertebrate animals the automatic powers are most highly developed in the highest type; not so in the vertebrate, for the highest development of the intelligence and the supreme domination of the will characterizes the highest type in them, while to these the instinctive powers and acts are subordinate. "The super-addition of these more elevated psychical endowments is coincident with the addition of a peculiar ganglionic centre, the cerebrum, to the automatic apparatus of vertebrated animals." In the vertebrata, therefore, there is the cranio-spinal axis, representing the whole nervous system of the invertebrata (with the exception of the rudiment of the sympathetic which they possess), with this superadded cerebrum; the cranio-spinal axis consisting of the spinal cord, the medulla oblongata, and the sensory ganglia. The spinal cord corresponds to the gangliated ventral column of the articulata; and each segment with its pair of nerves is a repetition of the single "pedal" or locomotive ganglion of the mollusca. The medulla oblongata corresponds with the cords that in the invertebrata pass round the œsophagus, connecting the cephalic ganglia with the first sub-œsophageal ganglion, but containing also ganglionic centres, which correspond with the respiratory and stomatogastric ganglia of the same class. Dr. Carpenter states:

"Under the term sensory ganglia may be comprehended that assemblage of ganglionic masses lying along the base of the skull in Man, and partly included in the medulla oblongata, in which the nerves of the 'special senses'—Taste, Hearing, Sight, and Smell, have their central terminations; and with these may probably be associated the two pairs of ganglionic bodies known as the Corpora Striata and Thalami Optici, into which may be traced the greater proportion of the fibres that constitute the various strands of the medulla oblongata, and which seem to stand in the same kind of relation to the nerves of touch, or 'common sensation,' that the olfactive, optic, auditory, and gustative ganglia bear to their several nerve-trunks." (p. 669.)

It is upon these that the cerebrum proper is superimposed, but wherever this is developed there also is found a cerebellum, having a general but by no means constant relation to the cerebrum. This arrangement is very similar to that adopted by Lamarck, who termed the hemispheres the *hypocephalon*, and, to use his own words, "le cerveau proprement dit, cette partie de la masse medullaire principale qui contient le centre de rapport des nerfs, et à laquelle les nerfs des sens particulier viennent se réunir."* Dr. Carpenter first explains the general anatomy of these distinct elements of the nervous system, copiously illustrating the text with wood-cuts. Having explained the anatomy, he treats of the physiology. The doctrine of spinal reflex action is lucidly developed, and extensions and new illustrations given. The following is one of these:

"There can be little doubt that the habitual movements of locomotion, and others which have become 'secondarily automatic,' may be performed by Man under particular circumstances, through the agency of the Spinal Cord alone, under the guidance and direction of the sensorial centres, or even without such guidance; the required condition being that the influence of the cerebrum shall be entirely withdrawn. There are numerous instances on record, in which soldiers have continued to *march* in a sound sleep; and the author has been assured by an intelligent witness, that he has seen a very accomplished pianist complete the performance of a piece of music in the same state." (p. 722.)

The functions of the spinal cord, as here stated, are so well understood, that we need not dwell upon that subject, but proceed to notice Dr. Carpenter's views as to the functions and offices of the various divisions of the encephalon, and analyse the considerations by which he has endeavoured to throw light upon this most obscure and most difficult department of neurology. Dr. Carpenter, as we have seen, arranges the ganglionic masses within the encephalon in two divisions, the sensorial and the intellectual, corresponding to the two divisions of self-consciousness known as sensation and perception. There seems little difficulty in coming to the conclusion, that the masses of grey matter in which the nerves of special sense terminate are the ganglionic centres of those nerves; and consequently, in part at least, the seat of those changes by which we become conscious of the impressions made on the external organs of special sense; and inasmuch as the nerves of four of the five senses may be traced to distinct masses of this kind, there is a clue to the function of the latter. Greater difficulty is found in determining the encephalic centre of the sense of touch and of common sensation. The optic and olfactive nerves can be traced into the thalamus; and as the sensory tract of the spinal cord is almost entirely spread out in the substance of that body, in Dr. Carpenter's opinion, he concludes that the *thalami optici* constitute "the chief focus of the sensory nerves, and more especially the ganglionic centre of the nerves of *common* sensation, which ascend to it from the medulla oblongata and spinal cord." The corpora striata being implanted on the *motor* tract of the *crura cerebri*, their relation to the motor fibres "appears to be essentially the same as that which the thalami bear to the sensory tract."

In determining the function of these *sensorial ganglia* Dr. Carpenter appeals to the series of experiments prepared for us by nature in the descending series of animal life, and examines the relative development of the sensory ganglia and cerebral hemispheres. We subjoin his argument drawn from this source:

"The sensory ganglia gradually increase, while the cerebral hemispheres as regularly diminish, in relative size and importance, as we descend from the higher mammalia to the lower—from these to birds, thence to reptiles; from these

* Philosophie Zoologique, vol. ii. p. 223.

again to the higher fishes, in which the aggregate size of the sensory ganglia equals that of the cerebrum; thence to the lower fishes, in which the size of the cerebral lobes is no greater than that of a single pair of sensory ganglia, the optic, and frequently even inferior; and lastly, to the *amphioxus*, or lancelot, the lowest vertebrated animal of which we have any knowledge, in which there is not the rudiment of a Cerebrum, the encephalon being only represented by a single ganglionic mass, which from its connexion with the nerves of sense must obviously be regarded as analogous to the congeries of ganglia that we find in the higher forms of the class. Descending to the invertebrated series, we find that, except in a few of those which border most closely upon vertebrata (such, for example, as the cuttle-fish), the *whole* Cephalic mass appears to be made up of ganglia in immediate connexion with the nerves of sense. These may appear to form but a single pair, yet they are in reality composed of *several pairs*, fused (as it were) into one mass. Of this we may judge by determining the number of distinct pairs of nerves which issue from them; and also by the investigation of the history of their development, the results of which bear a close correspondence with those obtained in the preceding method. It is further to be remarked, that the development of the cephalic ganglia in the invertebrata always bears an exact proportion to the development of the *eyes*; the other organs of special sense being comparatively undeveloped; whilst these, in all the higher classes at least, are instruments of great perfection, and are evidently connected most intimately with the direction of the movements of the animals. . . . . .

"Thus we are led, by the very cogent evidence which comparative anatomy supplies, to regard this series of ganglionic centres as constituting the real *Sensorium*; each ganglion having the power of communicating to the mind the impressions derived from the organ with which it is connected, and of exciting automatic muscular movements in accordance to these sensations. If this position be denied, we must either refuse the attribute of consciousness to such animals as possess no other encephalic centres than these, or we must believe that the addition of the cerebral hemispheres in the vertebrated series alters the endowments of the sensory ganglia—an idea which is contrary to all analogy." (p. 729.)

We do not at all perceive the necessity of being impaled upon either of the horns of this dilemma, without wishing to deny the truth of the proposition; and cannot quite accord with the assertion that the alternative idea is contrary to all analogy. Is it the analogy of structure or function—of material arrangement or dynamical condition—to which Dr. Carpenter refers?

Further proof is adduced in support of this view from the results of vivisections made by Flourens, Hertwig, Magendie, Longet, and others—that is to say, from the phenomena manifested by various vertebrata after the entire removal of the cerebrum. The complexity and intimate relations with each other of the constituents of the encephalon render the results of such experiments of little value in determining accurately the special functions of individual parts. The same remark applies to pathological observations bearing on this point.

There can, however, be no reasonable doubt as to the general proposition that the ganglionic masses into which the sensory nerves run, are the seat of those changes which excite in our consciousness the feeling of pleasure and pain; and it is not at all improbable that, in the invertebrata at least, they are also the seat of those changes which excite in the consciousness the [illegible] of [illegible] external sensations, in connexion with impressions resulting from them, [illegible] if these animals be capable of that idea. But Dr. Carpenter goes further than this. He maintains that all the changes which take place in the nervous ganglia of man, and excite ideas of the intellect and the emotions, must be sent downwards as internal impressions to the sensory ganglia, and excite passive changes therein before they give to the consciousness the ideas or the feelings which depend upon them corporeally. An ingenious and extended series of facts and inferences are adduced in support of this important doctrine, which has arisen in Dr. Carpenter's mind out of the fundamental principle in neurology and psychology, that the functions of the hemispherical ganglia are subject to the laws of reflex action. It has been generally understood for some time past, that the hemispheres are essentially a very extended surface of vesicular matter, with which the radiating white or conducting fibres—"the nerves of the internal senses"—are in immediate contact; and that the changes induced in the former by intellectual operations are transmitted downwards by the latter to excite motion. Dr. Carpenter sees in this view of the structure an analogy between the relations of the vesicular matter to the sensory ganglia, and those relations of the retina or any other peripheral expansion of vesicular matter, in an organ of sense, to the same ganglia.

"Ideas, emotions, intellectual operations, &c., have of late been frequently designated as 'states of consciousness;' and this psychological description of them is in full harmony with the physiological account here given of the material conditions under which they occur. For a sensation being a state of consciousness excited through the intermediation of the sensorium by a certain change (*e. g.*) in the condition of the Retina, it is not difficult to understand how a change in the condition of the cerebrum may excite, through the same instrumentality, that state of consciousness which may be termed ideational, or that another change may produce the emotional consciousness, another the intuitional consciousness, another the logical consciousness. And although it may be thought, at first sight, to be a departure from the simplicity of Nature, to suppose that the cerebrum should require another organ to give us a consciousness of its operations, yet we have the knowledge that the eye does not give us visual consciousness, nor the ear auditory consciousness, unless they be connected with the sensory ganglia; and in the end (the author feels a strong assurance) it will be found much simpler to accept the doctrine of a common centre for *sensational*, and for what may be distinguished as *mental*, consciousness, than to regard the two centres as distinct." (pp. 789, 790.)

Another function of these sensory ganglia is to originate the "guiding sensation," without which, as was first stated by Sir Charles Bell, and in Dr. Carpenter's opinion, no voluntary action can be performed.

"In the majority of cases the guiding or controlling sensation is derived from the muscles themselves, of whose condition we are rendered cognisant by the sensory nerves with which they are furnished; but there are certain cases in which it is ordinarily derived from one of the special senses, and in which the muscular sense can only imperfectly supply the deficiency of such guidance; whilst, again, if the muscular sense be deficient, one of the special senses may supply the requisite information." (p. 743.)

Dr. Carpenter adduces several interesting illustrations of this function, which we regret our inability to notice; but we cannot omit calling Dr. Carpenter's attention to two little works, containing somewhat similar views on muscular sensation and "adjustments," published by the venerable Dr. Richard Fowler, of Salisbury.*

* Dr. Fowler is one of our earliest experimental neurologists, his labours dating from the last century. We allude more particularly, however, to his two little works, 'Some Observations on the Mental State of the Blind, and Deaf and Dumb,' &c., Salisbury, 1843; and 'An Attempt to Detect the Physiological Processes by which Thinking is effected,' &c., Salisbury, 1852.

Having considered these points, Dr. Carpenter proceeds to analyse the operations of which the cerebrum is the instrument; "considering them in the ascending series, as founded upon sensational changes." He discriminates (with Sir W. Hamilton) between sensation and perceptive consciousness; the former being a change in the consciousness induced by impressions in which there is no reference to the cause—no idea of outness; the latter being such a change leading to a perception, if not of *the* cause, at least of *a* cause, external to the mind. A sensation may be accompanied with the simple feeling of pleasure or pain; so also a perception or an idea. This conjunction of the feeling with the idea produces an emotion or "emotional state."

"The formation of a true *desire* even for the gratification of some bodily appetite requires that an idea of the object of desire shall have been formed; and it is the expectation of the pleasure which will arise from the act in question, or of the pain which will be produced by abstinence from it, which makes the idea a ***motive*** to action. A careful analysis of the various Propensities, Moral Feelings, Sentiments, &c., which are ranked by metaphysicians under the general term 'active principles,' will show (the author believes) that such is the essential nature of all." (p. 785.)

Dr. Carpenter mentions illustrative instances. Benevolence is the pleasure in the happiness of others; malevolence in the unhappiness. Combativeness is nothing else than the pleasurable idea of setting oneself in antagonism to others; pride, the pleasurable contemplation of our own superior excellencies, &c. He mentions the circumstance that he first developed this view of the emotional states in October, 1846, and that it was only subsequently that he found Mr. James Mill had propounded similar views in his 'Analysis of the Human Mind.' Dr. Carpenter also refers to the doctrines of Sidney Smith on this point, and observes that they more nearly resemble his own than those of Mr. Mill. We think that the views of Dr. Carpenter are more generally the views of metaphysicians than he seems to be aware. Brown has something very similar; so also Baumgarten; in Unzer's 'Principles' Dr. Carpenter will find his ideas fully developed, as the views of Baumgarten applied to neurology. The difference between *volitional* and *voluntary* movements specially noted by Dr. Carpenter, is also distinctly laid down by Unzer.

Passing from the emotional to the intellectual powers, Dr. Carpenter discusses the relations to the cerebrum of what may be emphatically termed *Thought*. It is simple justice to say, that very interesting and very important doctrines are enunciated in this section, and that much light is thrown, by the arguments and illustrations adduced, on the more recondite (and to many mysterious) modes of mental action. Our limited space will not suffice for a detailed exposition of Dr. Carpenter's views; we shall therefore select one or two of the more interesting and novel points. One of these is the doctrine of "unconscious cerebration." This is partly a statement of facts, partly an hypothesis; a statement of facts *quoad* the phenomena that are included under the term unconscious cerebration; partly an hypothesis *quoad* the explanation of those phenomena which is based upon the doctrine, that the cerebral hemispheres—the sphere of mental operations—are not the seat of consciousness, but only the "sensory ganglia." First, as to the phenomena themselves. There are certain "intellectual ideas" which appear to be so necessarily excited by mental operations, and to be so little dependent on individual peculiarities, either inherent or acquired, that they take rank as fundamental axioms or principles. Of this kind are the belief in our present existence, in our past existence, in the existence of external things, the causes of our sensations, in our personal identity, the ideas of Right, Truth, Beauty, of a Great First Cause, &c. Dr. Carpenter shall, however, speak for himself.

"Upon the sensational and intellectual ideas thus brought under the cognizance of the mind, all acts of *Reasoning* are founded. These consist for the most part in the aggregation and collocation of ideas, the decomposition of complex ideas into more simple ones, and the combination of simple ideas into general expressions; in which are exercised the faculty of *Comparison*, by which the relations and connexions of ideas are preserved; that of *Abstraction*, by which we fix our attention on any particular qualities of the object of our thought, and isolate it from the rest; and that of *Generalization*, by which we connect together those properties which have been thus discovered to be common to a number of objects. . . . . . The foregoing are the purely intellectual processes chiefly concerned in the simple acquirement of knowledge, with which class of operations the emotional part of our nature has very little participation; and there is strong reason to believe that they may be performed *automatically* to a very considerable extent, without any other than a permissive act of Will. It is clearly by such automatic action that the above-mentioned 'fundamental axioms' or 'intuitions' are evolved; and there is not one of the operations above described which may not be performed quite involuntarily, especially by an individual who is naturally disposed to it." (p. 816.)

Dr. Carpenter mentions several illustrations of this *automatic* process of thought. The flow of thoughts in composition, when this automatic action may continue uninterruptedly for hours, is one. The habits of composition of Coleridge and Mozart are apt specific instances. The former had a remarkably weak will; with the most gigantic projects in his thoughts, he never could bring himself seriously even to attempt the execution of them. The composition of the poetical fragment, 'Khubla Kham,' in his sleep, was, as Dr. Carpenter remarks in a note, "a typical example of automatic mental action; and almost his whole life might be regarded as a sort of waking dream, in regard to the deficiency of that self-determining power which is the pre-eminent characteristic of every really great mind." Energy of automatic thought and weakness of Will are the characteristics of "Genius." When Mozart was only four years of age he wrote music which was in strict accordance with the rules of composition, although he had not been taught them. This characteristic of his childhood was the characteristic of his prime. There was no *intentional* elaboration of the separate instrumental parts of a symphony; the ideas came and arranged themselves spontaneously. Mozart, like Coleridge, was a man of extremely weak will, and when not under the guidance of his wife, was the sport of almost every kind of impulse. Other illustrations, of a singularly interesting kind, establish the existence of this automatic or unconscious cerebral activity. The most common instance of its occurrence is when a subject, which has been laid aside as hopelessly entangled, and then brought back to the attention, is presented to the mind under quite a new aspect. Part of the result may be attributed to the influence of repose on the material organ. But, as Dr. Carpenter remarks,—

"This by no means accounts for the *entirely new development* which the subject is found to have undergone, when we return to it after a considerable interval; a development which cannot be reasonably explained in any other mode than by attributing it to the intermediate activity of the cerebrum, which has in this instance automatically evolved the result without our consciousness. Strange as this phenomena may at first sight appear, it is found, when carefully considered, to be in complete harmony with all that has been affirmed in the preceding paragraphs, respecting the relation of the cerebrum to the sensorium, and the independent action of the former; and looking at all those automatic operations by which results are evolved, without any intentional direction of the mind to them, in the light of 'reflex actions' of the cerebrum, there is no more difficulty in comprehending that such reflex actions may proceed without our knowledge, so as to evolve *intellectual products*, when their results are transmitted to the sensorium, and are thus impressed on our consciousness, than there is in understanding that impressions may excite muscular movements, through the 'reflex' power of the spinal cord, without the necessary intervention of sensation. In both cases the condition of this form of independent activity is, that the *receptivity* of the sensorium shall be suspended *quoad* the changes in question, either by the severance of structural connexion, or through its temporary engrossment by other objects." (p. 819.)

This doctrine of "Unconscious Cerebration" has a wide appliancy. Emotional states may be thus developed by the operation of external impressions, and a great alteration induced in the feelings long before the individual is conscious of the change. Those external impressions are most likely to influence the individual which are derived from the emotional states of surrounding individuals—a mode of influence of peculiar and well-known potency in schemes of education, proselytism, &c. The power of congregated masses of men acting under a powerful emotion is something terrible in its irresistible action upon the individuals who come within the sphere of its influence. The theory explains also the obscure phenomena of sleep, mania, delirium, electro biology, animal magnetism, and is applied to this end in a very effective and able manner by Dr. Carpenter, who on this, as on other occasions, does not lose sight of the *practical* applications of science. Another point is shown—namely, that the doctrine of "unconscious cerebration" is perfectly compatible with the doctrine of a free-will and moral responsibility. While looking steadily at the facts on the side of cerebral action, we must look equally steadily at the facts which bear on consciousness, and the evidence derived therefrom. Perhaps the following quotation will indicate the nature of Dr. Carpenter's views on this point, and their practical uses:

"From the time when the Human being first becomes conscious that he has a power within himself of *determining the succession* of his mental states, from that time does he emancipate himself from the domination of his constitutional or automatic tendencies. It is a principle now recognised by all the most enlightened educators, that the development of this power of self-control ought to be the object of all nursery discipline; and the process of its acquirement is very gradual. When an infant is excited to a fit of passion by some unpleasant sensation, its nurse attempts to restore its equanimity by presenting some new object to its attention, so that the more recent and vivid pleasurable impression may efface the sense of past uneasiness. As the infant grows into childhood, the judicious parent no longer trusts to mere sensory impressions for the diversion of the passionate excitement, but calls up in its mind such ideas and feelings as it is capable of appreciating, and endeavours to keep the attention fixed upon these, until the violence of the emotion has subsided; and recourse is had to the same process whenever it is desired to check any tendency to action which depends upon the selfish propensities—appeal being always made to the highest motives which the child is capable of recognising, and punishment being only had recourse to for the purpose of supplying an additional set of motives when all others fail. For a time, this process of external suggestion may need to be continually repeated, where there are strong impulses whose unworthy character calls for repression; but if it be judiciously adopted and consistently persevered in, a very slight suggestion serves to recal the superior motives to the conflict. And in further space, the child comes to feel that he has *himself* the power of recalling them, and of controlling his urgent impulses to immediate action. The power of self-control, thus usually acquired in the first instance in regard to those impulses which directly determine the conduct, gradually extends itself to the habitual succession of the thoughts; and in proportion as this is brought under the direction of the will, does the individual become capable of forming his own character, and therefore truly responsible for his actions." (p. 849.)

But the Will may direct the mind and conduct in a *bad* direction, and in this direction the evil, or Satanic character, is developed. If the man choose the good, and seek to assimilate his Will more and more to the Divine, in the same ratio will he approach the Divine Ideal, "and in proportion" (we quote Dr. Carpenter) "as this assimilation has been effected, does it manifest itself in the life and conduct; so that even the lowliest actions become holy ministrations in a temple consecrated by the felt presence of the Divinity. Such was the life of the Saviour; towards that standard it is for the Christian disciple to aspire."

In reviewing, critically, the doctrines promulgated in this chapter on the Functions of the Nervous System, we shall confine our remarks to one or two of those more salient points which we have so very briefly noticed. It will be observed that Dr. Carpenter derives his doctrines mainly from two sources—namely, analogies in structure, and direct observations and experiments on function. The doctrines themselves centre, in his views, as to the sensorial functions of the sensory ganglia; it will therefore simplify our inquiry into the validity of these views, if we consider to what extent it is true that these ganglia are the seat or organ of consciousness,—or, in other words, inquire *what is consciousness* and *what its seat.*

The argument from analogy is based on comparative neurology, commencing, mainly, with the articulata; and it is attempted to show that the cerebral lobes of invertebrate animals ("more properly, their cephalic ganglia") are anatomically homologous with the sensorial ganglia of the vertebrata. To this doctrine there are two important objections. In the first place, it is stretching the doctrine of homologies farther than it will bear. If we survey the two large families, we are struck rather with their *parallelism* than with their *similarity*. Neither in the minute structure of the cephalic ganglia, nor in their physiological endowments, is there any transition from the highest invertebrata to the lowest vertebrata. On the contrary, while the nervous system of the amphioxus (the type of the latter) is much more homologous to the nervous system of the low helminthoid articulata, the mental endowments of the highest articulata (the entomoid, and especially *insecta*) are much more analogous to the endowments of the highest vertebrata. It is not possible to repel the conviction, that the social insects are at least as much higher amongst the invertebrata in the scale of mind, as birds amongst the vertebrata are higher than the fishes; and consequently, that there must be something in the cephalic ganglia of the articulata analogous (if not homologous) to the more highly developed cerebral lobes in the vertebrata. On what structures depend—if not on these cephalic ganglia—all those wonderful instincts which mimic in their operations the arts of man? There is hardly a mechanical pursuit in which insects do not excel. They are excellent weavers, house-builders, architects; they make diving-bells, bore galleries, raise vaults, construct bridges. They line their houses with tapestry, clean them, ventilate them, and close them with admirably fitting swing-doors. They build and store warehouses; construct traps in the greatest variety; hunt skilfully; rob and plunder; they poison, sabre, stab, and strangle their enemies. They have social laws, a common language, division of labour, and gradations of rank. They maintain armies, go to war, send out scouts, appoint sentinels, carry off prisoners, keep slaves, and tend domestic animals. In short, they are mentally a miniature copy of Man, rather than of the inferior vertebrata. If the cephalic ganglia be the seat of all these instincts, then surely they are as much the homologues of the entire encephalon of man, as of those ganglionic masses to which the nerves of sensation pass. If, further, we consider function, it may be observed, too, that the doctrine of "unconscious cerebration," or cerebral reflex action (a function of the hemispheres), applies to their acts; for it is no improbable theory, that the special adaptations to particular circumstances manifested by these animals, is dependent upon the same principle of action as unconscious cerebration; and that the term *instinctive*, as applied to the congenital talents of men like Mozart, is literally correct.

It must certainly be received as a fundamental truth in inquiries of this kind, that a special construction of the nervous system is necessary to the performance of these acts. Now that construction is derived from the parent, and transmitted from parent to offspring with such unswerving regularity that the lapse of many thousands of years, external circumstances remaining the same, has not perceptibly modified them. Hence we are led to the laws of generation and development for the antecedents or causes of this special material arrangement, and therewith to a new world of *quasi* mental operations. For what do we see in action? Some wonderful power is at work in vitalized matter, whereby its elements fall into certain pre-appointed forms, and the amorphous mass of the primary germ is transformed by a series of *associated* changes, each linked to and dependent on the other, *like associated ideas*, into a variety of instruments, exquisitely adapted to the uses of the instincts, which will finally wield them in exact accordance with the laws pre-writ in the nervous centre. So that there is not only a special construction of the nervous system, but also of the machines and instruments which the functional operations of the nervous system put in motion and use. This immanent *constructive* power exhibits more marvellous faculties than even the *instinctive* faculties. The geometrical accuracy with which the hive-bee constructs her cells is a wonderful performance, inasmuch as it displays a knowledge of mathematics, only in recent times attained to by man; but that ingenuity of construction is itself insignificant when compared with the scientific knowledge displayed in the construction of the *instruments* possessed by animals. Man's knowledge of natural philosophy is far below that displayed by this constructive energy. He has partly, and in a small degree, attained it, so that he can imitate these instruments. He can construct excellent telescopes, microscopes, galvanic batteries, pneumatic and hydraulic machines. In the internuncial telegraphic wires he almost rivals the nerve-fibrils, and he has even attained to a knowledge of some of the forces and means by which these results are obtained. But, then, others are so much beyond his knowledge that he cannot, even with the utmost stretch of his ingenuity, invent a suitable hypothesis. They are linked to properties of organized matter, of which he cannot so much as form a conception, and which seem utterly beyond any means of research at present within his reach. How, for example, can the invariable reproduction in the offspring of the characters of the parent be explained? By what means is it effected, that a microscopic portion of vitalized matter, formed in a distant, deeply concealed, and apparently unimportant organ, contains within itself the power to be evolved, and act, and enjoy, manifested by the parent organism? In higher animals this is even more remarkable than in the lower; but in them it is the most marvellous of all, for nothing is more certain than that the minutest peculiarities in gesture and temper of parents—peculiarities as plainly dependent upon a congenital construction of the nervous system as are the pure instincts—reappear in the children from generation to generation. Nay, periodic changes are re-produced in the same order; for paralysis, insanity, the loss of certain teeth, and other morbid changes, often of a trivial kind, will appear in the members of a family in succession, at exactly the same age as that at which the parent was similarly affected. Has it ever been even guessed by what mechanism this inevitable order of events is impressed upon the amorphous microscopic germ-cell or spermatozoon of the parents? That it is in accordance with the laws of the formative power is more than probable, but the inexplicable nature of the fact proves how very much we have yet to learn, even of the fundamental principles of physiology. With these convictions, as to the little we know about the source or nature of that power, which in man we term reason, and in brutes instinct, it is obvious that all arguments drawn from our present imperfect knowledge must be necessarily most hypothetical and inconclusive. We cannot, however, omit this opportunity of expressing our conviction that there is an exact parallelism between the evolution of the material structures and the development of the mental powers; consequently, that just as we seek to illustrate the anatomy, physiology, and development of Man, by inquiries into the structure, functions, and development of the lowest forms of organisms, so, likewise, must we investigate his mental and spiritual nature. The working of the *homæsthetic** or *reason-like* principle in organisms is the analogue of the working of *Man's* mental powers; and the whole scope of recent facts tends to establish the general proposition, that the sentient principle of man is derived from or rather evolved out of this *quasi* sentient principle of vital organisms. Nor is this doctrine derogatory to man's nature; for, inasmuch as these innate forces can only be attributed to a direct act of

* This term, and *homæsthesis*, may be usefully applied to indicate the primary force on which the *physical* adaptation of means to ends in *all* vital acts depends.

Will of the Divine Mind (just as the force of gravity is an emanation from the same source of all power and intelligence), the genealogy of the Human Mind (if the phrase may be permitted) is traced directly to Him who is the revealed Father of the Spirits of all flesh, and who created man especially in his own image. This grand principle, that the efficient cause of all cerebral operations in man is similar to, if not identical with, the efficient cause of all those exquisitely adapted operations, in living matter, whether they be attributed to the nisus formativus, or germ-force, the power of instinct, &c., leads us directly to the doctrine that the order of nature in mind must be unravelled by a study of mental or *quasi* mental phenomena in creation. It is here that man will find the reflection of his own powers; it is in this large volume, spread out for his use, that he can best study the intimate workings of his compound nature, and thereby learn to know himself.*

In referring to what is meant by consciousness, we would note, in the first instance, that Dr. Carpenter uses the term *sensation*, rather as expressive of a fundamental *idea* or "intuition." The feelings of pleasure and pain are separable from that "consciousness of *external* impressions" which is Dr. Carpenter's definition of *sensation*. They are, indeed, quite compatible with the absence of any such consciousness. Nor are we quite sure that Dr. Carpenter always uses this word in the same sense. In such phrases as "guided by sensations," and "excited by sensations" (phrases of frequent occurrence), there appears to be something more ambiguous than the definition would imply. If a movement be excited by a sensation, in the sense used by Dr. Carpenter, it is excited, in part at least, by the consciousness, but we cannot separate the consciousness *as an excitor of motion*, from the will. Or, to take another example, in pleuritis the side affected is held motionless—a restraint effected unconsciously, which the will cannot imitate. This limitation of movement, Dr. Carpenter argues, is dependent upon the direct stimulus of sensation—meaning, we presume, the stimulus of pain—yet pain is not simply the consciousness of an impression, but of something distinct and different which follows upon that impression; the consciousness of an impression (sensation) implies the idea of outness; feeling (pleasure or pain), in its simplest form, has no reference whatever to the antecedent—the impression.

To understand more clearly the nature of sensation, and of mere feeling (as just defined), and their relations to the mechanism contained in the sensorial and hemispherical ganglia, we must study the function of the latter through those simpler structures with which they are homologous—namely, the spinal ganglia—and consider them as being simply repetitions of those ganglia. Their afferent nerves must also be considered as homologous

* "Instead of bringing the soul under the category of mere physical life, we bring life under the category of the soul. The same principle which shows itself in the human organization—which gives form and feature to the body—which adapts all the organs to their several purposes—which constructs the nervous system as the great medium of mental manifestation—which implants the instincts and prompts the senses to their appropriate work—this principle rises in due time to a *self-conscious* activity, in which it can recognise its own Divine origin, and aspire towards its equally Divine destination." (Elements of Psychology, by J. D. Morell, A.M., Part I. p. 77.) [This work has only come into our hands since this article was written, or we should have noticed it more particularly in its relations to our subject. We can only say here that to the medical practitioner and student it is invaluable; and that it is the truest, most lucid, and most practical system of philosophy we know.]

with the nerves of touch; and having, like the latter, special adaptations for the reception of the impressions which they have to convey to the sensorium—namely, the undulations of an etherial or elastic medium, &c. The *functional* analogues of the spinal ganglia (there seems every reason to think) are to be found in the double-nervous cord of the articulata, and the analogues of these in the branchial and pedal ganglia of the *mollusca.* In the class *Acrita,* it is doubtful whether there be a distinct nervous system; yet the *tissues* perform those functions which are in higher animals the endowments of a nervous system; and in vegetable organisms this is certainly the case—facts which not only incontestably prove that the principle of adaptation is independent of nerves, or a nervous system, but showing that *to the cell we must look for its source and seat.* It was one of the errors of Unzer that *adapted* movements indicated the existence of a nervous system, although it might be invisible. A large generalization shows that the nervous system is only a development in accordance with that law of specialization of structure and functions which is seen in the ascending series, and which attains its culminating point in man, in whom a special structure is evolved for the conscious and voluntary performance of those instinctive acts of animal life which may be all traced in the natural history of the primordial cell.

By taking a general view of this kind we observe fundamentally, first, a general design or plan laid down; second, means adapted to carry out the objects designed. The main object is, that the organism shall subdue matter to its uses, for the security of its well-being and the continuance of its species. The "*lex nostri conservatio*" which Prochaska lays down as the law of reflex actions, is in fact the *fundamental* law of all vital organisms, whether animal or vegetable, inasmuch as even the continuance of the species depends upon the preservation of the individual. Now, it is not possible to study these phenomena without inquiry into the *nature* of the *design* which it is the object of all vital changes to fulfil. Hence, whatever objections may be raised to the doctrine of final causes in physical inquiries, none can legitimately apply to its use in the metaphysical, in which a knowledge of the order (*ratio*) of events is but another term for reason. Consequently, the means provided for carrying out a design will be best studied in reference to the design. Vital organisms have a mechanism so adapted to external things and material agencies, that the dynamical element shall react in a way previously fixed. In organisms, whether animal or vegetable, we have therefore *impression*—dynamical action *according to a pre-arranged beneficial plan*—and corresponding material reaction. In the animals endowed with a nervous system, not only are impressions *received* at a distance from the seat of dynamical action, but the results of that action *take effect* at a distance. Hence the necessity for afferent and efferent conductors (nerve-fibrils), and an intermediate special organ for exciting and directing dynamical action (ganglia, or *congeries of cells*). It is quite certain that a consciousness of the occurrence of these pre-arranged events is not a necessary part of the series, unless we be prepared to maintain that the primordial cell or the lowest organisms—as vegetables and zoophytes—are endowed with it. It is, in fact, as impossible to say positively where consciousness arises in the animal scale, as to indicate the state of development in which the human being is first endowed with it. But if we cannot point out the exact link in the ascending scale which connects conscious with unconscious existence, we can at least indicate the probable circumstances under which consciousness will be developed in those animals endowed with it. The doctrines of Unzer are the most complete on this point. We have said that the mechanism of every organism acts according to a pre-ordained plan. Now, if an organism be endowed with consciousness, *pain* is felt when the changes in the ganglionic or centric mechanism are not in accordance with the beneficial pre-arranged plan—*pleasure*, if those changes are in accordance with the natural object and arrangement. Hence beneficial changes are "con-natural" and pleasurable, and are induced by con-natural impressions; injurious changes are "contra-natural," or painful, and are induced by contra-natural impressions.* It is not probable that any changes which *reach the consciousness* occur in the animals below man that are neither the one nor the other; and it is yet a moot question whether such occur even in man. How far the addition of pleasure or pain intensifies the material reaction which so constantly occurs without either, is yet to be determined. In many instances we are satisfied these feelings alone have no influence whatever on the accompanying acts; they are solely *coincident* with them, being simply dependent upon the same series of ganglionic changes. On a previous occasion we expressed and maintained the doctrine, that in many of the so-called *sensational* acts, the *feeling* was by no means a necessary element; in opposition to the contrary opinion of the late Prof. Reid.† Dr. Cowan, of Reading, favoured us at that time with notes of a case in point, which Dr. Carpenter refers to on this, as on previous occasions, to prove the sensori-motor function of the sensory ganglia. But this case was certainly forwarded to us by Dr. Cowan to establish just the contrary doctrine. The involuntary *cries* this patient uttered from any sudden visual or auditory impression were purely reflex; for Dr. Cowan expressly states: "The sensorial impression and the motion consequent upon it, appear irrespective of any painful sensation or mental emotion, and are only noticed by the patient *in consequence of the resulting movement.*"

We are inclined to think that the same fallacy lurks in many minds with reference to the operation of feeling as to that of the will. Just as in an act of will we do not direct *what muscles* shall be moved, but simply *will* the act suited to the *object* we desire to attain; so in those movements in which feeling only is in operation, we do not act in reference to the cause or antecedent, of which we have often no knowledge, but simply in reference to the *end* to be attained by the movement. In both circumstances there is a state of the consciousness and a mechanism; in both, the mechanism may act adaptively and fitly, and yet the state of the consciousness be not excited.

It is only just to Dr. Carpenter to observe here, that he is not altogether unobservant of the important difference between the *feeling* of pleasure and pain and the *consciousness of an external impression;* and from time to time he gives an interesting illustration of the difference, as when he quotes approvingly Mr. Morell's definition of the state of an infant: "To it the

* Unzer's Principles: Trans. of Sydenham Society, p. 102.

† See Correspondence with the late Professor Reid and Mr. Combe: Lancet, 1845.

inward world is *everything*, the outward world is *nothing*." This is doubtless the condition of many animals; they have no consciousness of *external* impressions, or what may be more correctly termed "perceptive consciousness"—no idea of outness or externality. Their movements, therefore, in reference to external things are not *guided* by sensations, but by that adaptation in the ganglionic centres on which reflex acts depend; but in them, as *sensational* animals, there is, in addition to reflex acts, the excitation of pleasure or pain, when that adaptation is put into action.

What organisms, then, (we first inquire,) and what part of their structure, possess this elementary form of consciousness, which we term *feeling*, and none other? Upon this point opinions will differ widely. We can hardly place even the most animal-like vegetables in the number. Dr. Carpenter commences with those animals having a single ganglionic centre with internuncial nerves, in which, however, the vital actions have no necessary dependence on consciousness. Perhaps we might form some such conclusions as the following. The nervous system being essentially a co-ordinating apparatus, constructed to combine all the machinery of the body together, and cause it to act as one homogeneous whole, for the attainment of the pre-arranged beneficial ends, we should look for the seat of *feeling* in that portion of the nervous system in which all the subordinate divisions meet, for it is as a whole—as an *individual*—that the animal feels. In the tunicate mollusca, this centre is undoubtedly the single ganglion which gives nerves to the branchial orifice and tentacula, to the mouth, and to the motor apparatus. In the mollusca that have a symmetrical nervous system, as the oyster, this centre would appear to be the commissural band which unites the two branchial ganglia. The function of this band is particularly illustrated in the nervous system of the *chiton marmoratus*. In this animal the changes excited by an external impression are possibly propagated throughout the entire nervous circle by means of this band, and feeling takes place (if the animal be endowed with feeling) at the moment that the traject is completed. The absence of ganglia on this cerebral ring or ganglion is attributed to the absence of eyes and tentacles in the animal; it is therefore, probably, one of those animals without any sensation or *perceptive* consciousness. It may feel physical pleasure or pain, but probably even that in only a small degree. The ring is rather a co-ordinating *motor* than a co-ordinating *sensorial* apparatus; it is the rudiment, therefore, of the cerebellum, as much at least as of the medulla oblongata, or the sensory ganglia.

In ascending the scale to fix upon those animals which have *perceptive* consciousness, we are led to note those in which the senses of *hearing* and *vision* are developed more particularly, because in these the working of the adaptive principle is so highly developed as to prepare for and react upon *complex* qualities of matter. Nor can we deny to animals like the higher invertebrata, as the social insects (in which these senses are so highly developed), the power to act in accordance with the idea of causation—an idea which is the very basis of the idea of outness, or perceptive consciousness. Such an idea is only *perfectly* reached by the *co-ordination* of the senses, or more particularly of hearing and vision with touch, and when reached is a *general* idea, requiring a co-ordinating centre from which it can react on and put in motion the entire motor apparatus. Hence with perceptive consciousness we are to look for a co-ordinating sensorial and motorial centre, distinct, functionally at least, from the centre of mere feeling, or what has been termed *common* sensation, and in immediate relation with the nerves of sight and hearing. This centre will be developed in size and be complex in structure in proportion as these cerebral senses and the locomotive machinery are developed. To look for these in the encephalon of the invertebrata would be hopeless. We must turn, therefore, to the vertebrata.

To render this matter clearer, if possible, it would be well to inquire, first, what centric homologues *have to be looked for* in the encephalon of the vertebrata. 1st. There is probably a centre of the constructive and reparative instinct. Of this we know so little that its existence is hardly suspected, and will perhaps be altogether denied. There are, however, numerous unexplained facts which require some such theoretical centre—as, for example, all those obscure sensations caused by changes in the blood and tissues, attributed to the *vis medicatrix* and all its associated instinctive acts. 2nd. A centre of the reproductive instinct. This has been fixed in the cerebellum by the followers of Gall; but we fully agree with Dr. Carpenter in his objections to this doctrine. A large number of facts, anatomical, physiological, and pathological, demonstrate, however, that such a centre exists, and with relations little less extensive than the *conservative* instinct. In the invertebrata, life is hardly more than a series of phenomena in connexion with the function of reproduction. In the social insects this is most obvious. In the vertebrata this predominance of the instinct diminishes as we ascend the scale, but even in man the ovaria and testes are in intimate relation with the whole organism, and have a most important influence on the instincts, feelings, and intellect. Wherever this centre may be, it is certainly in as close and direct relation with the constructive as with the sentient centres. If it be placed with the conservative centre, in the cerebral commissure or nervous ring (as seems the most probable hypothesis), Dr. Carpenter's opinion, that either the central portion of the cerebellum or some part of the medulla oblongata has a special connexion with the generative function, harmonizes with the theory. In addition to these, we have to look, 4th, for the ganglionic centre of each nerve of sense, where the pre-arranged dynamical changes occur, which are induced when an impression reaches it and excites adapted acts. This perhaps *is*, although it need not necessarily be, the seat of feeling; but we have also to look for, 5th, the co-ordinating centre of *feeling* for *all* the nerves; 6th, for the corresponding centre of adapted motions; and, 7th, for the great co-ordinating centre of the perceptive consciousness. The three last may perhaps be hypothetically indicated, and as to the fourth there is no great doubt.

Various methods may be adopted to fix upon these encephalic centres. We may, firstly, trace the afferent nerves to their respective centres; but this is of doubtful validity, for our minute anatomy of the centres is as yet hardly settled. *Size* evidently bears an important relation to extent of function, and *colour* is a characteristic not sufficiently estimated. The clustering of pigment-granules about the roots of the spinal nerves in the amphioxus is an important fact in relation with similar deposits in the encephalic centres, as, *e.g.*, the *locus niger*. The *form and structure* of the nerve-centres is a valuable characteristic; it is not possible to compare sections of the corpora olivaria and cerebellum without seeing a *similarity* of structure and inferring *analogy* of function. The *difference* between these and sections of the corpora striata or optic thalami leads equally to the inference of *difference* in function. The *relative* sizes of parts in animals with opposite or different instincts is an important point. Thus in the ruminantia the olfactory ganglia are large, the corpora geniculata interna small, the posterior of the tubercula quadragemina small. In the carnivora —not without exceptions—the contrary is seen. The variations in *size* of these centres in different classes of mammalia is certainly a highly important fact. In the cat and stoat the posterior are the larger; in the pig, horse, ass, sheep, and deer, the anterior: here the comparative size points distinctly to a difference in function. It seems not improbable, indeed, that the anterior pair have a ganglionic relation to the olfactory ganglia; for where the latter are very large, so are the former, and *vice versâ*. Thus in the ruminantia with large olfactory ganglia, the anterior are the larger—in the cetacea with small olfactory ganglia, they are the smaller. The secondary instincts of the animal as regards food and defence, and its corresponding muscular movements, will be determined very much by the senses used in obtaining food and in self-defence. Where the eye is used (as in the feline carnivora) for directing the muscular acts necessary to capture the prey, the posterior are the larger, although the olfactory ganglia be large from the sense of smell being used to *discover* it. It is not improbable, therefore, that these ganglia constitute a sensorial co-ordinating apparatus to the cerebellum—or are its ganglia, as Langenbeck terms them.

Although we are very much in accordance with Dr. Carpenter as to the sensorial functions of the posterior cerebral ganglia or optic thalami, the general remarks we have made respecting the tubercula quadragemina apply also, we think, to the corpora geniculata. It is obvious, from various circumstances, that they are a structure distinct from the thalami. In the first place, they appear as if *appended*, because they destroy that flattened spheroidal or ovoidal form which is the characteristic of the true ganglion. Secondly, just like the tubercula quadragemina, they vary very much in size in different animals, and have varying relations to the latter. In the carnivora the internal pair are very large—in the ruminantia very small, except in the horse, in which they are connected with the posterior tubercles (testes) by a strong fasciculus of fibrils. In the monkeys they are in connexion with the anterior pair also, and with the optic nerve.

While these considerations impress us with the conviction that the anatomical relations of even the nerves of vision, and the course of the impressions transmitted along them, are still hypothetical, we cannot forget that the functions of certain important ganglionic structures situate within the encephalon are wholly unknown. The corpus dentatum of the cerebellum is one of these, the conarium or pineal gland is another; not to mention minor masses of *grey* matter. In view of these difficulties, the question so ably handled by Dr. Carpenter is as yet (we cannot but think) far from a satisfactory solution. Having both looked closely into the literature of this matter, and made anatomical inquiries, that we might faithfully fulfil our critical functions, we may perhaps be excused directing the attention of Dr. Carpenter, and of neurologists in general, to the necessity of guiding the scalpel and the microscope, not only by a well-considered theory of development of the nerve-centres, but also by a constant reference during anatomical research to the instincts and passions of animals—to their phrenology—if the phrase may be permitted. We will state briefly the special ideas we entertain. First as to the anatomy. In the simplest form of nervous system we have three elements—1. Afferent fibrils carrying impressions to a centre; 2. A centre made up of cells to co-ordinate the impressions so carried; 3. Efferent fibrils to transmit the results of that co-ordination. The first and third are simply *internuncial;* the second, corresponding to the concretious matter, is co-ordinatory (or analogically) *cerebral.* In all organisms above the simplest there are more ganglia than one, and since it is necessary that these should act together in unity, they have to be connected. Hence there are *internuncial structures* passing between the ganglia, varying, however, very much in anatomical composition and in function. In the earliest and simplest form the structure is made up of fibrils, and is *purely* and *simply* internuncial; but inasmuch as these fibres pass between ganglia, they are *commissural;* they would be better termed *internuncial commissures.* In the symmetrical animals another form of internuncial commissure is required—namely, to connect and combine into common action the two symmetrical halves of the body; these are internuncial but also *connecting commissures.* The anelata and articulata afford abundant illustrations of these structures. An instructive instance of this kind is presented by the nervous system of the sand-hopper (*Talitrus Locusta*).* In this animal the ganglia of each segment and of each half of the body are distinct, so that there is a double series of ganglionic centres united by longitudinal and transverse internuncial commissures.

The development of the insecta shows another form of connexion between ganglia—namely, fusion or *coalescence.* This is often indicated, as Dr. Carpenter remarks, by a bilobed form of ganglion, as when the ganglia of the two halves are developed and coalesce, and the longitudinal commissures remain; or by a quadriform ganglion, when the two coalesced ganglia of one-half of the body coalesce with the two coalesced ganglia opposite to them in the chain. This is well seen in the *Polydesmus.*† Sometimes this lobular form is lost from an equal coalescence of all the ganglia, as in the *Iulus.*‡ This is seen also in the cord of the vertebrata. The coalescence of ganglia may be real or apparent only, internuncial or co-ordinating. In the latter case, there is either a *fusion* of the grey matter very common in *invertebrata,* or a distinct commissure of grey matter, usually designated a *soft* commissure in the vertebrata. Now this structure being not simply connecting (as a commissure of *white* matter is), but having, in reference to the ganglia it connects, the same relation as a simple ganglion bears to its afferent and efferent nerve-fibrils, it is obvious that it ought to have another designation than *soft* commissure. What that designation should be we will not venture to decide; but when the structure co-ordinates the ganglia of the two sides of the

* See Grant's Outlines of Comparative Anatomy.

† Owen's Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals, vol. i. p. 201.

‡ Ibid. p. 199.

body, it is (we think) strictly analogous to those great central co-ordinating ganglia, the hemispheres or cerebrum, and the cerebellum. This all-important analogy deserves a little development as to one or two points. 1. This tissue is not peculiar to *soft* commissures, for we constantly find grey matter developed in white commissural structure, as in the œsophageal ring of the articulata, the *locus niger* of the crura cerebri (considered as longitudinal internuncial commissures), the *tuber cinereum*, &c. Now, as the histological composition of this grey matter indicates its co-ordinating or ganglionic function, it must therefore be considered in each case as a *ganglion*, superadded to co-ordinate certain of the fundamental or primary ganglia, and using the white fibres in which it is embedded as an internuncial medium. 2. This ganglionic, *superadded*, or *co-ordinating* commissural structure is sometimes seen to be projected from the linear axis. In this case it assumes the *form* of a ganglion, and is connected with the linear axis by means of strands of white or internuncial fibres, which appear as its *crura, peduncles, cords*, &c. The *conarium* or pineal gland, with its peduncles, appears to be a structure of this kind, and in the lower vertebrata, the cerebellum, the hypoarium, cerebral lobes, &c., are the same. This superadded ganglion may be considered as fundamentally constituted of two symmetrical halves, coalesced, like the spinal ganglia of anelata, &c., and may therefore, 3rdly, be projected from *each half* of the linear axis. In this case commissural structures will be necessary, such as the corpus callosum of the cerebral hemispheres; or the ganglion itself may divide and project lateral lobes, as we learn from the comparative anatomy of the cerebellum, which, in its earliest form, is a mass of ganglionic matter similar to the pineal gland, or ganglion, as the latter would be more properly designated. In the development of the cerebral lobes, and in their comparative anatomy, we can trace the operation of the same principle. They are far smaller in fishes and in the lower invertebrates than the sensorial ganglia which they co-ordinate; but in the higher vertebrata, and most distinctly in man, they constitute the great ganglionic commissure of the whole chain—the supreme internuncial and co-ordinating ganglion projected from the apex of the linear axis.

While, however, the physiologist is thus unravelling the central anatomy of the nervous system, he will do well to remember that *size*, as well as anatomical structure and relation, is an important clue to function, so far, at least, as regards the grey or ganglionic structures. It is from this general fact that we deduce the incontrovertible proposition as to the function of the hemispheres in man and the higher vertebrata. The same fact applied to comparative anatomy will enable us to determine the relation between the development of homologous ganglionic centres, whether primary or superadded, and the working of the instincts; and thus point out to us what in the human encephalon are the true functions of those masses of fused, coalesced, and intertwined internuncial structures, and primary and superadded ganglia, which make it up.

Although we are much in the dark as to the functions of these structures, there seems little doubt that the cerebral hemispheres are the seat of the *intellectual* operations and the region of ideas. It is probable, also, that they are *not* the seat of those various co-ordinating *motor* centres we have pointed out. The dynamical changes which go on in them do not *necessarily* lead to muscular movements, but often only to changes in the state of the consciousness; whereas it is more than probable that dynamical changes in the *striated ganglionic centres* are *necessarily* followed by movements, whatever change in the consciousness may be effected at the same time. The development of the hemispheres as distinct organs cannot be traced in the invertebrata; they are only first seen in the fishes as a rudimentary homologue of the *anterior* lobes. There can be no doubt, however, we think, that although there may not be a separate and distinct tissue discoverable in the cephalic ganglionic mass of the invertebrata, the acts of the highest of the order sufficiently prove that the analogue of the hemispheres is there. There is no more generally acknowledged doctrine than that the development of the anterior lobes marches *pari passu* with the development of those faculties in man by which he ascertains and orders cause and effect,—or, in other words, perceives and determines *the order of events*. The state of consciousness with which these powers (the reasoning and voluntary) are related, is altogether different from that of simple *feeling;* in the one there is a perception of outness, or externality, and, consequently, a consciousness of the immediate antecedent to the sensation in the relation of a cause of the feeling; in the other there is simply a feeling of pleasure or pain, with no reference to the cause. Hence, with the perceptive consciousness, there are indissolubly conjoined certain fundamental faculties, which the phrenological school associate with the anterior lobes—namely, "causality," "comparison," and "the perceptive faculties" in general. The very early development of the anterior lobes in the vertebrate scale has been usually adduced as an argument against these doctrines of phrenology; but it is clearly in their favour; for if the view here given be correct as to the nature and seat of perceptive consciousness, these lobes must *necessarily* be developed anteriorly to those that are in relation to the moral powers and the sentiments; inasmuch as a knowledge of the qualities of matter and its relations to the organism necessarily precedes a knowledge of moral relations. It is indeed not improbable, that, as the seat of perceptive consciousness, the anterior lobes have the special relations to the middle and posterior lobes of a co-ordinating apparatus, and perform those functions in mental operations which the co-ordinating sensorial ganglia perform in muscular acts.

The question then arises, whether these lobes be the seat of sensation or consciousness, in common with the sensory ganglia; or whether we should adopt Dr. Carpenter's views, which place it exclusively in the latter? To our minds there is nothing in known analogies, whether of structure or function, which precludes the idea that the seat of perceptive consciousness (as distinguished from the simple feeling of pleasure or pain) is in the seat of thought itself. Perceptive consciousness must be looked upon, according to our views, as a development of that innate power which adapts material organisms to pre-ordained ends. It is evolved as we rise in the scale of being, *pari passu*, with the material organ, and its sphere of action embraces a materially wider and wider field; but that action is fundamentally based on a presentation to the consciousness of the modes of action of the innate primary power itself. Just, therefore, as the feeling of pleasure is excited when the dynamic changes in the sensorium are in accordance with the pre-arrangements of the organizing and directing instinct; so the ideas of Beauty and Truth are most perfect, and give the most perfect pleasure, when the *ideas* of the instinct are so fully evolved in the consciousness, that the operations of the two powers are in perfect harmony. Whenever this evolution of the consciousness is not attained in the same degree, the ideas of truth and beauty are imperfect; for the quality of beauty in an external object is nothing else than the congruity of its external characters with the archetype of the Divine mind pre-writ in the material organism. Thus, then, to the feeling of perfect pleasure, with the idea of the perfectly Beautiful, *two things* are necessary: firstly, external characters in the object, perfectly attained according to the pre-arranged order of creation; secondly, a substratum in the sensorium, developed in perfect adaptation to the object thus perfectly developed. When an impression so originating from such an object reaches an organism so evolved, the pleasurable idea of the Beautiful is felt, and we cannot think that the seat of the pleasurable feeling which accompanies this mental process is elsewhere than in the seat of the process itself.

One great source of misapprehension in ideas of this kind is to be found in confounding different ideas together. Thus, a woman may be pleasing because she commends herself to the instinctive feelings of man as *the male*, and yet be wholly devoid of Beauty; while a woman may be as beautiful as the Medicean Venus and yet not be pleasing, because her form neither awakens a feeling in the mind, having its basis in the *instinct*, nor has a substratum been evolved in the intellectual sensorium in congruity with the impression derived from her form. That this evolution of the intellect requires a co-ordinate evolution of the material organ, is a proposition in accordance—not only with the entire series of facts observed in reference to the functions of the nervous system, but—with direct observation; it is, we think, undeniable. It appears to us also equally well-established, that in cases like that of Mozart, as in all cases of instinctive genius, there is a perfect evolution of one portion of the intellectual powers and of its corresponding material basis. If the same perfect evolution of *all* the mental faculties and their material organs were to occur in an individual, he would be a perfect man, able to appreciate, with the ease of intuition, ALL that is True and Beautiful; and the accompanying dynamic changes in his cerebrum would go on with that perfection, and freedom from painful feeling (fatigue), which are alike the characteristics of automatic acts, of pure unchecked instinct, and of exalted genius.

Much more might be written upon this singularly interesting and most important subject; but we forbear, having already transgressed beyond our prescribed limits. We are glad to see it taken up so zealously and earnestly by Dr. Carpenter, for we are sure that he will not rest satisfied until he has placed the doctrines of cerebral physiology on a wider and more intelligible basis. Nor will he shrink (as is proved by various admissions in this current edition of his Physiology) from the reconsideration of his views, and the disavowal of any doctrines which to him may appear without sufficient support. We cannot omit, however, stating here, that Dr. Carpenter has not done that justice to his subject which he might have done; for in the spirit of impartial criticism we must remark, that this chapter on the Functions of the Nervous System bears marks of haste and imperfect thought. There is also a defect in the arrangement of the whole subject, and, therefore, a want of lucidity and completeness. Dr. Carpenter's ponderous book is a miracle of industry: following so soon upon his 'Principles of Physiology,' one cannot but admire the gigantic force of mind which such labour indicates; but we must remind Dr. Carpenter, that no mind, however gigantic, can wrestle successfully with *time*, and that *sæpe vertere stylum* is a maxim as applicable to philosophy as to poetry.

Carefully renewed research is more than ever incumbent on the physiologist. Henceforth, he and the metaphysician must tread a common road—the metaphysician, by abandoning his exclusive researches into his own consciousness, and extending his inquiries into the phenomena of MIND, as displayed in creation; the neurologist, by abandoning his too exclusive attention to the material organization, and to merely *vital* phenomena;—while both seek the relations of mind and matter in the *quasi*-mental operations and co-ordinate material changes of the vast chain of organized beings. These are instinct with the DIVINE. Their phenomena constitute the book in which man may study the natural history of his own nature, and through which he may unravel the mystery of his being.

Thomas Laycock.

REVIEW II.

1. *On the Relations of Uterine to Constitutional Disorder.* By F. W. MACKENZIE, M.D. Lond., Physician to the Paddington Free Dispensary for Diseases of Women and Children, &c.—*London*, 1852. pp. 117.
2. *A Practical Treatise on Inflammation of the Uterus, its Cervix and Appendages, and on its Connexion with Uterine Disease.* By JAMES HENRY BENNET, M.D., Member of the Royal College of Physicians, &c. Third Edition.—*London*, 1853.

THE most striking peculiarity of the female sexual system is, that it is in a state of functional activity only during one-half of the ordinary term of human life. For the first fourteen or fifteen years it is undeveloped, isolated, entering in little or no degree among the organisms of the human body, showing no sympathies, receiving no impressions, and distributing no influences. After a period of about thirty years of physiological and pathological activity, this system again subsides into a state of functional quiescence, but it never becomes so isolated as at first; it ever retains a certain amount of sympathy with other organs and systems, and, subject to pathological changes, it exerts a considerable influence upon the general health. It is, therefore, chiefly during these thirty years of functional activity that the uterine system presents a subject of the highest interest both to the physiologist and to the pathologist. At puberty, the system starts into vivid vitality, and assumes that predominance which it is to maintain for a certain period. Not only is there an energetic development of these organs themselves, but under their influence the action of every organ of the body and of the whole collectively is controlled and modified. Nor is this power limited to the body, for the mental characteristics of the female are quite as much altered. From a child in thoughts, tastes, and habits, we see her suddenly expand into a woman, with higher aims, more exquisite feelings, and new aspirations. A still higher state of activity, and one more potential as regards the general condition, bodily and mental, is attained during gestation, during which process it seems as though the entire vital force were directed to the one object of reproduction. All the sympathies with the uterine system are more exquisite and intense, and, for the time, we may say that utero-gestation governs both body and mind.

This influence of the uterine system upon other organs, modifying their action for good or evil, is one great peculiarity which deserves our careful study. But another and an opposite characteristic is the reciprocal influence of other organs upon it. Not activity only, but exquisite sensibility also, characterizes the sexual system during its period of awakened energy, and this sensitiveness exposes it to impressions and modifications from other organs as well as from external circumstances. It is only by a patient observation of these peculiarities and mutual influences that the physiology of female life can be understood; and, in like manner, it is only by an analogous investigation that we can arrive at a large and complete comprehension of its pathology.

As regards the pathology of this system, with which alone we are concerned at present, the first and most striking circumstance is the local change which occurs at puberty, and is maintained until functional activity ceases, with its irregularities, excesses, diminution, or suppression, and the local organic diseases to which the parts are obnoxious. This portion of the pathology has naturally attracted the principal attention of those who have devoted themselves to this department of medical science. But the morbid influence exerted by these organs when diseased, and in some cases when apparently not diseased, is also a very wide field, and one that has scarcely been sufficiently examined. Yet our knowledge of the physiological influence so exerted, its universality and intensity, ought not to leave a doubt of the equally subtle and extensive morbid power which must result from any serious derangement of this system.

Lastly, the effect upon the uterine system of disorders or diseases of other organs or systems, cannot but hold an important place in a philosophical view of the female sexual system, endowed, as we know it to be, with a high degree of sensibility, and acknowledging the most delicate and distant sympathies.

Keeping this threefold view before us—that is, the effect of distant organic or functional disorders upon the uterine system, the local diseases of this system, and their effects upon other organs or upon the constitution generally—we propose, as opportunities may offer, to lay before our readers the result of modern research upon each division of the subject; and at present we gladly avail ourselves of Dr. Mackenzie's volume to illustrate the influence of certain general or local morbid conditions upon derangements of the uterine system:

"In the prosecution of this subject," Dr. Mackenzie observes, "much may be gained from physiology, for it is to the point at which physiological actions become pathological, that our inquiries should be mainly directed. Much, again, is to be learned by collecting the histories of individual cases; and this should comprehend not only their immediate symptoms and causes, but their remote antecedents also. By studying the particular circumstances or combination of events which may have preceded or given rise to any particular lesion, we are enabled to anticipate its occurrence whenever similar circumstances co-operate; and this knowledge is eminently equivalent to power: with such power, medicine may claim to rank foremost with those sciences which have contributed most largely to the happiness and welfare of mankind." (p. 2.)

At the same time we feel bound to say that a more stringent logic than is usual among medical writers will be necessary before we can arrive at so desirable a consummation, and we think that the investigations of Dr. Mackenzie would be improved by a more rigorous process of reasoning being applied to them. It is by no means always true, though in many cases it may be admitted, that one disease following another, or succeeding to some morbid influence, is caused by either; nor can we always consider the order of time in which the patient or her medical attendant becomes aware of the presence of certain symptoms as the test of the order in which the morbid actions which gave rise to them have occurred. We have made these observations to show the caution which ought to be observed in all inquiries into the causes and effects of disease, and especially of diseases affecting the uterine system, with its intimate and reciprocal sympathies.

Now, of the subjective sources from whence we might expect morbid influences to be exerted upon the uterine functions, we may enumerate as principal, the nervous system, the chylopoietic viscera, the kidneys, the blood, and certain constitutional diatheses: whilst we may observe certain modifications induced by the prevailing epidemics or the general medical constitution of the year, besides the ordinary external causes. All these subjects are worthy of a detailed investigation, but the two first alone occupy Dr. Mackenzie's present volume, and therefore to them our present inquiry will be limited.

No one familiar with the subject can doubt that certain disturbances of the nervous system may derange the uterine functions. For example, strong mental emotion, joy, sorrow, or fear, may produce immediately menorrhagia or amenorrhœa. We have had an opportunity of ascertaining that a large proportion of female prisoners have menstruation suppressed for some time after trial. Dysmenorrhœa has been the result of prolonged mental distress and consequent nervous disturbance. The effects of nervous disorder upon pregnancy and parturition are sufficiently common to have come under the observation of all practitioners. So that, as regards the effect of these greater disturbances, we may assume the fact to be admitted: as regards the minor states, it may require a closer observation to arrive at the same conclusions. Some difference of opinion will probably obtain as to the mode by which these effects are produced. Dr. Mackenzie, adopting the views expressed in Quain's 'Anatomy' as to the functions of ganglia, and considering them as nervous centres, receiving impressions through their afferent nervous fibres, and transmitting motorial stimuli, observes, that—

"Consistently with these views, then, it may be assumed that, so long as the uterine ganglia receive, through afferent fibres, impressions of a normal character, so long will normal ganglionic actions ensue, giving rise to healthy uterine action, both in regard to function and structure; whilst, on the other hand, impressions of an abnormal character will excite ganglionic actions equally subversive of both. The *modus operandi*, then, of disturbed states of the nervous system, irritative conditions of the nervous centres, and disorders of remote organs, in the causation of uterine disease, is either by the uterine ganglia participating in such states of disorder, or receiving impressions which tend to morbid actions. Regarded, then, in this point of view, we cannot be surprised that the majority of causes which tend to uterine disturbance should have a constitutional rather than a local origin; for whatever impressions are made upon the body, whether through the mind or otherwise, which are capable of being reflected upon the uterine ganglia, and are calculated to excite morbid actions, are so many ways in which uterine disorder or disease may be induced." (p. 14.)

The first nervous affection adduced by Dr. Mackenzie as a cause of uterine disorder, is *spinal irritation*, which he regards as a "distinct and idiopathic affection." Without entering upon this latter question, we shall quote Dr. Mackenzie's supposed case as an illustration of the connexion between this affection and the uterine disturbance:

"A female, perhaps in delicate health, receives a sudden fright or shock, which is followed, either immediately or remotely, by uterine symptoms. The spinal column is examined, and tenderness found in the lumbar or sacral region: possibly no other organic lesion is discoverable, and in this case the application of a sinapism or some other counter-irritant to the tender portion of the spine, is followed by a cessation of the uterine symptoms, without any other kind of treatment."

In support of this view, Dr. Mackenzie relates a number of cases in which the uterine disorder disappeared when the spinal distress was removed, and others in which there existed disorder of the chylopoietic viscera concurrently with spinal irritation and uterine disturbance, the latter disappearing when the former complaints were relieved. He has also given a table of 36 cases in which the uterine disease had been preceded by, and apparently connected with, mental causes, and in 17 the presence of spinal irritation existed in a decided manner, whilst in 5 only was it met with independent of such causes.

Now, with these facts before us, the question arises, that "if it be admitted that the causes in question can so affect the spinal cord as to give rise to functional disorder of it, can such disorder give rise to uterine derangement?" "To this question," says Dr. Mackenzie, "I have no hesitation in giving an answer in the affirmative. The cases I have selected point in their history to this conclusion, analogy is alike in favour of it, and it is impossible to reconcile the results of treatment with any other view." We think that to a very great extent the author has established his point, but there are two considerations which offer themselves as limitations:

1. In the more marked cases—for instance where spinal irritation has existed for some time, and has been followed by uterine disturbance—the sequence may perhaps be admitted to stand in the relation of cause and effect; but in the slighter cases of uterine pain or uneasiness, with spinal tenderness, both of which are cured by a sinapism, we confess we should hesitate in giving to the latter the name of "spinal irritation" (meaning by that "an independent and idiopathic" affection), or in dignifying the former by the title of disease at all. A slight uterine pain, which always gives a pain in the back, may be transitory, whether treated or not; but the supposed cause and its supposed effect are both too slight to afford much support to Dr. Mackenzie's argument.

2. But the second consideration is a greater obstacle than the first—viz.,

that it is extremely difficult and often impossible to determine which is the primary affection, especially in the severer cases. Even Dr. Mackenzie seems to feel this difficulty, for he remarks that "it is quite true that we cannot always determine whether the uterine or the spinal affection has the priority, because we have not often the opportunity of investigating the point at a sufficiently early period of the case; but some who have had this opportunity affirm that spinal irritation exists at least from the commencement of such cases." Our own experience does not, however, confirm this latter statement; and although our not witnessing such cases from the commencement, but being dependent upon the confused, uncertain, and inaccurate statements of the patient, may be one cause of the obscurity and difficulty, another and a far more important one is the fact that we cannot always date the commencement of uterine disease by symptoms, inasmuch as it may exist for some time without giving rise to any, or to any of sufficient magnitude to attract attention. So that, granting that spinal irritation and uterine disorder may be present in any given case, our own experience would lead us to conclude that either may have given rise to the other, and that, in point of frequency of causation, the latter as often gives rise to the former, as the reverse.

"With regard to the particular forms of uterine derangement which are met with in connexion with this affection (spinal irritation), I find, on referring to the twenty-six cases which I have tabulated, that hysteralgic affections occurred in 18; dysmenorrhœa in 9; menorrhagia in 4; amenorrhœa in 2; leucorrhœa in 13; and irregular menstruation in 2. But these affections were variously associated in different individuals; a fact which of itself would point to their constitutional origin. The predominance of hysteralgia is very striking, and has been noticed in general terms by other writers." (p. 29.)

This we should expect, and so far, we think, it is in favour of Dr. Mackenzie's opinion; but we must repeat our caution, that it is by no means easy or wise to pronounce hastily, whether the menstrual disorders and leucorrhœa are caused by, or have themselves caused, the spinal irritation.

Let us now turn to the consideration of the effects of disorders of the digestive system in the origination of uterine disease, and we do so with less uncertainty involved in this than in the former subject. No doubt that, to a certain extent, the observations already made upon the difficulty of determining which is the cause and which the effect in the case of spinal irritation co-existing with uterine disease, apply to this class of cases also, with this difference, however, that probably no one will be disposed to question the adequacy of such disorders to the effects they are supposed to produce. The influence of derangements of the stomach upon every organ of the body is so well established, and their power of exciting varied, minute, and distant sympathies, as well as of producing a more marked deterioration of the general health, is so completely admitted, that no well-informed physician will deny the possibility, and few the probability, of their originating uterine disease. The sympathy between the stomach and the uterus in disease is not more remarkable than in certain states of health. Upon this subject Dr. Mackenzie remarks, that—

"However regarded, there is no system of organs which is so liable to disorder as the digestive, whether from the operation of causes which are directly applied to them—constitutional derangement, mental affections or sympathy; and there is none, the disorders of which react so injuriously upon the constitution at large, or the uterine organs in particular. There are few instances of uterine disease which are not attended with some form of chylopoietic disorder, either as a cause, a consequence, or an accidental complication; and upon this association of morbid actions, or rather upon the reciprocal influence which the disorders of one organ exercise upon those of the other, may be said to depend much of the difficulty which attends the treatment of uterine diseases. On looking to the table published in the preceding part of this inquiry, it will be found that digestive disorder was an almost invariable concomitant in the cases referred to; that in by far the larger number it had existed as an antecedent; whilst the history and results of treatment which will hereafter be alluded to, tend to show that this antecedence was not merely casual, but that the chylopoietic disorder was generally the cause of the uterine." (p. 33.)

After distinguishing the circumstances in which these disorders are likely to arise, our author remarks—

"That it will be found that nearly all of them apply with particular force to the habits and constitution of the female as distinguished from the male. In her the brain and nervous system are more sensitive, and consequently more liable to be disordered by mental and other causes. In her the constitutional powers are less vigorous, and these are often still further reduced by sedentary habits, and a disregard of proper hygienic measures. Education in many instances, and particular pursuits in others, impair still more the tone of the nervous system, and proportionately injure the constitution: whilst, from the concurrence of these several causes, an impoverished condition of the blood, sooner or later, is induced, which co-operates with them in originating and maintaining an irritable and disordered state of the stomach and digestive organs; and the disorder thus secondarily induced seldom fails to react again upon the constitution, and thus to produce a state of general irritation, which is favourable to the production of various local diseases." (p. 34.)

We quite agree with the practical conclusions drawn by Dr. Mackenzie, after a full and cautious examination of the subject—viz., that disorders of the digestive organs may be followed by uterine disease: that in certain cases the former may be apparently slight, while the latter is very prominent: that the converse also occurs—that is, the continuance of uterine disorder almost always involves disturbance of the digestive system; and that, in certain cases, the formidable injury to the general health may be the effect rather of the secondary affection of the digestive system than of the primary disease.

"Impressed with the importance of these and similar considerations, I have for many years been accustomed to study uterine diseases with especial reference to their constitutional origin and causes; and in taking the histories of cases, I have more particularly sought for information on this point, which I have carefully noted down. The result of doing so has been to convince me that constitutional causes have more to do with the origin and continuance of uterine diseases than is commonly supposed; that chylopoietic disorders—sometimes singly, but more frequently associated with others—do in the great majority precede and give rise, first, to uterine irritation; and, secondly, to various uterine disorders; and further, that a proper and full appreciation of this circumstance is necessary to their successful treatment. The extent to which chylopoietic derangement is met with in connexion with this class of disorders, either as a cause, a consequence, or an accidental complication, would be altogether incredible, if facts did not directly support the observation. Thus of the hundred cases referred to, and tabulated in the earlier part of this paper, chylopoietic disorder existed as an antecedent in 82; and as a concomitant in 97. In 33 it coexisted with spinal irritation; in 87 with anemia; and in 20, anemia, spinal irritation, and chylopoietic disorders were met with concurrently." (p. 41.)

A very interesting subject for inquiry is, What are the symptoms of chylopoietic disorder most frequently observed in connexion with uterine disease? and though we do not think a very accurate limitation can be made, we are inclined to agree generally with Dr. Mackenzie, that they are characterized—

"By certain deviations from the healthy state of the tongue, more especially on awaking in the morning; by a degree of fretfulness of temper or despondency of mind, which is unusual to the patient, and unwarranted by her circumstances; by some uneasy sensation, either constant or occasional, about the epigastrium; by some degree of irregularity in regard to the action of the bowels; by some unnatural appearances in the urine or stools; by variability of the appetite, which may be either excessive, defective, or depraved; by restlessness at night and broken and unrefreshing sleep, whence patients arise in the morning with a feeling of weariness and fatigue; by a disposition to blotches and eruptions on the skin; the feet and hands being often cold and clammy, and perspiration readily induced by exercise." (p. 52.)

The uterine disorders to which affections of the digestive organs most commonly give rise, or to which they induce a susceptibility, are, pain and uneasiness in the uterus or ovaries, leucorrhœa, amenorrhœa, dysmenorrhœa, menorrhagia, and certain varieties of irregular menstruation. Whether they directly give rise to organic uterine disease may perhaps be a question; but undoubtedly they may produce that state of the system in which these organs are peculiarly susceptible of the operation of morbid stimuli which are calculated to cause organic disease. In all cases in which we can detect any deviation from the healthy action of the digestive system, whether cause or effect, our efforts for the relief of the local disorder will be much assisted by a careful attention to the digestive organs and to the general health. We think that this consideration has been too much forgotten of late in the disposition to treat uterine diseases as exclusively local, except so far as they directly produce certain general effects; and in this respect we think that Dr. Mackenzie has done the profession good service, by recalling its attention to the importance, practically as well as theoretically, of these concurrent affections.

The indications of treatment laid down by Dr. Mackenzie are threefold: first, to impart tone and vigour to the nervous system; secondly, to appease irritability and correct disorder; and thirdly, to improve and promote the secretions of the irritable organs. The first thing is, of course, to investigate, and, so far as we can, to remove, the causes of this weakness, irritability, and disorder; and

"Having urged the necessity of relinquishing various habits, pursuits, and customs, which may have been prejudicial to health; having enjoined due attention to matters connected with regimen, the necessity of keeping early hours, taking regular exercise and adequate rest and sleep; having also inculcated the necessity of proper dietetic measures; of adapting the quality and quantity of food to the wants of the system and the powers of the digestive organs, and of observing regularity in regard to the periods of taking it,"

—we may then consider the most appropriate medicinal remedies at our command. So long as the disorder of the digestive organs continues, Dr.

Mackenzie considers the employment of alterative medicines of the first importance, from their effect upon the mucous membrane, and their power of modifying its secretions: but the greatest care and caution should be observed, to guard against excess; the weaker the patient, the more sparingly should they be given. In slighter cases, small doses of hyd. c. cretâ or blue pill will be sufficient; but when there is much disturbance of the liver, stomach, and intestinal canal, Dr. Mackenzie prefers calomel. Our own experience is in favour of the milder preparations, continued at intervals for some time, and followed occasionally by a gentle purgative, as may be necessary. When this condition of the digestive system is corrected, we may have recourse to tonics; but, in some respects, their employment requires circumspection.

"They are, for the most part, inadmissible when there is much coexisting disorder; they should be given guardedly when this exists in a moderate degree only; while they are most beneficial where weakness and irritability are the sole pathological conditions which prevail. Again, the form of tonic to be given requires some consideration, as well as its dose, the period at which it should be taken, and the nature of the concomitant affection." (p. 60.)

We must refer the reader to Dr. Mackenzie's discriminating application of these principles, with which we cordially agree.

In certain cases, in which irritability is the prominent characteristic, it may be necessary to exhibit sedatives; and of these Dr. Mackenzie has found bismuth, hydrocyanic acid, and the oxides of some of the metals, particularly silver and zinc, eminently efficacious.

We have thus briefly examined the views of our author as to the influence of spinal irritation and digestive disorder in the production of uterine disease; the remainder of his volume is occupied with cases and commentaries illustrative of the positions he has laid down. He has shown considerable observation, and reasoning powers of no mean order. That upon the whole he has established his points, we think must be admitted; but we are not so certain that he has sufficiently guarded against opposite errors. His method is hardly so lucid as it might be, but these defects are far overbalanced by the practical value of the work. We shall gladly welcome Dr. Mackenzie's further researches as to the causative influence of certain diseased conditions of the blood and constitutional diatheses; to which we trust he will add diseases of the kidneys and morbid epidemic influences. This would complete the first division of the triple phase of female disease to which we alluded at the commencement of this review.

The second division would consist of the consideration of the local diseases; and if, in the present day, it has too exclusively occupied attention, the result has certainly been a vast amount of information added to our previous knowledge; and we have no doubt that the error of isolating these diseases will soon be rectified. Dr. Henry Bennet's book, which has reached a third edition, is a favourable specimen of this class of publications; and if we do not quite agree with him as to the local character of some diseases, and the comparative frequency of others, we should be sorry to doubt that he has added much to our knowledge of these affections. As his work was formerly reviewed in this journal, we shall not now enter into any analysis of it: the rapid sale of two editions bespeaks its favour with the profession, and in the preparation of the third Dr. Bennet has not been unmindful of the duty of every author to improve each edition to the utmost. He has added several new chapters, has recast others, and has so condensed the work that it is very little larger than the second edition.

A glance at the contents of the volume will show a large portion of the subjects included in our second division of local diseases. First, we have the functional disorders, which, however, may frequently have a constitutional origin, as we have seen, but which may also be purely local. Then we have inflammation of the cervix and body of the uterus, of the ovaries, of the vagina, and of the vulva. Next, lesions of nutrition, fibrous tumours and polypi; and lastly, cancerous and cancroid growths, and mechanical displacements. If to these we add the local affections of pregnancy and childbed, the second division will be complete.

The third division—that is, the influence of uterine and ovarian disease upon other organs, and upon the general health—has, perhaps, hardly received due attention; it has been fully noticed in certain well-marked diseases, as in cancer; but although it is traceable in almost all, and especially during pregnancy and childbed, it has not been as minutely and carefully investigated as its importance and very interesting character deserve.

We hope, from time to time, to call the attention of our readers to each of the departments we have noticed; and we have been thus particular in mapping out the subject, in order to give them a bird's-eye-view, as it were, of the field for investigation which lies before us.

Fleetwood Churchill.

Review III.

1. *Handbuch der allgemeinen pathologischen Anatomie.* Von Carl Rokitansky.—*Wien*, 1846. Band iv.

Manual of General Pathological Anatomy.

2. *Lectures on Nutrition, Hypertrophy, and Atrophy.* By James Paget, Esq., F.R.S, &c. ('Medical Gazette,' 1847.)

3. *Observations on the Clinical History and Pathology of one form of Fatty Degeneration of the Heart.* By E. L. Ormerod, M.D. ('Medical Gazette,' 1849.)

4 *On Fatty Degeneration of the Heart.* By Richard Quain, M.D. ('Medico-Chirurgical Transactions,' vol. xxxiii.)

5. *On Fatty Degeneration of the Small Bloodvessels of the Brain, and its Relation to Apoplexy.* By James Paget, Esq., F.R.S. ('Medical Gazette,' 1849.)

6. *On Fatty Degeneration of the Placenta, &c.* By Robert Barnes, M.D. ('Medico-Chirurgical Transactions,' vol. xxxv.)

7. *On Granular and Fatty Degeneration of the Voluntary Muscles.* By Edward Meryon, M.D. ('Medico-Chirurgical Transactions,' vol. xxxv.)

(*Continued from No. 22, p. 358.*)

We shall now proceed to inquire into the coincident pathological conditions which are found in instances of fatty degeneration, so far at least as they are not simply the result of the enfeebled state of the organ itself. Dr. Ormerod and Dr. Quain are our chief guides in this inquiry. The former shows, that out of 25 cases of fatty degeneration of one or both ventricles of the heart, the lungs were emphysematous in 7 cases; tuberculous deposit existed in 5; the liver was fatty in 5, cirrhotic in 1; the kidneys were granular in 7, cysted in 2, affected by capillary phlebitis in 2, and fatty in 1; the aorta was diseased in 5. Dr. Quain finds, in 33 cases of fatty degeneration of one or both ventricles, the coronary arteries ossified or obstructed in 13; chronic valvular disease, or endo- or pericarditis, in 6; in 8 the liver was large (probably from an early stage of cirrhosis), in 5 it was fatty or soft, in 5 it was granular; in 5 the kidneys were large (probably the hypertrophic form of Bright's disease), fatty in 2, granular or mottled in 6; the aorta was diseased in 7. Combining these two tables with respect to the liver, the kidneys, and the aorta, we have, out of 58 cases of fatty cardiac degeneration, 10 of fatty liver, 6 of granular or cirrhotic; 13 of granular kidney, 3 of fatty, 5 of large; in 12 the aorta was diseased, and the coronary arteries in 13 of Dr. Quain's cases :—in Dr. Ormerod's the point seems not to have been particularly examined. The only conclusion that can be drawn from the above numbers is, that no special connexion exists between fatty degeneration of the heart and any of the pathological conditions mentioned. Out of 10 cases of which we have notes, we find 5 where the heart's tissue was slightly affected by fatty degeneration, and 5 in which this process was more advanced. In all the first five the kidneys were granular and wasted. In the second set there was no disease of the kidneys in 2, incipient granular disease in 1, and confirmed in 1. It therefore appears, that in 8 out of 10 cases there was more or less renal degeneration, a proportion much higher than that given by the above authorities, even if we put together, as we believe we may do pretty safely, the large, cysted, and fatty varieties under one common head of renal degeneration. It seems, however, that the fatty change of the heart and the renal disease by no means advance *pari passu*. In the 5 cases of slight cardiac degeneration, the liver was remarkably fatty in 3, fatty and cirrhotic in 1, cirrhotic in 1. In the 5 cases of more advanced disease, the liver was fatty to some extent in 3 (in one of these it was cirrhotic also), in 1 it was not fatty, and in 1 it was not examined. Thus, in 7 out of the 10 cases there was coincident fatty change in the liver and heart. Between atheromatous disease of the aorta and fatty degeneration of the heart we have not observed any marked connexion. Dr. Ormerod does not consider that phthisis or emphysema has any causal connexion with cardiac degeneration; but he remarks on the not infrequent coincidence (12 per cent.) of ulceration of the alimentary canal with this state. Dr. Quain regards general or constitutional causes as of much potency in the production of fatty degeneration of the heart, and cites, from Dr. Ormerod's lists and his own, various instances illustrative of this point. We certainly think that these act in the way of promoting such change, but it would not be difficult to bring forward cases in which fatty degeneration occurred without such precedents, and also where it did not occur in spite of their existing in full force. These, then, we cannot regard as the prime cause; which, as already hinted, we believe to consist essentially in a lowered vitality of the affected tissue.

Ossification (so called) of the coronary arteries has been supposed to have much influence in bringing about fatty degeneration of the heart. Dr. Quain mentions his having seen the coronary artery extremely ossified, going to the only part of the heart affected. If a portion of the heart be starved, in consequence of a defective supply of blood, it is conceivable that its vitality will be lowered, so that it may degenerate fattily. However, neither in the case of the heart nor of other tissues does obstruction of the vessels at all invariably produce fatty degeneration of the part which they supply. Dr. Ormerod testifies, that in extreme cases of degeneration he has found the coronary vessels healthy; and, on the other hand, has found them diseased where there was no record of the structure of the heart being thus changed. In a case of cirrhosis of the liver, in which the obstruction to the circulation was so great that a very fine injecting fluid could not penetrate the lobular capillary plexus, the marginal cells for some depth were utterly atrophied—reduced to mere traces, small granulous globules, and nuclei: those in the central parts were tolerably natural. On the other hand, in cases of decided fatty state of the liver, it is certainly not common to find any obstruction to the passage of the blood. Mr. Simon mentions having several times observed obstruction of the branch of the renal artery supplying a part of the kidney which is softer than the adjoining, and in which the epithelium of the tubes is found extremely atrophied, but not in a state of fatty change. It seems, therefore, scarcely proved, that mere deprivation of blood can act as the sole cause of fatty degeneration of a part.

Inflammation of parts is recognised by Mr. Paget as a cause of their fatty degeneration. Rokitansky speaks of his third form of cardiac fatty degeneration as occurring more especially in hypertrophied and dilated hearts, in combination with the residua of endocarditis and carditis, or independently of them. In cases of adherent pericardium there is commonly a layer of fat spread over the heart's surface; and the muscular tissue is spoken of by Dr. Williams as being in a pallid, yellowish state. We are not inclined to regard inflammation as being a common cause of fatty degeneration; its own products not unfrequently undergo this change, but the involved tissues more often break down, or simply waste. Clearly, though it may promote the change, it is in nowise of the essence of it: the most marked cases of fatty degeneration of the heart, the liver, and the arteries, are those in which there is no trace of inflammation. No connexion has been observed between syphilis and fatty degeneration, so far as we know, though it is very probable that the cachexia induced by this poison might lead to such change.

The description which Rokitansky gives of the condition of the system in the so-called drunkard's dyscrasia, may here, we think, be quoted with advantage, as it illustrates very well the effects resulting from an unnaturally fatty state of the blood, and from a lowered state of the vital forces in the nervous centres, and throughout the system generally. He recognises an acute and chronic form. The latter appears as plethora, attended with a marked dusky colour and viscidity of the blood, and with a proportionate increase of its fatty contents. The condition of the solids results from this state of the blood, and is explained by it. The formation of pigment in the skin, the excessive, and at the same time qualitatively abnormal, formation of fat, and the mucous profluvia, are prominent symptoms. The bodies of old brandy-drinkers present very remarkable phenomena: the skin appears tinged of a dirty brownish hue (though parts naturally coloured—as the scrotum—may become pale); it has also a soft, velvety, fatty feel, like a negro's, and its epidermis is thin. In the subcutaneous tissue chiefly, and next in frequency, in the mesentery and omentum, fat is deposited, in a quantity either absolutely excessive, or at any rate disproportionate to the condition of the muscular tissue. The quality of this fat is similar to that of mutton suet. The muscular tissue is diminished in bulk, and is also pale. The formation of fat invades the muscles as the so-called fatty transformation; the liver, however, is especially thus affected. Even in the bones the formation of fat takes place in an excessive manner, at the cost of the osseous structure. All the mucous membranes, especially the bronchial and the intestinal, suffer from chronic tumefaction and blennorrhœa. The gastric mucous membrane, especially, is the seat of a chronic catarrh. A similar condition of hypertrophy is exhibited in the habitually congested membranes of the brain, in the form of turbidity, thickening, or chronic œdema. The brain is shrunken, as from senile atrophy, with or without enlargement of its cavities, and serous effusions in them. The blood appears dark, viscid, without fibrinous coagulum; has a sticky, greasy feel; often contains fat, mixed with it in the form of oil-drops, in great abundance; or, in rarer cases, presents a whitish, milky, chyle-like turbidity. In the acute form there is a marked tendency in the whole mass of blood to remain fluid, to lose its natural colour, and to allow the transudation of dirty red serum; while oil separates on its surface in the form of largish drops. The dead bodies exhale a remarkably sweetish smell, and quickly pass into decomposition. We have carefully perused the account which Dr. Huss gives in his work on 'Alcoholismus Chronicus,' of the morbid changes induced by spirit drinking. His description corresponds very closely with Rokitansky's, but is of course more detailed. He notices the frequent occurrence of granular kidney, and how this degenerative change is induced in such persons by intermittent fevers and rheumatic attacks more frequently than it would be in the healthy. The heart in drunkards becomes hypertrophied, afterwards overlaid with a fatty growth, while the muscular tissue wastes. The aorta is sometimes atheromatous; and the cerebral vessels, the smaller especially, are often dilated, and the coats of the larger brittle. The spinal cord is commonly healthy; sometimes it is wasted, like the brain. The voluntary muscles are generally flabby, their tissue thin, withered, pale, often seeming to be penetrated by fat; they waste, in extreme cases, to one-fourth or one-fifth of their natural size. The quantity of fat in the blood is shown by analysis to range from 117·0 (Le Canu) to 42·0 (Mareska) per 1000 parts; the highest estimate of the quantity in health not exceeding 8·65. The serum is often milky, and in a more marked manner in those cases in which the opacity is caused by oil, than in others where it depends on the presence of proteine matter. The following circumstances are especially mentioned as accounting for the unnatural condition of the blood :—(*a*) The absorbed alcohol interferes with the taking-in of oxygen into the blood and the giving out of carbonic acid, so that the blood remains in an unnaturally venous state. This is given as the result of Schultz and Klencke's experiments; but Vierordt and Prout have also shown that spirituous drinks diminish the quantity of carbonic acid expired, and this to a greater extent when taken upon an empty stomach, when they would of course be most rapidly absorbed. (*b*) The alcohol interferes with chymification and chylification, hindering the proper neutralizing action of the bile on the chyme, and thus producing a morbid condition of the materials out of which blood is to be formed. (*c*) The liver, and (*d*) the kidneys, becoming themselves diseased, do not depurate the blood properly. (*e*) In consequence of pulmonary irritation from the expired alcohol and bronchial catarrh, respiration is not properly performed, and the blood, from this cause as well as from (*a*), is not properly arterialized. Scharlau has found 30 per cent. more carbon in the blood of a drunkard than in that of a healthy man. The effects of continued abuse of alcoholic liquors may be briefly stated as disordered conditions of the primary and secondary assimilating processes; the latter appearing, in the more advanced stages, with nervous and muscular debility.

We shall defer any remarks on the foregoing interesting experiments, as we might almost call them, which the infatuation of man performs on himself, until we come to consider the nature of fatty change.

Dr. Ormerod's table shows, that out of 25 cases of fatty cardiac change, 10 were of drunken habits. Dr. Quain's series (1) gives only 4 cases noted as intemperate out of 33, while 9 are mentioned as temperate or regular. In 9 cases of our own, wherein the patients are said to have drunk hard, or been intemperate, the liver was very fatty in not more than 3. With us, indulgence in spirit drinking seems to tend more to the production of granular kidney-disease, cirrhosis of the liver, and allied morbid states, than to induce fatty changes. It must be remembered, however, that the fatty degenerations of alcoholismus belong to an advanced stage of the disease.

The relation of polysarcia to fatty degeneration is manifestly one of very great interest to inquire into, particularly as the first condition is palpable and external, and can be accurately watched and measured. From Dr. Quain's paper it appears, that out of 32 cases of fatty degeneration of the heart, 23 were fat and 9 thin, a sufficient excess, certainly, to make us watch with some degree of anxiety any unusual increase of bulk in patients who present at the same time an arcus senilis, or who suffer from any of the symptoms of feeble circulation. At the same time, the number, more than a fourth, who were in the opposite state, taken together with that of those in whom neither condition seems to have been prominent, shows convincingly that there is no absolute connexion between the two. The same may be gathered from Dr. Chambers' table,* where it is shown, that out of 38 cases of obesity, 7 professed themselves healthy, 5 of these weighed from nineteen to twenty stone, a size not inferior to that of several who suffered from serious disorders. We were recently present at the inspection of the body of a female who, though dying of bronchitis with tuberculous cavities in her lungs, was excessively fat. In her there was no remarkable fatty transformation in any part except the liver; the heart, in particular, was ascertained, microscopically, to be quite in a condition of average health. It is scarcely correct to speak of obesity as anything else

* Lectures on Corpulence.

than a disease; yet we think we must distinguish between this condition taking place as a simple hypertrophy occurring before life has begun to wane, which only acts injuriously by increasing the task of the heart, and mechanically impeding muscular play, and a similar state when it supervenes in the decline of life, or as the result of dram-drinking, a fever, an injury, salivation, or the syphilitic taint. The one involves no unhealthy nutrition of the various tissues; their life may remain sound and vigorous; they are only incumbered by the adventitious mass appended to them. The other is essentially connected with decay of the vegetative life, with a flagging of the various assimilative and eliminative processes; the blood is not properly depurated; it contains more oily matter and carbon than it should, and the nutritive power of the various tissues being impaired, this is either organized into fat-cells upon and among their elementary parts, or else settles down in their very substance as an imperfect replacement of the natural constituent. The first may be called sthenic, the second, asthenic corpulence. Doubtless in both there exists at the same time a peculiar diathesis, which gives the special impulse to the formation of adipose tissue, as is apparent from the occurrence of fatty degeneration in thin persons. Doubtless, also, the majority of cases of obesity are not typical instances of one or other condition, but approximate more or less to either. The distinction, however, between the two is a real one, and may be illustrated further by a reference to the first two forms of fatty disease of the heart mentioned by Dr. Ormerod. He says, "In the first form, the fat is accumulated in those parts where it is naturally deposited in the greatest abundance, as at the base of the heart. This is met with in those persons who have a general tendency to accumulate fat;" and, it may be added, in fatted animals. "It is certainly a condition of disease, but probably not an important one." "In the next form, the fat collects chiefly about the apex of the heart, and is not attended with so great enlargement as in the form first described, for the fat is deposited at the expense of the muscular tissue, not merely laid upon it." "This form occurs in persons who have, as is popularly said, an unhealthy disposition to grow fat, in those where the fat has generally throughout the body, as in the heart, a tendency to accumulate in the place of, not in addition to, the proper structures." He also quotes Dr. Latham's opinion, that it is not in those who have always been fat that he apprehends this morbid change (fatty degeneration), but where a man has been always thin, and in the decline of life becomes suddenly fat. Asthenic corpulence thus seems to acknowledge a close relation to fatty degeneration of organs in such wise, that it may with much probability be feared that the two are proceeding together in any given case. Again, even sthenic corpulence, though it may remain long unassociated with any fatty textural change, must yet exert a predisposing influence in this direction, from the increased amount of oily matter contained in the blood, and the diminution of active muscular movement, and of free respiratory action, which it tends to induce.

There exist other degenerations besides the fatty, and it is worth while to inquire whether there be any and what relation between them and the one which we are investigating. A fibrous change is one of the most important and common, depending, we believe, on the exudation of an unhealthy fibrinous plasma, which takes place in a slow and gradual manner.

One of the best examples of it is the white cartilaginiform patch which we have already noticed as occurring in the coats of arteries. Between this and true atheroma there seems to us to be no relation, but rather an opposition. Instances, however, are met with in which the two are commingled in the same part. The stiffened and puckered condition of the aortic and mitral valves, commonly supposed to be the result of endocarditis, is, we think, produced by a similar non-inflammatory exudation. This condition is a very different one from the atheromatous affection of the same parts. The change in cirrhosis is of the same kind, as well as in all the varieties of this process which thickens and condenses the Glissonian sheaths, and sometimes infiltrates the lobules themselves with a solidifying fibrinous blastema. The nature of this change appears to us to be totally diverse from that which produces fatty degeneration; but the two may exist together, the one having probably supervened on the other. We incline very strongly to consider the various forms of M. Brightii as all essentially instances of a degeneration of an allied nature. Not that we suppose the spoiling of the kidney to take place in consequence of fibrous development in the matrix, and atrophic pressure on the bloodvessels; but we think that, as the result of unhealthy plasmatic exudation, there comes to pass (1), a thickening of the capsule, and adhesion of it to the surface; (2) an alteration of the epithelium, such that its particles become more bulky, more sharply defined, more coarsely granular, and, as one would imagine, more inapt for their natural changes necessary to healthy secretion; or in other cases, such that the epithelium is converted into a coarse granular structure, not obstructing the canal, but equally unfit for its healthy function; (3) an altered condition of the membrane of the Malpighian capillaries, allowing serous fluid to escape readily from them on slight pressure; (4) in some cases an intertubular effusion thickening the matrix by an homogeneo-granular deposit, and interfering with the circulation of blood through its vessels. Repeated observation shows us the frequent coincidence of this change with cirrhosis of the liver, and thickening or cartilaginification (false) of membranous structures; and we may add, that our friend, Dr. Ogle, has formed the same opinion that we have respecting the correlation of these morbid states and their essential dependence upon an unhealthy condition of the fibrinous constituent of the blood. The reader will find his statement in the Pathological Society's Report for 1851-52, p. 355. Now we are perfectly satisfied that the condition of the kidney in question is in no degree whatever necessarily associated with any change of the nature of fatty degeneration. Decaying epithelial particles or fibrinous casts may degenerate fattily, but this is a secondary and an epiphenomenon, and has no relation to the nature of the original degeneration. The *calcareous* degeneration is in many cases a secondary change; it seems most prone to occur where organized products of low vitality have undergone, or are undergoing, a kind of decay (such as the softening of tubercle), and are becoming inert. Thus it is often observed in obsolete tubercle, or masses of fibrine, in atheromatous deposits, and in fibrous tumours. It does, however, often occur primarily, as in the ossification of the persistent cartilages, and the wrongly-named ossification of the arteries in old persons. In this case we have little doubt it may occur quite independently of atheroma, though it is more commonly consecutive to this.* The only relation detectible between calcareous and fatty degeneration seems to be this, that in a part where life is already weakened by fatty change, calcareous matter is especially apt to settle down as in a suitable nidus.

We have already exceeded our limits, and must hasten from this survey of a wide field, to draw, if possible, some general conclusions, to make our "vindemiatio." Few have attempted any exact inquiry into the nature of fatty degeneration; the only authorities that we can refer to are Mr. Paget, Dr. Quain, and more recently, Professors Lehmann and Valentin. Mr. Paget says—

"As to the very nature of the wasting atrophy, and the exact mode in which the quantity of a tissue is decreased, we really know for certain extremely little. For explanation of the mode in which the fatty degeneration takes place, I before suggested that fatty matter is probably one of the products of the spontaneous transformation of the tissues at the end of their period of vigorous existence; and that this form of atrophy only represents the state of a tissue remaining unrepaired after it has fallen into the ordinary course of degeneration. The possibility of fatty matter being formed in the transformation even of proteine compounds is certain from the observation of Wurtz, that butyric acid is one of the compounds formed by the decomposition of fibrine in the open air. And that the fat which we find in the muscles and gland-cells is really not a deposit put into them from without, but one of the products of the change of their own contents, is made probable by the frequency with which, in muscular fibres, we find the fat-particles arranged in the same manner as the proper constituents of the fibrils,—sometimes in transverse, sometimes in longitudinal rows. Indeed, one is constantly tempted, in the examination of these specimens, to think that we can trace all the transitions from the 'sarcous elements' of the muscular fibre, and the granules of the gland-cell, to the little oily particles which, by clustering and then fusing with others, at length make the great oil-globules which fill the cell. What we can see in the degenerating normal tissues is fully confirmed by the corresponding changes taking place in anormal products. In some of these, indeed, the fat is found in parts to which exudations from bloodvessels could scarcely gain access; as, for example, in the substances enclosed in old, shrivelled sacs with non-vascular wells; and in most of them it is found most abundantly in the centres of parts—such as coagula in the bloodvessels—which are most remote from the supply of blood."

Dr. Quain expresses himself thus with reference to fatty degeneration of the muscular fibre of the heart, distinguished from fatty growth:—"I seek to establish the fact, that the molecular fatty matter in the fibre is the result of a chemical or physical change in the composition of the muscular tissue itself, independent of those processes which we call vital."

Lehmann, at p. 222 of the 3rd vol. of his 'Physiological Chemistry,' says—

"The interpretation of this so frequent phenomenon may be one of the two following. Either it is assumed that fat already present is disposed by some molecular force to collect in those cells which are becoming decrepit, or whose life is less vigorous, and replace therein the vanishing nitrogenous constituents: or the hypothesis is proposed, that the fat arises immediately out of the nitrogenized basis substance of the cells or fibres, and that their nitrogen disappears under the form of ammoniacal salts, or other simple combinations, leaving fat behind as a second decomposition product of the albuminous material."

The first view mentioned by Lehmann, which is rejected by Paget and

* Mr. Dalrymple has recorded a case in the Medico-Chirurgical Transactions, vol. xxvi., which seems to have been primary calcareous degeneration of a scaly epithelial formation.

Quain, might be expressed as follows. The living organized tissues are not transformed into oily matter, as pieces of dead tissue are by a mere chemical or physical action; but as their assimilative power gradually decays, they become less and less able to maintain the normal constitution of their elementary parts, which gradually liquefy, and are absorbed and not replaced, while oily matter, atom after atom, settles down in their room. Or the oily deposit may be primary, and the wasting of the tissue consecutive after a longer or shorter interval. We shall now endeavour to exhibit fairly the evidence that may be produced on both sides. The change of dead animal tissues into adipocire is a very striking instance of the conversion of proteine substance into fatty matter. Dr. Quain describes a portion of muscular flesh from a horse, which had been thus changed, as resembling spermaceti in its general appearance, emitting a strong ammoniacal odour, floating on water, and being nearly all soluble in ether. His own interesting experiment with a healthy human heart, which, after some weeks' maceration in weak spirit-and-water, was found so changed as to present the same appearance under the microscope as a fattily degenerated organ, is exactly an instance of the same kind. We have repeated Dr. Quain's important experiment with very much the same result. The heart of a child, aged about five years, who died of a burn, was ascertained, by microscopic examination, to be perfectly healthy. A small piece was placed in common water, in a room not warmed, and so left for a fortnight; at the end of that time the fibres were found in great measure broken up, and their débris mingled with multitudes of largeish glistening yellow corpuscles, which had quite the aspect of fat, and were soluble in ether, but not in acetic acid, nor readily in liquor potassa. Opaque molecules were seen in the remnants of the fibres, disposed just as those in the muscular fibre degenerated during life. The tissue was very much softened, and of a dirty greyish white. A piece of boiled white of egg, which had been kept in water three weeks, showed scarce any trace of oil; it was extremely softened, and was swarming with infusoria. The liver of a cat was macerated in water for one month; it was then found of a pinkish colour on the surface, and white internally. It had contained, shortly after death, only some local deposit of oil in the cells. After maceration, it appeared, under the microscope, to be converted into a mass of granular and oily matter; the oily masses sometimes had the appearance of having resulted from conversion of the cells, but were usually much smaller. Voluntary muscular fibre from the same animal presented numerous large glistening corpuscles of fatty aspect scattered here and there; some seemed to be within, others, perhaps, were external to, the sarcolemma. There was also some deposition of molecular fat in the sarcous structure. The white fibrous tissue of tendons of these muscles showed no fatty change whatever. Neither did the nuclei of the thymus; they retained at the end of this time almost completely their natural aspect. Mr. Lindsay Blyth kindly undertook, at our request, a chemical determination of the increase of oily matter produced in the muscular tissue of the heart by maceration. The specimens he experimented with were from the same heart above mentioned. After maceration in water acidulated with nitric acid for nearly a month, we examined the tissue by the microscope, and found that some amount of fatty change had taken place in the fibres, though not to a great extent, not nearly so much as in the portion macerated in water only. He states that he divided a portion of the heart weighing 100 grains into two equal portions of fifty grains each. One of these was chopped up, and after thorough extraction by ether, yielded 1·003 of fatty matter. The other was kept in a bottle containing three drachms of water acidulated with a few drops of nitric acid, from December 9th to January 4th. On analysis, it yielded 1·500 of fatty matter. The increase of fatty matter, which must have proceeded from the conversion of proteine substance, amounted, therefore, to nearly one-third. The following experiment relating to the like change in a morbid product, seems also worth mentioning. The aorta of a man, dying suddenly of heart-disease, was extensively affected with the white cartilaginiform patches. Having examined these microscopically, and ascertained that they were produced by the presence of an homogeneous translucent matter, containing no oil except in some parts where it was mingled with a few spots of atheroma, we placed it in water. At the end of about thirteen days we examined it again, and were much interested to find that at one part near the middle, over a space nearly the size of a half-crown, where it had reached the surface of the water, it was of a bright yellow colour. In this part, the whole of the fibrinous deposit was converted into oily matter, while in the other parts which had been more submerged, scarce any change had taken place. The result seems to indicate, that access to the oxygen of the air accelerates the change, though it is certain that this is not essential. It is therefore quite certain that dead tissues out of the body will change into fatty matter.

But Wagner has performed some experiments which go to show that a similar change may take place in dead tissues within a living body. He took the testes, crystalline lenses, eyes, and pieces of intestine of frogs, and coagula of blood, portions of muscle, and coagulated ovalbumen, and put them into the abdominal cavities of doves and fowls, closing up the wound, and allowing them to remain a certain time. He found in almost every case that these substances became surrounded by a capsule of false membrane, that they gradually diminished in size, and underwent a change into fatty matter. Frogs' testes, which when fresh in the winter contained 3 per cent. only of fat, increased their fatty contents, in proportion to an equal weight of fresh testes, from 5 to 15 per cent. In dry lens-substance there was $\frac{1}{3}$ to $\frac{3}{4}$ per cent. of fat; in those which had lain 6 weeks in the abdomen there was 7, 10, 12, 14, 15, even 47·86 per cent. Together with this increase of fat there was always a considerable diminution in volume: an ox's lens was diminished in 43 days from 2·825 grammes to 0·528; a sheep's lens in 40 days from 2·640 to 0·136 gramme; a rabbit's in 54 from 1·544 to 0·542 gramme. Wagner concludes justly, that in these cases the fatty change did not consist merely in the setting free of pre-existing fat; but the objection cannot quite be so easily disposed of, which the cautious experimenter himself suggests, that the fat might have been secreted by the vascular capsule, and made its way by infiltration into the inclosed substance. To our minds, however, the.e seems to be no reasonable doubt that there took place an actual fatty transformation, and not a deposition of fat. The next step in the line of evidence is to those instances, of which we have related two, where oily matter is formed abundantly in coagula which are contained in the vessels or in the heart. Here we think there can be no question that the oil is actually the product of the decomposition of the proteine substances, that it results not merely from the setting free of pre-existing oily or saponified compounds, but from the adipocerous transformation of the nitrogenized principles. A step somewhat further brings us to the metamorphosis of fibrinous deposits, such as occur often in the spleen and (as we think) in the placenta, and in other parts. In these instances the disintegrating substance is somewhat more in the position of a tissue, more in contact with the circulating blood, and its changes may be supposed to be more under the influence of the vital forces. Here, however, also, we find a fatty change taking place almost exactly in the same way as in the before-mentioned instances. Fatty degeneration of pus-corpuscles in an abscess would be an instance of this change occurring in organized structures, and this clearly by a chemical conversion of their own substance, and not by a nutritive change, which cannot be considered possible. We do not here allude to the development of pus-corpuscles into granule-cells which some speak of, but simply to their disintegrating into oily matter. In the two instances we have mentioned where fatty degeneration occurred in tissues surrounded and pressed upon by fibrous formations, the one being the columnæ papillares of the heart, and the other a nodular mass in the liver, we think the fatty change may have been one of mere conversion. In the liver especially, the appearances were very different from anything that is observed in ordinary fatty degeneration.

Thus far we have traced fatty changes from purely chemical actions occurring in dead matter out of the body, up to transformations apparently of a like kind occurring on the confines of organization and vitality. Can we advance yet further, and include in one category the fatty changes of living and acting structures? The theory which is most prevalent affirms that we may, but we confess that there appears to us to be a vast hiatus between the two groups. It seems to us to require the strongest evidence to prove that a tissue which is in any degree alive, and carries on a process of nutrition, however feebly, can undergo changes identical in mode and kind with those incident to dead matter. Moreover, we do not think that all instances of fatty textural alteration are alike; we think that some are much more degenerative than others. Let us briefly review the fatty conditions of various organs as detailed in the foregoing pages, and endeavour to see in what light we should regard each of them. The glomerulus presents one of the simplest instances of a cell with albuminous contents undergoing fatty change. Does this take place by the cell wasting, as Wagner's bits of dead matter did, and the contents, much diminished, turning into oil? No; the cell *enlarges* as it receives the oily matter; it manifestly carries on a process of nutrition, though that consist in receiving an excess of oil in exchange for a less quantity of albumen. The oily matter is beyond all doubt "put into it from without;" and though the oily granules themselves may be in part the result of a chemical change in proteinous blastema, this does not alter the case with regard to the living organism of the cell. The varieties in the formation of glomeruli which we have noticed, show that the organizing force can use fatty granules in the construction of a cell, as well as purely albuminous matter. When glomeruli form from the epithelial particles of healthy lungs, or by alteration of cancer-cells, we think there is often evidence to the same effect—there is no shrinking, but an enlargement of the affected cell. So with regard to the hepatic cells; they distend and enlarge as they become full of oil, and this occasions the general enlargement of the whole organ. Surely they cannot degenerate by breaking up of their albuminous contents; the oil they contain must be "put into them from without." This is especially evident in the case of a fatted sheep, where the marginal cells are loaded with oil, which must be derived from the blood. As the glomeruli, when their period of growth and of fatty impletion is over, break up into shapeless masses of oily granules, so do the fat-laden cells of a degenerated liver. They may, however, exist, we believe, for a long time without disintegrating, though containing much oil; and so long as they retain their form, so long, we must suppose, their life continues. It seems, however, that the more oil they contain the less sugar can they prepare, and, *pro tanto*, the less fit are they for one of their functions. In Dr. G. Johnson's account of fatty degeneration of the kidney, it is implied that the epithelial particles enlarge and become distended with oil, so that the walls of the tubuli are pressed against the capillaries of the venous plexus, causing obstruction in them, and congestion of the Malpighian tufts. Here the *enlargement* of the cells by the intus-susception of oil is clearly recognised, and it is manifest that this involves a depositing of oil within them by a process of nutrition. On the other hand, it seems to us very possible that fibrinous casts contained in the tubes may degenerate fattily by conversion of their substance, and the same may be the case with some effete epithelial particles; but we entertain no doubt that in the commoner form of fatty kidney, which is only a variety of the granular disease, the oil which is seen in many of the cells is a deposit, and not a conversion product. This seems to us proved by the not unfrequent development of such cells into glomeruli. We may class together the fatty condition of the epithelium of the gall-bladder and of the ducts in London dogs, and that of the central portions of the kidneys in dogs and cats, as remarkable examples of the replacement of albuminous granular matter by oily without any degenerative change. As we are utterly ignorant of the mode in which the epithelium of glands fulfils its function, so we are quite unable to judge in how far and what an alteration in the chemical composition of this substance unfits it for taking part in the process of secretion. It may be that the oily matter in these instances is capable of supplying the place of the granular. At any rate, we may note these, and many of the commoner, instances of accumulation of oil to some extent in the epithelium of the gastric tubuli in apparently healthy animals, as proofs that the presence of much oil in a part, and fatty degeneration of that part, are by no means identical. There are other cases in which a true fatty degeneration of gastric glandular structure appears to take place, the nuclei and cells appearing as masses of fatty matter. We have no evidence to offer as to the nature of the change in these cases, but only the following remark. As there is good ground to believe that the epithelial cells of the stomach tubes are continually shed during digestion, and pass off in the chyme, while fresh ones are produced within the tubes, it seems far more probable that the particles and their nuclei should become fatty by a process of malformation and deposition of oil, than by actual conversion into this matter. In the testis it has appeared to us that the epithelial cells enlarge as they become the seat of oily deposit, and that they thus degenerate in a way of mal-nutrition. In whatever way a fatty change of the thymus takes place, it is clear that it is not by a conversion of its nuclear growth into fat. The fatty change of the thyroid of which we have recorded an instance, seems to have been clearly a peculiar textural change attended with exudation of oily matter from the bloodvessel, rather than a transformation of the glandular tissue into fat. From the instance of the supra-renal capsules, no evidence that we know of can be gathered one way or the other. The arcus senilis appears to be an instance of simple oily deposit in the tissue of a part, as evidenced by the intumescence of the ring affected. Subsequently, no doubt, the involved tissue may liquefy and be absorbed. The atheromatous change in the coats of arteries seems to us to be precisely of the same kind as the change in the cornea.

Dr. G. Johnson notices the existence of microscopical quantities of oil even in the texture of the coats of apparently healthy arteries; and we feel quite convinced that the oily globules which constitute the common atheromatous patch are the primary deposit, and not the result of the conversion of another, or of the tissue itself. We would remark that, considering the large amount of oily matter in the blood, which, though in great part saponified, is also in notable quantity free and visible—as we have seen in numerous examinations of the blood, carried on in conjunction with our friend, Dr. Sieveking—it surely cannot be considered improbable that this matter should be deposited in different parts. When we read of the stream of blood flowing from the jugular vein of an elephant depositing on each side a considerable quantity of a fine fatty matter, having exactly the composition of ordinary fat, can we be surprised that the same thing should happen within the body? Rather, is it not the marvel and the miracle of healthy assimilation, that it does not happen everywhere—that every tissue and every part is not infiltrated with exuding oil? Oil has manifestly the greatest tendency to separate from the blood, and to settle down in the interstices of the tissues. Why does it not settle down within them, except in that small proportion which belongs to the healthy constitution of each organized substance? We feed an animal on oil cake, and we find the marginal cells of its liver, or perhaps all of them, loaded with oil; fat cells formed in streaks all through the interstices of each muscle, yet the muscular fibre itself is free from oil; there are no fat-cells in the brain—no more oil in the kidney-cells than appears quite natural. What keeps the tissues that do not become the seat of fatty deposit, either in their own textural elements or in their interstices, free, but their own peculiar assimilative power? Why does the liver-cell fill itself with oil, and the renal or the salivary forbear; the difference of their condition lies surely not so much in the difference of their supply, as in that of their natural endowments. We see the plasma that exudes from capillaries on the outer surface of the aorta deposits oil-drops in abundance, which coalesce, and become, by the addition of an envelope, fat-cells; but at an eighth of an inch distance the exuded plasma deposits no oil, so long as the arterial wall remains healthy. One can but conclude, from these trite facts, that it is the tissue itself which, by its own vital assimilative power, determines or regulates the kind of material that shall separate from the plasma, exuded and penetrating between its elements. Commonly, it opposes no let or hindrance to the separation and organization of oil into fat-cells; there are very few parts where fat does not form, at least in microscopic quantities. It is very interesting to notice, in conjunction with its entire absence from the brain, the existence of an unusually large proportion of combined oil in the tissue of this organ. The non-deposition of oil in healthy cartilage, or in the cornea, is certainly a wonderful phenomenon. How peculiar must be that force which, while saturating these textures with nutrient plasma, does not permit that constituent to separate, which would impair the cohesion of the one and the transparency of the other.

We think the foregoing observations, supported as they are by our examination of the point, justify us in regarding the atheromatous deposit as a primary fatty one, taking place amid the elementary parts of the tissue, and doing so actually in consequence of the flagging and failing assimilative power of those parts which in the healthy state would not permit of it. As the abnormal unorganizable deposit increases, the decaying tissue wastes and disappears, not undergoing conversion into oil—or at least only partially — but rather, simply liquefying, and becoming absorbed. What we have seen of the fatty degeneration of the cerebral vessels quite corresponds with this view; it is altogether impossible to suppose that the wasting coats are themselves converted into oily matter, or that this results from any previous exudation undergoing change; the oily matter evidently settles down on the wall of the artery, which is unable to nourish itself healthily, and there accumulates till it may form groups resembling even granule-cells. In one of our examinations we found a vessel, whose wall consisted of homogeneous membrane, closely beset with nuclei, representing the commencement of the circular fibrous coat, so atrophied in one part that there remained nothing but the homogeneous membrane; this was, however, quite free from fatty deposit, and it seemed clear, that while many of the other vessels were affected by fatty deposit, this, on the contrary, had undergone simple atrophy.

Proceeding next to the case of the heart, and remembering the ceaseless activity of this organ, is it a thing conceivable, that the living working fibres should undergo fatty change in the same way as a piece of dead muscle does? While they act at all, their nutrition must go on; and is it not far more probable that their fatty change occurs in the way of replacement of albuminous matter by oil, molecule for molecule, than by an actual conversion of the substance of the fibre into oily matter, as takes place in the formation of adipocere? Is it not more conformable to the analogies of living tissues to regard fatty atrophy of muscle as a mere variety of simple atrophy of the same? We know that the muscular fibres of the heart may simply waste, without being fattily degenerated. Dr. Lankester records an instance of rupture of the right auricle, with thinning of its walls, to such an extent that at many points they seemed to be held together merely by the serous covering; there was, however, no fatty degeneration, only perhaps a less decided appearance of the transverse striæ. We think we have seen a similar atrophic state of the cardiac muscular fibre ourselves; and we believe the only difference between these and the state of fatty degeneration, consists in the albuminous matter being replaced by oil in the one, and not at all in the other.* It is the difference between starving and bad food. We must here refer to what we have said respecting the relation of fatty growth upon the wasting tissue of the heart, to fatty degeneration occurring alone. In both cases the fibre wastes, in both there is a tendency to the deposition of oil from the blood; but, from some cause which we know not, in one the oil is deposited *outside*, and is organized into that lowest and least useful of all organic structures, fat; while in the other, the oil is deposited within already prepared proteinous envelopes.† The two conditions are often combined together, and both essentially depend on the decay and disintegration of the muscular fibre. It scarcely seems credible that the nature of the change should be essentially different in the two cases. When a paralyzed muscle wastes, we consider that its assimilative power languishes: it is unable to construct healthy sarcous elements as before; its former substance liquefies, and returns to the blood just as it did in former periods of activity, but now it is only replaced by inert oil. The supply, however, is equal to the demand; the sarcous tissue, were it formed, could not be used, and therefore it is not formed.

Mollities ossium, though, as we have said, not quite identical with fatty degeneration, yet seems very well fitted to illustrate the nature of this change. The earthy constituents are absorbed, and oil is deposited in their room; these are the main features of the morbid process, and surely they give no support to the view that fatty degeneration of living tissues consists in a chemical conversion of their substance into oil. If it should be proved that the uterus, during the period of inversion, *constantly* undergoes a fatty change, this would most probably be an instance of a tissue degenerating *viâ conversionis*, and not *nutritionis*. But we strongly suspect that the normal kind of retroceding change will be found to be a simple and not a fatty atrophy. We will quote here a remark of Mr. Paget, in which we quite coincide:—"No doubt a person who has a natural tendency, even when in health, to become corpulent, would, *cæteris paribus*, be more likely to have fatty degeneration, than to have a wasting atrophy in any organ which might fall into the conditions in which these changes originate." Thus, in a person predisposed to deposit oil, the wasting uterus might be fatty, while it would not be so in another. We observed, the other day, a difference in two fibrinous casts, from two kidneys which we were examining, which seems worth mentioning in its bearing on this question, though we by no means wish to lay too much stress upon it. The two casts were of that oval shape which they often assume when some time has elapsed since their formation, and which, by the way, often may cause an inexperienced observer to mistake them for cysts. One was in a large, mottled, fatty kidney; it was full of oily molecules, dispersed through its substance. The other was in a contracted granular kidney; it was quite free from any oily matter. The fatty matter in soft cataract might be the result of a conversion of the lens substance; but as Lebert says

* From some observations we have recently made, it appears to us that the degenerated fibres of fatty hearts are at least quite equal in diameter to those which are healthy. The inference from this is the same as in the case of the glomerulus.

† Lehmann says, p. 222, vol. iii. of his Chemistry—"If the organism does not find in the food material enough to form the envelope of the fat-cells, it borrows the material from the muscular fibre in order to dispose of the fat in those proteine envelopes."

the laminæ are softened and *hypertrophied*, we think it more probable that it is a deposit arising from unhealthy nutrition.

We think that we have evidence, in the foregoing pages, of the following varieties of fatty change:

(1) Fatty conversion of mere masses of dead matter, organized or non-organized, without or within the body.
(2) Fatty degeneration of organized and living tissues, reducing them gradually to a lower grade of organization and vitality.
(3) Fatty replacement, by a growth of fat-cells taking place on an organ which is either in a state of simple or fatty atrophy.
(4) Fatty replacement of granular matter of epithelium without any degeneration.

We doubt not that these several conditions are in some measure allied, especially (2) and (3); but it seems to us, that to attempt to bring them all together under one common head, when there are such manifest differences between them, would be an unwarranted generalization.

The causes of fatty degeneration have already been incidentally referred to. They are recognised by all observers as those which lower the vital energies. Defect of activity in the various excreting organs of course causes an accumulation of oily matter in the blood, which finds vent by being deposited in all the interstices of the organs, and taking the form of adipose tissue. The same will occur if, by means of the food, an excess of oily matter is introduced into the circulation. So long, however, as the organic life of the tissues persists, they will nourish themselves healthily out of this oil-laden blood; but when that begins to decay, then oily matter settles down in situations from whence the organizing force before excluded it. The effects of spirit drinking, as we have above seen, illustrate extremely well the two principal conditions of the change. Impairment of excretory action, and pouring in of an hydro-carburet into the blood, cause it to be loaded with oil; while the debilitating action of the alcohol on the nervous system, and through it probably on all other parts, lowers their vital energies, and at length so enfeebles their organic life, that they can no longer maintain their healthy construction.

We have so often made use of the terms vitality, assimilative force, and the like, that we think it just necessary to state, that we do not mean to advocate the doctrine of a vital principle, an "organic agent," controlling and regulating all the actions of the system, and preserving the due composition of each part. We mean, by the terms in question, to convey no more than the bare, inexplicable, but all-important truth, that some force distinct entirely from ordinary chemical or physical agencies, causes organic matter to assume and retain a certain definite form while it makes part of a living body. The matter thus organized is endowed with peculiar properties, which it loses when it abandons the form given to it and returns to the condition of organic matter.

We do not suppose that we explain any phenomena by using these terms, but we wish thereby strongly to assert the existence and supremacy of a force which has been too little considered by the chemical school of the present day. He who has watched, day after day, the shapeless organic substance of the ovum gradually taking form, and evolving from its own homogeneity the several organs and tissues, each in its destined place, and of its predetermined figure and composition, can never believe that any modification of chemical or physical agencies can work so "wonderfully and fearfully." The practical physician well knows that to aid in maintaining this force unbroken and vigorous, is a principal aim of his art: he knows, when it is once seriously decayed, how little remedies avail, and how like the labour of Sisyphus it is to try to uphold a system where it is naturally languishing and defective.

To attempt to enter upon the subject of the treatment of fatty degeneration would be out of place here; but we cannot close without expressing the feeling which is strongly fastened on our mind, that a large portion, probably the majority, of the diseases of the present day, are of the nature of degenerations, commencing unperceived, advancing gradually, and often scarcely attracting attention, till irreparable mischief is done. We do think it behoves medical men to impress strongly on the minds of parents and of friends, the deep need there is for watching carefully the first symptoms of failing health; and of not deferring to seek aid until serious disease has sounded its alarm, and disorganizing processes have commenced that can never be thoroughly repaired.

Handfield Jones.

Review IV.

Ueber das Albumin als Bestantheil des Harns in Krankheiten. Von J. Flor. Heller. ('Archiv. für Phys. und Path. Chem.,' N. F. 1852.)

On Albumen in the Urine in various Diseases. By Dr. F. Heller.

The discovery of the coagulability of the urine, to whomsoever it is to be attributed—now a difficult task to determine—and its association with structural disease of the kidney, a discovery undoubtedly due to Dr. Bright, are justly regarded as two of the most important facts in medical science. It does not detract from the merit or interest of the former, that in the first instance it was made by means of but a rude chemistry, while it certainly leads us to hope and to believe that if so great results have followed facts ascertained in a manner so simple, we shall yet, by a nicer chemistry and by more refined analysis, arrive at still more important and more valuable truths; for it is quite plain that upon this subject our knowledge can by no means be considered as already perfect.

Without attempting to write a history of the discovery of coagulable urine, and of the import which more recent researches have given it, we may nevertheless allude to these. It has been remarked—and the observation has much dependence upon truth—that there can be no discovery made in medical science without the possibility of a reference, in some instances sufficiently remarkable, being found in one or other of the works of the ancient authors. The justice of this remark those who have distinguished themselves by the most important discoveries, and whose names are connected with the most direct advances in medical science in our own time, have not been the last to acknowledge, while the perusal of the observations of the Father of Medicine himself, on the urinary secretion, and more especially of his aphorisms as relating to this subject, will not detract from its truth. But this is not the portion of the history of coagulable urine which we intend even for a short time to delay us. We come to much more recent times. In 1795, Dr. Latham, who had devoted much attention to the study of diabetes, and who published a work on that disease, speaks of a patient who had a remarkable and copious discharge of serum from the kidneys; and Dr. Blackall, who saw the patient under Dr. Latham's care in St. Bartholomew's Hospital, thus alludes to the case in his interesting work on Dropsies:

"Two months before his admission he had been attacked by severe shiverings and other febrile symptoms, with vomiting and constipation. On admission he was emaciated, had œdema of the right leg, a small quick pulse, an excessive feebleness and dejection of spirits, a sallowness of complexion, and a foul tongue. The urine was made in larger quantities than natural, and in the night more copiously than in the day-time. A parcel of it was subjected to evaporation, with some expectation of obtaining a saccharine extract. To my great surprise, when the heat rose to 160° F., the fluid became uniformly opaque and white, and a considerable precipitate took place, which, when strained, but not much dried, amounted in weight to more than half an ounce. A similar effect was produced by nitrous acid. The patient quitted the hospital after remaining about three months, and six weeks afterwards died."

Whether or not there was a post-mortem examination does not appear, but at all events, in the words of Dr. Blackall, "facts of this kind had up to that time been but slightly alluded to by medical writers." Dr. Blackall himself, and about the same time Dr. Wells, did investigate the subject with great labour and patience, and they certainly nearer than any others approached the discovery which has since made the name of Dr. Bright so deservedly distinguished in medicine. Dr. Blackall and Dr. Wells both published, in 1812, the former a separate treatise, the latter two papers in the Transactions of a society for the improvement of medical and chirurgical knowledge. Both speak of the participation of the kidneys in certain of the cases of dropsy they describe, as ascertained by post-mortem examination. "The kidneys," says Dr. Blackall, "have been diseased in an unusual proportion in such dissections. In no less than three instances out of the eleven here referred to—viz., three in Dr. Wells' work, and eight in mine—they were thickened or hardened, and even with a confused structure, in two or three containing hydatids or vesicles." Only a few years subsequent to the publication of Blackall's and Wells' researches, Dr. Bright made his valuable and universally-known observations; and these, shortly succeeded by the labours of Christison, Gregory, Prout, and Rayer, and more recently of Owen Rees, Gluge, Bence Jones, George Johnson, John Simon, W. T. Gairdner, and Frerichs, have placed the structural diseases of the kidney, as connected with dropsy and albuminous urine, on a basis as complete and thoroughly understood as is the case with any other organic disease and its allied symptoms with which we have to deal. But with this advance in our knowledge of the organic diseases of the kidney, we have further learned that it is not only in cases where real structural change has taken place in the urinary organs, that we are to look for the presence of albumen in the urine, and we now know that there are a great many other circumstances under which it occurs. In these cases the coagulability is certainly not so permanent; yet, though properly termed temporary, its manifestation is in the highest degree interesting, and the causes on which it depends worthy of investigation. This is a subject which of late years has occupied the attention of many observers, both in this country and abroad. In particular, the albuminous urine which frequently or occasionally occurs in certain febrile diseases has received great attention, as, for example, in Asiatic cholera and in scarlatina, &c.; and in regard to it, differences of opinion have arisen, from the varying nature of the observations which determined them. In many chronic diseases, too, the investigation of the urine has shown its frequent coagulability. It is to the consideration of the occurrence of albumen in the urine under these circumstances, and a few others to be immediately named, that we propose to devote the remainder of this article. In so doing we shall refer to the recorded experience of various observers and to our own—more especially to that of F. Simon, Becquerel, and Martin Solon, whose researches are well known, and are esteemed alike for accuracy and extent. To the 'Journal of Physiological and Pathological Chemistry and Microscopy,' published in Vienna, and of which he is the editor, Dr. Heller has during the past year contributed five papers, the title of which we have placed at the head of this article. The first two of these are chiefly occupied with the subject of the detection of albumen in the urine, and the remaining three with the consideration of its occurrence in different diseases. Though in these papers of Dr. Heller there are recorded many very interesting facts, we do not think that in any very important point he has added to our present knowledge, while in most particulars, his observations, both as regards number and importance, fall short of those of some of the authors already named. Availing ourselves of his researches as well as theirs, we shall now attempt to lay before our readers a short summary of what is known on this most important and interesting subject.

We proceed, then, to the consideration of the presence of albumen in the urine, when there exists no evidence of any serious affection of the kidney. We are obliged to say *serious*, because the desquamative process to which we shall have occasion to refer, is a change, but in very many instances, in our opinion, not a serious one. We propose to view the subject under the four following heads:

I. Coagulability in the urine of persons perfectly healthy, and in certain states of the system, as during pregnancy.
II. In the cases of acute and inflammatory diseases.
III. In certain chronic diseases.
IV. Occurring after indulgence in particular kinds of food, after certain remedies, and after the application of blisters.

Before proceeding to the consideration of these four cases of albuminuria, and in order to avoid misconception, and to save repetition, we may for a moment allude to the coagulability of the urine arising from the admixture of blood, or pus, of the seminal fluid, and of the discharge from an inflamed or irritated mucous membrane, as in vaginitis or vesical catarrh, or gonorrhœa in the male. In these cases only a little care is requisite to render apparent the immediate cause upon which the presence of albumen in the urine depends. For this purpose the microscope is the means to employ; by it the presence of blood-corpuscles or pus-globules can be easily ascertained, or the characteristic appearances in the case of an inflamed mucous membrane. This subject we now dismiss, and pass on to the consideration of the first of the four divisions noted above—viz.,

I. *Coagulability of the urine of persons in health*, &c.—This need not detain us for more than a very short space. The fact of albumen having been found in the urine of persons in perfect health rests on the authority of a most competent observer, F. Simon, who says, that he found the urine coagulable in the case of a perfectly healthy and vigorous young man, aged 26. We refer to this observation of Simon, because it has been so very generally quoted and used to support the argument of albumen in the urine being no certain test of renal disease. We are aware of no other observations which would tend to confirm this one of Simon. The generally entertained opinion is certainly opposed to it, but the facts upon which this opinion is based cannot, on the other hand, be said directly to gainsay it. The amount of observation required to determine its probability would be very great, and considering the kind of experiment required—namely, the examination of the urine of persons in health, not at all likely to be undertaken.

During pregnancy, the occasionally albuminous condition of the urine has been noticed by several observers, and a variety of opinions has been entertained in regard to its cause. The relation subsisting between puerperal convulsions and the coagulable state of the urine, notwithstanding the study which has been devoted to it, still requires more complete investigation. Dr. Blot, who wrote his inaugural dissertation for the Faculty of Medicine at Paris on the subject, in 205 instances found albumen in the urine in 41, the greatest proportion of the number being first pregnancies. Dr. Charles Campbell, a distinguished lecturer on Obstetric Medicine in Paris, and who witnessed almost all the observations of Dr. Blot, informed the writer, that in his experience the urine towards the completion of pregnancy was not unfrequently charged with albumen to a greater or less degree. Many other interesting observations on this point have been made by different authors, particularly by Dr. Cormack and Dr. Simpson, and by Professor Litzmann. The albuminuria of pregnancy is not unfrequently connected with œdema of the cellular tissue, particularly of the lower portions of the body, the feet and legs. Sometimes, also, this œdema exists without the albumen in the urine. The view which ascribes both of these phenomena to a mechanical cause of pressure, is, in our opinion, the most probable. The fact of albuminuria being more common in first pregnancies, and in cases of twins, renders this view all the more likely: it certainly is not unreasonable to suppose that the pressure exercised by the enlarging uterus leads to renal congestion, and that to the temporary albuminuria. We avoid entering here on the subject we have already incidentally alluded to—namely, the connexion between the condition of the urine and that of the kidneys in cases of puerperal convulsions. In it there remain some interesting points to reconcile; we refer to the absence of the albumen, though diligently looked for, in cases where complete, or at all events considerable, disorganization of the renal structure has been found on dissection; further, to the presence of albumen in large amount, where no affection of the kidneys has been discovered after death; and last of all, to cases of puerperal convulsions, most severe in character and even fatal, in which no albuminuria existed during life, and no renal disorder was found after death.

II. *Albuminuria in cases of inflammatory or acute diseases.*—"In many acute diseases," says Lehmann, "unconnected with affections of the kidneys, albumen not unfrequently appears for a short time in the urine." To the truth of this remark, all who have paid any degree of attention to the subject will subscribe. The existence of albumen in the urine can no longer be looked upon as certainly indicative of renal disease, for in reality it is not so; indeed, in some instances, in cases of acute diseases, the manifestation of albumen in the urine may be looked upon as a favourable symptom, in so far as it is indicative of the return of a diseased part to a healthy state, and to the exercise of an impaired or even suspended function. That in some instances, in cases of acute diseases, the presence of albumen in the urine may serve to show a greater liability, either, on the one hand, in the disease itself, or, on the other, in the individual suffering from it, to affection of the kidneys, it is not unreasonable to suppose,—and the present state of our knowledge on the subject renders it impossible for us to deny, but we feel disposed to believe,—that, however jealous and zealous a circumspection be exercised over the future progress of many who have, during the continuance of acute or inflammatory diseases, suffered from a temporary albuminuria, no recurrence of the affection will ever ensue, and that no fear need be entertained from that circumstance of the ultimate development of organic renal disease. These views may not, indeed are not, in unison with those still entertained by many able and accomplished physicians, who, too, by their study of renal diseases and the morbid qualities of the urine, have extended our knowledge, and constituted themselves authorities on this very subject; and this consideration would lead the writer to speak with becoming deference. In directing attention to some of the acute diseases in which albuminuria is apt to occur, it is proposed, for the very reason of the difference of opinion which still prevails, to consider each disease, and to seek the cause for the production of the albuminuria in it, *per se.* In febrile diseases, properly so called, and particularly among the exanthemata, a temporary albuminuria may frequently be observed at two distinct and different periods of the disease; the first, at nearly the outset of the febrile excitement; the second, towards the termination of the case, after the crisis is passed and the process of convalescence commenced. We shall speak, firstly, and very briefly, of the urine in erysipelas; secondly, in scarlatina, and the dropsy which succeeds it; and lastly, in cholera and in variola.

In *erysipelas*, during the acute stage of the disease, the urine always presents, in a high degree, the characters of febrile urine. In two out of five cases during this period, Becquerel detected albumen. In four out of six cases, Dr. Begbie found the urine at the same stage of the disease albuminous, while in three of the four the albumen was associated with blood. At the period of desquamation after acute attacks of erysipelas, more especially if a considerable extent of the skin has been affected, it is not unusual to find the urine coagulable. Occurring at this the desquamative stage of the disease, the coagulability may last for several days, and while it exists, the urine is generally charged with epithelium derived from the minute renal tubes. Lehmann speaks of the presence of albumen in the urine during the desquamative stage of erysipelas, as nearly as frequent as after scarlatina; and this, as we have elsewhere observed, coincides with our own experience. In many cases of erysipelas, occurring both in hospital and private practice, we have found the urine coagulable, and in all such, the coagulability has been associated with the presence of epithelium. To the association of the epithelium with the albumen, in common with Lehmann and many others, we are inclined to attribute great importance, as will appear more fully in speaking of the urine in scarlatina. But of this we feel certain, that a temporary albuminuria, more particularly occurring in acute febrile diseases, will hardly be present without desquamation of the epithelium from the tubes of Bellini; that desquamation is often speedily accomplished, and so the albuminuria may be short-lived; and it is for this very reason that we have often practised the examination of every specimen of urine passed, and in no case neglected to examine it for more than twenty-four hours. The truth is, that unless very frequent examination of the urine is practised, the coagulability may escape detection, and this applies as well to erysipelas as to those other diseases in which the temporary presence of albumen in the urine has been noted. In this way the different statements and varying observations of authors may be partly reconciled. The albuminuria, in the case of erysipelas, is most apt to occur in those instances where the inflammation of the skin is idiopathic in its origin, and in which a large extent of surface has been affected. The condition of the urine in erysipelas is of great interest in connexion with a point which, though it has never been brought prominently forward, yet we know to be familiar to not a few—namely, the not unfrequent development of organic renal disease in those who have suffered repeated attacks of the inflammation of the skin. It is possible that the want of due and necessary care and precaution during the period of convalescence from erysipelas, more especially in cases where the temporary presence of albumen in the urine is noted, may lead to the production of serious structural change. Again, we know well that the habit of intemperance predisposes, in a very marked manner, to Bright's disease, and also in a scarcely less so to erysipelas. These two diseases are also in a great degree dependent on long-continued gastric derangement. Again, it would not be difficult to show, from the success of treatment, that the affection of the skin is in some way or other connected with the renal function, and is often most efficaciously treated by remedies which sensibly affect the secretion of urine. Nothing can in this respect be more marked than the effect of the tincture of muriate of iron, a remedy of unquestionably great virtue in the treatment of erysipelas. This is a subject which invites further attention, and is well deserving of it.

Secondly, in *scarlatina*. As in other acute or febrile diseases, the urine during the stage of greatest febrile excitement occasionally manifests the presence of albumen. Becquerel says it is by no means rare. We have seen bloody urine on the second day of the disease in one case which ultimately recovered; it was accompanied by a greatly decreased secretion and by severe pain over the loins. But this is not a usual occurrence; and while the characters of the urine in the early stage of this disease partake entirely of the inflammatory nature common to other diseases of a similar kind, it is a rare circumstance to find any such marked deviation from a healthy standard as the presence of albumen or blood. Let us now regard the urine during the desquamative stage. On this point recorded observations are very various. We shall note some of these, and then attempt to reconcile their differences. Martin Solon, of Paris, a most accurate observer, found albumen in the urine in 22 out of 23 cases. In the case of a man, aged twenty, which occurred in Schönlein's clinical ward, the urine was very albuminous during the period of desquamation, and continued so for four days without the occurrence of dropsy. F. Simon says albumen is commonly, but not always, found in the urine during desquamation. Philipp, in Berlin, where scarlatina was very prevalent, and anasarca could not be warded off, found at least sixty cases in which the urine was tested, both by heat and nitric acid, and no trace of albumen could be detected. This experience of Philipp is perhaps the most uncommon of any, for as a general rule, and, indeed, a rule admitting of very few exceptions, the urine in dropsical cases, as we are led to presume his all were, is coagulable. These facts will serve to show the variance of experience and opinion on this point. That of the writer corresponds in a great extent with the observation of M. Martin Solon. In the course of very careful and repeatedly-instituted experiments, he has found the coagulability of the urine to be very common during the progress of desquamation, commencing generally a day or two after its beginning, and lasting sometimes during several days, but generally for only two or three. This coagulability he has further invariably found to be associated, as Lehmann and others have observed, with a considerable amount of kidney epithelium, but never with the fibrinous casts of the renal tubes. He has in these instances found no diminution in the quantity of the urine secreted, and no other alteration in any of its sensible qualities, nor has he ever observed any indication of renal congestion, as evidenced by lumbar pain or uneasiness, or any of those symptoms which accompany the dropsical sequelæ of the disease. From this experience the writer was led to conclude that the albuminuria and the desquamation of the epithelium of the minute renal tubes stood to each other in somewhat of the relation of cause and effect. On this subject we must beg to refer to a paper in the 'Monthly Journal of Medical Sciences' for October, 1852.

Reverting now to the various experiences, some of which have been cited above, it is important to inquire what means we possess capable of accounting for this difference of opinion, or rather of observation. It has been suggested by Dr. Christison as not unlikely, that during the prevalence of certain epidemics, or at different periods of the same epidemic, the albuminuria may be present or absent, just as we find the presence or absence of certain characters which mark the features of particular epidemics, as well of scarlatina as of other diseases of a like kind. There exists a good deal to favour this view, so far as the experience of observers in Edinburgh is concerned, and a short reference to that may not be uninteresting. Scarlatina, which previous to 1848 had not been for some years prevalent in Edinburgh, became towards the middle of that year decidedly epidemic. The writer was then residing as physician's clerk in the Royal Infirmary, into which there was admitted a large number of patients suffering from scarlatina. The urines of almost all these cases were examined daily, and the result was, that in twenty-one consecutive examples the presence of albumen was detected during the progress of desquamation of the skin. At nearly the same period Dr. Newbigging, with most enviable opportunities in John Watson's Hospital, was engaged in making observations on the urine, and arrived at the same results; which, in common with some other interesting facts, he laid before the Medico-Chirurgical Society of Edinburgh, and afterwards published in the 'Monthly Journal' for November, 1849. At a period subsequent to this, Dr. Andrew Wood, and with opportunities very much the same, examined the urine daily in a large number of cases, and found coagulability in less than a half. Since then, Mr. Benjamin Bell and Dr. James D. Gillespie, the former in George Watson's, the latter in Donaldson's Hospital, have made frequent examinations of the urine in all the cases of scarlatina which occurred during the epidemic prevalence of the disease in these institutions, and have found no case of albuminuria separate from dropsy. We shall presently show that there exist certain circumstances in some of these different observations which demand inquiry before they shall be considered as entitled to full weight. But dismissing these objections for the present, and assuming the examinations to have been made with the greatest possible care, and with attention to all the necessary and required circumstances, then the recent experience in Edinburgh amounts to this: that at the early period of the epidemic, albuminuria existed in every case—the experience of the writer and of Dr. Newbigging; that shortly afterwards it was detected in less than one-half of the cases—the experience of Dr. Andrew Wood; and that more recently it has never been found—the experience of Mr. Benjamin Bell and Dr. Gillespie. So far this gives a measure of probability to the suggestion already alluded to, that possibly during certain epidemics, or at certain periods of the same epidemic, albuminuria may be looked for in vain.

Let us now consider the circumstances we have referred to as causing a variance in the observations. It appears that the same care was not exercised in the conduct of the experiments. A daily examination of the urine was considered by the foreign observers, who first drew attention to the subject, as imperative; and it is sufficiently remarkable that the daily examination, as practised by the writer, by Dr. Newbigging, and Dr. Wood, detected the albumen; in the experience of the two former, almost invariably, in that of the latter, in a considerable number of cases. The fact of the urine not having been examined daily invalidates to a certain extent, in our opinion, the experience of Mr. Bell and of Dr. Gillespie. We are fully warranted in holding this opinion, from having seen the presence of albumen in more than one instance limited to a day, and in several not continuing beyond two. Further, it has been suggested by Dr. Christison and by Dr. Wood, as not improbable, that the difference of treatment may have some effect in causing, or rather in preventing, the occurrence of albuminuria; and in particular reference has been made to the use of the warm bath, a most powerful, and, in the hands of Dr. Wood somewhat modified, a most useful, means of treatment. It is argued that the derivation from internal organs, and determination to the skin, secured by the warm bath, have been instrumental in banishing the albumen from the urine. This is a point which further experience is required to determine. Some circumstances render it tenable; for example, the efficacy of the warm bath in the treatment of the scarlatina dropsy, and in Bright's disease, both before and after the first occurrence of dropsy. In connexion with this, it is further of interest to know that none of the cases referred to in the experience of the writer were treated with the warm bath, the inconvenience of the bath arrangements rendering that plan, however desirable, inexpedient. It is possible, as already hinted, that the severity of the cases may determine the presence of albuminuria; and the experience of the writer has convinced him that in those cases in which the desquamation of the cuticle is most abundant, it is most apt to occur. A question suggests itself, whether or not the age of the patient may affect the changes in the urine. The writer's experience had reference almost entirely to adults. The dropsy occurring after scarlatina we know to be most common among children. Is it possible that the temporary albuminuria may prevail more among adults?

Such are some of the suggestions worthy of consideration, which the experience of observers in Edinburgh on this subject gives rise to. That experience is in part confirmatory of, and in part opposed to, the observation of the writer, who may in the first instance have expressed his opinion too strongly, but who is very willing to allow the subject to stand the test of further experience and careful experiment, and so to rise or fall.

Both of the diseases the urines of which we have now considered are by many classed under the division of Exanthemata; but in regard to one of them—namely, erysipelas—it is contended by some that its claim to be admitted into that class has not been made out. For our own part, the observation of its contagious property, of its mode of origin, &c., has satisfied us that, as an idiopathic disease, it is, as Dr. Alison teaches, rightly so included. To some who are still doubtful on the point, it may constitute another feature of resemblance, when they discover, as upon careful examination they not unfrequently will do, a temporary coagulability of the urine associated with a desquamative change in the renal apparatus, in cases of erysipelas.

The urine first passed in reaction of *cholera* has been noticed by a great many observers to contain albumen. Heller found more or less coagulability of the urine in all the instances he examined. In the Cholera Hospital of Edinburgh the proportion of cases in which coagulability was detected was upwards of sixty per cent. The presence of albumen in the urine of cholera is associated with several other very interesting and important characters—viz., extreme deficiency, often absence of urea, the presence of bile or biliary colouring matter, and of renal as well as bladder epithelium. In numerous instances which have fallen under our own observation, the urine was not unfrequently highly coagulable, but more generally the amount of coagulability was not great, though quite decided. In these instances, the presence of albumen did not continue long, in most indeed for only a few days, generally decreasing in amount from day to day. These facts, in regard to the urine in this disease, have been borne testimony to by the observations of Dr. Parkes in London; of Levy, Martin Solon, and Rostan, in Paris; and of Dr. William Robertson and the Writer in the Cholera Hospital of Edinburgh, besides many others. One material point of difference between the urines of the exanthemata and that of cholera, as we have now considered them, exists in the scanty elimination of urea in the case of the latter. One very interesting and important point of resemblance exists between them in the presence of the renal epithelium, evidencing the occurrence of desquamation, upon which, in our opinion, the passage of the albumen from the blood depends. The epithelium and the albumen always appeared together, and when the latter failed to be detected the former had again disappeared. To some it may appear inconclusive to argue that, independent of congestion in the renal capillaries, the albumen, owing to the constant separation of the epithelium lining the minute tubes, finds its way in the current of urine, but where there exists during life, as in the instance of scarlatina, no evidence of renal congestion, no lumbar pain or uneasiness, no diminution of the quantity of urine, and no other alteration in any of its sensible qualities; and, as in the instance of cholera, no trace of that congestion after death, it does not appear to us unphilosophic to suppose, that such an alterative change as desquamation in an organ so delicate in function as the kidney, and the secreting portions of which are the parts concerned in the change, is the only cause of the passage of the albumen. The function of the kidneys in the case of cholera is no doubt more seriously impaired than in either scarlatina or typhus, &c., because the urea, whose presence in the blood is a fruitful source of mischief, is either absent from or in very diminished quantity in the urine; but even this difference, striking and all-important as it is, does not render less likely the dependence of the albumen in the urine of all, on a common cause.

We have spoken of the albuminous urine of simple scarlatina—let us now consider the condition of the urinary secretion in the *dropsical* disease. As upon the former, so as regards this, there is a variety of opinion dependent upon the different results of observations. But though some have gone the length of dividing the cases of post-scarlatina dropsy, according as the urine contains albumen or does not, so calling it albuminous and non-albuminous, it will, we think, be acknowledged as the almost invariable experience, that in the dropsical cases the urine is always more or less charged with albumen. To all who have attended with any degree of care to the condition of the urine in this affection, the dissimilarity subsisting between its characters and those of the urine in the simple scarlatina must have been very noticeable. First of all, the quantity of urine in the dropsical disease is greatly diminished, indeed, the very diminution of the secretion is a most valuable prognostic of coming dropsy. Then, blood, even to the extent of causing an abundant sediment, and very frequently so as to give its peculiar colour to the urine, is present. Besides these characters, others equally marked are very commonly present, exudation-corpuscles, much epithelium, and the fibrinous casts of the small tubes. These changes in the urine afford evidence of renal congestion, and they do so in common with the symptoms of the patient, for in such cases a diminished and altered secretion is not more invariable than the existence of febrile excitement, often to a very high degree, and of severe lumbar pain and uneasiness; other symptoms, too, such as vomiting, often exist. The coagulability in the dropsical disease is generally much more lasting, and though commonly temporary, yet there is reason to apprehend the establishment in not a few of such cases of permanent renal disease.

In *variola* there are, just as in the other febrile diseases, two periods when the presence of albumen in the urine may be detected, in the early febrile, and in the suppurative or critical stage. In five out of eleven cases, M. Solon found the urine coagulable. In a very bad and ultimately fatal case of variola, which we saw in the Royal Infirmary, the urine was highly charged with blood on the second day after the appearance of eruption, and continued to be so till the eighth day, when death occurred. In another case, which also proved fatal, and in which the variolar eruption was found after death in nearly the whole course of the intestine, albumen, and subsequently blood, were present in the urine from an early period of the disease. During the period of suppuration albumen is sometimes present, and after desquamation has commenced it is not unfrequently so. At this stage, Simon says, sediments of purulent mucus are thrown down. We are unable exactly to corroborate this statement, for we have not found the presence of true pus to be common in the urine after any acute disease. Abortive epithelium, and albumen, together with amorphous or crystalline urates, on the contrary, we have found to be very common; and this observation we have frequently had occasion to make, that in those diseases, whether febrile or more truly inflammatory, whose height or critical period is marked by suppuration, these ingredients of the urine, and the deposit their presence occasions, are most common. The urine of variola has in our experience formed no exception to this rule.*

We have thus considered, under the head of febrile diseases, the urine in erysipelas, in scarlatina, in cholera, in dropsical scarlatina, and in variola; and with these examples we propose to leave this division of the subject, and pass to the condition of the urine, so far as its coagulability is concerned, in certain inflammatory diseases; and first, in pneumonia.

The presence of albumen in the urine of *pneumonia*, about the period of the resolution of the disease, has been noticed by Becquerel, Simon, Andral, Finger, Heller, William Aitken, and others. We have ourselves frequently observed it. Heller makes the interesting remark, that he has found the albumen in something of an inverse ratio to the chlorides, that is, at the commencement of exudation, when the amount of chlorides continues normal, he has failed to detect albumen, but as the exudation increases, and the chlorides diminish, he has found the albumen to appear, and to continue present for a considerable period. Though by no means constant, Heller has found the coagulability of the urine in pneumonia to be very frequent. Simon, at the period of crisis, found the urine albuminous in twenty-two out of twenty-four cases. Becquerel and Andral have noted the coagulability of the urine during the inflammatory stage of the disease in a few cases. Our own observation has satisfied us that the appearance of albumen in the urine, even in considerable amount, is by no means an unfrequent phenomenon at about the period of resolution in pneumonia. In eleven cases where the inflammation was highly sthenic in character, where condensation rapidly occurred, and in which the period of resolution was well marked, we have found the urine coagulable. The coagulability of the urine in pneumonia has been associated, and apparently upon reasonable grounds, with the resolution of the disease, with the absorption of the pulmonary exudation, and the return of the lung to its healthy condition. The period of its recurrence and certain other of the characters the urine presents, favour this idea. Schönlein and others have indeed shown, and their view is generally believed, that it is by the kidney as an emunctory that the great mass of the exudation in cases of inflammation of the lung, as well as of other

* Since this review was written, a most interesting case of variola, in so far as the phenomena presented by the urine were concerned, has come under our notice. After the continuance of severe hæmaturia for three days,—from which the patient, a gentleman aged thirty, had never suffered before,—the eruption of variola and the disappearance of the blood from the urine occurred simultaneously on the morning of the fourth day. Till the ninth day of the disease the urine continued faintly coagulable, when the amount of albumen increased amazingly, and the urine became, and for three days continued to be, very highly coagulable. (A specimen of the ninth day's urine was shown to the Medico-Chirurgical Society of Edinburgh.) At the end of the third week the coagulability was scarcely discernible, and now, after the patient's return from a month's residence in the country, the urine is entirely free from albumen.

organs, is removed from the system. In the case of pneumonia the albumen is accompanied by a deposit of amorphous, sometimes of crystalline lithates. These, too, have been accepted as the pulmonary exudation chemically transformed, passing from the system. It is possible, that, in many cases, the exudation altogether undergoes this chemical transformation, but it is equally certain, that in others it appears only to have been partially so, and a portion at least passes in the current of urine as albumen.

In acute *pleurisy*, Heller has occasionally found the urine coagulable at the period of absorption. In acute inflammations of the heart, our author has also found albumen in the urine, more frequently in pericarditis than in endocarditis. In peritonitis he has frequently found coagulability of the urine.

In the case of pleurisy, we can to a certain extent confirm Heller's observation, having found albumen in two or three instances; to these, however, we have always hesitated to refer, as, in all, the application of blisters was employed just about the period of commencing absorption, and blisters, as we shall immediately have occasion to state, are a frequent cause of the temporary presence of albumen in the urine.

III. *Albuminuria in certain chronic diseases.*—Heller asserts, and probably many would at first sight be apt to join with him in the belief, that in all kidney lesions albumen will be found in the urine. For our own part, we are disposed to accept this statement with some reservation; for while it is scarcely possible to conceive the continual absence of albumen from the urine in cases of advanced or even confirmed renal disease of all kinds, still there are many instances in which—during, at all events, a considerable period of time—the presence of albumen cannot be detected. Several examples of this nature have from time to time come under our own notice. In particular, we have been struck by the presence of a very considerable tubercular deposit in the kidneys of a patient whose urine during life, repeatedly examined, had never contained albumen. Again, the scrofulous abscess of the kidney we have seen after death, in cases where no coagulability had been found. Both of these lesions of the kidney we know, however, are sufficient, in many other instances, to cause the manifestation of albumen; and perhaps the correct explanation of the absence of coagulability in the former, lies in the consideration of the amount of the renal substance which has been involved in the morbid deposit. In the case of scrofulous abscess, it is very probable that during the active process of its formation there existed albumen in the urine, but that after a time, when the formation of the scrofulous and putty-like mass of which such abscesses are composed, and after the abscess itself had become surrounded by parietes of its own, either the function of the kidney was carried on altogether independent of that portion in which it was situated,—or, as in some instances, the whole kidney being implicated in the scrofulous degeneration, the secretion of the urine was entirely carried on by the other, either altogether or at least tolerably healthy organ. Such considerations tend to limit the universal application of our author's statement, and others of a similar nature might be started. But even in cases where there can be no reasonable doubt of the existence of extensive renal disease, even of Bright's disease, the urine may, for a time longer or shorter, contain no albumen. Whatever the true explanation of its limited absence in such cases may be—whether it may depend on certain changes in the kidneys themselves, or, more probably, on the altered chemical constitution of the blood—of this, at least, there can be reasonably no doubt, that such cases do occur. We have ourselves seen individuals presenting all the appearance of labouring under renal disease, suffering moreover from dropsy, and, so far as most careful examination would go, free from disease of any other internal organ, yet passing a urine containing no albumen, and that during a considerable period of time. In such, however, there does appear, sooner or later, the presence of albumen in the urine; and the nature of the case, which was at first somewhat doubtful, then becomes established.

Did the nature and limits of this review permit, we might here allude to many very interesting phenomena in connexion with the urine of Bright's disease. One very remarkable feature in certain cases of this affection has been pointed out by Dr. Parkes—viz., the occasional greatly increased quantity of the albumen after meals; though in many cases, of course, this increase cannot be detected.

In all organic affections of the abdomen, but particularly when associated with ascites, albumen is occasionally found in the urine. Thus, in cases of cirrhosis of the liver, it is so; and Schmidt remarks, that in affections of the spleen, when anasarca exists, the urine is often albuminous. Schmidt has recently recorded the following case of this nature:—A man, æt. 18, who had lived for a long time in a malarious district, became anasarcous, and had albuminuria. The anasarca vanished after the appearance of a miliary eruption, accompanied with desquamation over the whole body. The albuminous urine remained unaltered for eight months, when an attack of intermittent fever came on, which yielded to bark: upon his recovery the urine was quite healthy.

In more than one case of disease of the liver, in which there existed no evidence of any renal affection, we have found a small quantity of albumen in the urine; but in these there also existed ascites, and at the time the albumen appeared, diuresis to a considerable extent was being produced by remedies; and to the increased stimulation so excited, it is not improbable the albuminuria was owing. Certainly, coagulability of the urine in chronic diseases of the liver is rare.

In diseases of the heart albumen is sometimes present, but generally when the cardiac disease is advanced; and in such cases we know that there exists a tendency to congestion in all the viscera, and not least so in the kidneys; and thus the albuminuria, which is often very temporary, is caused.

In dropsy from disease of the ovaries, albumen is often found in the urine,—the consequence of the pressure maintained on the renal apparatus, as after the operation of paracentesis the albumen sometimes disappears.

In phthisis the urine is not unfrequently coagulable, when no evidence of Bright's disease is present. The albumen, in such cases, is generally due to the deposit of tubercular matter in the kidneys, though, as we have already noticed, the existence of tubercle in these organs is not invariably accompanied by albuminuria. We have, moreover, seen albumen in the urine of phthisical patients pretty constantly, for many weeks before death, in which dissection revealed no tubercular deposit or change in the renal structure to account for it.

In rheumatic chorea, M. See has found the urine albuminous in two instances: in one, that of a child, who had three attacks of the disease, and along with the first two, dropsy and albuminuria.

In diabetes, albumen sometimes exists in the urine; indeed, it is contended by Schönlein, that in the first stages of the disease no sugar, but albumen, is present, and that as the albumen disappears, the formation of sugar commences. Be this as it may, it is certainly correct that albumen may occasionally be detected in diabetic urine. Simon found it in two instances, and Brett has found casein and butter in the urine of a patient labouring under diabetes.

IV. *Albuminuria occurring after indulgence in particular kinds of food, after certain remedies, and after the application of blisters.*—To these various causes of temporary albuminuria allusion is here only necessary, in order to complete the sketch which we made in the first instance, and none of them need detain us. The facts in regard to all these are well known. Indulgence in pastry has, it is said, been in some the cause of the temporary presence of albumen. Of internal remedies, juniper and cantharides may be mentioned as examples; the exhibition of which, in larger doses than are required, causes the presence of albumen, and even of blood, in the urine. In regard to the third,—the occurrence of albuminuria after the external use of cantharides in the form of blisters,—all are familiar with the degree of dysuria, in some cases amounting to severe strangury, which so frequently follows. When the blisters are applied over cupping-marks, the presence of albumen in the urine is almost certain to succeed; and even where the blister had been applied over a sound portion of skin, we have seen M. Bouillaud, at La Charité, in Paris, demonstrate the existence of albumen in the urine. In the two latter examples, we seek an explanation in the known congestion of the renal vessels, which it is the specific effect of juniper and cantharides to produce. Why the presence of albumen should follow the eating of pastry is more difficult to explain; and as the experiments on which the statement rests are believed by some not to be authentic, and by others to be capable of other explanation, we shall not here attempt it; the more so, that in our experience we have never seen albuminuria so induced.

In the foregoing consideration of various diseases, acute and chronic, febrile and organic, and of different circumstances in which the temporary presence of albumen in the urine occurs, we must not be considered as having in any degree discussed the whole subject; for there are, besides the diseases we have named, several others in which coagulability of the urine occasionally manifests itself. Of these may be mentioned, continued fever, and typhus, about the critical period of which albuminuria is not unfrequent, and more particularly so in cases of abdominal typhus.

In regard to the coagulability of the urine in typhus, we have been informed by Dr. James Sidey, that for some time past he has been engaged in making careful experiments in the Royal Infirmary of Edinburgh, and that in a very large proportion of cases he has found albumen in greater or less amount in the urine. The period of its occurrence has, in these instances, invariably been towards the crisis of the fever, and, in a large nnmber, on exactly the sixteenth day of the disease. The albumen Dr. Sidey generally found to continue for four or five days.

In many cutaneous diseases, too, the urine is sometimes coagulable. In these the occurrence of the albumen, as in other diseases alluded to, bears relation to the extent of the surface of the body affected, and to the desquamation which occurs. Boeck found the urine albuminous in elephantiasis Grecorum. In eczema we have found the urine albuminous; and in one case of eczema rubrum, affecting nearly the whole body, which we saw in the Saint Louis Hospital, in Paris, the coagulability of the urine lasted so long as to cause apprehensions of the development of renal disease; it disappeared, however, after a time, and simultaneously with the last desquamation which preceded the cure of the case. Heller remarks, that in many diseases of the skin the urine very rapidly becomes putrid; and in herpes zoster he has particularly noticed it. This observation of our author we have frequently had occasion to confirm; and indeed, as a general rule, we have found that in those diseases in which a large portion of the cutaneous surface has been affected, the urine, if not alkaline on being passed, becomes very speedily so. Such urine very frequently deposits crystalline phosphates. The importance of this fact is at once apparent in testing such urine for albumen, for if the previous addition of an acid be not attended to, the employment of heat will assuredly fail to detect the albumen, whose presence in many instances, under just such circumstances, we have seen.

There are also many chronic and organic affections in which coagulability of the urine occurs, besides those we have alluded to; experiments and observations are, on this subject, however, still wanting, and it is here hardly worth while to mention the particular instance in which, in any particular disease, the occurrence of albumen in the urine has been noted and recorded.

We have completed the space allotted to us without entering on the subject of the modes of detection of albumen in the urine, of its quantitative analysis, or of the relation which, under various circumstances, it bears to other ingredients of the urine in disease. These, too, are subjects, especially the last, on which our author has written fully, and has recorded some interesting observations, more particularly as regards the relation of albumen to, and its alternation with, the chlorides; but to these points we may hereafter, in connexion with the investigations of recent authors, and chiefly those of Dr. Lionel Beale on the presence of the chlorides in the urine of pneumonia, have an opportunity of directing attention. Meantime, we trust that we have succeeded in showing how much careful observation is still required in regard to the particular part of the subject which has at this time engaged our attention; it is certainly one of very great importance, and the study which some portions of it has already received from different members of the profession, shows that it is regarded as one of no small interest. These investigations will, we have no doubt, be still carried on and extended; and before long we shall, by their means, have determined under what and how many different circumstances albuminuria may occur; what causes its temporary presence, or more continued manifestation; and, most important of all, what, as a symptom, is the real value of its presence under all the varying circumstances in which it occurs.

J. Warburton Begbie.

REVIEW V.

Unsere Aufgaben. Ein Versuch zur Anbahnung gemeinschaftlicher Arbeiten für die Rationelle Heilkunde. Von Dr. F. W. BENEKE, frueherem Hausarzte am Deutschen Hospital in London, &c. &c.—*Goettingen*, 1852.

Our Problems. An Essay to Facilitate United Labours for Rational Medicine. By Dr. F. W. BENEKE, late Resident Physician to the German Hospital, London, &c. &c.—*Goettingen*, 1852.

MANIFOLD are the concussions which medical science has experienced in the short time of the last half-century; all the errors created by an imperfect generalization, as depicted in the history of medicine, have once more appeared before our eyes in rapid succession. Even now some of them have not yet vanished from the scene; but the conviction more and more is gaining power, that the only hope for the continual and lasting progress of medicine lies in the patient and energetic pursuit of the same method to which the so-called exact sciences of nature owe their flourishing state; we mean the way of sober, unprejudiced observation, and strictly logical induction. Not insignificant is the profit which has already accrued from the exertions of those who have clearly recognised the correctness of this view. No one can, however, help feeling, that the subject before us is enormous in comparison with the limited power of a single being. Many are certainly deterred from entering upon it by seeing how much is wanted, how little can be done, when the observer stands alone; where to begin, and how to begin, is unclear to not a few, who feel a real desire to work, and who are also able enough to give aid in the investigation of many important points.

Great is the progress made in anatomy and physiology, in physical diagnosis and pathological anatomy; but with much reason, Beneke remarks that we have, as yet, gained very little through these collateral sciences for the rational *treatment* of disease, which always must be the main end of our labour. The cause of this sad truth he sees in the circumstance, that our materia medica and our therapeutics are based, to too great an extent, on rude empiricism, uncontrolled by physiologico-chemical experiments, or by pathologico-chemical experience. To remedy this deficiency, Beneke urges the necessity of co-operation according to a certain plan.

If we look at medical and medico-chemical literature, we find that a great number of men, and of able men too, have spent much of their time in pathologico-chemical researches, without at all promoting the progress of science. The principal cause of failure in these investigations is, that they have been working without a *guiding idea.* In order to avoid this, Beneke, in the first instance, developes his views on the origin and nature of disease in general, and on the ways in which the metamorphosis of material may become anomalous. Our limited space prevents us from giving in detail his idea concerning the essential nature of health and disease; it may be sufficient to state here, that disease is considered as originating either in disorders of the alimentary material, or in one or other of the agencies operating upon this (the organs of the body and the atmospheric air). As the abnormal changes in the constituents of the food and of the blood (the product of the food) form the more frequent source of disease, Beneke gives a brief sketch of the normal metamorphosis of aliments, under their different heads of azotized, non-azotized, and inorganic. The normal course of the metamorphosis of these three groups is indispensable for perfect health; it may depart from the standard by being either *retarded* or *accelerated;*—*retarded,* when the single constituents do not run through the complete series of changes; *accelerated,* when the metamorphosis takes place too rapidly. As the series of changes of the azotized components is greater than that of the non-azotized, Beneke concludes that the latter are sooner decomposed than the former, and proposes, as the first *law of dependency* (*Abhängigkeits-Gesetz*) for the metamorphosis of matter, that, according to the larger or smaller quantity of the non-azotized constituents (entering the blood), the metamorphosis of the azotized is more or less rapid. Although we may admit that this is the case in general, we cannot agree with Beneke in thinking that it always must be so; the circumstance, at least, that the series of changes in the azotized nutriment is the longer one, cannot be a sufficient argument to us, as a longer series may be finished, under certain conditions, more rapidly than a shorter one.

As a second law of dependence, Beneke proposes, "that the metamorphosis of the azotized and non-azotized alimentary material is, to a certain extent, dependent on the quality and quantity of the inorganic material." (p. 12.)

After these preliminary remarks, our author proceeds to the consideration of the question:—" Of what nature are the abnormal conditions of the food? What is the nature of the morbid changes of the blood resulting from them, and which is the connexion between this and the disease of the whole organism?" (p. 13.)

Although food may deviate from the normal in *quantity* and in *quality,* yet the quantitative disorders are the most frequently met with, the more so, as those generally considered as qualitative are in reality quantitative, consisting in the presence of a *plus* or *minus* of one or several of the normal constituents. Guided by the idea, that from the disproportion of one group of the alimentary materials, at first a disorder in the metamorphosis of the *corresponding* group in the blood must ensue, and later only in the *other* groups, Beneke divides these disorders into *direct, indirect,* and *complicated* ones. If the quantity of the azotized class exceeds the measure which can be fully digested and carried through the normal series of changes, *direct* disorder in the metamorphosis of the materials of this class results, which will show itself, in the beginning, in the abnormity of its ultimate products of decomposition, and later only in its earlier products. A transitory increase of the ingesta will be the cause of a transitory augmentation of urea and lithic acid in the urine; if the increase is continued for a long time, and is considerable, not only urea and lithic acid, but also oxalic acid, will be excreted in large quantity; and doubtless, also, the azotized constituents of the bile, and the proportion of the albuminates in the blood, will be augmented. In a similar manner, by the use of too large a quantity of the non-azotized and inorganic materials, the metamorphosis of these classes may become disturbed.

Indirect Beneke calls those disorders in the metamorphosis of one group, which are not effected by disproportion in the ingestion of material belonging to the same, but of the materials belonging to another group. If, for instance, the quantity of azotized substance taken within a certain time be not above the normal, but the quantity of the non-azotized be increased, then, according to the first law of dependence, the disorder would show itself in the series of the azotized material, and the same form of disease might result as from direct augmentation in the ingestion of the azotized substances. Beneke considers these retardations of the azotized constituents of the blood as essential in the development of scrofulosis, tuberculosis, and those conditions which are attended by oxaluria; in their highest degree they may cause the appearance of sugar in the urine, which, in such cases, probably has its origin in the azotized elements. Another instance of indirect disorder is found in the fact, that after the habitual use of soda the symptoms of acidity in the stomach and intestinal canal are increased, as well as the proportion of mineral acids excreted in the urine; which phenomena can only be interpreted by a change in the metamorphosis of the azotized and non-azotized organic material of food, effected by the disorder in the ingestion of inorganic materials. The results which Dr. Parkes has gained from his researches on the action of liquor potassæ illustrate, it may be, the same order of facts.

By the coincidence of direct and indirect disorders in the metamorphosis of the single groups, the *complicated* disorders result, which are in reality the most frequent.

Although it is frequently difficult to trace the direct, indirect, and complicated disorders, yet they form in many instances only the first link of a long chain of consecutive conditions, of which the one results from the other, and frequently remains in reciprocal connexion with it. As an instance of a more simple kind, Beneke mentions the connexion between the abnormal production (or we would rather say, abnormal accumulation and excretion) of oxalic acid and the impaired growth of cells. His view is, that by the directly or indirectly retarded metamorphosis of the azotized substances, the quantity of oxalic acid in the system becomes increased; phosphate of lime being easily dissolved in a solution of oxalic acid, the presence of the latter in an augmented proportion tends of course to deprive the system of a certain quantity of the phosphate. This being further essential to the formation of cells, a deficiency of the latter function must result as a third link in the chain; emaciation is the necessary consequence, originating, in this case, in an abnormal accumulation of the azotized ingredients of the blood.

Although we do not yet think ourselves entitled to consider the morbid increase of oxalic acid in the human organism as a product merely of the azotized substances to the exclusion of the non-azotized; and although we cannot admit, in these morbid states, an abnormally increased production, but only the presence in an increased proportion (probably accumulation in consequence of retarded metamorphosis); yet we highly value the manner in which Beneke investigates the origin of disease. If we look only at the dietetical indications resulting from it, without entering into the medicinal treatment, we see at once that the alimentary material must be diminished to such an extent, that all the ingredients can run through the normal series of metamorphosis; we cannot doubt, that by increasing the quantity of the so-called strengthening food, a practice to which patient and medical man not rarely resort, the root of the disease is nourished, and the emaciation must necessarily increase.

After having thus alluded to the abnormalities in the mixture of the blood which may arise from the alimentary material, as the source of the blood, and which he calls therefore primary disorders, Beneke passes to the consideration of the *aggressive agencies*—i. e., the agencies causing changes in the blood—namely, the *atmospheric air* and the *organs of the body*. The former of these acts only upon the blood, as the product of the alimentary material—the latter as well on the blood as on the nutriments.

Without further entering into the contents of this chapter, we may only say, that here again the disorder may be quantitative or qualitative. By the diminished proportion of oxygen brought in contact with the blood, the metamorphosis of the latter must be retarded; disorders must arise, similar to those which are effected by too large a quantity of food. On the other side, by an abnormal introduction (absolutely or relatively) of oxygen, the change of matter must be accelerated.

In considering the action of the *living organs*, their *structure* and *function* must be always considered simultaneously. Dr. Beneke, with much reason, particularly dwells on the importance of the influence of the *nervous system* in the operation of the organs on the alimentary material and on the blood, and in the process of metamorphosis in general—a circumstance which we frequently find neglected in physiological chemistry. The specific agency of the living organ—as the effect of the anatomical structure, modified by the presence of nerves, and their connexion with the nervous centres—Beneke calls *typical energy*. This is, however, not to be understood as if he were admitting a so-called vital power independent of the laws of inorganic nature, but he uses it merely as a term for that as yet inextricable complexity of various factors. The "*typical energy*" of an organ might therefore be shortly interpreted as the product of all the physico-chemical processes under the control of the nervous system of that organ. We can never be permitted to take only into account the mechanical construction of an organ as a merely physical apparatus, but we must always look at the fact that it is endowed with nerves and connected with the nervous centres. Whatever excites or depresses the latter must exercise an exciting or depressing influence on the metamorphosis of the organic matter. Hence the striking influence of joy or grief on the physical state in general. We have twice had opportunities of observing the correctness of Dr. Beneke's remark, that under continued depressing mental influences oxalic-acid crystals appeared in the urine constantly and in very large number, and that at the same time the quantity of lithic acid became increased, while no change had taken place in the manner of living. In another subject we almost daily examined the urine during seven weeks of great excitement and intense mental activity: the quantity of twenty-four hours was not changed; the colour was clearer; the specific gravity, in general 1026—1028, had decreased to 1022—1024; not a single crystal of oxalate of lime could be detected under the microscope, while they were seen almost always in a small number, as well before as after that period; the diet had not been altered; no change had taken place in the amount of exercise.

After shortly alluding to another source of disease, in the disorder of the *egesta* (through the skin, the kidneys, the liver), Beneke advances to the proposition and discussion of the *subjects for inquiry.*

Whatever may be the case we have to deal with, we shall always have to answer the questions: What is the nature of the disease? How did it arise, and how may it be removed? To these questions, however, we cannot answer, without knowing what is the state of health, and what are the means of preserving it. Our knowledge on these points is, as yet, so limited, that they must be placed among the principal subjects of inquiry. If we may say that health exists when the quantity and quality of the ingesta and egesta are normal, then we see arising immediately the following points for investigation:

"1. To find the standard measure for the quantity and quality of the ingesta of twenty-four hours.

"2. To find the standard measure for the egesta of twenty-four hours; for the ultimate bodies of decomposition of the different series of metamorphosis.

"3. To find the standard measure for the atmospheric influences.

"4. To find the standard measure of the effect of single organs, and of the whole living organism on the ingesta and egesta." (p. 46.)

Many points connected with these questions are still in great obscurity, and an exact solution of the problems is as yet impossible; much advantage may, however, be gained already now for scientific as well as practical medicine, by an approximative investigation of the normal quantity of the ingesta and egesta, of their dependence on the difference in the atmospheric conditions, and in the state of the living organism.

The author, therefore, draws our attention, with reason, to these points, as being the principal desiderata for pathology and therapeutics. As the best way to form a correct idea of the normal measure of the ingesta, Beneke advises us to collect as many accounts as possible of the quantity and quality of food consumed in twenty-four hours by apparently healthy persons, in the greatest possible number, in different ages, under different atmospheric influences, &c. Much information would be certainly gained by such observations, but we are inclined to think, that we should gain more by the accurate investigation of the process of nutrition and metamorphosis of organic matter in animals, according to the manner shown to us in the admirable work of Drs. Bidder and Schmidt.* The whole amount of the ingesta and egesta, in the same animal, must be examined during one period, when it is allowed to take as much food as it likes; during another period, when it takes only a limited quantity and special quality; then all the principal excretions (kidneys, lungs, intestines) during a third period, that of starvation; the gain or loss of weight is carefully to be marked on every day. Accurate notes are to be taken of the atmospheric influences, of the activity of the animal, &c. &c. Manifold might be the variations in these experiments; food might be withdrawn and water permitted, according to the desire of the animal; solid food without any fluid; mere vegetable or mere animal food; food without large quantities of chloride of sodium, of earthy phosphates, &c.; food as

* Die Verdauungssaefte und der Stoffwechsel. Milan und Leipzig, 1852.

much as possible deprived of the inorganic constituents, &c. &c. By this method we should find at least how much substance is consumed and decomposed in a starving animal, of a certain species, and of a certain weight, under certain atmospheric influences;—we should further learn, through the quantity of nitrogen contained in the urine and in the alvine excretions, through that of the carbonic acid and water, the proportion of the decomposed albuminous and fatty substance; we should perceive how much food of a certain quality is necessary for keeping the same animal, under certain circumstances, in the same state; and what is the effect of an increase of food, according to the desire of the animal, on the weight, on the excretions, on quickness of movements, &c.—similar observations, although they would seldom have the same accuracy, might be made also on ourselves.

After having shown the necessity of enlarging our knowledge concerning the state of health, Beneke proceeds to the duty of giving the history of cases of disease, which he divides into four parts:

"I. The account of the *development of the disease*—given in so accurate a manner, that, by an accumulation of many cases, *the ætiology of certain classes of disease*, principally of the so-called dyscrasic states of the blood would become more elucidated.

"II. Description of the *present state*, in which not only the physical, but also the physiologico-chemical symptoms must be included.

"III. *Plan of treatment*, according to rational and empirical principles, from which is to arise the *science of dietetics and pharmacodynamics*, based on *physiologico-chemical investigations* or on *undeniable empirical facts*.

"IV. *Carefully given daily records*, not only of the apparent general course of the disease, but also of all the external conditions, of all the ingesta and principal egesta, as well in quantity as in quality." (p. 52.)

I. In the history of the development of the disease, Beneke divides the influences which have acted on the patient into three classes: (a.) *the more remote;* (b.) *the less remote;* (c.) *the proximate influences.*

(a.) *The more remote* influences concern the parents and grand-parents, their occupation, manner of living, the atmospheric influences under which they had been, the diseases they have suffered from, &c. &c. We agree with Beneke, in attributing much more importance to this part of the history of diseases than it is generally considered to deserve. Not only in the so-called hereditary diseases ought we accurately to examine into the state of health of the parents, but in every single case; we must try to find out why in one child of the same parents the tuberculous, in the other one the gouty, diathesis is developed, while a third child may enjoy so-called full health — what may be the state of children whose father was suffering at the time of procreation from syphilitic disease of the bones, while the mother was of a gouty disposition or affected with osteomalacia?—what was the state of health, the manner of living, the residence, of non-tuberculous parents whose children die of tuberculous phthisis, &c.?

(b.) The period of the *less remote influences* commences with the birth. The infantile management, the food and treatment at different periods of life, the former and present residence, the manner of living as well physical as psychical, the previous diseases the patient has been suffering from, should be taken into consideration. We need not dwell on these points; they are in a similar manner treated of in 'What to Observe at the Bedside, and after Death, in Medical cases.'* We must mention, however, that Dr. Beneke does not forget to allude, under this head, to the very interesting subject, as yet only little investigated, in how far we are entitled to judge from the presence of a certain group of diseases on a certain state of the blood and of the whole organism—of a certain crasis, as Beneke expresses himself, and from this, on the absence of another crasis.

As a single instance, he mentions that those suffering from oxalic-acid diathesis are not liable to typhus fever; that we may conclude, therefore, from the previous occurrence of typhus, on the absence of the first-named diathesis in the same period. The great importance of the establishment of many facts of this kind is striking enough; it would throw light on the essential nature of the different groups of diseases. It is a subject we must certainly aim at; but the history of medicine has also taught us, how careful we ought to be in forming a conclusion, lest it may prove fallacious. Although we do not think ourselves entitled to doubt the correctness of the instance adduced by Beneke, we are as yet unable to corroborate it, and we may refer to it to show the difficulty of establishing such a fact. Three times we have had an opportunity of watching, during an acute affection (twice of acute rheumatism, and once of severe angina tonsillaris), patients whose urine usually exhibited a large number of crystals of oxalate of lime. In all these three cases, during the whole of the acute attack, and during the time of convalescence, not a single crystal of oxalate of lime could be detected; in two of the cases, they appeared again after some weeks; in the third case, no examination was afterwards made. Whether the disappearance of the oxalic acid during the acute affection depended on the increased aggression of the atmospheric air and living organs on the blood, or on the diminished ingestion of material during the time of pyrexia, or on both causes, is of no importance to the present question; but these observations show, that it would be erroneous to conclude, from the absence of oxalic acid during the course of acute diseases, that there is a want of susceptibility for acute disease in those suffering from the so-called oxalic-acid diathesis.

(c.) The duty of describing the *proximate* influences or the exciting cause of the disease is a comparatively easy one after the investigation of the circumstances creating the disposition. We must in this manner gradually find the cause, why the same influence in one case produces pneumonia or bronchitis; in another, acute rheumatism; in a third, diarrhœa; in a fourth, ophthalmia; and why a fifth and sixth remain unaffected by it.

II. The general outlines which Beneke gives of the description of the *present state*, and of the value which he attributes to each symptom, fully show, that he is equally practised in the *physical* and in the *physiologico-chemical examination*. Every physical symptom must be carefully observed; this is to be done, however, not merely for the diagnosis of a physical change, but it must also help us in the solution of the principal question—the cognition of the forming material of the organism, i. e., the blood. The observation of those phenomena, from which we may judge of the amount of the typical energy of each organ, and of the whole organism, appears to him of great importance.

* London, 1853. By the London Medical Society of Observation.

Concerning the *physiologico-chemical examination*, Beneke considers the analysis of the blood of less importance than the accurate examination of the process of metamorphosis of the quantity and quality of the ingesta and egesta. Such an examination of the present state, combined with the previous history, must lead to a more complete diagnosis than is generally made, and must immediately guide us to a rational plan of treatment. In order to make Beneke's views better understood, we may be permitted to quote the outlines of a case described in the shortest manner. The patient is a young man, aged 22 years, who has suffered for the last three days from pain and slight swelling of the joints of the hands and knees. Knowing this, Beneke inquires first for the more remote, then for the less remote, and then for the proximate influences. The patient is the son of a consumptive mother, who died eighteen years ago; his father is still alive, and is a healthy man. He was suckled by his mother. After being weaned, he had milk-and-water with bread for breakfast and supper; and after the 15th year, coffee, with bread-and-butter. Has always enjoyed his dinner; seldom missed meat, but has been used to eat, also, many potatoes and much rye-bread. He generally took much salt with his food; drank soft water; and after his 15th year, he had the allowance of a quart of small beer daily. He had been always very fond of sugar and sweet things. In his earlier years, the patient lived in the country (north of Germany); later, he was employed in a sugar-house in London, where he was frequently exposed to intense heat, producing profuse perspiration. Has never been in poor circumstances. When a child, he began late to walk; has since suffered much from toothache (principally from the 10th to the 15th year); has at present eight decayed teeth. When a child, he had also slight swellings in the neck. Has not been affected with typhus or ague; but has had measles (without epidemic cause) in the 6th year. As a boy, he also suffered frequently from epistaxis. On one of the last days before the outbreak of the disease, the patient took very large meals; and on the day previous to the appearance of the affection of the joints, he was exposed to a noxious cause likely to give origin to cold (working in a cold draught of air, with the upper part of the body sparingly covered). The patient is 22 years old, weighs (without clothes) 140 pounds, measures 5ft. 4in. His body is moderately developed; muscles rather lax; bones thin; colour chlorotic; hair light; iris bluish gray; conjunctiva pure, slightly-bluish white. Glands of the neck slightly but not visibly swollen. A few spots of acne and ecthyma on the front and on the back of the trunk. Physiognomy in no respect characteristic; expression cheerful. During the consultation, patient appears anxious; rather hasty in his answers; the extended arm is slightly trembling. The pulse beats 110 times in the beginning, only 86 times towards the end, of the visit; is not very elastic, feels soft, and of middle size. The patient sleeps during seven or eight hours, but is awoke by every little noise. The physical examination exhibits: formation and motion (Sibson's chest-measurer) of the thorax normal, size and situation of heart normal, slight murmur with the first sound at the base (anæmic murmur is heard also in a slight degree in the vena jugular dextr.); diameter of the liver rather increased; sound of percussion perfectly dull. The tongue is red, with a thin, bluish-white covering on the surface. Appetite not altogether absent; taste acid after meals. The wrist-joints are slightly swollen, their temperature is increased, they are tender and painful. The quantity of carbonic acid expired is not measured. The perspiration constantly covering the skin for the last two days has a strongly acid reaction, and a faint smell of acetic acid; saliva slightly acid. Urine and fæces at the first visit not seen; during the following twenty-four hours, about 8 ounces of fæces, of moderate consistency and light-brown colour. Quantity of urine, 34 ounces, specific gravity = 1030; acidity (ascertained by neutralization) = + 48 (+ 15—20 being the standard in health); high gold-yellow colour; on the bottom of the clear fluid, a sediment of lithates (soluble by warming), and some crystals of uric acid and of oxalate of lime (microscope); the quantity of urea (nitrate of) does not exceed the average; but that of lithic acid, sulphuric acid, and the earthy phosphates, is increased; no sugar is detected, but by boiling and nitric acid some albumen is precipitated. Patient has taken, during the last twenty-four hours: water, 48 oz.; milk, 10 oz.; gruel and barley-water, 10 oz.; bread, 10 oz.; butter, $\frac{3}{4}$ oz.; mixtur. gummosæ, 6 oz.; with one drachm of nitrate of potash.

From the previous history, and the examination of the present state, Beneke concludes, in this case, on the presence of a complicated disorder of the blood. The scrofulous dyscrasia which the patient inherited is connected with an (indirectly) retarded metamorphosis of the nitrogenized constituents of the blood, and in the former diet of the patient several things were present to promote this dyscrasia. The circumstance of his having been late in beginning to walk, the insufficient development of the bones, the morbid condition of the teeth, and the flaccidity of the muscles, all point to an abnormal loss of earthy phosphates, and therefore, also, according to Beneke's views, to an abnormal production of oxalic acid. The result of the examination of the "status præsens" shows an increased expenditure of nitrogenized material; the presence of albumen in the urine; an absolute increase of lithic acid; no absolute increase of urea, as only a part of the lithic acid reaches that stage of decomposition. The ingesta being diminished, we have reason to conclude that the proportion of the albuminates of the blood is abnormally augmented. The high colour of the urine, especially if present for a long period, indicates an impaired function of the liver. The quantity of sulphuric acid in the urine being only moderately increased, corresponds to only a slight degree of pyrexia and increase of change of matter. (Beneke holds the opinion, that the quantity of sulphuric acid in the urine, provided no combination of sulphur have been taken except in the food, is proportional to the intensity of the metamorphosis of matter.) The high degree of acidity of the urine principally depending on the acid phosphates, is frequently caused by an increase of the azotized components in the blood, and is, also, often a sign of the presence of a morbid excess of soda in the blood.

From all this we may conclude, with a high degree of probability, on an increase of the azotized materials of the blood; and, from the diet, we must consider this increase as an indirect one, occasioned by retarded metamorphosis of the albuminates. Concerning the inorganic constituents, we may deduce, from the acidity of the perspiration and of the saliva, and from the acid taste, an augmented production of organic acid, by which the presence of an abnormally great quantity of soda in the blood becomes very probable; the large proportion of salt previously taken, and the impaired expenditure of soda through the liver, make this inference still more plausible. The increased quantity of earthy phosphates points to the pathological production of oxalic acid, which is afterwards proved by the microscope. (p. 71.)

Our space does not permit us to give the whole semeiology of the case, with the diagnosis and the therapeutical indications founded on it. We will only add, that Beneke is not contented with the name attributed to such an affection, "*articular rheumatism*," as signifying only a single group of the morbid symptoms; but his diagnosis would be: *chronic disease of the blood at present augmented, (increase of albuminates and alkaline bases, diminution of earthy phosphates;) articular rheumatism; hyperæmia of the liver; anæmia.* (p. 73.)

We must bear in mind, however, that Beneke intends to give by this only the outlines of a case as it may be taken by men much occupied; and he wishes merely to show how the process of disease is to be observed, and how the physiologico-chemical examination may help in the diagnosis and treatment. He has given a more elaborate case in an appendix to this work. It may be remarked, also, that in calling the adduced case of rheumatic affection a disease of the blood, Beneke must not be understood as if he was considering the ultimate cause of the disease to lie merely in the blood; in other parts of the same work he has sufficiently shown that he is always aware of the intimate connexion between the solids and the blood.

There are some expressions in the above case which appear to state as facts what are, as yet, merely suppositions; but this we may attribute to the tendency of a concise style in writing. For instance, we meet several times with "abnormally increased production of oxalic acid," whereas the increased quantity of oxalic acid in the urine is no proof of its absolutely increased production, but only of the formation and excretion of some oxalate which has not been further transformed. "An increased production of organic acid" is another instance of the expressions we allude to.

Concerning the treatment, the diagnosis formed in the manner above described leads Beneke to the following indications:

"1. Diminution of the nitrogenized material of the blood (small venesection of about six to eight ounces, and exclusion of nitrogenized food); 2. Acceleration of the metamorphosis of the remaining nitrogenized material (increase of diuresis simply by drinking more water, and moderate doses of salts of potash, particularly the acetate)."

He of course carefully avoids repeated or larger venesections; he does not continue the low diet too long, nor the salts with an alkaline basis, especially soda, which already exists in excess; he does not resort to frequent purgatives, by which, according to *Schmidt's* excellent investigations (Cholera, &c.), the albuminates of the blood are augmented. After having subdued the acute attack, Beneke would not permit too much nitrogenized food, and would warn his patient against taking those non-nitrogenized substances, by which the metamorphosis of the nitrogenized ones is most retarded (sugar, starch, &c.); he would combat the increase of soda, and the diminution of red blood-globules, by mineral acids, iron, &c.

III. In the determination of the *therapeutical indications* in general, Beneke reminds us again to look for the origin of disease either in disorders of food and its product the blood, or in some of the aggressive agencies.

As the organism cannot be retained in the state of health without a certain supply of alimentary material in the proper quantity and quality, the establishment of a system of *rational dietetics* must be considered as one of the *principal subjects* for inquiry. It must comprise:

a. The consideration of all the articles of food;
b. Of the other influences of life, which all have a direct or indirect influence on the metamorphosis of the alimentary material.

We must therefore form tables showing the exact composition of all articles of food—the quantity of water, the quantity and quality of nitrogenized and non-nitrogenized, as well as of the inorganic constituents. Much has been done already, particularly as regards the organic ingredients; but much remains to be fulfilled, and most in regard to the quality of the inorganic ones, which are of the greatest influence on the nutrition of the organism. We must further learn to fix what is the *proper quantity and quality for each individual case.* In doing so it is essential never to omit taking into consideration weight, measure, age, occupation, residence, with atmospheric influences, and principally also the typical power of the nervous system. It is doubtless the case that in a person measuring six feet, and weighing 170 pounds, more matter is decomposed within twenty-four hours, than in another only five feet high, and weighing 120 pounds, whose other conditions are, however, the same as those of the former subject. It is ascertained that two persons of the same height and weight, of whom the one is low-spirited and slow, while the other is cheerful, active, and quick in his motions, require different quantities of food, as the metamorphosis of matter must be slower in the former than it is in the latter. Every influence, whether psychical or physical, by which the act of respiration is promoted, must increase the metamorphosis of matter, and must not be forgotten in regulating the diet. Concerning the *quality*, we must likewise choose it according to each individual case. We must learn at first, whether there is an acceleration or a retardation in the metamorphosis of a certain class of alimentary materials—in case of retardation, whether a direct or an indirect one—whether there is a plus or minus of phosphates, a plus or minus of soda, &c. &c.; and according to the result of these investigations we must prescribe the diet.

As another subject for inquiry, Beneke proposes the investigation of the action of those substances the use of which is not necessary for the maintenance of health, which may therefore be looked at separately from the nutriments in the proper sense of the word—as coffee, tea, spices, wine, spirits, &c. &c. These *articles of relish* (*Genussmittel*) certainly exercise an important influence on the digestion and on the metamorphosis of matter. Researches of some value have been made already by Dr. Boecker, of Bonn,* according to which most of these so-called excitants do not accelerate, but retard, the metamorphosis.

For the establishment of rational *therapeutic treatment,* besides the

* Beitrage zur Heilkunde.

knowledge of the nature of the disease or of what is to be remedied, it is necessary to know the action of the remedies. To achieve this, Beneke seems to put little confidence in the statistical method as recommended by Louis and Wunderlich, but entertains much hope in the *physiologico-chemical experiment performed on the most healthy individuals.* It would lead too far to expose here the preferences and deficiences of the one method and the other, but we must think the combination of both of them indispensable for the formation of the rational pharmacology.

IV. We need scarcely remark on the necessity of not only elaborate, but frequent records of all the symptoms in each case, the condition of all the egesta, and the kind of ingesta.

After having thus given the outlines of what is wanted for the promotion of a rational system of pathology and therapeutics, Beneke briefly, but with much clearness, alludes to the manner in which the different observations and examinations are to be conducted. It cannot, of course, be required from a much-occupied medical man, that he carefully examines and records all the different points of importance; but most of us might have always one or two cases in hand, of which we could make out the history of the development of the disease; in which we could form, besides the physical, also the physiologico-chemical diagnosis, in which we could daily note the progress of the physical symptoms; the quantity and quality of the ingesta, the atmospheric influences, the quantity of the urine, its physical quality, its specific gravity, reaction, its degree of acidity or alkalescence; the approximative quantity of lithic acid and urea; of earthy phosphates, of sulphuric acid and of oxalate of lime; the presence or absence of sugar; the quantity and quality of the alvine evacuations; that of the perspiration, and also the reaction of the saliva, if possible, at different times of the day.

We must not forget to mention that Beneke gives very complete schemes for the examination by which the development of the disease and the *status præsens* may be investigated; he gives very practical diagrams for taking record of the most important points in the course of the disease, and another scheme for the *post-mortem* examination. He proposes also the outlines of a useful plan, according to which the chemical examinations may be performed. We will, however, at present, not dwell any longer on these points, as we hope to be able to return to them ere long, when analyzing the plan for the physical as well as physiologico-chemical examination which is to be adopted by the Society for Clinical Observation in Germany.

True it is that many of the most important points cannot be easily observed in private practice, but doubly great is therefore the duty which devolves on the medical staff of hospitals. No hospital ought to be without a laboratory, and without the instruments necessary for accurate examination.

But the more we enter into the field of scientific examination for the promotion of rational medicine, the more we must see how much is to be done, and how little a single individual can do; with a full conviction of this fact, our author has urgently invited his colleagues, as well in Germany as abroad, to unite with him, to distribute the labour, and to work according to a certain plan. And we are happy to say that his exhortation has been efficient, as at the last *Versammlung deutscher Naturforscher und Aerzte*, in Wiesbaden, in September, 1852, by the co-operation of Professor Vogel, of Giessen, and Professor H. Nasse, of Marburg, a Society for Clinical Observation, or, verbally translating the rather long German title, "A Society for United Labours towards the Promotion of Scientific Medicine," has been formed, which counts already among its members many well-known German, and some English names.

Hermann Weber.

Review VI.

1. *Statuta Universitatis Oxoniensis—Addenda; Titulus* VI. (Printed at the University Press.)
2. *Remarks on the Extension of Education at the University of Oxford, in a Letter to the* Rev. W. Jacobson, D.D. By Henry Wentworth Acland, M.D., F.R.S., &c.—*Oxford*, 1848.
3. *Advertised list of subjects for examination: School of Natural Science.* 1853.
4. *Report of H. M. Commissioners appointed to inquire into the State, Discipline, Studies, and Revenues of the University and Colleges of Oxford.* (Presented to both Houses of Parliament.) 1852.
5. *The same, for Cambridge.* 1852.

To ascribe the commencement of a change to 1848 is not to give it a good character to European ears. So many goodly boughs were lopped from the strongest trees, so many brave trunks were being prostrated by the axe of revolution, so false and ephemeral were the fungous forms that sprouted from their stumps, that the very date stinks in our nostrils. Yet, amidst this dreariness of decay and leprous aftergrowth abroad, a home fruit was humbly maturing, most wholesome and nourishing to the body politic; a fruit which future ages will prize in proportion to its worth. During this ill-omened year, our ancient universities were engaged in introducing into their scheme of education, means of mental training —new, untried, and hitherto somewhat suspected. It is unnecessary to go through the history of hebdomadal boards, graces, convocations, and other machinery, which, whatever may be the defects, is useful at any rate in impeding over-rapid legislation in such democratic bodies as our universities;—suffice it to say, that at Cambridge in 1849, and at Oxford in 1850, a series of statutes became law, which established as the acknowledged pursuits of the place, as claims for reward and means of distinction, those which have fairly been called "Progressive Studies," that is, the Moral and Political and the Natural Sciences. It is with the latter that we naturally, in this place, are most concerned; and we seize on the occasion of the present time, when it is fixed that the new statutes shall come into operation, to make some comments upon them.

Possibly our readers may not see the full bearing of this change either on the empire at large, or on our profession of medicine in particular. Yet we believe they will fully go along with us when they have thought-fully reviewed our reasons for attaching so much importance to it. In the first place, let us not, from feelings of rivalry or fondness for other uncongenial theories, avoid freely confessing the fact, that at Oxford and Cambridge are brought up the great working majority of those who chiefly move the machinery of our social state. Most of our barristers, our clergy, our statesmen, some of the most influential in the profession of medicine, our landowners, and many traders whose wealth, rank, and talents give them influence in society, have received there their education. Let it also be called to mind, that the studies pursued at the universities have a regulating force over public schools, and these over preparatory establishments, and that private tutors are supplied from them, and even teachers to institutions designed to supersede them. It is impossible to deny these facts, or to deny, therefore, that any one receiving the merest rudiments of aught beyond a commercial or industrial education, must have the course of it influenced by the systems pursued at Oxford and Cambridge. Justice forbid that we should be supposed to depreciate here our noble Metropolitan University. That was founded for the accomplishment of a most delicate and difficult task—viz., to give their dues to those whom a variety of honest reasons prevented from participating in the benefits of the old collegiate institutions; and this task it is performing manfully and well—God speed it!—but the system so admirably adapted for its special purpose can never supplant, and was never intended to supplant, that of its elder sisters. It is *their* wisdom or folly which is, and always will be, reflected in a thousand mirrors through the land. The contagiousness of their wrath has become proverbial,* and their harmony spreads also with proportionate, though not equal, rapidity.

Mark, we are speaking here, not of professional instruction, not of the acquirement of knowledge for use in the special modes of gaining bread, but of that general primary training which makes the mind more or less capable of holding this working capital of facts, "that which fits a man" as Milton phrases it, "to perform justly, skilfully, and magnanimously, all the offices of peace and war." It is this bringing out of the mental powers to which the name "education" is most strictly due—from *educo*, "*I lead out*," while instruction is from *instruo*, "*I furnish*" with facts. It is this education which is directly influenced by our university systems; but it must not be forgotten that even professional instruction receives a colour from the sort of individuals thus formed.

The question of introducing the progressive physical sciences into the scheme of primary education in England is not one of yesterday. Even before fruit had been borne by Bacon's teaching of the right method of studying nature, one of our greatest men, himself an instructor of youth, saw the advantage to be derived from early imbuing the mind with the principle of looking into the natural objects around it for truth. Milton† advises that the acquisition of Latin should begin with Cato, Columella, and Varro, and occasion taken, while reading these agricultural authors, to draw the attention of the pupils to the tillage of their own country. Greek, too, he would commence with "the historical physiology of Aris-

* *Chronica si penses, cum pugnant Oxonienses,*
Post aliquot menses, volat ira per Angligienses.

† Tractate on Education to Mr. Samuel Hartlib.

totle and Theophrastus," while the lads are proceeding "leisurely from the history of meteors, minerals, plants, and living animals as far as anatomy . . . . and this will give them such a real tincture of natural knowledge as they shall never forget, but daily augment with delight. Then, also, the poets which are now counted most hard, will be both facile and pleasant, Orpheus, Hesiod, Theocritus, Aratus, Nicander, Oppian, Dionysius; and in Latin, Lucretius, Manilius, and the rural part of Virgil . . . . . ." After this, comes the study of history, jurisprudence, moral philosophy, logic, rhetoric, and poetry. "These are the studies wherein our noble and gentle youth ought to bestow their time in a disciplinary way from twelve to one and twenty; *unless they rely more on their ancestors dead than on themselves living.*"

It is probably the magnificent impossibility of the details in Milton's scheme, which has prevented its being seen that he has hit upon the true theory of the use of physical science to education. He says, with justice, "this is not a bow for every man to shoot in, that counts himself a teacher," but he has made it more stiff than necessary, by not adapting it to the existing establishments of the country. The principle is not the less valuable for being found in what is fortunately perhaps a Utopia, nor has the use of the physical sciences in education any chain to bind it to the objectionable parts of the scheme.

Mr. Locke, unfortunately, was infected with some of the notions of what has since been called the "useful knowledge" school, and though he urges the study of physical science as a component part of education; he urges it solely with a view of forwarding the knowledge required for exercising arts and professions. He is curiously blind to the importance of cultivating the mental powers by its means. Indeed, it is singular how, throughout the treatise, he ignores the influence which the instilling general principles into the mind exercises over its power of acquiring branches of knowledge dependent on those general principles. For instance, he refuses to see how instruction in one department of philology can make a man apt at an allied branch. Thus, while sneering at those who are "bred up amongst Greek and Latin," he says, "If any one among us have a facility or purity more than ordinary in his mother tongue, it is owing to chance, or his genius, or anything rather than to his education, or any care of his teacher,"* apparently without a notion that this very purity and facility is the result of the pupil's being early imbued with the principles of grammar before his mind was overburthened with a multitude of words and ideas. Though his principles are erroneous, yet Mr. Locke's suggestions as to the details of teaching are more available than Milton's scheme, and have been made useful to succeeding writers on the subject.

The grave and unintentional caricature of the "useful knowledge" school contained in the theories of Edgeworth, where it is seriously proposed to commence professional instruction in the cradle, to give the future inquirer mechanical playthings, and to confine theology to the unbreeched parson, ought to have proved a *reductio ad absurdum* to the whole system. But instead of doing this, it has had the bad effect of associating itself in men's thoughts with all use of science in education;

* Locke on Education: Works, vol. viii.

so that any proposal to employ the observation of nature in training the mind has been denounced as part of the absurdities above named. This is a serious impediment to those truer views which we have expressed in the words of Milton.

A further impediment is the great authority and overwhelming genius of those who have supported opinions and encouraged systems adverse to this employment of natural science. The thirty years' teaching of Socrates nipped it in the bud at Athens, and the mighty schools of philosophy, founded by his pupils, effectually turned the ancient world from the study of nature to the intellect of man—from the laws of matter, which they said it was God's business to look after, to the laws of mind, which they considered as a home concern.* Hence, when true philosophy actually did come down from heaven at the bidding of One greater than Socrates, it was "to the Greeks foolishness." A different result might have followed, had Socrates turned his shrewd mind and the apprehension which he first seems to have gained for himself of Genus and Species† to the rectification of the physics which he learnt from Archelaus and Anaxagoras; Lord Bacon might possibly have been anticipated by 2000 years, and the Grecian race prepared by a careful study of God's creatures to receive joyfully a revelation of his nature.

Let not all this recapitulation of by-gone days be as a dead past; it is our own faults if history is to us an old almanack. It is not as if the influences of the Socratic school had passed away to be smiled at as things that were; they are still living and acting for good and for evil—for *great* good and for *some* evil—on the vast accumulation of human knowledge collected since their day. Who can see that demand for definition in argument, so common in all speculative minds, and a variety of perverse intellectual habits arising out of it, without perceiving that it is traceable to the long-established dominion of Euclid and Aristotle over education? We do not mean that all persons who argue in this way have learned it from those masters; they may very likely have never drunk from the fountain-head, but the habit has come through several hands exaggerated and ingrained. Dr. Whewell‡ has well pointed out of what importance the classifications of natural history, pursued as the science of *natural classes,* are in correcting this faulty bias. No commendations of ours are needed to draw attention to his valuable reasonings on this subject, which ought to be themselves perused by all who take an interest in education.

* Cicero, Tusc. Disp., Lib. V. cap. 4: "Socrates philosophiam devoravit è *cælo*. et in *urbibus* collocavit." In the present day it is "human" and "material," "divine" and "spiritual," that are convertible terms in popular parlance; of old, "divine" and "material" were synonymous, and with about equal reason.

† "So familiar has every student been rendered with the ordinary terms and gradations of logic and classification—such as genus, definition—individual things as comprehended in a genus, &c., that some mental effort is required to see anything important in this. But familiar as these words have now become, they denote a mental process, of which, in 440—430 B.C., few men besides Sokrates had any conscious perception."—Grote's History of Greece, vol. viii. p. 578, note.

‡ Philosophy of Inductive Sciences, Book XII. cap. iii. § 12. "It is a curious example of the belief in definitions, that elementary books have been written in which natural history is taught in the way of question and answer, and consequently by means of words alone. In such a scheme, of course, all objects are defined; and we may easily anticipate the value of the knowledge thus conveyed. Thus: 'Iron is a well-known hard metal, of a darkish grey colour, and very elastic.' 'Copper is an orange-coloured metal, more sonorous than any other, and most elastic of all except iron.' This is to pervert the meaning of education, and to make it a business of mere words."—*Id.* § 16.

But what influence can be expected to be exerted over the mind by these studies, if they are taken up only after its habits are formed? What extensive good can they do unless they are in fact a part of its formation? If we wish to disabuse our country of its fallacies, we must ingrain their refutation in those minds which lead public opinion on a great scale. It is of no use encouraging science, as it is called, by petting a few individual workers, unless we enable society to understand their reasonings. Without this, their words are profitless babblings—nay, worse than profitless; for educated men, from ignorance of their mode of argument, despise it, and teach others with authority to do the same.

We are disposed ourselves to go further still, and to think that a more scientific turn of the English mind would serve not only the cause of speculative truth and of material well-being, but raise also the moral and religious feeling of our country. We would say with Dr. Acland, in the pamphlet which appears in our heading, that—

"Every one who is acquainted with the present condition of physical science will agree that a general acquaintance with it, given and received in a proper spirit, *must* ennoble the learner, and give him a more true knowledge of his condition, and a more thankful appreciation of it.

"Indeed, so certainly does this appear to be the case, that it is for the effect upon the moral quite as much as on the intellectual character, that the cause of this kind of education should be advocated; and any man really anxious for the full development of the mental powers of his pupils, is doing himself and them a serious disservice, nay, an injury to his country, who does not set himself to ascertain what is the most feasible method of adding to the study of language, logic, history, and religion, the study of the general nature of the planet in which he is placed, and of the material conditions under which his work of probation is to be performed."

We cordially agree with Dr. Acland, and think her Majesty's Commissioners have done wisely in quoting *in italics* the latter clauses. They are a bold, manly expression of true patriotism, and cannot be brought too prominently under the eyes of our legislators, who are anxious to ascertain how the Universities are willing to fulfil the trusts committed to them: for, be it remembered, these words, written in 1848, were the precursors —how far the *cause* it is not for us to say—of the great changes under our notice.

Hitherto we have been speaking as citizens only, and the arguments used might have occurred to any lawyer, divine, or country gentleman; but let us now look on the matter as medical men—as those whose opportunities have led them to feel the true grandeur of their mission to heal and to teach the art of healing—as those who are daily impressed with the intimate way in which the position of their profession in a state is bound up with the best interests of society. As medical men, then, let us say, without reserve, that we consider the status held by our brethren as far, very far beneath what it ought to be. We refer to our status in social life, and in the estimation of our compeers. However much our friends respect us as individuals, our profession never adds in its due degree to that respect. A stranger never looks upon us as entitled to any particular rank or place on account of it—such, for example, as is conceded to a clergyman. A family that parts with a daughter to us never looks upon her as raised in the scale of society, but rather as having made a sacrifice on the altar of Hymen. All of us have felt this, and all of us have said, "These things ought not to be."

Now, far be it from us to claim the martyr's palms—we have little sympathy with martyrs of society. Besides, we think we are not so far gone: "*sanabilibus ægrotamus malis,*" and the remedy is even now being prepared. The remedy we believe to be the general diffusion among educated men of a knowledge of the principles on which we act, of the philosophical nature of our inductions, of the powers of mind called into action in our deliberations, of the real solid foundation of our opinions—in fact, what a *Man* a medical man is.

We have gone on too long endeavouring to raise our profession by paltry little props, by denouncing unworthy members, by scrupulously abstaining from what might appear "unprofessional," by drawing as strongly as we can the line of demarcation between ourselves and quacks, and sometimes annoyingly acting as our own seneschals; and we have gained some ridicule and little sympathy. The fact is, that it is not the presence of some base bricks which makes our edifice despised; it is the want of knowledge of its noble points. The town does not think worse of the legal profession from the acknowledged cases of roguish attorneys and mean barristers, nor do the many "disgraces to their cloth" which we read of in the papers lower its estimation of the clergy as a body. The reason is clear: a knowledge of the principles of law is forced on every educated man—on the landowner by his magisterial duties, on the trustee, proprietor, litigant, master, servant, legislator, by various claims more or less agreeable; so that he is able fully to appreciate a man who shows himself capable of handling those points better than his rivals. The same might be repeated of the value set on worldly success in the church. So is it with the medical profession: society does not rank us the lower for the instances of vulgar selfishness which we are fond of parading as exceptional cases; it is obvious to English sense that there are rotten stones in every wall; but it undervalues us because it does not perceive how our great men are its great men also; it despises our palace because it cannot see the strength of its buttresses, the glory of its pinnacles; and it cannot see, because its eyes are untaught. To recur to what we said before, *the raising of our profession to its due rank depends mainly on the scientific education of the upper classes throughout the country.*

But perhaps the gulf which lies between us and our countrymen, though it consists principally in their not understanding us, may be deepened in no slight degree by our not understanding them. Let us put a case (perhaps an extreme one) to illustrate strongly both these points. Jack Squills, a gentlemanly lad of good abilities, is at seventeen taken from Rugby, where he was distinguished as a manufacturer of Greek iambics, and sent to London to be educated for the medical profession. He lives with a surgeon, whose duty it is to look after his linen and morals, and, wisely enough, does not rush immediately into hospital practice with the majority, but attends Graham, and Grant, and Potter, and Todd. He learns to know what has put Berzelius, and Hunter, and Cuvier, on their pedestals. He then enters with zeal into the dissecting-room, and soon into the hospital wards; he goes through the ordinary courses, and gains the ordinary prizes; he spends a winter industriously at Paris, is at the Hôtel Dieu every morning by sunrise, does not neglect the St. Louis or Hôpital des Enfants, nor Dumas and Andral in the afternoon; he becomes F.R.C.S. (by exam.) and M.B. (Lond.) Is he not right in thinking he has made the most of his opportunities and abilities? Having a little independence of his own, he looks well about him, and then settles in the outskirts of an assize town, near the park fence of his old schoolfellow and study chum, Broadlands. Now Broadlands was always a clever fellow, and a good fellow too; he got an exhibition, but managed to resign it in favour of the next to him, a hardworking son of a hardworking curate, with whom he went to college. He read moderately, from his very first term, Thucydides and Grote, Livy and Niebuhr, Aristotle, Butler's Analogy, &c., and did not omit mathematics, and, in fine, took a good double second. Nor, on leaving Alma Mater, did he neglect the further cultivation of his mind; he kept up frequent intercourse with his old associates, who were taking their chance of the woolsack or the mitre, not forgetting Jack Brag, a first-rate oar and debater at the Union, who was writing leaders for the *Times*. Broadlands is right glad to see again his old chum Squills, calls directly, and asks him to dinner to meet the sheriff and the county member, and some neighbouring magistrates and clergy, several of whom were old Rugbeans also. Squills is certainly well received, and both neighbours hope to renew their school friendship to their mutual edification. But alas! the allusions and the conversation and the prejudices of the company are a Babylonish dialect to him; he despises them because he knows not their origin or their course; their heroes are to him empty names, and he thinks them ignorant because they never heard of those whom he reveres. The worst of it is, that both are right in seeing impediments to the cultivation of friendship; the squire *is* ignorant most profoundly of what makes the medical man a useful member of society; and the latter's blindness to his neighbour's merits is not wilful, but the sheer result of want of eyes. We must seek a remedy, for the loss is very great on both sides; each equally losing that "sharpening of the countenance" as iron by iron, which Solomon rightly tells us the friendship of men of different occupations bestows.

Now, would the result have been the same if these men had each received a tincture of one another's learning?—if the student of nature had been more imbued with the works of man's mind, and still more if the collegian had gained a knowledge of natural science? Would the friendship have ceased had our medical student continued at the same place as his schoolfellows his university course up to the period when it was necessary to begin professional instruction?

We believe the Roman schoolmaster was right in saying, that at the time of commencing manhood, common studies are as strong a bond of union as a common religion,* and lead to as firm friendships in after-life. The cause of which is, that they are, in fact, a common language, which those who are without cannot fail to find a cause of estrangement. To make this common language as perfect as possible, we would defer to the latest moment that necessary separation of young men intended for different pursuits, which the final preparation for gaining their bread requires; and we look upon everything that tends that way as a step in civilization, and as a means of giving a fair social rank to every exertion of mind.

* "Nec enim est sanctius sacris iisdem, quam studiis initiari."—Quintilian, Instit. Orat. I. 2, 2.

It is not only such model young men as we have described our heroes, that would receive benefit from opportunities of mutually acquiring one another's language. It may be safely conjectured, that they will always constitute a small minority of the population, and claim, therefore, less attention than that large heterogeneous body denominated "the average" by statisticians, and the "poll" by Cantabs; consisting of those whose idleness has sunk good abilities, industry raised bad abilities, whose want of determination, funds, or physical strength have stood in their way, or a large supply of these requisites has carried through difficulties, or whose mental powers are actually in that state of balance which the arithmetician supposes. We are far from thinking this respectable class unworthy of notice; nor would we dispose of them, as Milton summarily does, by calling them "stocks and stubs," and saying no more about the matter. In truth, we think that the teaching of the physical sciences at the universities will benefit these in a more bountiful proportion than the superior men. In the first place, the offer of a new field of study may possibly excite them to throw off their bad habits, and improve their minds in a novel way. Then we must remember, that it is not only the actual engagement of the mind in a particular study, which is beneficial to it; the mixing in social intercourse with others who are so employed, leads the less industrious students to appreciate in a considerable degree the modes of argument, and understand the phraseology. A modified light is diffused through the whole place, which those share who indeed can scarce be said to deserve it. This is certainly the case with logic, classics, and ethics; and we cannot doubt that the infusion into collegiate society of a number of young men who are studying natural philosophy, would do more than anything yet accomplished to spread abroad a knowledge of its principles, and a reverence for its professors.

Of this double boon which we desire for our profession—viz., the opportunity of being understood, and the opportunity of understanding others, we see promise, nay, *the only promise* offered to our generation, in the changes adopted at the ancient universities. At Oxford especially seems the fulfilment nearest, where to extend the university in a ratio to the population, if not further, is a fixed determination; and the various plans of "Affiliated Halls," "Private Halls," "New Colleges," "Larger Colleges," in the city, out of the city, and with various relations to their alma mater, amicably divide that energetic body, "the Tutors' Association." By these means, it is intended to reduce the expenses to 70*l.* or 80*l.* per annum, even to those whose abilities do not allow them to gain assistance from endowed funds; and a golden age is looked forward to, when our attorneys, country surgeons, civil engineers, &c., will receive the benefits of the very highest form of education, conjoined with judicious discipline. At Baliol, the chemical laboratory is now completed; at Exeter, it is designed; at Christ Church, the anatomical museum, with its beautiful physiological preparations, is thrown open, and the short courses of anatomical lectures attended; in Convocation, an application of university funds to the building of common university museums and laboratories is proposed, and all this with a good-natured allowance for honest opposition, which warrants success.

At Cambridge, we cannot say that so much is doing or so well. By the evidence given to her Majesty's Commissioners, it would appear a notion prevails, that the chief use of scientific instruction is as a part of a purely medical education, and hence a tendency to turn the natural science classes into part of a medical school. The authorities see, of course, that it must be a provincial one, and rank in usefulness and estimation to an infinite degree below London. Wisely enough, they are chary of associating themselves with a scheme of doubtful dignity, and take little interest in the matter. However, the natural science schools are established, and therefore the deepest part of the Rubicon is passed.

To show that at Oxford minds as well as voices and votes are engaged in smoothing the way for the car of science, we would refer to a paper put forth in the spring, with a modest but rather embarrassing omission of the authors' names. However, its being posted everywhere by the governing bodies was probably sufficient authorization. This paper purports to be simply an enumeration of the subjects to be taken up for a common degree, and for honours in the natural science schools; but in it is contained, to the diligent observer, a most philosophical division of natural knowledge, and a most accurate appreciation of what constitutes true excellence in the scientific diathesis.

In this paper we read, that for a common degree or minimum is required an acquaintance with the principles of mechanical philosophy, chemistry, and physiology, or at least with the principles of two of them; and in every case an acquaintance with some branch of physical science dependent on mechanical philosophy.

"1. *Mechanical Philosophy* includes the sciences of mechanics, hydrostatics, pneumatics, sound, light, heat, and electricity. The general principles of mechanical philosophy may be studied in such books as Fownes' 'Manual of Chemistry,' Part I.; any particular branch in Peschel's 'Elements of Physics,' Walker's 'Text Books,' or any work of the same character. The principles of mechanics generally need not be brought up by every candidate for a common degree; but all must be acquainted with some one of the sciences dependent upon mechanical philosophy.

"2. *Chemistry.*—The subjects noticed in the syllabus of lectures on inorganic chemistry, annually delivered by the Professor, except those which relate to heat, electricity, magnetism, and the less common of the elements; copies of which syllabus may be obtained at the Botanic Garden. Daubeny's 'Atomic Theory,' and Fownes' 'Manual' may be consulted with advantage, though no particular text book is enjoined.

"3. *Physiology.*—The general principles of human and comparative physiology. Agassiz and Gould's 'Comparative Physiology' may be used as a guide, chapters xiii. and xiv. being at the option of the candidate. Any one of the following subjects—viz., the several properties of the textures; the functions of the brain and the nervous system; nutrition, including digestion;—may be read at greater length in Carpenter's 'Manual of Physiology,' or Todd and Bowman's 'Physiology of Man,' or Kirkes' 'Handbook of Physiology.'"

Such is the scheme of examination designed to mark ordinary abilities and application; and, in truth, we think that here is fully carried out the wise and kind intentions of the legislators, who have thought fit to append to their new statutes the following remark:—"*Hoc semper animo infixum habeant Magistri Scholarum, Moderatores, Examinatores Publici, nos nihil triste aut asperum moliri. Lenitati ubique consultum volumus, modo ne sit ea, quæ Juniorum socordiæ patrocinari videatur.*" A knowledge of principles is required, but no very intimate acquaintance with details; and we are sure that, under judicious professors, such knowledge may be acquired without much expenditure of time, and will be most valuable in forming the mind.

Some may cry out, and especially among prejudiced second-rate men of science shall we hear this said, "What is the use of such superficial smattering?" What, indeed—if it *be* superficial! But that is not by any means a matter of necessity; for, "just as the most detailed instruction is not always philosophical, so *elementary* teaching need not be *superficial*, but indicative of the deepest truths. Some men are always superficial, and some never; and some laws which, in the advance and progress of human knowledge, were not reached for some thousands of years, may be made to reveal themselves in a short and simple way, and to leave an impress never to be effaced from any mind capable of receiving it." This, we honestly believe, will be the case at Oxford. A professor wastes the time of himself and his class, if he tries by details to save the pupils' personal labour. Private study alone can give them this knowledge, and to attempt to impart it by a royal road is *cramming*, not teaching. To prevent cramming, we never read a scheme for examination more adapted than what we have quoted for the minimum degree.

In Honours there are four classes, and any one going up for a common degree, who exhibits a more detailed and correct knowledge of the above-mentioned subjects than is required merely to pass the examination, may be placed in the fourth class. This is an honourable distinction for a clear-headed, accurate man, not quick, but sensible and useful. But for the three other classes, not only is more accuracy required in the same subjects as for the minimum, but an exact knowledge of some one or more of the particular sciences comprehended under the general head of Natural Science. Here, we think, great wisdom is shown: the pupil is not required to overburden himself with the individualities of a variety of subjects, but to show the capabilities of his mind by the practical or detailed investigation of one or two special branches. He may take up, for example, in chemistry, the researches relating to the alcohol series, or to some of the nitrogenous products of animals or vegetables; or, in physiology, Owen's 'Homologies of the Vertebrate Skeleton,' the comparative anatomy of circulation, respiration, &c.; or Cuvier's 'Ossemens Fossiles,' or the details of human anatomy; and perhaps a proof of his power of dissecting for demonstration. Much better can a man of real mental power prove his ability by mastering one of these subjects, and submitting to severe examination on it, than if he laid encyclopædias at the feet of his triers. True men are aware of this, and will put aside the cumbrous weapons of over-much knowledge, as David did Saul's armour, in favour of that which is light, but a proof of a skilful hand.

Those who read, and with us admire, this scheme of examination, will naturally inquire what opportunities are to be afforded to the student for acquiring the knowledge to be tested; how is to be effected that fusion and interpenetration of it with other forms of knowledge which we have tried to show is so desirable; and what security there is that the practical working of the examination will approach the perfection aimed at. It is well known to all who possess a stout pocket-book or directory, that there is at Oxford a considerable staff of professors of natural sciences; that there are two foundations for courses of lectures on anatomy, three for medicine, one for chemistry, for botany, for geology, for mineralogy, for natural philosophy, for experimental philosophy. The lectures have hitherto been attended by a few B.A.'s and M.A.'s, who chance to have some leisure hours; and the principal income has been derived from the foundation funds. It is proposed in future to require attendance at certain courses from those who go up in the physical science schools, and thus to augment the salary of the distinguished men who occupy these positions, in return for the extra trouble which a younger class will occasion. Besides this, we hear that several junior fellows of colleges are fitting themselves to act as private tutors in those sciences—such as chemistry, for example—which require practical manipulation. But the latter design of course awaits the completion of those laboratories, museums, &c., which we spoke of as having been commenced. In order to prevent sole attention being paid to one or more of those sciences, to the exclusion of other forms of knowledge, students are not to be encouraged to take them up during their earlier terms of residence. Indeed, at the "little go," which comes about the end of the first year, and at the "primary examination," which is at the end of the second, the subjects are as of old, philology, history, logic, pure mathematics, only under improved regulations. After this is passed, the student is obliged to choose some direction in which to extend his feelers for knowledge, at the same time that he keeps up and increases what he has already stored. At the final examination he must be prepared to be tested in two of the following schools—viz., of "Literæ Humaniores," of Mathematics, of Natural Science, of Modern History and Jurisprudence. Of the two schools selected, however, "Literæ Humaniores" must still be one.

We must confess that here we are rather at issue with the framers of the new statutes; we think that the free choice of studies, during the last year or so of residence, should have been more extended, and that the necessity for making philology still a constant pursuit should not exist. The object, no doubt, is to keep up the floating capital of this form of knowledge throughout the country. This object is a good one; but would it not be as well attained by requiring, not a certain period of devotion to it, but a certain proficiency? For example, those who have taken honours in the second examination might be allowed to give themselves entirely to the acquirement of natural science, or jurisprudence, or mathematics, at will. Thus a double motive for exertion in both examinations would be afforded, and those who are studying the special sciences be rather marked as superior men; fulfilling thus the intention of the University, by gaining respect for the pursuits in which they are engaged. It is well known that from good schools many lads come up to college, who require very little more knowledge to enable them even to take honours in classics; these often become idle when they find they are already furnished with that which others are learning, and lose, perhaps, much of what was already acquired. These cases would be especially provided for by our proposition, for the invigorating influence of a totally new study and new method would entirely remove the listlessness of over-repletion, and stimulate the mind to a healthy curiosity. The mental philosophy and logic, which are very properly made essentials for honours in the final examination *in literis humanioribus*, might easily be made also essentials in the other schools: thus no valuable subject would be passed over.

But a further advantage would arise; many would be attracted to the University whose parents, either from not appreciating philology, or other causes, are unwilling to detain their sons so long from the special studies of after life. Of late years, many physicians, and barristers even, have become so without an university education; while our desire is to see it extended even to the lower grades of all intellectual occupations. We think that if, as a reward for exertion, a young man were allowed, during the latter part of his college course, to study the elementary principles of the knowledge required for his future profession (so that, on taking his degree, he might proceed to more practical matters), many surgeons, attorneys, clergymen, and others, would see that this mode of education was well worth their while; though at present we fear that the philology will act as a bugbear. As regards our own profession, it is certain that chemistry, botany, anatomy, physiology, might be much better learned in the retired regularity of a collegiate town than amidst the bustle of working life. And if the student came up to London well prepared, even with the principles of some of these sciences, he would have more hours for his hospital attendance, and ten times the power of making a good use of those hours. It is well known that the elementary part of scientific teaching is very unremunerative to the teachers; they would consult their own dignity and pockets much better if they had nothing to do with it, confining themselves to the higher parts of practical science, its application to our art, and actual instruction in medicine and surgery.

At the risk of superfluous repetition, we wish again to call attention to the strongly-marked line which we think should be drawn between *elementary* and *final* professional studies. The most important practical distinction is the different influence which the mind of the teacher can have over his class. In the former, it is principles which are of the greatest weight; and good lectures may make the audience as well acquainted with these as the lecturer himself; so that, in coming to details, both stand on nearly equal ground. Details, too, being mostly dependent on experiment, are in the instructor's own hands, and at any time and in any locality may be placed in those of the pupil. The great thing here, then, is to have a good professor, and a man of power may create a school of science almost anywhere: he would be followed into the wilderness if his lot was cast there. This Haller did for physiology, at Göttingen; Liebig for chemistry, at Giessen. But this is impossible in the studies which are finally to fit a man for his duties in life. Here the details are matters of observation, not of experiment; they cannot be created, but must be sought where alone they exist—in the law courts and barrister's chamber by the young advocate; in the wards of large central hospitals by us. The crowd of a great city may impede study, but it is there only that the true objects of study are to be found. The professor is as nothing; the school is everything. Our idea of a perfect medical school would be, that no pupil should be received till he had completed his courses of anatomy, physiology, chemistry, &c., so that his whole mind might be devoted to the study of disease and its cure, without the thousand distractions of the present system. And such an idea can only be carried out by our universities undertaking earnestly the elementary or semi-professional part of medical education.

It is doubtful whether this amplification of the principles of the new statutes will be realized any quicker because it is recommended by Her Majesty's Commissioners; for learned corporations are frequently very averse to changes proposed in this way. It is stated in Hanna's life of Dr. Chalmers,* that all the valuable recommendations of the Royal Commissions in 1830 have lain hitherto entirely unheeded by the Scotch Universities, so as to make Dr. Hanna reasonably "wonder why such commissions were appointed." In England, however, the commission and the Universities have been so thoroughly working together, so animated by a common spirit, that we trust the recommendations of the one are merely the intentions of the other put into words. The further suggestion of the commission to encourage the study of natural science by the assignment of some existing fellowships and other rewards to it, they have thought fit to defend at such length, that a doubt occurs whether a cause which requires so much legal acumen is very tenable. We believe rewards enough will be assigned to such studies, when once the nation is practically convinced of their importance. Are we less philanthropic than our fathers?—not a bit.

Now comes the question, which is almost a ridiculous one to those acquainted with Oxford and Cambridge, as to the security that the intentions of Convocation in framing those statutes will be carried out by the individual examiners. Perhaps, however, some readers may not be aware that the most important part of the pass examinations, and a very necessary part even of that for honours—namely, the *vivâ voce* questioning—is public. Indeed, till within the last two years a very useful rule prevailed, which not only *permitted*, but *obliged*, each under-graduate to have sat for a day in the schools before his examination, so that he could never make the excuse that he was unprepared for the mode of questioning. We are not only sorry that this rule has been altered, and desire its restoration, but we would wish to see it adopted by our professional examining boards. By this compulsory attendance of some, and permitted attendance of all, such publicity is secured, that to dream of unfairness in an examiner never enters into the head of an under-graduate; not so much on account of the oath taken, as of the impossibility of concealment. It was before our time that Dr. Reynolds, as Censor of the College of Physicians, was summoned to mortal conflict by a gentleman he had rejected; but even in this less warlike day, pamphlets and party spirit have been hurled at the heads of persons in the same position. Such proceedings would be completely checked by admitting the public, or at all events the profession, to each examination; and by requiring previous attendance from those who are about to submit themselves to it, so as to secure an *ex officio* audience when the other fails.

Our allusion to the possibility of other examining bodies learning wisdom from the universities, leads us to notice the rich fund of experience on this subject which is afforded by the sketch which the Report

* Vol. iv. p. 417. Edinburgh, 1852.

gives of the history of Oxford, from the time of its great reforming chancellor, Archbishop Laud. Much that he did is permanent, because it was really useful, but much also has passed away. It has passed away because it was unsuited to human nature, and not, as the fashionable phrase runs, because it was unsuited to the age. That which is really unsuited to one age is unsuited to another, and that which has benefited our fathers would, if used aright, equally benefit our sons. But in spite of this failure, we see practices often revived over and over again, in different forms, and as rapidly falling into disuse. Among them conspicuously appears the attempts to make the senior degrees a test of ability; the futility of which experiments we would commend to the serious attention of the College of Physicians, who, in their new charter, propose to aim at the same thing. We are convinced that, in a few years, their examination for the Fellowship must share the fate of that which was tried several times at Oxford for the Mastership of Arts, and again revived for the Doctorate of Divinity. The evidence of Archbishop Whately so clearly shows the reasons of this, that we are induced to extract it. (See Evidence, p. 25.)

"When first I went to Oxford, and for some years after, there was a regular public examination for the degree of M.A. It was soon found that no examiners could be induced ever to *reject* a candidate, however ill prepared. Hence the whole soon degenerated into an empty form, and was discontinued. . . . .

"Then, a good many years after, when I was a member of the Hebdomadal Board, a scheme was proposed for making the Divinity exercises something real. It looked well on paper; but I inquired, 'Suppose a candidate for the degree of B.D. or D.D. fails to exhibit the requisite proficiency, will the examiner reject him?' They were compelled to admit that rejection was a thing not to be thought of, considering that several of the candidates would be elderly men, and clergymen, perhaps dignitaries."

Dr. Whately then alluded to the story in the 'Spectator,' of the Indian, who, on seeking his wife in the other world, found Paradise surrounded with an alarming barrier of thorns, but was eased of his fears by the discovery that they were only the *ghosts* of briars, and no impediment to his progress.

"'So,' he said, 'this examination will have some effect till it is discovered—as it soon will be—that it is only a shadow.' And thus it proved, on the experiment being tried."

We trusted, that in a Charter, to be dated 16th of Victoria, no "shadows" would have existed to have obscured the real meaning; but we fear that it is now almost too late for the warning to be taken. Anything would be better than a private form, which pretends to be something real, and is, in fact, an empty bladder. Election to the governing body by the votes of the fellows, or of the licentiates, or even selection by responsible persons, or simple progression into it by seniority, with or without the power of exclusion by ballot, would be something tangible, and capable of being carried out in detail, or discussed as a principle. Our objection to the examination is, its impossibility.

To return to the banks of the Isis. One difficulty objected to the employment of the physical sciences in general education, is the want of books, as a means of communicating them. Since the invention of printing, the professional system of oral instruction has been gradually giving way to its material rival, and it is true enough, that books, and good books, too, are absolutely necessary. To expect to find these as well suited to our purpose as those we employ in philology, or history, or religion, would be asking too much of a form of knowledge which we are setting to a new work. But we believe the demand will generate the supply, and text books, which now command a limited sale, for the use of medical students, may be soon improved by being a more remunerative style of writing. Monographs, too, of the higher class of scientific literature would be disengaged from Cyclopædias, and other serial publications, in which they are now buried from the want of sufficient sale to justify a separate form. But, in point of fact, there is at least a sufficiency to begin upon, of books elegant in style and philosophical in tone. Our physiologists have furnished several, of which mention is made, in the list of subjects for the natural science schools; and the chemists have made laudable efforts to wipe away the old reproach:

"*Chemicus in libro non valet ova duo.*"

As we said before, a new demand will produce a new supply.

On the whole, then, we anticipate from the recent movement at the universities great advantage to society in general, and to our profession in particular: to our profession as a science, and to those who practise it as individuals; much from our further knowledge of which others are acquainted with, but much more from their greater acquaintance with our forms of intellectual cultivation. But when? Are we to see it soon, or before we grow grey? In truth, we know not: prophets are not obliged to date their predictions; and the growth of solid reform is always so gradual and steady, be it slow or fast, that it is difficult to point to a period when it can be said to take place. We believe, then, that the good is beginning, nay, is already begun, and will be completed — perhaps never; perhaps when other things become perfect.

To this unseen and plantlike growth of all lasting reform we would especially call the attention of our friends on Cam and Isis: first, as a warning; and secondly, as an encouragement. As a warning, not to be led into sudden alterations by what happens to be the fashionable cry of the year. The pressure from without may sometimes, with great caution, be used as an instrument, but if it once masters the good sense within, real constructive progress is at an end, or at least seriously checked. The change to which we have been calling attention is only one step in the middle of a long ladder; it was not really more important than many of its predecessors and followers, but is capable of more comment, because it is marked by printed statutes. It will come into operation quietly, and its benefit will be gradual; if any part of its progress is accompanied by a flourish of popular trumpets, let its friends march with caution and suspicion. The slow advance of beneficial change is yet more an encouragement to perseverance. No reforming movement can be good which allows of lying on the oars after it is made, and therefore the mere fact of so much remaining to be done is evidence of the value of former exertions. The more they gain, the more overwhelming should appear the ground yet to be gained; for "the broader the diameter of light, the larger the circumference of darkness."

T. C.

REVIEW VII.

An Essay on the Action of Medicines in the System; or, "On the mode in which Therapeutic Agents introduced into the Stomach produce their peculiar effects on the Animal Economy." Being the Prize Essay to which the Medical Society of London awarded the Fothergillian Gold Medal for 1852. By FREDERICK WILLIAM HEADLAND, B.A., M.R.C.S. —*London*, 1852. 8vo, pp. 346.

THE intelligent physician is not satisfied merely to observe and record the results of treatment, but seeks, by inquiry and reflection, to ascertain *how* his remedies produce their effects, or, in other words, their *mode of operation.* From the earliest times to the present day this has justly been regarded as the key-stone of therapeutical science, and the history of medicine consists chiefly of the detail of systems based on abstract views of the intimate action of remedial agents.

That our knowledge of the subject has not attained a degree of advancement commensurate with the attention bestowed on it, is due, partly, to its highly complex character and consequent great difficulty, partly to its necessary dependence for progress on the previous advance of physics, chemistry, physiology, and pathology, but more especially to the faulty method pursued in its study. In place of adhering rigorously to the positive method, by which alone this or any other branch of medical science can be advanced, the highest intellects formerly wasted their powers in profound reasonings on speculations not founded in observation, but in the delusive fancies of ingenious men. Sects and schools were raised, to give place in turn to their rivals; but the subject in dispute received little real elucidation. In more recent times, the majority of the competent minds devoted to medicine have sought too exclusively in the simpler studies of physiology and pathology for those positive and unchangeable results which they have erroneously despaired of finding in therapeutics, so that even now, when sounder methods of inquiry are understood and employed, the progress realized in our knowledge of the mode of action of drugs is small, compared with that of other branches of medical science, and the results are precarious and uncertain in their practical applications.

Time has accumulated, nevertheless, a considerable amount of fact and opinion of more or less value, relating to the mode of operation of medicines. To arrange these, and bring them, if possible, into connexion with scientific principles, would render an important service to therapeutics, by making our existing knowledge more available as a guide to practice, by disclosing more clearly the extent of our ignorance, and suggesting means for its removal. But this is a most difficult task, and we are not disappointed to find that Mr. Headland has failed in its accomplishment. It demands an experienced intellect, whose thoughts have been for years given to the subject, to grapple successfully with its heterogeneous and intricate materials, so as to separate the wheat from the chaff, and generalize the former; and its practical nature requires a large amount of that individual knowledge which is obtained only by long experience in the observation of the effects and uses of medicines.

While we recognise with much pleasure the efforts of the London Medical Society to direct attention to therapeutical inquiries, we regard the selection of the subject of the present volume for prize-competition as unhappy. It is too extensive and difficult for original treatment within reasonable time and space, and the consequence is, that in place of fostering experimental inquiry and the discovery of new facts—the proper objects of a prize essay—it results in the production of a long and ill-digested compilation. The subject of a prize essay should necessitate original inquiry, and admit of complete investigation within a limited period, and thus make the competition one more of talent than, as it too often is, of mere industry. We feel assured, that if the same industry and abilities (and these are excellent) which have been devoted to the work before us, had been given to the elucidation of the action or mode of action of some one medicine, we should have now the pleasure of recording a contribution to therapeutics of substantial value, and the author would have laid a permanent ground-work for his reputation. We trust yet to meet him with a cordial welcome in this capacity, and meantime proceed to satisfy the reader that our judgment of his present essay is well founded.

The *first* chapter contains some introductory observations, setting forth the importance, extent, and great difficulty of the subject, and hence the need of "exact precision of language and thought" in its treatment. The author urges attention to this point earnestly and justly, yet his own work is disfigured by even more than average carelessness. His language is often inaccurate, his argument incorrect, and his reasoning inconsequential. Therapeutics suffers from many deficiencies, but especially from an imperfect nomenclature and the absence of an exact understanding as to the meaning of many of its words; and every writer in this science should early indicate clearly and precisely the sense he attaches to the technical language which he has occasion to employ. But the very title of the work under review is vague and undefined, and leaves the reader in doubt as to whether he is about to peruse a treatise on medicinal therapeutics, or an essay on a section only of that subject, or the mode of action of drugs. In several places throughout the volume, physiological and therapeutical actions are confounded together, and the author has nowhere attempted to distinguish, with scientific precision, two very different things, mode of action and mode of cure—the *modus operandi* and *modus curandi* of writers on the *materia medica*. That an accurate conception of the meaning conveyed by these expressions is most desirable, if not absolutely essential, to both author and reader at the outset of this inquiry, will appear from the following observations.

Physiological action and mode of action.—When a medicine is brought into contact with the living healthy body, two sets of changes result, one in the drug, and the other in the organism. The sum of the latter is named its physiological action, and commonly embraces several effects and symptoms, which it is desirable to analyze and arrange before inquiring into the manner of their production. We shall now do so very briefly, and only so far as is necessary for our present purpose. Physiological effects differ as to extent and place of operation, and are hence distinguished into local, general, and special; and also as to order of development, on which is founded their distinction into primary and secondary.

Distinction of local, general, and special actions.—Local action precedes absorption, and is confined to the part to which the drug is applied; general action is subsequent to absorption, and is manifested on the entire frame; while special action, also subsequent to absorption, is displayed on one system or organ. Irritant and emollient effects are examples of the first; tonic and stimulant, of the second; narcotic and diuretic, of the third. The same drug may act in only one or two, or in all three capacities: thus turpentine is a local irritant, general stimulant, and special diuretic; ergot of rye is a general sedative and special parturient (uterus); chloroform is a local irritant, general sedative, and special narcotic (brain). These examples already direct attention to the fact of there being often no perceptible correspondence or necessary connexion between the local, general, and special actions of the same medicine. Thus tartar emetic and ammonia are both local irritants, but the first is a general sedative, and the second a general stimulant: and pareira, nitric ether, and digitalis, are respectively tonic, stimulant, and sedative to the general system, but are all special diuretics. In an inquiry, therefore, into the operation of a medicine, we must ascertain separately its local, general, and special actions; and so, also, in attempting to penetrate the secret of its *modus operandi*, we must examine it as regards each of these actions respectively.

The same drug may have one mode of action for its local, another for its general, and a third for its special effects. Thus, as a local astringent, the mode of action of the muriate of iron is a chemical and injurious influence on the living tissue; but as a general tonic, it operates by contributing to the blood and system an element essential to their normal constitution. As a local irritant, the mode of action of alcohol is chemical. This drug is also a general stimulant; but the nature of the first impression or material change in the production of the symptoms so designated is unknown. It may be the same as that which originates the local action; but the fact of other local irritants, as colchicum and tobacco, being general sedatives, and not stimulants, is unfavourable to this supposition. Lastly, alcohol is a special diuretic. It is eliminated in part with the urine, and, irritating the inner surface of the urinary apparatus, induces, by reflex action, an augmented flow of blood to the kidney, and diuresis. This remote irritation is probably produced in the same manner as the local action. There are other cases in which the general and special effects acknowledge the same mode of action, or in which the special effect is a more distant link in the chain of symptoms arising from the same cause; and it is important, in analyzing the operation of a medicine, to ascertain whether the special action arises independently of the general, or is simply its physiological consequent. This leads us to the

Distinction of Primary and Secondary Effects. — When we examine remote (general and special) action carefully, a certain order of occurrence may be traced in the phenomena. There is, first, a material impression on the blood or solids, or both. This excites one or more functional changes in the system or organs with which the medicine is brought into direct contact. These are the primary effects. The drug may have no further action, or its primary effects may cause others in parts having physiological connexion with those primarily affected. There are the secondary effects which, again, are either ultimate and non-productive of other changes, or, as happens sometimes, they may originate a third order of phenomena.

To which of this series of changes do we refer in speaking of the "mode of action"? Some authors refer to the structural impression, some to the primary effects, some to both of these, while others employ the expression vaguely, and indicate the former at one time, and the latter at another. For scientific accuracy, the expression should apply always to the same thing, and we are of opinion that it should refer exclusively to the first order of phenomena, or the material changes, which originate and give their peculiar character to all the subsequent effects; and besides, this conception of the phrase is, on the whole, the most in conformity with usage. It is true, we find in the secondary effects the explanation of the tertiary phenomena, and in the primary the *modus operandi* of the secondary; but we are now trying to give a precise application to the term "mode of action," as it is employed in a general sense in reference to the operation of medicines. One or two examples will suffice to illustrate these remarks. In analyzing the remote action of potassa, we find that it produces one or more structural changes of the blood (and perhaps also of the tissues), by which the circulating fluid becomes impoverished and overcharged with effete matter, and we have, in consequence, the primary functional effect of impaired nutrition; this, again, causes diminution of tonicity and irritability, and increased activity of absorption, and the secondary effects of relaxation, weakness, wasting, and altered secretion, while these in excess may permit the transudation of the fluid portion of the blood, and occasion the tertiary phenomenon of œdema. The mode of action of potassa is the destructive alteration of the blood from which the other changes are derived as necessary physiological consequences. On examining the operation of digitalis, we find that the first or material impression of the drug, or its mode of action, is unknown, but that the primary effects are manifest depression of the vascular, nervous, and muscular systems, and of the first especially. From an enfeebled circulation arise the symptoms of paleness, weak pulse, coldness of the extremities (and, by diminished pressure on the brain), dimness of vision, vertigo, noise in the ears, and delirium. These—and the diuresis according to some authorities—are the secondary effects of digitalis.

Therapeutical Action and Mode of Cure.—We have spoken as yet only of physiological action. When the body is diseased, to this are added one or more new effects resulting from the curative agency of the drug upon the disease and its symptoms, and the sum of which is named the therapeutical action. While the physiological action is constant, both in health and disease, the therapeutical is uncertain, and relative to the various causes and modifying conditions of disease. The "mode of cure" is that one or more of the series of physiological changes which immediately serve to control the disease and its symptoms. It may be identical with the mode of action, as in the cure of chlorosis by iron; or it may be found in the primary effects, as when digitalis abates cardiac palpitation; or in the secondary effects, as in the cure of amenorrhœa from the sympathetic uterine excitement of a drastic cathartic. When the therapeutical action cannot be referred to the physiological, it is said to be *specific;* but it would be better to say that the "mode of cure" is undetermined, than to conceal our ignorance by the use of a mysterious word.

In the *second* chapter the author reviews "some of the more important classifications of medicines, and opinions of authors respecting their actions." He observes—

"The opinions of authors on the general action of medicines are in most cases best ascertained by observing the manner in which they have arranged and classified them, grouping together those which they consider to be alike in their mode of operation.

"Differences of opinion respecting individual medicines will be best considered afterwards, when we come to discuss those medicines. We are now to make inquiry as to the action of classes and groups. So that, in examining classifications as a key to the opinions of writers on this matter, we are only concerned with those which are founded in some way on the effects and operations of medicines.

"Now there are three different points of view from which the action of a medicine may be regarded. We may ask—1. What is the ultimate effect of its action on the system? 2. To what organ or tissue is its action directed? 3. In what way does it operate?

"In other words, we may speak of the *result of the action* of a medicine, of the *direction of the action* of a medicine, or of the *mode of operation* of a medicine. . . . . .

"Though the arrangements and theories of authors have generally taken into account all three of these questions, yet they have usually given greater prominence to one or other of them. And according to this their predominant idea, I will take the liberty of grouping them into three schools for the sake of convenience; considering, first, some theories and therapeutical arrangements which are based upon the ultimate effect of medicines; secondly, some that depend upon their local tendencies; and, thirdly, some others that concern their mode of operation." (pp. 17, 18.)

The arrangements of Young, Duncan, Murray, Eberle, Pereira, and Schultz, are passed in review, and are made the subject of brief, and, for the most part, of judicious comment. Mr. Headland has himself endeavoured to construct a classification founded on the mode of operation, that being the subject with which he is more particularly concerned, and being also of opinion that such an arrangement, though difficult of formation, must have "great practical and scientific utility." As the classification of medicines according to their action on the healthy body has formed the subject of a separate paper* by the writer of this notice, our remarks here will be few.

Mr. Headland has thought it necessary to select either the "action" or the "mode of action" as the exclusive basis of his arrangement, and has failed to perceive that a natural or scientific classification of medicines must be formed on the comparison of *all* their characters, so as to place *those drugs together whose properties present the greatest number of important points of resemblance, the relative importance of the properties being determined by their value as guides in the application of the medicines to the treatment of disease.* In accordance with this principle, the physiological properties assume obviously the first rank, and of these sometimes the general, and at other times the special and local actions, have respectively the highest value as guides to the employment of the drugs in disease, and determine their arrangement into classes; the distinctive

* Dublin Journal, November, 1852. See also a review of the classifications of Pereira and of Trousseau and Pidoux, in the same journal for May, 1850.

characters of which are sought in the primary effects, in preference to the *modus operandi*, because the former are in most cases a safer guide than the latter to the uses of the medicine, and because, in the present state of therapeutics, our knowledge of the former is comparatively accurate and complete, while of the latter it is of many drugs altogether undetermined, and of the remainder obscure and imperfect. When the mode of action is known, and suggests good rules of practice, it will direct usefully the subdivision of the class into orders.

In the following outline of his arrangement, it may be noticed that Mr. Headland has *not* confined himself to the *modus operandi*, but has also taken into consideration the general action and the presumed seat of action in the formation of his divisions:

"Class I. HÆMATICA. Division I. *Restaurantia.* Ord. 1, Alimenta; 2, Acida; 3, Alkalia; 4, Tonica; 5, Chalybeata; 6, Solventia. — Division II. *Catalytica.* Ord. 1, Antiphlogistica; 2, Antisyphilitica; 3, Antiscrofulosa; 4, Antiarthritica; 5, Antiscorbutica; 6, Antiperiodica; 7, Anticonvulsiva; 8, Antisquamosa.

"Class II. NEUROTICA. Division I. *Stimulantia.* Ord. 1, Generalia; 2, Specifica. — Division II. *Narcotica.* Ord. 1, Inebriantia; 2, Somnifera; 3, Deliriantia. — Division III. *Sedantia.* Ord. 1, Generalia; 2, Specifica.

"Class III. ASTRINGENTIA. Ord. 1, Mineralia; 2, Vegetabilia.

"Class IV. ELIMINANTIA. Ord. 1, Sialogoga; 2, Expectorantia; 3, Cathartica; 4, Cholagoga; 5, Diaphoretica; 6, Diuretica." (pp. ix. x.)

The general principle of this arrangement is stated (pp. 102 and 150) to be therapeutical, but it is obvious from the names of the majority of the classes and orders (as the narcotics, stimulants, sedatives, astringents, emetics, and cathartics), that they are founded on the action of the medicines on the healthy body. The catalytics are treated in an exceptional manner, and are grouped into orders "according to the morbid states in which they are employed." We would willingly waive the scientific requirement of a uniform principle of arrangement were difficulty experienced in distributing the antiphlogistics, anticonvulsives, &c., in a purely physiological classification, which, however, is not the case; but a glance at Mr. Headland's catalytic orders* must also convince the reader of the unsuitableness of the therapeutical character, as the basis of a scientific arrangement. It is too uncertain and relative. Thus, mercurials are placed among antisyphilitics, but more deny than believe in their efficacy in syphilis; silver and copper rank among anticonvulsives, yet many doubt altogether their utility in epilepsy and other spasmodic affections. At best, the only common proposition that can be made of Mr. Headland's antiphlogistics, antisyphilitics, and antiscrofulics — which ties them together, and is implied in the name—is, that they are useful in inflammation, syphilis, and scrofula. They include medicines having very different actions on the human body, and are no more orders in science than a happy family is a natural order of animals, or a nosegay a natural order of plants.

* "Ord. 1. *Antiphlogistics:* Antimonials, mercurials, alkalies, salines. 2. *Antisyphilitics:* Mercurials, preparations of gold, iodide of potassium, sarsaparilla (?). 3. *Antiscrofulics:* iodine, bromine, chlorine, iodides and bromides, potash. 4. *Antiarthritics:* Colchicum, nitro-hydrochloric acid, lemon-juice. 5. *Antiscorbutics:* Citric acid, lemon-juice, fresh vegetables, salts of potash (?). 6. *Antiperiodics:* Arsenious acid, arsenite of potash, alum, chloride of sodium. 7. *Anticonvulsives:* Preparations of arsenic, silver, zinc, lead, and copper. 8. *Antisquamics:* Preparations of arsenic, sulphur, pitch." (p. 153.)

Even as respects their application to the disease in which the name suggests their curative power, the members of a therapeutical order have little in common; for experience shows of each order, that the individual medicines have their special indications in certain stages and circumstances of the disease, and are useless or injurious in other stages and circumstances. Thus, each of the three periods of syphilis has its own treatment. In the primary, a cooling regimen and mild local applications are employed with advantage; in the secondary, slight mercurialism, with diaphoretics, is often of service; while in the third, the iodide of potassium, and, according to some, the salts of gold, are useful. And it is unnecessary to illustrate the familiar fact, that these rules of treatment may be further modified, or that the drugs named may be altogether contraindicated by circumstances of age, habits, and constitution peculiar to each patient. To inform a student, therefore, that mercury is an antisyphilitic, guides him a very short way to its successful employment, even in syphilis; and this drug is used in so many other diseases, that there are few therapeutical classes from which it can with accuracy be omitted. Thus, employed as the basis of classification, the therapeutical character is wrong in principle, and of small value in practice.

The consideration of the *modus operandi* of medicines is conveniently divided into two parts—general and particular; the first embracing a general account of the physical, chemical, and other forces by which drugs operate on the living structure; and the second, the particular modifications of those forces which characterize the modes of action of individual medicines.

General Modes of Action.—Although this forms the title of the third chapter, it is not treated of there, but in the latter part of the second, in connexion with classification. Mr. Headland there observes:

"Attempts have been made to account for the *modus operandi* of therapeutic agents generally, in three different ways:

1. On mechanical principles.
2. On chemical principles.
3. On general or vital principles.

"1. Mechanical theories of the action of medicines were greatly in vogue during the seventeenth and eighteenth centuries. There is a tendency in the human mind to explain everything; and it was only natural for men who knew little of chemistry or of physiology to resort to the science of physics, which they could comprehend, in attempting the explanation of observed phenomena.

"John Locke, in his Essay concerning the Human Understanding, published in 1689, gave it as his opinion, that the shapes of the minute particles of medicines were sufficient to account for their several operations.

"'Did we know,' said he, 'the mechanical affections of the particles of rhubarb, hemlock, opium, and a man, as a watchmaker does those of a watch, whereby it performs its operations, and of a file, which, by rubbing on them, will alter the figure of any of the wheels, we should be able to tell beforehand that rhubarb will purge, hemlock kill, and opium make a man sleep.' This idea did not originate with the great metaphysician. The first rudiments are to be found in the doctrines of the *Methodic Sect* among the Romans, a medical branch of the Epicurean school. They held that diseases depended either on constriction or relaxation of the tissues, and that medicines operated by mechanically affecting these conditions." (pp. 31, 32.)

Here follows a rambling account of the crude mathematical and chemical notions on the actions of medicines taught by our predecessors, from Galen downwards, which might have been omitted with advantage in a work that the author has difficulty in keeping within proper limits. There is, indeed, "very little that is tangible to be discovered in these old theories, and it is not to be wondered at that most of them have faded away before the advance of science." (p. 37.) In the remainder of this section the author introduces views relative to general physiological and therapeutical effects, and finally gets involved in such a jumble of subjects essentially different, as to defy useful criticism; and we content ourselves by extracting the first and last paragraphs:

"The most plausible explanations of the mode of operation of medicines have been founded on *vital* or *general* principles. By *vital* I mean that these theories concern actions which could only take place in the living body. They may be termed *general* principles, because the grounds on which they are based are neither mechanical nor chemical, but something different from both. The term *dynamical* has sometimes been applied to an ill-understood vital action of this sort. . . . . .

"Concluding, then, that it is impossible to account clearly for the actions of most medicines on *Mechanical* or on *Chemical* principles, we are led to infer that their influence must for the most part be *vital* in its nature—that it must be such as could only be exerted in the living body. Even then we are unable to fix upon any single rule or formula which shall be capable of accounting for the actions of all at once. So it seems that the only general explanation which we can offer of the *modus operandi* of medicines in the cure of diseases, is to say that they operate by *various counteractions.*" (pp. 39, 47.)

This reminds us of the reply of Argan, in the 'Malade Imaginaire:'

"Mihi a docto doctore
Demandatur causam et rationem quare
Opium facit dormire.
A quoi respondeo,
Quia est in eo
Virtus dormitiva,
Cujus est natura
Sensus assoupire."

Medicines influence the phenomena of life only by modifying the organs and constituent elements, solid or fluid, of the body. The first effect of some is accompanied by an obvious organic change, and admits of explanation by physical or chemical laws, or by both combined. We attribute to *physical* forces the changes caused by form and weight, by modifications of imbibition and endosmose, and by alterations in the distribution of heat and electricity; and to *chemical* forces, the molecular changes of combination and of decomposition induced in the fluids and solids with which the medicine comes in contact. But there are medicines whose operation is unaccompanied by any visible organic change, and which cannot, in the present state of our knowledge, be referred to known physical or chemical laws. Hence a third general mode of action is admitted, and named by some vital,—by others, dynamical. The first term seems to imply in the drug the existence of the forces peculiar to living beings, and is therefore incorrect. The term dynamical is also open to objection. It includes, in its ordinary sense, all forces—physical, chemical, or vital—resulting in motion; and its employment in a new and restricted sense in therapeutics is inconvenient. Moreover, it veils our ignorance by giving a formal designation to forces the nature of which is unknown. We prefer to confess our ignorance, and describe these forces as *undetermined.* There is little doubt that many of them will be brought, by careful examination, within the known domain of physics and chemistry; and there may be others different from, or new modifications of, the forces now recognised in these sciences. Galvanism was first manifested by its action on living bodies; and physiological experiments may yet be the means of throwing much light on the more latent properties of inorganic matter.

The forces by which medicines operate are, then, conveniently arranged under the three heads of physical (including the thermal and electrical), chemical, and undetermined. Here we shall not go further into their consideration, as the reader interested in the inquiry will find, in the recent volumes of this journal, several articles in which the nature and operation of these forces on living bodies have been discussed. In connexion with this subject, Mr. Headland has omitted to notice the important influence of isomorphism in determining the reactions between medicines and the living economy, as ascertained by the beautiful researches of Professor Blake, and for an account of which we refer to our second volume.

We come now to the *third* chapter, which treats of the absorption of medicines, and their particular modes of operation when introduced into the stomach. The affirmations made on these subjects are arranged in ten propositions:

"The first four of these concern the general conduct of medicines after their introduction into the stomach, and before their passage into the blood. . . . .

"The remaining six treat of the subsequent behaviour of those medicines which pass into the blood and fluids of the body. Of these, the fifth specifies their general course. The sixth states that they may undergo certain changes in the system. And the concluding four treat of the various modes in which these agents may operate in the cure of disease.

"The first proposition lays down the great fundamental rule of the action of medicines through the medium of the blood and fluids.

"PROP. I.—*That the great majority of medicines must obtain entry into the blood, or internal fluids of the body, before their action can be manifested.*

"This is to say, that the mere contact of a medicine with the stomach is not in general sufficient for the production of its peculiar action. It will be seen that the only apparent exceptions to this rule consist of agents having a mere local action on the mucous membrane, for which simple contact is all that is required.

"Even when acting on any part of the system removed from this mucous surface, as when applied to the skin, it is necessary that the medicine pass away from it to enter the blood or internal fluids. In the great majority of instances it enters the blood directly. But we know that it would be sufficient for its operation if it were to enter through the chyle, or into the serous fluid which exists in the interstices of the tissues throughout the body. For by these it might at length be conducted to distant parts. This is what is meant by *internal fluids.*" (pp. 48, 49.)

The part of the doctrine of absorption referred to in this proposition is not correctly stated. All medicines must enter the blood before their remote action can be manifested. Their local operation is quite independent of absorption; and as to the qualification of "internal fluids," we are not aware of any facts showing the development of remote effects from the presence of drugs in the chyle or interstitial serum. On the contrary, all our knowledge tends to prove that their passage into the blood is essential. It is not so clearly determined, as laid down by Mr. Headland in his *fifth* proposition, that they are in all cases carried with the blood to the parts on which they operate primarily. The evidence in favour of this view gives it a very high degree of probability, and we may accept it as respects medicinal action; but our author has not even alluded to the difficulty in the way of its unreserved adoption presented by the extremely rapid influence of certain poisons. He joins Mr. Blake in affirming that "there is no poison whatever which acts so quickly on distant parts, that the circulation cannot previously have had time to conduct it to them" (p. 52), and ignores completely the experiments of Christison, Brodie, and Taylor, on conia and prussic acid, from which it appeared that these poisons may begin to operate, and even prove fatal, in a shorter space of time than is necessary for their conveyance with the blood to the organs affected.

In his *second* proposition Mr. Headland examines the process of endosmose, and the manner in which medicines insoluble in water are reduced by the animal fluids in the stomach and bowels to the state of solution, and fitted for entering the bloodvessels. He arranges the drugs and articles of food which are insoluble in water in four groups:—1. Mineral substances soluble in acids, as the oxide of iron and the carbonates of lime and magnesia. 2. Mineral substances soluble in alkalies, as sulphur and iodine. 3. Animal and vegetable substances soluble in the gastric juice, as albumen and gelatine; and 4. Fatty and resinous substances soluble in alkalies, as cod-liver oil and copaiba. The acid solvent is furnished by the gastric juice, and the alkaline by the biliary and pancreatic fluids. No notice is taken of the important part played by the alkaline chlorides in the solution of calomel and other metallic drugs, as determined by Professor Mialhe, and described in his treatise on the 'Art of Prescribing,' a work with which Mr. Headland seems not to be acquainted; yet we find little in his remarks on the changes which medicines undergo in the intestinal canal preliminary to their absorption, which is not better and more fully stated by Mialhe, whose small volume contains by far the most valuable contribution to our knowledge of this subject.*

In the *third* proposition it is affirmed that insoluble bodies cannot pass into the circulation, an assertion which is not justified by our present knowledge:

> "PROP. III.—*That those medicines which are completely insoluble in water, and in the gastric and intestinal juices, cannot gain entrance into the circulation.*"

That every substance capable of exerting a *remote medicinal* action is soluble, or susceptible of becoming so, in the fluids of the body, is a safe and sound doctrine in practice; and it has been shown that nearly all, if not all, our active medicines which are insoluble in water, such as calomel and the oxide of antimony, undergo chemical changes in contact with the gastro-intestinal secretions, which fit them for solution and passage into the bloodvessels. The drugs not thus acted upon are very few, but charcoal is an unequivocal example of a perfectly insoluble medicine. The simple metals and nitrate of bismuth, cited as such by Mr. Headland, are acted upon by the secretions.

* Reviewed in vol. iv. p. 128.

But Mr. Headland affirms that solid bodies cannot under any circumstances obtain entrance into the circulation; and, indeed, this doctrine was generally believed until a doubt of its accuracy was suggested, in 1847, by the experiments of Œsterlen, who found that finely-divided charcoal, administered by the mouth to fowls and rabbits, passed into the blood, in which fluid the carbonaceous particles were detected by the microscope. Other animals fed on Prussian blue gave the same results. Our author is disposed to reject these observations as inaccurate, and as opposed to what he says is "an absolute physical law"—that insoluble bodies "cannot pass through the homogeneous wall of the capillary or absorbent vessels." But we cannot assent to the affirmation of so important a law until furnished with more conclusive proof of its truth than we now possess.

All we know of physics would lead us to believe in the possibility of finely-divided solids passing through organic membranes. We are told, on the one hand, that solid bodies may be reduced to particles of unlimited minuteness, and, on the other, that organic membranes are porous; and we see no reason why solid particles suspended in water, and smaller than the pores of the membrane, should not pass through them. Sulphate of baryta and charcoal in fine division go through filtering paper; and it is probably only a question of relative size, as between their particles and the pores of an organic membrane, that determines their passage or non-passage through the latter. We have found that the particles of charcoal and indigo may be reduced to a size ten and twenty times smaller than that of the interstices of the animal tissues, which are stated by Matteucci to vary in diameter from $\frac{1}{2540}$th to $\frac{1}{5080}$th of an inch. In the black consumption of coal-miners, carbon passes from the air-passages into the blood; but it is uncertain whether the bronchial membrane is ever quite sound at the time of entrance.

Mr. Headland had misgivings about his "absolute physical law," and has made the following experiments "to satisfy himself on the point:"

"Exp. 1.—Ten grains of calomel were given to a large dog. It was killed after three hours, allowing this time for digestion. A considerable quantity of blood was collected from the portal vein, and submitted to analysis to determine whether it contained any compound of mercury in an insoluble form. The blood was dried and pulverized. The result was boiled for some time in water, and the insoluble part collected. It was dissolved in a small quantity of aqua regia, and the clear acid solution placed in a test tube. A slip of zinc foil was folded round a narrow plate made of gold foil, and introduced into the solution. A galvanic current being thus set up, the minutest quantity of mercury, if present, would have been deposited on the gold, so as to tarnish it. But this did not take place, and when at last the zinc was completely dissolved, the gold remained as bright as before. Thus there was *no* calomel, or compound of mercury, present in the insoluble form.

"Exp. 2.—Ten grains of strong mercurial ointment (containing half its weight of metallic mercury, with some oxide) were given to another dog. He was killed after the same time, and the portal blood analyzed carefully in the same way, but here also *no* mercurial compound was present in the insoluble form.

"Exp. 3.—To a third dog five grains of oxide of silver were administered. After three hours he was killed. The portal blood was dried in a water-bath, and reduced to powder. This was boiled for some time in water, which was separated by filtration. Aqua regia was then boiled on the insoluble part. This would convert any silver into chloride. The acid was evaporated off as much as possible, and the solid remainder heated in a small porcelain crucible to dull redness. The result was powdered, and digested in liquor ammoniæ. It was filtered, and excess of nitric acid was added. There was not any precipitate. Had chloride of silver been present, it would have been dissolved by the ammonia, and precipitated by the acid. Thus no insoluble silver compound was contained in the blood analyzed.

"EXP. 4.—Ten grains of sulphur were administered in the same way to a fourth dog. On killing it and opening the body, the thoracic duct was found to be full. A considerable quantity of chyle was collected from it. Now, as it is asserted by some that fat passes undissolved into the chyle, and as I believe that sulphur is digested in the neighbourhood of the bile duct, this chyle was chosen for analysis in preference to blood, as more likely to contain any insoluble sulphur. Besides, the blood would be less satisfactory, on account of the large quantity of albumen and fibrine contained in it, both of them also containing sulphur. The insoluble part of the chyle was obtained in the same manner as with the blood. It was then boiled in a small quantity of a weak solution of caustic potash. By this any free sulphur would be converted into a soluble sulphuret of potassium. The solution was filtered, and a few drops of a solution of the nitro-prusside of potassium added. (This is a salt lately discovered by Dr. Playfair. It is a delicate test for soluble sulphurets, with which it strikes a deep purple colour.) No change was produced. Therefore no insoluble sulphur was present in the chyle." (pp. 76, 77.)

Our author considers that the results now detailed are in direct opposition to those of Œsterlen's experiments, and justify his assertion that insoluble bodies cannot pass into the bloodvessels; but a little inquiry shows that they do not warrant this inference. He should have employed powders reduced to the finest possible state of division (with particles measuring less than $\frac{1}{8000}$th of an inch in diameter), perfectly insoluble in the animal fluids, light, and at the same time of easy recognition with the microscope or with chemical reagents. They should then have been exhibited in considerable quantity, and well mixed with fluid, so as to facilitate in every way their passage through the membrane. A more unsuitable substance could scarcely be found, than that employed in the first experiment. Calomel is heavy; its particles are large (averaging $\frac{1}{2000}$th of an inch in diameter), and in contact with the secretions it undergoes partial change and solution, by which its absorption and remote action are readily understood; and its detection in the blood by chemical means is always difficult. Ten grains are given to a dog, and the portal blood is examined for it unsuccessfully three hours afterwards; but even if it had got to some extent into the circulation in the solid form, it might all have passed from the portal blood within that period. Some slight value might have been given to the experiment had it been shown that mercury was at the same time present in solution; but this has not been done.

In the second experiment, a fluid (mercury) is oddly enough employed in an inquiry into the absorption of insoluble powders. The third and fourth experiments are open to the same objections as the first. Ordinary sublimed sulphur was probably the variety of this drug employed in the fourth experiment. Its particles vary from $\frac{1}{617}$th to $\frac{1}{2466}$th of an inch in diameter, and are too large. Besides, this element, when exposed to the action of alkaline carbonates in solution, is transformed in part into the sulphuret and hyposulphite of the alkali, compounds which are soluble and capable of easy absorption. And it is singular that Mr. Headland seeks for *undissolved* sulphur in the chyle, the very place where he has previously endeavoured to show that it should be present in solution. "The blood," he says, "would be less satisfactory, on account of the large quantity of albumen and fibrine contained in it, both of them also containing sulphur." But chyle has fibrine and plenty of albumen, yet his "delicate test" does not seem to have made out any sulphur in it. If insoluble bodies pass into the circulation, all that we know of absorption points to the veins as their probable channel of entrance.

We cannot admit the validity of the conclusion, that in these experiments the powders, or any portion of them, did not pass undissolved into the blood; and if that were shown, the same powders, under other circumstances, as finer division and larger quantities, might still obtain entrance. Even if it were proved that calomel, oxide of silver, and sulphur, were never taken undissolved into the circulation, the same does not follow of charcoal and Prussian blue.

Further investigation is necessary for the solution of this question. Œsterlen may have been deceived in his observations;* but the best way of determining their truth is to repeat his experiments on the living animal, and attend carefully to the details which he has given.

In the *fourth* proposition it is stated—

> "*That some few remedial agents act locally on the mucous surface, either before absorption, or without being absorbed at all. That they are chiefly as follows:—A. Irritant emetics. B. Stomach anæsthetics. C. Irritant cathartics.*"

We have described local action as that which precedes absorption, and is confined to the part to which the medicine is applied. It is manifested by the great majority of medicines, and not by a few only, as our author states. A large proportion are local irritants, and produce effects varying in degree from simple increase of secretion to inflammatory redness, vesication, suppuration, and gangrene; many are local emollients; some are local astringents, as tannin and catechu; a few are local anæsthetics, as chloroform and aconite; and others are local paralyzers, as prussic acid and lead. The topical impressions may induce changes in parts at a distance, through the reflex nervous system; which should be distinguished, as Mr. Headland observes, from primary medicinal effects. Lachrymation, sneezing, salivation, and menstruation, from irritants in the eye, nose, mouth, and rectum, are familiar examples.

The operation of emetics is discussed here. Mr. Headland adopts their common division into direct or irritant, and indirect or specific. The former are said to act by local irritation of the gastric mucous membrane; and the latter on the vagus nerve, remotely, or after absorption. The sulphates of zinc and copper are described as irritant, and tartar emetic and ipecacuan as specific emetics. The tardy action of the latter is thought to indicate their solution in the stomach, and passage into the blood; but it is due as probably to their acquiring acridity only in virtue of chemical changes, brought about by their contact for some time with the gastric juice. On the skin their irritant influence is likewise slow of development, and is obviously secondary to some chemical reaction between the drug and the cutaneous secretions. As to the proof derived from their causing

* In the fifth edition of his 'Materia Medica,' Œsterlen continues to adhere to the conclusions deduced from his experiments. We may also refer to the confirmatory experiments of Eberhard, Donders and Mensonides. (Henle's Zeitschrift, Band i. (N. F.) pp. 400—15.)

emesis when introduced into the circulation, every experimenter knows that most substances capable of exciting sufficient disturbance, produce both vomiting and catharsis when thrown into the vessels.

Like most drugs, they produce both local and general effects; but as emetics, we believe them to operate by direct irritation of the interior of the stomach. And daily experience abundantly confirms this view: for the more decided their local irritation, and consequent emetic operation, the less manifest are their remote effects, the medicine being rejected from the stomach before absorption has taken place, and *vice versâ*. The same is remarked of all the narcotico-irritant poisons; and the circumstance of tartar emetic causing vomiting when in the stomach, and purging when in the bowels, is referable only to local irritation.

The *sixth* proposition contains no pretence to novelty, and need not detain us. We pass to the seventh, which, with the eighth, ninth, and tenth, embraces the exposition of the author's views of the particular modes of medicinal action. He divides medicines into four classes of hæmatics, neurotics, astringents, and eliminatives, according to their presumed *seat* of action on the blood, nervous system, muscular fibre, and organs of secretion.

The *seventh* proposition relates to the first class, the members of which are said to " act by influencing the blood itself, simply and solely." (p. 95.) It includes iron, mercury, potassa, quinine, iodine, and a long list of valuable drugs, having the most varied actions, and of which almost the only common characters are, their inducing sensible changes in the circulating fluid, and their effects being more or less permanent; so that the hæmatics are very deficient in the scientific requirements of a class. But we cannot admit that their immediate operation is confined exclusively to the blood. The changes in this fluid form an essential, and at present the best understood, part of their action; but some of them, as potassa and the iodide of potassium, act undoubtedly on the solids, and it is premature to assert of any one of them that it has no direct influence on the tissues. This is obvious from a little reflection on their action. Let us take, for example, iron and quinine, two of the more important.

Iron is a normal constituent of all the tissues and fluids of the body. It is present in largest proportion in the blood, and is found both in the serum and blood-globules; but is attached especially to the hæmatin, or red colouring matter of the latter, being essential to its chemical constitution. Thus, too little iron in the system will diminish the normal proportion of hæmatin, and consequently of blood-globules. The diseases—chlorosis and anæmia—in which it is useful, are characterized especially by deficiency of red corpuscles; but this is only one of many symptoms, such as pale complexion, impaired appetite, œdema, mental languor, physical weakness, and general atony of the tissues. When iron is given, it is absorbed and retained in the blood, which improves in colour, and becomes richer in globules, and, at the same time, the healthy nutrition, tone, colour, and strength of the entire body are restored. The metal enriches the blood by adding to it the wanting element, and the question now before us is, whether the improved tone and strength of the solids result solely from the higher nutritive power of a richer blood, or are due in part also to the direct action of the metal on, or addition of it to, the tissues—the blood serving merely as the vehicle to carry it from the stomach to the capillaries. We cannot, in the present state of our knowledge, give a definitive answer to this question; but it is certainly *not* proved that the drug acts only on the blood. It may be conveyed directly to the tissues, without previously forming part of a blood-globule. It is always present in the serum as well as in the corpuscles; and in vegetables whose tissues contain iron as an essential constituent, there is at least no intermediate stage of corpuscle between the absorption of the metal and its addition to the substance of the plant.

It is most inaccurate to assert of quinine, that its primary operation is confined to the blood, when we have no knowledge whatever of its seat or mode of action. Improved tonicity may result from a direct action of the medicine on the solid fibre, or from the supply of better blood to the tissues; or both causes may co-operate. That quinine has some action on the solids appears probable, from its invigorating influence on the coats of the stomach, the comparative rapidity of its curative operation in some diseases, and its speedy action in large doses as a poison. Agents whose primary influence is closely confined to the blood manifest their action slowly. We may add, that the researches of Professor Blake already referred to, in which many of Mr. Headland's hæmatics were made the subject of experiment, place beyond doubt their power of disturbing immediately and powerfully the functions of the tissues, but whether in conjunction with or independently of changes on the blood has not been determined.

The blood-medicines are divided into *Restoratives*, which "act by supplying, or causing to be supplied, a material wanting, and may remain in the blood;" and *Catalytics*, which "counteract a morbid material or process, and must pass out of the body." The first are said to be natural to the blood, and to be useful in those diseases caused by their own deficiency. They are said to "restore what is wanting;" and hence the name, which is inconvenient, as long familiar to us in the sense of a cordial stimulant. They correspond nearly to our tonics, which are reduced in Mr. Headland's arrangement to an order of the restoratives, and include only the vegetable bitters; but the word tonic is almost universally used to designate any influence which increases permanently the strength and weight, and this arbitrary restriction of its application can only cause confusion.

The catalytics* are unnatural to the blood, and are separated from it by the excretory glands:

"What then is the curative action of these remedies?

"A large class of diseases depends on the presence in the blood of a morbid material, or, what amounts to the same thing, on the constant working of a morbid process in that fluid. . . . . .

"Now the object in the treatment of such diseases is to obtain in each case some remedy that shall be able to counteract this process, something that shall destroy the morbid influence at work, and thus restore health. Medicines that are used with this intention form the second division of Hæmatics, which I have named Catalytics (Catalytica), from a Greek verb signifying to destroy or to unbind.

"Now though I have a probable hypothesis to advance as to the action of some of these, I would not have this considered as more than hypothetical. I would not speak positively of the action of any one of them, any more than to say that each of them tends to neutralize one or more particular morbid poisons. . . . . .

* See p. 93, note.

"Now the mode of counteraction is not defined, because it is only in a few cases that we can even guess at it. In the majority of instances it seems inexplicable. We know that Syphilis is a poison in the blood. Mercury also is a poison in the blood. But why does Mercury antagonize and annihilate Syphilis? The case is the same with Scrofula and Iodine; with Lepra and Arsenic. It is very humiliating to be baffled when we have got thus far; when, led by the hand of Science, we have been conducted almost to the end of this interesting inquiry, to find that we are perfectly unable to take the last step, and thus to conclude our adventure." (pp. 97, 98, 167.)

The *modus operandi* of catalytics is thus, according to our author, all in darkness, and he carries us a century backwards into the mysticism of specifics. We certainly know little of their mode of action, but we can say something of their mode of cure, and can refer their remedial agency in several diseases, more or less satisfactorily, to their operation on the healthy body.

Many diseases are unquestionably caused by the presence of a morbid material in the blood, either introduced from without, as in typhus and hydrophobia, or generated within the body, as in the lithic and oxalic diatheses; but our existing knowledge does not warrant our author in including scrofula, scurvy, and psoriasis, among poison-diseases; nor has he given any satisfactory evidence in support of his proposition, that the catalytics act in poison-diseases by destroying the morbid virus. This would be the most efficient mode of treatment, had we the means to effect it; but, though we have an obscure suspicion of such an antidotal agency in one or two cases, we are, for the most part, reluctantly thrown upon more general principles in explanation of the *modus curandi.* Though we cannot neutralize the poison, as we neutralize an acid with an alkali, we can improve the general health, and prevent its further generation when it originates within the body; and, in all cases, we can so modify the functions as to subdue its effects and promote its elimination; and these are the means by which we control the course of typhus, small-pox, gout, and rheumatism; our main indications for their employment being obtained by careful study of the curative process of nature.

The assertion, that in the treatment of inflammation our remedies are directed against the disease *in globo*, or are intended to neutralize its supposed essence, scarcely merits correction. We are obliged to analyze this morbid process into its numerous organic and functional elements, and try to favour the natural tendency to resolution, by attacking one or more of these separately. And as the elements vary greatly according to the stage and duration of the inflammation, constitution and habits of the patient, &c., so its cure may be promoted by remedies having different and often opposed actions. We relieve congestion and lower action by the withdrawal of blood, sedatives, evacuants, and low diet; soothe irritation by emollients; abate heat by cold; swelling by pressure; pain by anæsthetics, and restlessness by narcotics. When the vessels are weakened and distended in the chronic form of the disease, resolution is promoted by tonics and astringents. These are some of the many elements comprised in antiphlogistic treatment, and a few of the varied agents which may co-operate effectually to the solution of inflammation. In justice to Mr. Headland, we must state, that in his detailed account of antiphlogistics he almost abandons his general catalytic doctrines.

A considerable number of our author's catalytics, as the mercurials, antimonials, alkalis, and iodides, exhibit important resemblances in their physiological action. They impoverish the blood, impair nutrition and tonicity, and finally cause weakness and emaciation. And their beneficial influence in disease, especially as antiphlogistics and absorbents, can in some measure be accounted for by reference to these effects.

Their physiological operation induces a condition of system opposed to that of *inflammation*. They depress the functions, lower the plasticity of the blood, soften textures, liquefy secretions, and promote elimination, and thus, doubtless, favour the natural process of resolution. Their *absorbent* action is an obvious consequence of their effects on the healthy body. Impaired assimilation and defective sanguification—whether produced by drugs, starvation, dyspepsia, or bloodletting—always increase absorption. Hence the value of mercury in promoting the removal of inflammatory effusions, and of iodine, mercury, and the alkalis in discussing hypertrophies and scrofulous swellings. The augmented activity of absorption is communicated to the entire body, and is not confined to the adventitious growth or effused fluid; but these, being of recent formation, are taken up early and quickly, like cicatrices and callus in scurvy and other exhausting diseases. Potassa and the iodide of potassium appear to have likewise a direct solvent action on lymph and other new deposits, by which their absorption is promoted.

The cure of gout by colchicum is included by our author among the inexplicable phenomena of therapeutics, and yet it may be reasonably enough referred to the sedation, purging, and increased elimination of uric acid, and perhaps also of bile, caused by the medicine. Its curative influence is never decided until some amount of physiological action—as indicated by languor, frontal headache, and slight diarrhœa—has been developed, and other remedies which occasion symptoms in health like those of colchicum, are similarly useful in gout.

We agree, however, with Mr. Headland, that the therapeutical action of several of his catalytics is involved in obscurity, and that in these circumstances we should found our practice on the simple results of experience, and not upon vague hypotheses of the essential nature of disease and the intimate action of medicines. Nor are we indifferent to the value of specifics. The possession of a dozen such, equal in efficacy to the curative power of quinine in ague, would much enhance the importance of our art, and we should gladly welcome their discovery, however inscrutable their mode of operation; but with the conviction that this knowledge, if acquired, would enable us to exhibit them with more accuracy and success, and extend the sphere of their usefulness.

In the *eighth* proposition, Mr. Headland states, of his second class of medicines, or *neurotics*, that they

"*Act by passing from the blood to the nerves or nerve-centres, which they influence.*

"*That of these, some, called* STIMULANTS, *act so as to exalt nervous force, in general or in particular.*

"*That others, called* NARCOTICS, *act so as first to exalt nervous force, and then to depress it; and have also a special influence on the intellectual part of the brain.*

"*That others, again, called* SEDATIVES, *act so as to depress nervous force, in general or in particular.* . . . . .

"The action of neurotics, or nerve-medicines, is rapid; but it is transient, and is soon over. A neurotic medicine does not cause any change in the blood; and it cannot remain in it, but soon passes out. It acts by contact with nerve, apparently producing no lasting change even in nerve-fibre; and as the cause of the action cannot remain, the effect also soon passes away." (p. 205.)

Of their mode of action he observes:

"I must now venture to repeat an idea which I have already referred to at the commencement of this Essay, and state my belief in the bare possibility of the operations of neurotic agents being explicable upon mechanical grounds. It is generally believed among scientific men that each particle of a compound body is made up of a number of indivisible atoms, each of which is inconceivably minute in size. And as these compound bodies have each a peculiar chemical constitution, so must each of their ultimate parts be composed of a peculiar arrangement of simpler atoms, and thus have a certain shape of its own, more or less different from the shape of every other compound atom. Both the substance of a nerve, and the active part of a nerve-medicine, consist of a number of definite compound atoms. And it is possible that the atom of a stimulant medicine may be of such a shape as that it shall be unable to coincide with, or to fit into, the series of atoms forming the sensitive surface of the nerve, and thus irritate this when brought into contact with it; and that the compound atoms of a sedative may so arrange with these nerve particles as to fit among and extinguish their salient points, and annihilate their natural sensibility. We learn from the phenomena of the senses that the nerves are very much under the influence of mechanical impulses of all kinds, and particularly minute and inappreciable impulses of this description. Another fact which gives additional credibility to such an idea is, that those neurotic substances which are chemically alike, are in general alike also in their influence on nerve." (pp. 207, 208.)

We coincide with the author in his opinion of this speculation: "It is purely conjectural and fictitious, and is indeed likely to remain so; for the thing is not by its nature susceptible of proof, nor is it even possible to inquire into it."

Mr. Headland advances no proof, nor can any be given, in support of his affirmation, that the action of neurotics is confined to the nervous system. Every tissue and system has its independent vitality, and may be influenced separately by external agents. Stimulants and sedatives act more or less equally on all the functions, and augment or depress the vascular and muscular, as well as the nervous systems; and there is reason to believe that their effects on the circulation and muscles may be produced independently of the nervous system. Prussic acid and aconitina, when painted on detached muscular fibre, paralyze it, and the weakness from their internal use may be in like manner caused by primary action on the muscles. The depression of the heart may be due to paralysis of its muscular wall thus produced, or it may arise from some peculiar impression on its lining membrane, or from some transitory change in the organization of the blood, which has hitherto eluded observation. That a nervous system is not essential to the action of the neurotics is proved by some of them, as prussic acid, aconitina, and sulphuretted hydrogen, destroying the irritability and life of plants in which none exists. We may have some vague ideas of the mode of action of these drugs, but we really know nothing certain of the nature of their first impression, or of the system or systems on which it is made; and all we can do at present is, to observe carefully their effects.

There are many points in the section on neurotics to which we object, but we can allude to a few only of them, and must do so very briefly.

Strychnia and brucia are placed among stimulants, and in the same division, therefore, with ammonia and valerian, with whose action, we need hardly say, they have not the slightest analogy. Their very peculiar effects —commencing with rigidity and muscular twitches, and terminating in convulsions and spasmodic asphyxia—are not associated with general stimulation; give them special uses in disease; and demand for them a separate and independent place in an arrangement of drugs. To force them, as Mr. Headland and others have done, under stimulants, with the prefix of special, can have no other result than to create error and confusion.

Mr. Headland's theory of narcotic action is one of the few traces now existing in therapeutics of the Brunonian system, the author of which taught that narcotics were stimulants, which, acting with great energy, rapidly exhausted the excitability of the body, and brought on indirect depression. But several narcotics produce no perceptible previous excitement. True narcotism (hypnotism) is a peculiar and independent phenomenon, and has no essential relation either to stimulant or sedative action. This is obvious from the circumstance of its being associated with both of these actions. Some narcotics, as ether, are stimulant; some, as chloroform, are sedative; while others, as Indian hemp, act feebly in either capacity. The doctrine which refers narcotic action to the exhaustion resulting from previous stimulation is untenable on other grounds. Were it true, all stimulants should be also narcotics, which is not the case. The operation of alcohol itself affords it no support, as, in intoxication, consciousness is suspended long before the secondary stage of depression, as indicated by the pulse, is established. Lastly, were narcotic action a symptom merely of nervous exhaustion, we should have it in a high degree from certain sedatives, as hydrocyanic acid, conium, and aconite, which powerfully depress the cerebro-spinal system. But the slight disposition to sleep sometimes induced by these drugs cannot be likened to true narcotism.

Mr. Headland divides his narcotics into three orders:

> "1. Inebriantia: Alcohol, wine, ethers, chloroform, camphor, Indian hemp, tobacco, lobelia.
> 2. Soporifics: Opium, lactuca, hops, nutmegs.
> 3. Deliriants: Hyoscyamus, belladonna, stramonium.
>
> "These orders are named from the secondary action of these different narcotics on the intellectual functions. In the production of inebriation these functions are impaired and deranged; in sleep they are lulled or extinguished for a time; and in delirium they are excited and led astray." (p. 224.)

We question the propriety of separating narcotics into inebriants and soporifics. The first stage of narcotism, whether produced by wine, chloroform, or opium, is always marked by confusion of thought and intoxication, and the final result is in all the same, deep sleep and coma. Chloroform and Indian hemp, placed among inebriants, are most valuable soporifics. Chloroform is a general sedative, and is incorrectly placed with drugs whose primary action is stated to be stimulant. Tobacco, again, is neither stimulant nor soporific, but sedative.

It has long appeared to us a serious error to confound the action of stramonium, hyoscyamus, and belladonna, with narcotism. They are very different. Thus, opium causes sleep, contraction of the pupil, and checks secretion; while belladonna produces delirium, dilatation of the pupil, and promotes secretion. The first, in medicinal doses, has no very decided effect on the circulation, but, if anything, exalts it; the second manifestly depresses it, and also the nervous and muscular systems. On the whole, these solanaceous medicines are much more closely allied to sedatives, both in their action and uses, than to narcotics; while from other sedatives, as digitalis and aconite, they are best distinguished by their effect on the iris, which is at the same time characteristic and of considerable practical value.

Astringents and eliminatives are discussed under the ninth and tenth propositions. The fourth and best chapter treats of the "action of some of the more important medicines in particular." It concludes the work.

Our remarks have not been written from a wish to parade the author's shortcomings, but simply from a desire to enforce greater exactitude in the statement of fact, and closer reasoning in the treatment of a subject for whose solid progress we are most anxious. We have passed in silence many parts of the essay more open to unfavourable criticism than those noticed. The taurine-hypothesis of the action of bitter tonics; the chemical theory of the alleged remote refrigerant effect of vegetable acids; the attempt to establish the blood-nature of all inflammatory, arthritic, scorbutic, periodic, and spasmodic diseases; the hypotheses of special poisons in scurvy, scrofula, and psoriasis; of the oxidizing action of lemon-juice in rheumatism, &c., do not merit serious criticism. Such displays of therapeutical romancing are scarcely pardonable in a speculative chemist, and are inexcusable in a physician. Hypothesis is useful in medicine, as in the simpler sciences, but it must be used most sparingly, and with a caution corresponding to the subtle and intricate nature of the phenomena. When abused, as in the many crude speculations of the work under review, it tends greatly to generate error and scepticism.

We have already spoken favourably of the author's talents and industry, and we gladly repeat our conviction that, if properly directed, they are very capable of doing good service to medicine. But in future efforts at authorship, Mr. Headland must carefully avoid the errors of his present work, the general contents of which cannot be reconciled with the positive method of inquiry, exact expression, and sound reasoning, so justly and so emphatically insisted on in the introductory observations, and without rigorous attention to which we can never hope to give therapeutics a sure footing among the sciences, or to improve materially our skill in the treatment of disease, and so to strengthen its claim to public confidence as an art.

Alexander Fleming.

Review VIII.

1. *Traité Pratique des Maladies des Nouveaux-Nès et des Enfants à la Mamelle, précédé d'un precis sur l'Hygiène et l'Education physique des Jeunes Enfants.* Par E. Bouchut, Laureat de l'Institut de France, &c. Deuxième Edition.—*Paris*, 1852. 8vo, pp. 924.

A Practical Treatise on the Diseases of New-born Children, and of Infants at the Breast, &c. By E. Bouchut. Second Edition.

2. *Journal für Kinderkrankheiten.* Herausgegeben von Drn. Behrend und Hildebrand. Band xviii., Heft 5 und 6.—*Erlangen*, 1852. 8vo, pp. 153.

Journal for the Diseases of Children. Edited by Drs. Behrend and Hildebrand. Vol. xviii., Parts 5 & 6.

Seven years have now elapsed since M. Bouchut was introduced to the notice of the profession in this country by one of our predecessors,* and the opinion then pronounced upon his work was, that it was a good and an useful one, and could be safely recommended to those engaged in the investigation of the diseases of infancy. Since the above period, our author has not been idle, as his various excellent papers in the 'Gazette Medicale' bear witness, and in particular, his 'Treatise on the Signs of Death,' which received the approving selection of the Institute of France. Independent of these labours, however, the enlarged and improved condition of the work now before us would be sufficient reason for our bearing honourable testimony to the author's industry and talents. From a duodecimo of 600 pages, it has passed into an octavo of 900, and changed its title of 'Manual' for that of 'Treatise.' Many of the subjects treated of in the first edition have received considerable extension, and points previously omitted have been found in the present 'Treatise' a place. Congenital affections and "vices of conformation" are included, as are also many surgical diseases. The systematic arrangement of the contents of the work is greatly altered, several woodcuts inserted, more extensive facilities for reference afforded, and the book generally forms, in our opinion, a good example of what second editions should endeavour to be made. We recommend M. Bouchut's treatise in all confidence, and particularly to students acquainted with the French language; not that it can supersede the classical labours of MM. Rillet and Barthez, or the able compilation of M. Fabre, in the 'Bibliothèque du Medécin Praticien,' but as less voluminous, and more immediately adapted to their requisitions. To such, also, it will be but of slight importance, whether M. Bouchut always addresses them with the voice of his own experience, or very frequently shows himself but the exponent of that of M. Trousseau and of others we might name. The occasional incompleteness which we have noticed, also, of certain topics treated of (arising from an attempt at too great completeness), including some upon which the author evidently cannot boast of much personal experience, and concerning which he has not troubled himself to read very deeply, will scarcely be felt a drawback by the student and junior practitioner. These

* British and Foreign Medical Review, vol. xx. p. 357.

points being—as it is but fair to add to the above qualifications of our praise—points generally of lesser practical importance.

The first portion of M. Bouchut's treatise is, as in the former edition, occupied with the hygiène and physical education of the child from birth. Nearly one hundred pages are devoted to the subject, and although, as was remarked in the review of the first edition, the author shows himself very much indebted to the labours of M. Donné, his chapters are well worthy of attentive perusal. The section which treats of Lactation has received most addition, and embraces the results of some very recent investigations of MM. Becquerel and Vernois. The second part constitutes one of great importance to the practitioner, embracing as it does the "General Pathology of Infancy," and the symptoms of disease as afforded by physiognomical expression, and the general appearance &c. of the body. We quote the following from its first section:

"In a general way, we may affirm, without fear of erring, that the anatomic lesions of the diseases of early infancy are less decidedly inflammatory than those of older children and of adults; they are more fatal, it is true, but death is less often the result of the material derangements they produce than of the blow given to a feeble constitution by too great a dynamic reaction.

"In fact, if we carefully examine the anatomic lesions of a case of pneumonia, and compare them with those of acute pneumonia in the adult, searching, at the same time, in each instance, for the essential element of that which we are wont to designate *phlegmasia*, we shall find that we cannot establish a connexion in either case.

"Inflammation is without vigour at this tender age, has less power of plasticity; it is, if we may so express it, feeble, like the subject in which it is developed. Suppuration rarely follows; the matter deposited in the cellules of an organ remain there, for the power of absorption is diminished, and insufficient for their removal. If the child does not succumb, the disease often passes into the chronic state. . . .

"In the infant, as in the aged, the febrile reaction is not in exact ratio with the material lesion; in the former, it is acute, and seems to indicate a disorder somewhat considerable; whilst in the latter, it is weak, sometimes scarcely perceptible, notwithstanding the existence of very serious anatomic lesions. It is in the adult only where the scales are found to some extent equipoised, and in whom we may take the febrile reaction as a guide in judging of the material changes.

"This want of accordancy between the reaction and the lesion is one of the most curious phenomena of infantile pathology, and has, in my opinion, an important practical signification. Thus, for example, the increase of action which runs so high and so differently in the pneumonia of the child, in that of the adult, and in the pneumonia of the aged, at least bears witness once more to the truth of the principle, that these lesions being assumed of the same import, each patient, according to age or other circumstances, has a mode of undergoing them which constitutes such patient's autocratia. . . . . .

"The affections of early infancy differ then from the diseases of the adult in many respects. The facility of action in the causes producing them, the acute, often extreme reaction, which rapidly abates; the feeble plasticity of inflammation bestowing particular characters on the anatomic changes; the frequent remittency of the febrile state; the rapid progress of complications; the precipitous termination, whether towards cure, death, or the chronic state; in fine, everything entitles us to say, that the diseases of children at the breast exhibit the character of remarkable debility, which is in relation to the poverty of constitution in the patient." (pp. 102—104.)

Space will not permit us to dwell longer on this portion of M. Bouchut's treatise. We pass, therefore, to the third great division of the book, which treats of the special diseases of Infancy. Here considerable additional matter will be found, as well as many changes of arrangement. After some introductory observations on the separation of the remains of the umbilical cord, and on exfoliation of the epidermis, the diseases of the nervous system are entered on. In the first edition those of the alimentary apparatus were here discussed. In our opinion, the first five chapters of the second book of the present division are the weakest points in M. Bouchut's treatise; and had we not, in some recent articles, discussed the subject of Cephalhæmatoma, and since that 'Vices of Conformation,' connected with the Brain, &c., were fully entered upon in the review of Dr. Churchill,* we should otherwise have felt disposed to have mingled our commendations of M. Bouchut with some remarks of a less laudatory and more critical character. Under these circumstances, we shall pass to his chapter on Hydrorachitis. In this, a detailed account is given of a case in which M. Chassaignac lately and successfully operated on a child five months old, puncturing the tumour, and injecting into it a diluted solution of the tincture of iodine. In three weeks' time the disease was considered cured. M. Laborie's views respecting those specialities of the disease which indicate where operative interference may be resorted to, appear to have been well illustrated in the above instance. M. Bouchut's *resumé* of the matter is, that

—"all operations put in force, in cases of spina bifida, present great difficulties and great danger. They generally induce acute inflammation of the sac, and soon afterwards rachidian meningitis. It is the latter affection which proves fatal." (p. 181.)

Under "Facial Hemiplegia" an interesting case of M. Danyau's is alluded to, in which, after the use of the forceps during labour, the child was born with paralysis of the left side of the face and of the left arm. After death, which occurred in a few days, extravasated blood was found near the origin of the brachial plexus, and on the facial nerve, at its exit from the stylo-mastoid foramen. "Paralysis of the deltoid" is referred to as illustrated by the case given in M. Jacquemier's work on Obstetrics. Spasm of the glottis is described under the term "Phreno-glottism," the anatomic cause of death, in fatal cases, being confessed to be as yet unknown. In the chapter treating of this affection, a paper by M. Betz, of Tübingen, is quoted, concerning goitre in new-born children. This writer states it to be a rather frequent, but not generally known disorder. It occurs in children of good constitution, so that the enlargement of the gland is often regarded as a fold of the skin charged with fat; in other cases, no external swelling occurs. The affection consists in a hypertrophied and extra-vascular state of the thyroid, and is hereditary. The symptoms it gives rise to are connected with the function of respiration, and the power of sucking and swallowing. Local depletion, the use of emetics and of iodine, constitute the treatment advised by M. Betz. We may remark, that Bednar, in his recent work,† records ten cases in which the thyroid was found enlarged, in some instances even to three times its original size. He denies, however, the rectitude of associating with this lesion the existence of any such respiratory disturbances as have been included

* Vol. vi. p. 140.

† Die Krankheiten der Neugebornen und Säuglinge, &c. Dritter Theil, § 78.

under the term "thyroid asthma." Amongst the "aphorisms" terminating the ninth chapter, we meet with the following:—"Phreno-glottism followed by general convulsions constitutes a fatal malady." (p. 194.) That general convulsions form one of the worst complications, or sequences, of spasm of the glottis, we admit; but that such event is *always* of a fatal character, we know to the contrary.

Chorea is scarcely ever met with in children at the breast. Baron, after thirty years' experience at the "Enfants Trouvés," had not met with such a case. M. Bouchut refers to two examples, one recorded by M. Michaud as occurring in a child immediately at birth; the other, observed by M. Constant, in an infant four months old. In this country, we have been chiefly indebted to the labours of Badham, Kennedy, and West, for information upon what is usually termed by the continental writers "essential paralysis;" and the observations of the latter author may in particular be referred to.* In 1851, an able paper was published by M. Rilliet in the 'Gazette Médicale,' and republished in 1852 in the 'Journal für Kinderkrankheiten.' With the assistance of the latter, and his own experience, M. Bouchut gives a fair epitome of the subject, and ventures a peculiar opinion as to the nature of at least one chief class of cases. It is necessary for the reader to bear in mind, that under the terms "Essential paralysis of Children," writers on pædiatrics imply those instances in which there exists more or less complete loss of power, with or without loss of sensation, and assumed to be unproduced by appreciable lesions of the nervous centres. It is true that some forms of the affection follow or are accompanied by convulsive action; but in these cases the complication is accidental, or at any rate it is believed there is no proof of there having been produced, during the fit, such organic changes as can be regarded as the permanent cause of the paralytic condition. There are some, however, who demur to this latter opinion. All cases, also, of facial paralysis, mydriasis, &c., are left out of the above category. M. Bouchut's views are as follows:—the greater number of cases not preceded by "febrile convulsions," and when the part affected is painful, always depend upon a local affection of the muscular system. The disease is a "myogenic paralysis" in its nature—"*toute rhumatismale*"—its chief exciting cause being the application of cold.

"The myogenic paralysis of young infants is a severe affection. Whatever may be its origin, its result is an alteration of the nutritive functions of a portion of the muscular system, almost always followed by incurable deformity. Death never occurs. In its incomplete form it is more readily curable than when the paralysis is entire or complete." (p. 208.)

We have lately been paying close attention to this malady, and we feel inclined, from our present experience, to accede to *a limited* application of our author's views, but by no means to so absolute an one as he appears to enforce. We would remark, at the same time, that it appears to us M. Rilliet has placed scarcely a fair interpretation upon some views lately broached by M. Ozanam,† in respect to paralysis following eclampsia. We certainly do not regard M. Ozanam to have implied, that in all cases where the loss of power immediately follows general convulsions, meningeal

* Lectures, p. 145, second edition.

† Archives Générales de Medecine, Mars et Juin, 1850.

apoplexy has necessarily occurred; but simply to have sought to prove that such producing cause *has* occurred. Whether M. Ozanam's 7th and 9th cases substantiate even this opinion may of course be a question for debate.

In the present edition M. Bouchut substitutes "granular meningitis" for "tuberculous meningitis," because

"Microscopic analysis has demonstrated, in an incontestable manner, that these granulations of the serous membranes, and of the pia-mater, are only composed of fibro-plastic tissue, and not of tuberculous matter." (p. 235.)

This is a point which must be considered at present *sub judice*, and, so far as M. Bouchut's views are concerned, scarcely interferes with the correct opinion he holds of the essential nature of the malady.

"We often find, along with granular meningitis, masses of a tuberculous nature in the interior, or on the surface of the brain.

"Those who die from granular and tuberculous meningitis almost constantly present similar lesions in the other tissues. . . . . This is not the least important circumstance to consider in determining the nature of our present malady. The tuberculous cachexia reigns through the body." (p. 245.)

"We do not pretend to maintain that this general condition suffices for the development of granular meningitis. There must exist with it the concurrence of circumstances capable of giving rise to congestion or inflammatory attacks of the encephalic membranes. These causes, which would have had no effect upon a healthy and vigorous child, become the source of very severe cerebral disorder under the above circumstances." (p. 236.)

In reference to the treatment of this very fatal malady, we greatly differ from our author as to the "real efficaciousness" of "bleeding from the arm, the foot, or the jugular," even at the commencement of its second stage (p. 259). But we must pass to the fourth book, with strongly recommending its preceding seven chapters to the attention of the reader. We now enter upon the diseases of the respiratory organs. M. Bouchut continues to teach, with respect to the nature of croup, the same doctrines he advocated previously, and on which point, we must admit, he is supported by far the greater number of continental pathologists.

"The disease commences in the fauces before invading the larynx and the bronchia; it is a true diphtheritic angina which becomes transformed into croup. If we carefully examine the patient at the commencement of the disorder, we shall find this to be almost always the case. Nevertheless, some accurately observed, but very rare facts, prove that croup may break out at once in the larynx and bronchia. In these instances the first stage, such as we have described, is entirely wanting, and disturbances of the respiratory function first signalize the existence of the malady." (p. 305.)

We need scarcely say that most of the higher British authorities support the doctrine, that the general form of croup met with in these islands is the rare, exceptional form of M. Bouchut and his *compatriotes*. The existence of the diphtheritic form is not only admitted by us, but is known to be very prevalent under particular circumstances; but it is as a primary, idiopathic affection of the parts *beneath* the glottis, and not of the fauces, that most practitioners here consider the more general malady. Upon this subject we have so lately enlarged,* that we shall simply allude to the

* Vol. viii. p. 67.

fact of the probability of the above variance in opinion having its origin in the prevalence of certain geognostic, endemic, and epidemic influences, in either climate, which are sufficient to stamp the malady we both call croup with particular characters. Had our own experience always been derived from patients subject to such a combination of climatorial and epidemic influences as we have seen in operation during the past year, whilst an epidemic of scarlatina was prevailing along the low Surrey bank of the Thames, we might have felt somewhat disposed to agree with M. Bouchut's opinion. But the knowledge thus obtained has to be counterbalanced by equally trustworthy observation under very different circumstances. We do not deny that it may *possibly* be the case, that very early and minute investigation may hereafter authorize us to admit that some amount of redness, congestion, or of inflammation of the posterior fauces, &c., does in most cases precede the great outbreak of the laryngeal affection. But we cannot conceive that we shall be brought to allow of the general primary existence of the distinct, frequent, abundant, nay, often extreme, anginose exudation upon which so much stress is laid by the continental authorities. The subject of tracheotomy is fully discussed by our author, and representations given of the instruments employed by M. Trousseau. The concluding aphorism of the chapter is the following: "If croup has so far advanced as to be marked by fits of dreaded suffocation, or by apparently approaching death, tracheotomy should be immediately performed." (p. 332.)

In the present number of the 'Journal für Kinderkrankheiten' will be found a clinical lecture by M. Guersant, on the above subject. Formerly, both M. Trousseau and M. Guersant, following the practice of M. Bretteneau, strongly recommended the operation as affording means for sponging out the trachea and larynx with a solution of the nitrate of silver. Both gentlemen have now relinquished the practice, except under very particular circumstances, as they have found danger may arise, for—

"If the sponge is too fully loaded with the solution, the latter may flow down the bronchia, and cause serious accidents; so that we only employ it when the formation of the pseudo-membrane re-occurs, and can be removed only with the greatest difficulty. Often the wound itself must be cauterized, which becomes covered with false membrane a day or so after the operation. I always cauterize it at once, and so prevent this complication arising." (Jour. f. Kind. p. 447.)

In the same journal is an account of a case treated by Dr. Alley, of Boston, in the way recommended by Dr. Green, of New York—viz., applying a strong solution of the nitrate of silver to the larynx, &c., through the glottis. Much false membrane was thus brought away, or detached and afterwards ejected. The child (æt. 4) died.

"The post-mortem examination showed the lungs to be quite healthy, with the exception of the upper lobe of the left lung, which could not be inflated. From the rima of the glottis to the first bifurcation, false membrane was plainly perceptible, and the bronchial ramification leading to the left lung was filled with it. But it was interesting to observe that the course of the sponge along the trachea was easily recognisable by the false membrane being completely detached in this spot." (p. 451.)

On the subject of pneumonia we have twice dilated;* we shall therefore

* Vol. iii. p. 424; vol. vi. p. 152.

but remark that M. Bouchut still maintains the opinions he first broached in his inaugural dissertation, and repeated in the former edition of his work, and now conjoins with them the following critique upon his opponents, MM. Bailly and Legendre:

"These facts [inflatibility &c. of hepatized lung] have been contested by MM. Legendre, Bailly, and Barthez, who have often tried, with variable success, the insufflation of lung invaded by *lobular pneumonia*. They would, in consequence, separate pneumonia into two affections—one in which the lung cannot be inflated, constituting true pneumonia; and one in which insufflation is easy, the simple fœtal and congestional fœtal states of M. Legendre, but the lobular congestion and bronchio-pneumonia of M. Barthez. These writers, from thus taking the result of pulmonary insufflation as the chief foundation for a nosologic division, are found necessitated to place two or three new pulmonary affections between bronchitis and pneumonia, and to separately describe them; exactly as others would slip in a special pleuro-pneumonia between pneumonia and pleurisy. Endeavours of this kind, however, have always been unsuccessful, as such ought to be the case with regard to the opinions I am directly combating." (p. 366.)

"Pulmonary phthisis," unalluded to before, is treated of in the present edition. Some particular views are propounded as regards the minute anatomy of the disease; but as the author promises us a distinct treatise by M. Robin and himself upon the subject, we shall refrain from any comment now. The seventh book concerns diseases &c. of the heart. In it we find nothing that need detain us, except the notice of an interesting case communicated by M. Thibierge, and occurring in the Hôpital Saint-Antoine. It is an example of hypertrophy of the ventricles, communication between the four cavities of the heart, with displacement of the aorta, which opened into the two ventricles. The child was eight months old. Diseases of the mouth, stomach, and intestines follow, the account of which is able and complete. In the fourth chapter (p. 484) is related a case of "abscess of the posterior wall of the pharynx, in an infant four months old." The symptoms led to the belief that it was a case of croup, notwithstanding a tumour was felt by the finger when introduced down the throat. After death, an abscess as large as a hen's egg was discovered. The following remarks of M. Bouchut, relative to the practical value of the recent much-vaunted discovery of the "vegetable nature" of many diseases, are quite in our own way of thinking:

"As regards natural history, this discovery is very interesting, but in a medical point of view it has far less importance than might be supposed. What matters, after all, whether the disease we are occupied with [Muguet] be characterized by the presence of a cryptogam or of a false membrane? Is it the less a pathologic production, having its origin in a morbid state of the individual? Does it alter the character of the disease and its aggregate symptoms? Not a whit. The treatment is not even modified, for in therapeutics experience selects beforehand the means to be adopted, without awaiting the sanction of theoretic views." (p. 509.)

We shall pass over various matters well discussed by the author, to enter a little into detail upon the interesting subject of "intestinal hæmorrhage." It is a very unfrequent affection, often unnoticed in works on pædiatrics, and one upon which we have not hitherto dwelt. Slight discharge of blood (or what might be termed hæmorrhage) from the bowels is not uncommon during the course of inflammatory diarrhœa and entero-colitis in early childhood and in elder children. Hæmorrhage is also, as we remarked in our last article,* one of the diagnostic signs of invagination of the bowels, and is also met with in cases of "polypus of the rectum," and one or two other local affections. It is not to these forms of the disorder that we now particularly allude. It is to the more or less profuse evacuation of blood immediately or soon after birth in most instances, the intestinal discharge being sometimes accompanied with marked hæmatemesis, whilst in a few cases the latter is most evident, and the intestinal hæmorrhage but slight. M. Billard we believe to have been the first systematic writer treating specially upon the subject. He alludes to 15 cases, occurring from the first to the eighteenth day after birth, the majority being during the first week, 9 out of the 15 patients females, and the larger number of a full or plethoric habit. M. Billard was at a loss to indicate the source of the hæmorrhage; and although not expressing himself very determinately, it may be assumed from his observations that he considered the termination of the disorder as generally of a fatal character. In 1835, a paper was published in the 'Gazette Médicale,' by M. Rahn-Escher, in which cases were related to prove that the infant may recover from the intestinal hæmorrhage, even when combined with that from the stomach; that its source is the mucous membrane of the small intestines, particularly of the upper part; and that hereditary tendency is probably an important cause of its production. M. Barrier afterwards promulgated the opinion that the infants may have been born in a kind of apoplectic condition, or that a sort of congestional state continued for some time after birth. Sanguineous congestion, therefore, of the internal viscera and mucous membranes might be considered to exist, giving rise to the hæmorrhage, sometimes from the small, sometimes from the larger bowels, and at others from the mucous membrane of the stomach itself. In 1841, Kiwisch published a memoir on the 'Abdominal Apoplexy of New-born Children,' in which premature ligature of the cord is assumed to be an active cause of it; and in 1848, M. Rilliet produced the most complete history that we possess of the affection.† Besides the above writers, we might have alluded to several others who have detailed one or two cases; but suffice it to say, that Dr. West, in his Lectures, relates three examples—one occurring soon after birth, another between the second and third month, and the third at the tenth month. We ourselves have seen two cases in which, during the first month, some amount of blood has been ejected, both by the stomach and bowels, both patients recovering, but reduced to a severe condition of anæmia; and we have been favoured by Mr. Lovell, of Chelmsford, with the history of another, which we shall condense in the form of a note.‡

* No. xxi. page 129. † Gazette Médicale.

‡ Child, of the male sex, born on the 5th of April; labour quite natural; no inordinate pressure of the infant; the cord not cut or tied prematurely; and no signs of apoplectic stupor, asphyxia, purpura, or unnatural lividity of any kind. Child cried loudly when born, was vigorous and lively, and rather beyond the average size. On the fifth day after birth, the appetite failed, child became drowsy and inactive, obscure symptoms of general *malaise* continued to prevail, when, on the 15th of April, it suddenly appeared sinking, the surface becoming cold and clammy. A stimulant was given, which was soon afterwards ejected from the stomach with a considerable quantity of blood. The bowels were afterwards relieved, the motions containing blood, which, together with the hæmatemesis, continued to occur at intervals until the 17th of April, when the patient died.—*Post mortem:* "Stomach greatly distended with a dark reddish-brown thick fluid; œsophagus full of the same, so that on slight pressure it oozed from the mouth; mucous membrane of the stomach had a reddish blush, and was covered with a thick layer of blood and viscid mucus; colon with patches here and there apparently marked by portions of coagula escaped from the stomach. . . . Liver very large, hard, of a deep mahogany colour, and evidently loaded with blood."

We shall now refer to M. Bouchut. He alludes to three forms of intestinal hæmorrhage. One accompanying *purpura hæmorrhagica*, as illustrated by MM. Billard and Richard's (de Nancy) cases, and by one communicated to the author by M. Gubler. Another variety, depending upon a state of passive congestion of the intestines and transudation of blood into their cavity, resulting from inordinate compression of the fœtus during labour. A third form arising from the effects of acute inflammation of the bowels as early as four months after birth. After making allusion to a fatal case of simple chronic enteritis in a child nine months old, in which melæna occurred, M. Bouchut remarks—

"Thus we perceive there exist even in new-born children several varieties of intestinal hæmorrhage, which may be referred either to the accidental constitution of the patient, as in purpura (whatever may be the view we take of this affection), to a circumstance like compression of the cord or of the entire fœtus during the travail of parturition, to ulcerative action in acute or chronic inflammation of the bowels, or to invagination, polypi of the rectum, and anal fissures, as M. Trousseau has shown.

"The whole of the blood which escapes from the vessels does not, unfortunately, find an exit from the bowels; it may remain and accumulate, rendering the situation of the patient very perilous." (p. 603.)

A case, highly illustrative of the latter statement, is given by M. Bednàr.* On the eleventh day after birth, the boy's skin (then of a pale yellow colour) diminished in warmth, the impulse of the heart became dull and prolonged, and the respiratory murmur scarcely perceptible. The child lay almost motionless and slumbering. The day following, the surface could scarcely be kept warm, and the little patient had to be aroused to suck. On the twentieth day after birth it died. The brain was found to be anæmic, the lungs plethoric, whilst blood was effused into the duodenum and stomach.

"The blood may pass from the intestines by the rectum, or by the mouth from efforts to vomit. This, though rare, has nevertheless been observed, and is naturally explained by the seat of the hæmorrhage being the stomach, or the upper part of the alimentary canal." (p. 603.)

The hæmatemesis is not quite so rare as our author imagines; it may be said to be less frequent than the intestinal discharge, but often more copious when it does occur than the latter.

"It would be difficult in the present state of science to indicate the means of recognising the precise seat of an intestinal hæmorrhage. In this respect there is the same difficulty with the child as with the adult, and all the attempts as yet made to arrive at a safe result have been fruitless. . . . .

"The hæmorrhages of new-born children, arising from passive congestion of the intestine, are in general profuse, the blood passing from the bowels in a fluid state, and with its black colour. The flow is less in such cases as we have associated with purpura, whilst, again, it has been considerable in the hæmorrhage which we have seen coinciding with acute enteritis. Here, also, it had particular characters, and which are, perhaps, special to this variety. But on this point experience must decide. The blood was bright red in lieu of being black, as in the preceding varieties, and stained freely the linen. From the fact, also, that several days after the cessation of the hæmorrhage the child evacuated black matters, formed from half-digested blood, the hæmorrhage must have been considerable.

"The dangers resulting from these hæmorrhages in young infants are easy to

* Krankheiten der Neugebornen, &c. Erster Theile, p. 65.

foresee. The nature of their determining causes adds still further to their gravity, which is the greater the younger the child. Nevertheless, a cure is possible; but, as we have said, it is very rare, and one cannot exert too much endeavour to obtain it." (pp. 603, 604.)

We make a stride of thirty pages to lay before our readers some information which, we have no doubt, will be as startling to them as it was to us when we first met with it. In the first edition of his work (p. 549), M. Bouchut affirmed, respecting the jaundice of new-born children, that although it was incontestable that diseases of the liver may produce it, yet *elles n'existent presque jamais* in the patients in question. Now we are told that this latter statement is an error, and that "the jaundice of new-born children always results from an inflammatory affection (slight or severe) of the liver, which impedes the circulation of the bile, and determines its passage into the blood." (p. 638.) It is stated to be the sign chiefly of *acute* inflammation, and with which a third of the children born are affected (p. 638).

Now we are aware that Billard some years ago drew attention to the *passive state of congestion*, so frequently seen in the liver of new-born children, as also that he stated such a condition would, in most cases, only be known after death, as during life it afforded no special symptoms for its diagnosis. In Bednàr's late work, it is also laid down, that "it is impossible to diagnosticate hyperæmia of the liver, if it be uncomplicated with increase of size of the organ.* From Billard to Bednàr we have searched the various foreign works on pædiatrics we have at hand, and we must say, that neither in them, nor from our own experience, have we met with aught that could support M. Bouchut's remarkable statement. Of course our author has as much right to be original as anybody else, and it *may* be that he is as right as he is original. He will excuse us, however, if we await a little further investigation on this subject. With the next 150 pages we cannot tarry; they are pages, however, which will well repay perusal, particularly those on "intermittent fever." Upon "sclerema" (768) we have something to say. When it first became the duty of the writer of this article to apply himself to the study of the diseases of children, he had necessarily to make himself acquainted with the writings of the continental pathologists upon the subject of pædiatrics. He found in them certain diseases treated of pretty minutely, which, however frequently met with in the large hospitals abroad, were apparently uncommon in this country. But from the accounts given of them, he assumed that *when* meeting with these disorders at home, he would have no difficulty in recognising the similitude of their likeness, as he had previously become acquainted with it from books. With respect to one or two he found himself mistaken—e. g., with *sclerema.* Of this he has now seen three cases, two of them fatal ones. From it being a rare affection with us, and from these cases having occurred very lately, the writer carefully watched their character and progress, and although he could easily say, "these must be cases of some forms of the *sclerema* treated of by our foreign brethren," yet he was forced to acknowledge he had not as yet seen *the* form so many of them have described. If difficulties have occurred to us, not the less,

* Op. cit. Dritter Theil, p. 133.

as it appears, does M. Bouchut experience them; indeed, he expresses himself rather more strongly than we should wish to do.

"It is quite impossible, after having read what has been written upon the induration of the cellular tissue of new-born children (called also œdema and sclerema), to form a precise idea of this malady.

"Some assure us that the skin is livid, others, again, affirm that it is of a yellowish-white colour; and whilst some say that the limbs are as hard as if they had been frozen, others maintain it to be an error, and that they are soft, and retain on their surface the impression of the finger. There, we are told, it is a local disorder; here, that it is a general one; in fact, there is not a single point in the history of the affection which has not been argued in this controversial manner." (p. 769.)

M. Bouchut, judging from his own experience, gives the preference to M. Billard's description of this malady. He (M. Bouchut) describes two forms of it—simple and œdematous sclerema—thinks "it results from an obstacle to the circulation in the cutaneous capillaries" (p. 782), and that although so frequently complicated with certain pulmonary lesions, the latter "are rather the result than the cause of the malady." (p. 774.) We recommend M. Bouchut's chapter for perusal, warning our readers, however, that in it they do not get any exposition of the views of MM. Bailly and Legendre in respect to the connexion of "imperfect expansion of the lungs" with induration of the cellular tissue. Many writers strongly deny the identity between the osteo-malacia of adults and the rickets of children in any condition or stage of the two affections. Our author, on the contrary, lays it down that "rachitism and osteo-malacia form but one and the same malady modified by the age of the individual. Rachitism is the osteo-malacia of infants." (p. [illegible])

We pass over several chapters—many of a surgical character—to Book the XXVth, which treats of "Growth in connexion with the diseases of children" (p. [illegible]). This portion of our author's treatise contains much interesting matter, collected from various sources, and so condensed into [illegible] (in so small a compass) elsewhere. We [illegible] the following:

"[illegible] Nevertheless, there [illegible] the cause of a [illegible] we may ask if there does not [illegible]" (p. [illegible])

[illegible] cases. In respect to one [illegible] weeks it has been ailing;* [illegible]

[illegible] having no other disorder [illegible] as only ten teeth were [illegible] whether the "exagge-[illegible] was "en rapport" [illegible]

[illegible] that it would cause him [illegible] a child, and [illegible] Bouchut's posi-[illegible] concludes the

* [illegible]

volume, and if space permitted, we should draw attention to the instructive manner in which it is here treated, as also in the 7th chapter, which considers "the influence of the previous and present diseases of the nurse upon the health of the child." (p. 64.) We take leave of M. Bouchut with regret, whose "treatise," we are sure, we shall find an useful and very "handy" book.

We have already referred to one or two of the articles in the present number of the 'Journal für Kinderkrankheiten;' we may further draw attention to the useful practical papers contained in it, from the pens of Dr. Hauner (on cerebral affections), Dr. Meigs (on atelectasis), M. Rilliet (on tracheo-bronchitis and suffocative catarrh), and Dr. Marchant (de Charenton) on infantile asphyxia. From the latter we quote the following illustration, in answer to the question proposed by the author, as to how long asphyxia can continue without producing death?

"Dr. Grénet de Barbezieux relates, in the 'Press' of November 29, 1851, the following circumstance:—In the year 1844, I delivered a lady in Paris of a child presenting all the characters of five months' gestation. The skin was colourless, the infant motionless, shrivelled, and flabby, and evinced no signs of the establishment of respiration and circulation. Nevertheless I attempted its resuscitation, inflating the lungs and applying friction to the skin, but it was all in vain. After four hours, when preparations were made for burying the child, it was observed, with astonishment, that the skin had a blush upon it; fresh endeavours at resuscitation were had recourse to, and the child soon began to breathe. Death took place in forty-two hours." (p. 397.)

Dr. Marchant is therefore of opinion, that the present state of science does not warrant us in placing any definite limit to the prolongation of viability under the condition of asphyxia, and that we cannot be too cautious in coming to a conclusion as to whether a child be really dead or not. The grand resources of our art in such a case are, insufflation of the lungs and the maintenance of the warmth of the body.

W. Hughes Willshire.

Review IX.

Die Heilung und Verhütung des Cretinismus. Mittheilungen an die schweizerische naturforschende Gesellschaft. Von Dr. J. Guggenbühl.—1853. pp. 122.

The Cure and Prevention of Cretinism. By Dr. Guggenbühl.

The name of Dr. Guggenbühl is well known to many of our readers as that of the director of the establishment at Abendberg, near Interlachen, for the treatment of cretins. To those who are not acquainted with the labours of this gentleman (labours which have thrown no little light on the treatment, physical and mental, of idiots in general), a brief account of the Cretin Hospital may prove interesting.

Until the year 1839, the condition of the cretins was supposed to be irremediable. Whole villages of them lived and died in Switzerland, as now in many other parts of the old and new world, without the most sanguine physicians of the country ever dreaming that their condition was otherwise than hopeless. In that year, however, a young physician of Zurich, Dr. Guggenbühl, witnessed an incident which convinced him that even a cretin could be taught something. He saw a cretin muttering, without understanding it, a prayer beneath a crucifix. The prayer had been taught by the mother, and the words, though not the sense, had been remembered by the idiot. Convinced that with so much memory the mind could not be utterly extinct, Dr. Guggenbühl resolved to devote his life to the single purpose of restoring, as far as possible, the priceless gift of intelligence to these unfortunate beings. He first entered on a systematic study of cretinism, and convinced himself that mental without bodily improvement would be impossible. In 1842 he succeeded in buying the mountain of Abendberg, and with the aid of some benevolent individuals of Germany and England, has treated since that time a vast number of cretins. The work before us consists of a report read before the Swiss Society of Naturalists. The first part is occupied with a general history of cretinism, and with an account of the means of combating it. The latter and more interesting part refers more immediately to Abendberg, and to this we shall very briefly direct attention:

"The Abendberg (says Dr. Forbes, in his 'Physician's Holiday,') is one of the green barriers already noticed as inclosing the plain of Interlaken. It lies to the south-west of the village, its northern base abutting on the eastern extremity of the lake of Thun. Its elevation above the level of this lake and the plain of Interlaken may probably be 3500 English feet—that is, about 5300 feet above the sea-level. The Cretin-establishment of Dr. Guggenbühl is situated on the southern slope of this mountain, within probably a thousand feet of its summit; it took me exactly an hour and a half to reach it from the village, at a good walking pace. On emerging from the mule-path, which has also its termination here, I came at once upon a small open terrace, surmounted by a green slope stretching to a considerable distance up the mountain, and surrounded on all sides by the forest. It is on this small terrace, which looks like a step in the mountain, that the Cretin Hospital is built; and the green slopes above serve the double purpose of meadows for pasture and hay, and as an exercising ground for the patients. The spot is a remarkable one, and remarkably beautiful. It looks as if the wild forest had withdrawn itself on all sides purposely to open a spot for the abode of man, yet remained sufficiently nigh to defend and shelter it, leaving it only exposed to the south and east, where exposure is desirable. It can hardly fail to remind the traveller, when its green, sunny fields first greet him on emerging from the gloom of the forest, of some of those open yet sheltered solitudes which Spenser is so fond of introducing amid his forest scenes. In descending towards the house, I encountered midway, on the green slopes, some twenty of Dr. Guggenbühl's patients or pupils, climbing the hill for air, exercise, and amusement—all combined—under the superintendence of a well-dressed young man and two of the sisters of charity belonging to the establishment. They were all children, from the age of twelve or thereabouts down to three or four: one was carried by a servant, being incapable of walking. They were running and waddling and tumbling on the grass, and playing in their own way, with the servants, with one another, and with a fine, good-natured dog, which made one of the party, and which was probably of nearly the same intellectual caliber as some of his poor biped companions.

"The exhibition at once satisfied me of the enlightened character of Dr. Guggenbühl's views; and I felt much greater pleasure in thus observing and examining the poor objects of his benevolent care, amid their humble enjoyments, and as it were in Nature's own presence, than if I had seen them cooped up in a ward or schoolroom, under restrictions which they probably could neither understand nor well brook. They were all neatly and cleanly, but plainly dressed, and, like most individuals of the pitiable class to which they belong, were cheerful and apparently happy. The motherly care shown to them by the excellent sisters was delightful to witness. Dr. Guggenbühl justly considers cretinism as a physical malady, consisting in an imperfect development of most of the bodily organs, and of the brain in particular, on the imperfection of which latter organ all the mental incapacity depends. Whatever be the special cause of the affection, he concludes that it is only by improving the bodily health generally, by strengthening and improving—that is, developing to a higher degree of functional activity—all the organs of the system, and among the rest, and in an especial manner, the brain, that any rational hope of benefit can be founded. It was therefore a beautiful and most philosophical principle which he adopted as the indispensable basis of all his practice—that, namely, of having the infant cretin removed from the low, close valleys in which the malady generally originates, to the free, dry, cool, bracing air of the open yet comparatively sheltered and sunny slopes of the Abendberg." (pp. 109—111.)

The institution is both a school and an hospital. With a correct appreciation of the true principles of treatment, Dr. Guggenbühl brings to bear on the cretins every influence which can improve their general health and rouse their mental activity. As far as medicine is concerned, the general treatment is tonic, for it is found that the evacuating and lowering plan is hurtful, even in the hydrocephalic forms. The first great tonic is the pure and highly electrical mountain air; the second, a good but simple diet, into which milk largely enters; cod-liver oil, carbonate of iron, phosphate of lime, and the expressed juices of tussilago, leontodon, &c., are frequently employed. In epileptic cases, copper, oxide and valerianate of zinc, are also used. Iodine (except in the form of iodide of iron) appears to increase the weakness and atrophy; stimulants, such as valerian, arnica, and serpentaria, given with the intention of stimulating the nervous centres, are not beneficial, but phosphoric ether certainly lessens the torpor, and increases the power of learning. Wine causes congestion of the head, and is little used. Warm baths, douches, frictions with aromatic spirituous fluids, &c., are employed to stimulate and improve the functions of the skin, and electro-magnetism is used to stimulate the muscles in all cases with a tendency to paralysis. Dr. Guggenbühl, in his zeal for the cause, has tried mesmerism, but no cretin has as yet been thrown into sleep. Gymnastic exercises are of course an important item of education: the swinging-pole, the dynamometer, the triangle of Clias, and the American "baby-jumper," are the apparatus chiefly used. Afterwards, garden and field-work for the more robust prepare the children for their future occupations.

The body thus provided for, the cultivation of the senses simultaneously proceeds. The eyes of the cretin of the valley, dull, observant, and misty as they are, are naturally attracted by the magnificent mountain scenery of Abendberg; he is taught to look on the moon, the clouds, and the many-coloured glaciers; pictures cover the walls of his house; he learns his letters in a dark room, where the alphabet burns in phosphorus. His taste is excited and cultivated by strongly-tasting substances, such as quassia, salt, sugar, and vinegar. The sense of smell is stimulated in the same way by various odours. But it is through the inlet of the ear that the most powerful influences are poured into the mind, for the hearing of a cretin is often acute when his other senses are blunted or dormant. The human voice, the tones of the organ, and music of various kinds, arise around many times a day, and as soon as it is possible, every cretin is taught to take his share in the harmony. In this way, the great feeders of the mind, the senses, are called into action, and the mind itself is soon ready for the seed, which floats with it in the most natural way. Occupied in his garden, the cretin learns to note the growth of the plants he manures and waters, to observe the effect of his labours, and amidst the maze, to dimly discover a plan. Then his curiosity and interest (for these powerful agents of cultivation are by this time roused) are excited by a few striking physical experiments, until at length he is able to fix his attention, and to regulate the direction of his mind. Then he is ready for the more formal instruction of the school; soon he is taught to read and write, and from amidst the confusion and chaos of a mind diseased, arise the harmonious proportions of an intelligent soul.

The success of this elaborate and careful training is wonderful; some cases, indeed, are rebel and intractable, but the majority improve greatly both in mind and body. In the report before us are many examples of the fact, which our space obliges us reluctantly to omit.

We need scarcely remark that this result has by no means a merely local interest. Cretinism is not confined to the lower valleys of Switzerland, or to those other mountain districts of the old and new world which resemble it in physical conformation. All over Europe, more or less, the victims of this disease are found. Virchow, in his late official inquiry, found in the villages of Lower Franconia no less than 133 decided cases.* In other parts of Germany, in Sweden and Norway, in England, even in its very metropolis, isolated cases are met with.

The causes of this singular complaint are not very obvious. Dr. Guggenbühl ascribes it to a peculiar malaria which, in Switzerland, does not ascend more than 3000 feet above the level of the sea, and the operation of which is assisted by a wretched sanitary condition, by impure air, bad water, miserable dwellings, want of light and of good food, and by the pernicious practice of continual intermarriage. Virchow also inclines to the opinion that some special malaria is its cause. Whether or not the absence of iodine from the air has anything to do with it, future inquiry must determine. The observations of Chatin on this point are at present much too imperfect to warrant any conclusions.

Among the many noble works of philanthropy which, rather than the triumphs of an extended empire, or the spoils of an universal trade, form the true glory of this country, can we find anything more noble than this cretin seminary on the green heights above Interlachen? To have detected the capacity for improvement concealed in the distorted body and malformed head, was an evidence of no little wisdom, but to have formed the plan by which that improvement might best be brought about, and to have abandoned the easier paths of life for an office apparently so repulsive as that of a teacher of cretins, are proofs of uncommon genius and benevolence.

It is impossible not to draw a moral from this brief history of the establishment at Abendberg, and to wish that all the teachers of youth would recognise the wisdom of the mode of tuition there pursued. In just proportion the material and the mental are cultivated together. The body is nourished with the mind, and neither blunts the mind nor is exhausted by it. In its essentials, the education of the cretins should be

* Verhandl. der Phys.-Med. Ges. in Würzburg, Band iii. p. 247.

the education of all children, and if from so impoverished a soil Dr. Guggenbühl has drawn such fruits, what might not be expected if the bodies and minds of children amply blest with vigour and intelligence were developed with an equal care?

Our space compels us to take leave of this important work, but we trust that even these few observations may excite the interest of some among our readers who may possess the power of aiding Dr. Guggenbühl in his self-imposed task. He has already found potent aid from English pens, and the cause which has received the earnest support of Forbes and Conolly can scarcely want an advocate now. Still we cannot refrain from adding our tribute of sympathy and admiration for a man who has so nobly endeavoured to remove one of the greatest afflictions of humanity, and to restore to the poor cretin his birthright of intelligence.

REVIEW X.

Urinary Deposits—their Diagnosis, Pathology, and Therapeutical Indications. By GOLDING BIRD, A.M., M.D., F.R.S., Fellow of the Royal College of Physicians; Assistant Physician to, and Lecturer on Materia Medica and Therapeutics at, Guy's Hospital. Fourth Edition, Revised and Enlarged.—*London*, 1853.

No subject in the range of medical science has made greater progress within the last few years than that relating to the chemistry and pathology of the urine; this is partly owing to the rapid advances which have of late been made in organic chemistry, through the labours of Liebig, Mülder, Lehmann, Simon, Robin, and many others; and partly to the vast importance of the subject in a pathological point of view. One consideration alone will serve to bring home to the mind the value of a correct knowledge of the composition of the urine in health and disease.

The office of the kidney is to separate from the blood the various excrementitious substances contained in that fluid, a variety of salts, and many other bodies and compounds, the products of deranged or morbid action; and hence, the composition of the urine fully made out, affords an accurate and important indication of the many changes which are constantly going on, either in the system at large, or in particular organs.

While we are very desirous that the following review should be characterized by the strictest impartiality, we yet reserve to ourselves the right of freely and frankly commenting upon any portions of the work before us which appear to call for observation.

Although but little more than eight years have elapsed since the treatise by Dr. Golding Bird on urinary deposits was first published, it has already gone through no less than four editions; a fact, which, in itself, affords very strong evidence both of its merits and of the interest felt in the subject of which it treats.

The great characteristic of this work is, that it is eminently practical; it gives a clear and simple account, under separate heads, of each of the more important normal and abnormal constituents found in the urine, with instructions for treatment. To the student or practitioner, therefore, desirous of acquiring merely a general knowledge of the chemistry and pathology of the urine, and of the treatment of the more ordinary deviations from health, the work in question is an admirable guide.

This treatise, however, must not be regarded as of a complete and comprehensive character, for it does not attempt to embrace all the facts and observations connected with the urine which the united labours of many observers have made known to us. We notice many deficiencies in this respect; and the truth of the remark is acknowledged by Dr. Bird himself, in the following observation, which forms part of the preface to the present edition:

"If in any case I may appear not to have noticed some of the more recent contributions to the literature of the profession on the now popular subject of urinary pathology, it has not been from oversight or want of respect to their authors, but simply from their having been unsupported by my own observations."

The reason assigned by Dr. Bird for these omissions is evidently of a very unsatisfactory character. If an author is to be permitted to exclude all matter which does not happen to be supported by his own observations, he would, in a case like the present, have to reject a very large proportion of what is published on the subject; for in an extended and complex subject like that of the chemistry and pathology of the urine, no one observer can possibly verify for himself all the facts and observations brought to light by the efforts of many inquirers. The course adopted by Dr. Bird is neither fair to his readers nor just towards his contemporaries. The correct course to take is to notice all new contributions in their proper places: if erroneous, to refute them; and if not within the range of the author's own special observations, to admit them on the responsibility of their respective writers.

Nor has Dr. Bird himself strictly followed out the line of conduct which he has laid down, inasmuch as he treats of many things connected with the urine which could not possibly have fallen under his own immediate observation; but still he has acted upon it to such an extent as to expose himself to the charge of partiality, and also to deprive his work of that completeness and comprehensiveness which it ought to possess.

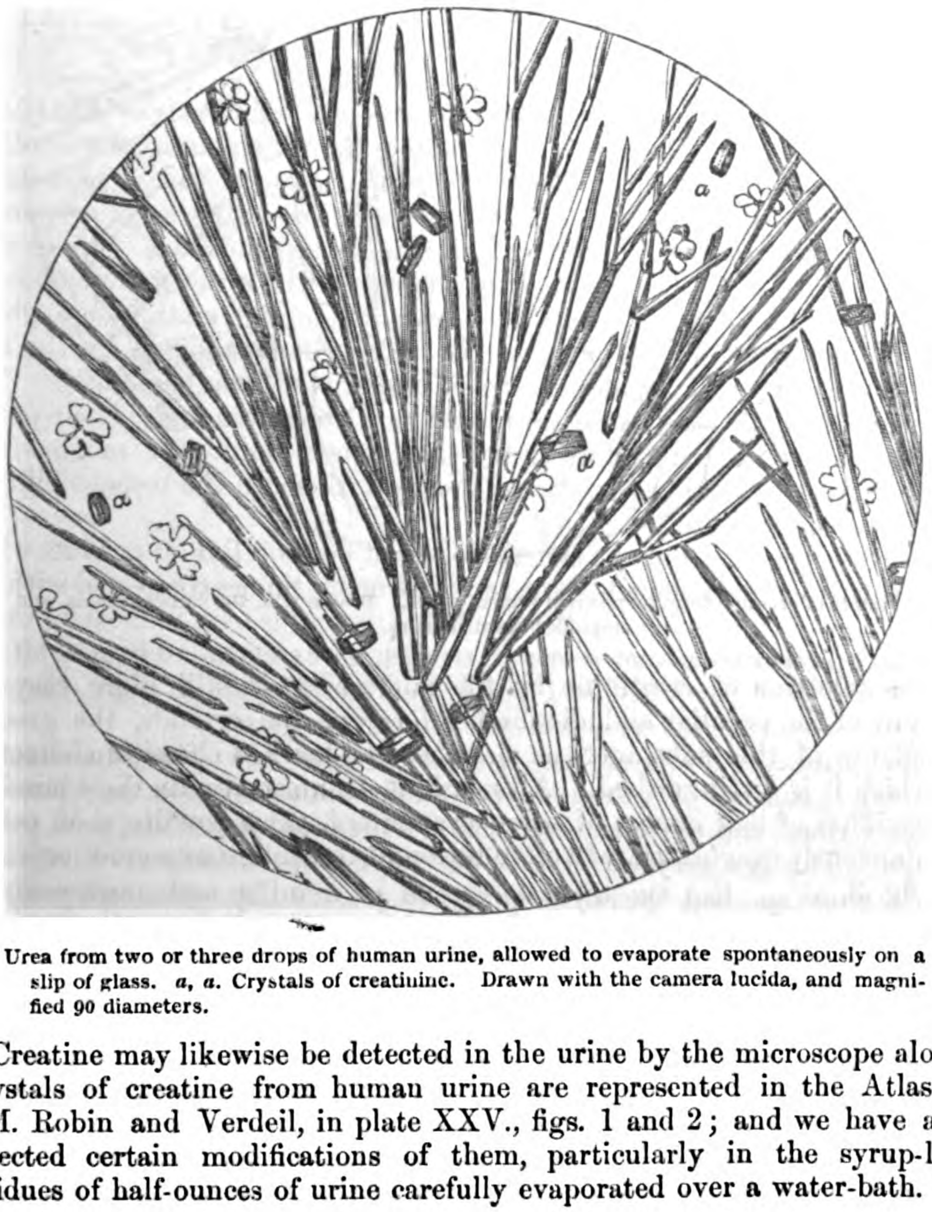
The present edition of the work is divided into fourteen chapters. Chapter I. consists of "Preliminary Details connected with the Chemistry of the Urine," and contains directions for the detection of its chief constituents. This is a very useful and important chapter, and would have been much more so, had the directions been of a fuller and more complete description. To have done justice to this subject, the chapter ought to have been as long again.

The details with respect to the mode of detecting and obtaining creatine and creatinine especially are very imperfect, a short process only being given for the formation of a compound of zinc and chlorine, with creatine and creatinine. No directions are furnished in this place for procuring these important constituents of the urine in a separate form. But it is mainly by the absence of full microscopical instructions that the chapter is rendered so defective. Nothing is said with respect to the microscopical characters of urea, creatine, or creatinine, in the urine; although these are in many cases so well defined that their detection by means of the microscope becomes extremely easy.

It was long since pointed out by the reviewer,* that urea almost constantly crystallizes when urine is evaporated, presenting several well-marked modifications in the character and form of its crystallization. If containing urea in large quantities, a few drops only of urine, allowed to evaporate spontaneously, leave a considerable satin-like crystalline crust upon the glass, which, viewed under the microscope, presents a beautiful radiated arrangement, resulting from the union of numerous more or less elongated and linear crystals. See fig. 1.

UREA.

Fig. 1.



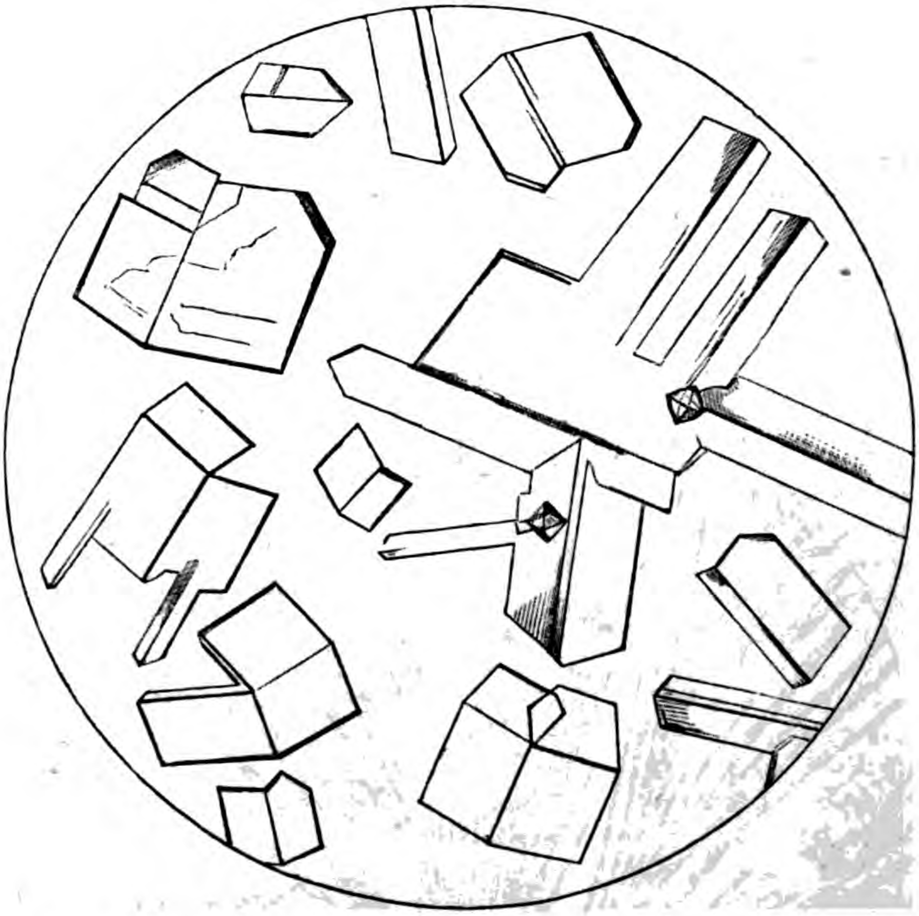
Urea from two or three drops of human urine, allowed to evaporate spontaneously on a slip of glass. *a, a.* Crystals of creatinine. Drawn with the camera lucida, and magnified 90 diameters.

Creatine may likewise be detected in the urine by the microscope alone. Crystals of creatine from human urine are represented in the Atlas of MM. Robin and Verdeil, in plate XXV., figs. 1 and 2; and we have also detected certain modifications of them, particularly in the syrup-like residues of half-ounces of urine carefully evaporated over a water-bath.

* Lancet, February, 1850.

CREATINE.

Fig. 2



Crystals of creatine from inspissated human urine. Drawn with the camera lucida, and magnified 100 diameters.

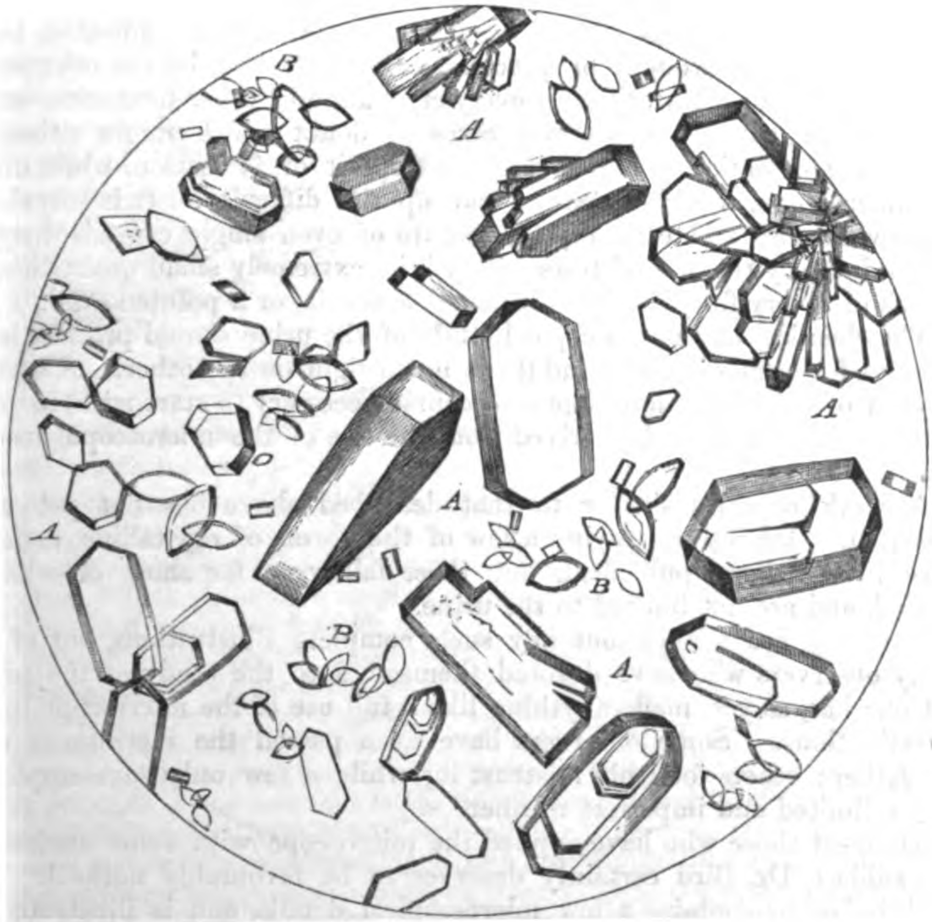
The detection of creatinine by the microscope is still more easy, on account of the peculiar and distinctive form of the crystals, the greater abundance of this substance, as also the well-marked chemical characters by which it is distinguished. Although it is most readily discovered in the residues of half ounces of urine evaporated, yet a few drops of urine, spontaneously evaporated, will often be found to contain numerous crystals. See fig. 3.

It may be of use, in this place, to offer a few general observations on the application of the microscope to the study of the urine.

A knowledge of the composition of the urine may be arrived at by two means, which may be employed either separately or in combination, as may be required: the one chemical, the other microscopical. The latter, in many cases, possesses great advantages over the former, particularly in the rapidity and simplicity with which results may be obtained by it. So long as it is necessary to institute complicated and tedious chemical analyses, in order to acquire a knowledge of the composition of the urine, the study of that fluid cannot be generally pursued by those who are

CREATININE.

Fig. 3.



Creatinine from a few drops of human urine, allowed to evaporate spontaneously on a slip of glass. *A, A.* Crystals of creatinine; *B, B.* Crystals of uric acid. Drawn with the camera lucida, and magnified 100 diameters.

actively engaged in the treatment of disease, and who, consequently, ought to possess an accurate acquaintance with the subject; it must still continue, as at present, to be for the most part confined to a few individuals who devote themselves specially to it. When, however, the application of the microscope to this subject is fully and rightly made and understood, this will no longer be the case, and every medical practitioner will be able to determine for himself, in a few minutes, by means of that instrument (aided in some cases by reagents), the nature of the chief constituents, normal and abnormal, contained in any urine.

This correct and extensive application of the microscope has, however, yet, to a great extent, to be made; and the difficulties in the way of such an application are, in some respects, very great, but by no means insuperable. For this purpose it is requisite that a large number of figures be prepared, accompanied with full descriptions. These figures should be most carefully outlined with the camera lucida, coloured after nature, and drawn, as far as practicable, to a uniform scale. They should exhibit every chief variety and modification in the form, size, and colour, of not merely those salts and substances which subside as deposits, but also those which assume a crystalline form, and so become revealed on the evaporation of the urine. A comprehensive atlas of the description indicated being once formed, the student or practitioner would then, by the microscope alone, be able to identify nearly every crystalline or other formation occurring in the urine. In the few cases of doubt which might arise, the judicious application of reagents to the deposit or formation, while under the microscope, would speedily clear up the difficulty. It is surprising with what ease and certainty the nature of even single crystals may be determined by the use of tests applied, in extremely small quantities, by means of a very fine camel's hair pencil, a needle, or a pointed quill.

The chemical and microscopical study of the urine should proceed hand in hand, for each will assist and throw much light on the other. A certain amount of chemical knowledge is of course necessary to start with, in order that the full benefit to be derived from the use of the microscope may be secured.

A work or atlas similar to that described above has not yet been executed. Atlases portraying a few of the forms of crystalline deposits have indeed been published, but these fall very far short of what is needed, and are not limited to the urine.

Not only are we without any such complete illustrations, but of the many observers who have devoted themselves to the study of the urine, not one has, as yet, made anything like a full use of the microscope in his investigations. Some observers have even passed the instrument over altogether; others foolishly mistrust it; while a few only have employed it in a limited and imperfect manner.

Amongst those who have applied the microscope with some success to this subject, Dr. Bird certainly deserves to be favourably noticed. The work before us contains a few microscopical details, and is illustrated by sixty-one small woodcuts; a number, however, which is wholly insufficient to convey any idea of the great variety in the characters of the crystalline formations which are found in the urine. Moreover, these illustrations are rough diagrams, rather than complete and artistic drawings.

The want of complete and instructive figures, executed with every advantage of form and colour that can be obtained, has often forcibly struck us, and in the hope of one day or other being able to supply this desideratum, we have been for some years engaged in having accurate illustrations made of all the chief modifications of the various crystalline and other deposits met with in the urine. The number already executed amounts to about two hundred and fifty, and it is calculated that nearly one hundred more will be required to make up the series.

We have dwelt thus long on the application of the microscope to the study of the urine, because we are satisfied that its importance is in general by no means fully appreciated. The microscope should be used in the examination of every urine; not only should the urine be examined when it contains a visible deposit, but even when there is no apparent precipitate; since the deposit may be too small to be seen by the naked eye, or the crystals themselves forming it may be exceedingly minute.

Not only should the urine at the bottom of the vessel be examined, but the surface of the fluid should be carefully looked to, as well as the sides of the glass in which it is contained. Moreover, the urine should be kept for some days, and submitted to microscopical examination from time to time, as precipitates and other formations or developments frequently present themselves, even many days after it has been passed. In this manner much valuable information, procurable in no other way, is constantly obtained.

Chapter II. is devoted to the subject of the "Physiological Origin and Physical Properties of the Urine." It treats of the metamorphosis of tissue, the density, colour, and consistence of the urine, with the amount of solids contained in it. The subject of the metamorphosis of tissue is ingeniously discussed and considered. It should be understood that the indications afforded by the tables given by Dr. Bird, calculated from the formula of Dr. Christison, and professing to show the amount of solids contained in any urine according to its specific gravity, are correct only within certain limits; this is well shown by Dr. Bence Jones, in the remarks which follow:

"Tables have been constructed, professing to tell how much solid matter is contained in urine of any specific gravity. It is said, that by taking the specific gravity, and referring to the table, the quantity of solid matter may be immediately determined. If the urine were simply a solution of one substance—as, for example, urea in distilled water—such tables could be made to give the truth; but when many different substances are dissolved in water, no tables can be trustworthy. A small quantity of one substance may increase the bulk of the urine more than a larger quantity of another substance; or equal quantities of different substances may increase the bulk of equal quantities of water in which they are dissolved to a very different degree; so that the solid residue in each might be equal, while the specific gravities of the solutions might be different. Experiment proves this in the case of the urine. There is no short road to any accurate results. The acid urine must be carefully evaporated, at a very low temperature, in the vacuum of the air-pump, over sulphuric acid, until, on being weighed and reweighed, it ceases to lose weight. If the urine be not acid, the result will be worthless. The following experiments were thus made on the specific gravity and solid residue of the urine before and after dinner. About 500 grains of urine were in each case evaporated.

	Specific gravity.		Solid residue.			Per 1000 grains of urine.	
Before dinner . . .	1028·0	found	67·03	grains;	by table	65·2	grains.
After dinner . . .	1028·5	,,	65·59	,,	,,	58·5	,,
Before dinner . . .	1028·2	,,	64·77	,,	,,	65·0	,,
After dinner . . .	1034·3	,,	84·65	,,	,,	79·0	,,
Before dinner . . .	1024·7	,,	60·77	,,	,,	58·0	,,
After dinner . . .	1024·8	,,	64·61	,,	,,	58·0	,,
Before dinner . . .	1024·8	,,	56·67	,,	,,	58·0	,,

These experiments show that the quantity of solid contents cannot be determined by taking the specific gravity."

At page 46 is a useful table, by Mr. Ackland, of Hatton Garden, for reducing the indications of a glass urinometer to the standard temperature of 60° Fahrenheit. This is the only new matter contained in the second chapter.

The chapter concludes with an account of the optical properties of urine containing sugar. In the application of the polariscope to the detection of this substance in the urine, many difficulties have to be overcome; and then the test is of use only where a large quantity of sugar is present, in which case more simple and certain tests are at our command.

Chapter III. treats of the "Chemical Physiology of the Urine;" under which head the physiological origin of its more important constituents is considered. At page 80 the following remarks occur, on the decomposition of urea into carbonate of ammonia:

"The mere act of boiling the urine is sufficient to convert a portion of urea into an ammoniacal salt; and by long keeping even in close vessels a similar change occurs. The rapidity with which this conversion is effected varies remarkably in different specimens of urine. I have known urine become alkaline within an hour of its emission; and yet, in one instance, I detected urea in a specimen of urine which had been preserved in a closely-stopped bottle upwards of ten years. The presence of a mucoid body in a state of change, acting as a ferment, certainly explains the rapid conversion of urea into carbonate of ammonia in some urine."

In the 9th Lecture by Dr. Bence Jones, on Animal Chemistry, page 90, we meet with these observations on the transformation of urea:

"Pure urea may be kept dissolved in distilled water, or it may, as you see in this test-tube, even be boiled without being changed into carbonate of ammonia; but if a few drops of ammoniacal urine, or a small quantity of mucus, is added, decomposition begins. By careful experiments more may be made out on this subject than the general fact, that some substance in a state of change is requisite to cause the change in the urea to begin; and the influence of the monads and vibrios, which are sometimes found in acid urine, may be determined."

It will be perceived that the statements of Drs. Bird and Jones on this point are in direct opposition; and as it appeared one of importance, we some time since instituted certain experiments, with a view to determine, more fully and accurately than had yet been done, the conditions which regulate the conversion of urea into carbonate of ammonia. The principal conclusions arrived at were as follow:

"1st. The simple act of boiling an aqueous solution of urea is sufficient to determine the gradual dissolution of that substance, and its conversion into carbonate of ammonia.

"2nd. This conversion of urea takes place, after a time, in distilled water, even without the aid of the spirit-lamp.

"3rd. The decomposition of urea is effected, either with or without heat, much more readily in fluids which are alkaline, and especially in those the alkalinity of which arises from the presence of lime in any form.

"4th. The conversion of urea is retarded, and sometimes altogether prevented, by an acid condition of the fluid in which it is present; and this is equally the case whether the solution be subjected to the heat of the spirit-lamp or not. The more acid the fluid, the greater its power of resisting the decomposition of the urea.

"5th. Animal matter, in a state of decomposition, exercises a powerful influence over the transformation of urea; and this it does partly by producing an alkaline condition of the fluid in which the two substances are contained, the alkalinity being produced by the carbonate of ammonia generated during putrefaction." *

It is always desirable, when the statements of authors are opposed, as in the present case, to point out the differences, and to ascertain, if possible, which are correct. Conflicting statements, occurring in different

* Lancet, vol. i. June 12, 1852, p. 567.

works on the same subject, are calculated to puzzle and mislead the reader, who is not, in all cases, himself in a position to decide between them.

At pages 83 and 84, under "Uric Acid," we find these remarks:

"The deposits most frequently occurring in the urine on cooling, by evaporation in vacuo, or exposure to a freezing mixture, are, however, neither crystalline, nor composed of uric acid alone. They consist of urate of ammonia, sometimes mixed with urate of soda or lime, more or less contaminated with colouring matter; are amorphous, and readily dissolve in warm water, which scarcely acts on uric acid. We are hence compelled to seek for another explanation of the proximate formation of these deposits; and this, I believe, is found in the action of uric acid on the microcosmic salt, or double phosphate of soda and ammonia, which salt, or its elements, may be regarded as a constant constituent of healthy urine. When uric acid is mixed with a warm solution of this triple phosphate, urate of ammonia is formed, and phosphoric acid evolved, either free or combined with a base, and forming an acid salt. This urate of ammonia is not decomposed on cooling, but is simply deposited in delicate microscopic needles, readily redissolving on the application of heat, if sufficient water is present. On the addition of urine to a hot solution of these minute needles, they are deposited on cooling, combined with the colouring matter of urine, completely amorphous, and presenting all the characters of the commonest forms of urinary deposits. If, after the separation of the urate of ammonia, a fresh quantity of uric acid be heated in the supernatant fluid, more of the ammoniacal salt is formed, up to a certain point, when phosphate of soda yields, and urate of soda is generated, which on cooling is decomposed in the manner already described.

"I therefore ventured, some time ago, to propose the following as a probable explanation of the mode in which uric acid exists in healthy urine. Uric acid, at the moment of separation from the blood, comes in contact with the double phosphate of soda and ammonia, derived from the food, forms urate of ammonia, evolving phosphoric acid, which thus produces the natural acid reaction of urine. If the whole bulk of the urine be to the urate of ammonia formed not less than about 2701 to 1, the secretion will, at the ordinary temperature of the air, remain clear; but if the bulk of fluid be less, an amorphous deposit of the urate will occur. On the other hand, if an excess of uric acid be separated by the kidneys, it will act on the phosphate of soda of the double salt, and hence, on cooling, the urine will deposit a crystalline sediment of acid sand, very probably mixed with amorphous urate of ammonia, the latter usually forming a layer above the crystals, which always sink to the bottom of the vessel."

These observations are ingenious, but certainly are not altogether correct. That uric acid is present in urine, in combination with a base, or bases, is certain; but that base does not consist, as Dr. Bird and some others suppose, almost exclusively of ammonia. Lehmann most distinctly asserts, that what is usually described as urate of ammonia is indeed urate of soda. We find, at page 214 of Lehmann's 'Physiological Chemistry,'* the following observations in relation to this point:

"The sediment which is deposited from acid urine in fever, and in almost all diseases accompanied with fever, has long been misunderstood in reference to its chemical composition. Originally, it was regarded as a precipitate of amorphous uric acid, and subsequently (and almost to the present time) it was regarded as urate of ammonia. It has, however, been fully demonstrated, both by myself† and Heintz,‡ that this sediment consists of urate of soda, mixed with very small quantities of urate of lime and urate of ammonia.

* Translation, by Dr. Day. † Jahresber., d. Phys. Ch., 1844, § 26.
‡ Müller's Archiv., 1845, pp. 230—261.

"It would be both superfluous and wearisome to recapitulate the arguments adduced by Becquerel, myself, and Heintz, against the opinion of Bird, who maintains that this sediment is always urate of ammonia, as the actual nature of the deposit has been so completely established. I will here only remark, that as I long ago found, and as Liebig has since confirmed, scarcely any ammonia occurs in urine, and that according to the direct analysis of the sediment made by Heintz, scarcely 1$\frac{0}{0}$ of ammonia could be found in it."

Farther on, at page 216, Lehmann proceeds to observe:

"Even in alkaline urine it is very seldom that urate of ammonia occurs as a sediment: in these cases it is found in white opake granules, which, as has been already stated, when seen under the microscope, appear as dark globules, studded with a few acicular crystals. It scarcely ever occurs except in urine which, by long exposure to the air, has undergone the alkaline fermentation. Even in the alkaline urine of patients with paralysis of the bladder, dependent upon spinal disease, it is very rarely that I have found these clusters of urate of ammonia. In the alkaline urine that is sometimes passed, in other conditions of the system, it is never found."

It is of much importance that the chemical constitution of the urates occurring in the urine should be correctly determined, and considerable difference of opinion being still entertained on this point, we have instituted certain qualitative and quantitative analyses of different specimens.

Analysis of First Specimen.

Colour, bright rose-red. Under the microscope it was found to consist of minute amorphous particles, which dissolved in warm water, and re-appeared on cooling in their original state. Acetic acid slowly developed a multitude of rhombic crystals of uric acid.

The filter containing the deposit was treated with half an ounce of cold water in three portions. This removed a quantity of urea and a little of the urate. It was then drenched with boiling water, and the filtered liquid allowed to stand for twenty-four hours. The deposit was collected and dried at 70° Fahr. It weighed three grains.

One grain was dried at 212°, by which means it lost 0·07 of moisture. This was afterwards incinerated, and it furnished 0·092 of a white ash, which was very alkaline to turmeric paper, though not permanently so; it was infusible before the blowpipe, but tinged the flame of a violet white colour. When dissolved in acetic acid, and tested with oxalate of ammonia, it gave a precipitate of oxalate of lime.

One grain was distilled with two drachms of weak potash, and it gave an alkaline liquid which contained 0·012 of ammonia. While boiling, the potash solution acquired a bluish green colour, showing the presence of uramile or murexide, substances which doubtless gave the rose-red tint to the precipitate.

One grain was treated with one drachm of weak acetic acid, by which means 0·66 of nearly colourless uric acid were obtained; the acetic solution gave a copious precipitate with oxalate of ammonia.

These results prove that the precipitate consisted of bi-urate of lime, with a little bi-urate of ammonia, and a still smaller quantity of bi-urate of potash, together with colouring matter. The following is the per-centage composition:

Bi-urate of lime	61
„ ammonia	13
„ potash	traces.
Moisture	19
Pink colouring matter (uramile or murexide)	07
	100

Analysis of Second Specimen.

The colour was at first of a bright rose-red, but after washing with cold water it lost a great deal of its colour, and became of a foxy or yellowish red tint.

In its treatment with reagents it gave the same reactions as the last, and furnished very nearly the same proportions of the several products. Its per-centage composition was,

Bi-urate of lime	70
„ ammonia	9
Moisture	16
Colouring matter	5
	100

The solutions from which the preceding were deposited, after cooling, were evaporated to dryness at a temperature of 100° Fahr., and on examination they were found to contain a very small quantity of urea, together with bi-urate of lime and a little bi-urate of ammonia, but *no urate of soda.*

Analysis of Third Specimen.

The urate, as it appeared when diffused through the urine, which was of a deep brown colour, was of a pale, fawn-tint; but when collected on the filter it was of a light rose pink. After some hours this colour disappeared, and the urate became of a greenish hue, to the eye resembling somewhat purulent matter. On submitting a portion of the urate in this state to the microscope, it was seen that it had lost its usual granular form, and had become aggregated into small, globular, crystalline masses, of a very pale colour and deliquescent appearance. The alteration of colour observed was no doubt owing to this change in the form of the urate.

The ash of the incinerated urate amounted to about ten per cent.; it gave a permanent stain to turmeric paper, was soluble in water, and tinged the flame of the blowpipe of a violet colour, showing that it was composed chiefly of potash. 100 parts gave

Bi-urate of potash	57·12
„ lime	18·37
„ ammonia	10·06
Moisture	11·74
Colouring matter and loss	2·71
	100·00

Analysis of Fourth Specimen.

This was of a bright deep pink colour, and was obtained from a urine having a specific gravity of 1024, which, on the evaporation of a few drops on a slip of glass, gave a crystalline crust of urea. It was treated with a large quantity of alcohol, by which a great deal of urea and a very little amorphous pink matter were dissolved out. It was dried at a temperature of 70° Fahr., and its density taken, which was found to be 1100.

Ten grains were treated with half an ounce of cold water, and set aside for twenty-four hours: the water dissolved two grains, and became coloured of a pale, sherry-tint. When evaporated, it yielded a fawn-coloured deposit, which, on incineration, furnished 0·25 of a white ash, which was found to be lime.

Two grains were treated with two drachms of dilute acetic acid, which was allowed to act for twenty-four hours; by this means 1·2 grains of nearly white uric acid were obtained.

Two grains were dried for several hours over a steam-bath, and they lost 0·2 from escape of moisture. The remainder was then distilled with two drachms of weak liquor potassæ, and the distillate contained 0·04 of free ammonia.

Two grains were incinerated, and they furnished 0·2 of white ash: this was found to be potash.

These results show that the composition of the urate deposit was as follows:

Bi-urate of potash	42·0
„ lime	20·0
„ ammonia	19·5
Moisture	10·0
Colouring matter and loss	8·5
	100·0

Analysis of Fifth Specimen.

Colour, dull pink. The ash which resulted from the combustion of this amounted to 12·5 per cent.; it was strongly and permanently alkaline, and tinged the flame of a full yellow colour, from which it is manifest that it consisted in great part of soda. The acetic solution was made slightly turbid by oxalate of ammonia, which shows that it also contained lime; when distilled with potash, ammonia was detected. This specimen, therefore, consisted in great part of urate of soda, but it likewise contained a small quantity of urate of lime, and probably, also, of urate of ammonia.

In making analyses of urates, it is absolutely necessary that certain precautions should be observed. The best mode of proceeding is as follows:

The filtering paper used should be digested in acetic acid, in order to free it from lime or other salts which may be contained in it. The precipitate should be examined under the microscope, to ascertain whether it be free from oxalate of lime, uric acid, triple phosphate, or other deposits. It should be collected on the filter, well washed with proof spirit to remove urea and chlorides, and then dissolved in hot water, which should be poured upon the precipitate on the filter, stirring gently with a feather. Of course as the water becomes cold the urates are thrown down, when they may be collected and dried for analysis. If either triple phosphate or uric acid be present, the filter will retain these while the urates pass through; but if there be any oxalate of lime, there is danger that the crystals of this salt will pass through the filter, and so vitiate the analysis.

It has now been proved, that the deposits considered by Dr. Bird and others to consist principally of urate of ammonia, have a variable composition, being made up of the urates of lime, potash, and soda, with very small quantities only of ammonia. From the very great difficulty which is experienced in freeing these deposits entirely from urea, it may be questioned whether the ammonia is not derived, in some cases at least, from the decomposition of that substance. It is, therefore, an error to regard the urates as consisting ordinarily of urate of ammonia. Dr. Bird, as already noticed, is not singular in his opinion; as most other observers in this country have taken a similar view of their composition; for example, Dr. Bence Jones, and the late Dr. Prout. By medical men in general they are almost constantly spoken of as urate of ammonia.*

The omission previously referred to, with respect to a formula for the separation of creatine and creatinine from the urine, is supplied in this chapter, and Liebig's process for obtaining those substances is given.

The only new matter contained in Chapter III., is a short reference to a paper by Dr. Lionel Beale, on the "Chlorides in Pneumonia."

Chapter IV. is on the "Chemical Pathology of Uric Acid and its Combinations." The urates of the urine are treated throughout as consisting of urate of ammonia, although in one place Dr. Bird refers to the recent researches of Heintz, who, Dr. Bird states, has found that they always contain appreciable quantities of the urates of lime and soda, and often of magnesia and potash. "It by no means follows," writes Dr. Bird, "as assumed by Zimmermann, that this lime existed, combined with uric acid, as an admixture of a calcareous phosphate will produce the same reaction."

The results of Heintz's analyses of the urates are not correctly given by Dr. Bird; for that chemist found that they consisted chiefly of urate of soda, with very small quantities of the urates of lime, potash, and magnesia, and scarcely any ammonia. This conclusion of Heintz and Lehmann we have already shown to be correct, so far as the ammonia is concerned. The chief differences in the results of our investigations and those of Heintz and Lehmann are, that in many cases these deposits consist almost entirely either of urates of lime, potash, or soda, or of all these combined in various proportions.

The woodcuts illustrating modifications in the form of the crystals of uric acid, and the so-called urates of ammonia and soda, are the same as in the last edition of the work. The only additions to this chapter are notices of the artificial Vichy water of the Spa at Brighton, and of a secret remedy, known as "constitution water," which has acquired some celebrity in the treatment of calculous affections.

As no quantitative analysis is given by Dr. Bird of "Vichy water," and an incomplete one only of "constitution water," the following results of a chemical examination of these waters will not be without interest:

Results of the Analysis of a Sample of the Artificial Vichy Water of the Spa at Brighton. By Dr. Letheby.

The water is effervescent from free carbonic acid. It is alkaline to test paper; and it has a specific gravity of 1005. 2000 grains of it were

* It should be stated, that in the above analyses of the urates we have received great and valuable assistance from Dr. Letheby.

evaporated, and they yielded 9·88 grains of a very white saline residue, which consisted of—

Sulphuric acid	0·34
Chlorine	0·70
Carbonic acid	3·93
Soda	4·19
Potassa	traces
Lime	0·34
Magnesia	0·18
Silica and alumina	0·12
	9·88

These constituents may be arranged in two ways, thus:

	1.	2.
Bicarbonate of soda	10·14	9·50
„ „ magnesia	·58	·58
„ „ lime	·26	·87
Sulphate of lime	·58	—
Sulphate of soda	—	·60
Chloride of sodium	1·16	1·16
Silicate of alumina	0·12	0·12
	12·84	12·83

The first is the most probable arrangement of the elements; but the second form is introduced in order to show how closely this water resembles that from the wells of Vichy, examined by Henry in 1847. The following analyses are from his paper on the subject:

	1. Source Grande Grille.	2. Source Nouvelle.	3. Source Prè-Salé.	4. Nouvelle Source Célestines.
Bicarbonate of soda	9·80	9·68	9·40	8·27
„ „ lime	0·21	·18	·89	·55
„ „ magnesia	·13	·11	·81	·42
Sulphates of soda and potash	·97	·82	·52	·38
Silicate of alumina	·46	·46	·14	(?)
„ „ soda	·80	.68	·55	·28
Alkaline chlorides	1·08	1·00	·60	·76
	13·45	12·93	12·91	10·66

The water also resembles the alkaline springs of Cusset and Hauterine in France. In fact, there is but little difference in the composition of the alkaline waters of Vichy, Cusset, and Hauterine; and the artificial water from the Spa at Brighton is a very good imitation of all of them. It will be observed from these analyses, that the active ingredient in the whole of these waters is bicarbonate of soda. The dose of Vichy water is a small tumblerful two or three times a day, which equals, perhaps, about a pint —a quantity which contains 35·5 grains of bicarbonate of soda.

Results of the Analysis of a Sample of "Constitution Water."

Taste and feel soapy; specific gravity, 1031; very alkaline to turmeric, and reddened litmus paper; it effervesces briskly with acetic acid; gives a rather copious precipitate with nitrate of silver acidulated with nitric acid;

gives a small precipitate with nitrate of baryta and nitric acid, and with oxalate of ammonia; it shows the presence of traces of alumina and silica; 400 grains of the water left, after evaporation, 12·6 grains of a white solid residue, which does not char by heat, but fuses into a clear globular mass before the blowpipe, and gives a violet tinge to the flame. It therefore contains 3·2 per cent. of common or impure carbonate of potash, or pearl-ash, which is always contaminated with chlorides, sulphates, and silicates, with a little lime and alumina.

The dose of constitution water is the eighth part of a bottle about the size of a wine-bottle, four doses to be taken daily, this being equal to about 184 grains, or upwards of three drachms, of carbonate of potash. The price is 62*s.* per dozen; its actual cost, exclusive of bottles, is not nearly as many farthings.

Furnished with the above recipe or analysis, the admirers of "constitution water" may supply themselves with it at a very much cheaper rate than at 5*s.* 2*d.* per bottle. The remedy is indiscriminately recommended in all cases of "strangury, gravel, and stone." The only cases to which it is really applicable are those of uric-acid diathesis. In some forms of urinary deposit and stone it would be productive of incalculable mischief, and therefore it should on no account be taken excepting under medical advice. Persons are too apt to imagine that there is some hidden charm in these secret and empirical remedies; for the removal of this delusion no method is so effectual as the publication of their actual composition.

The whole of the observations contained in this chapter relating to the treatment of uric-acid diathesis are sound, practical, and of much value.

From the researches of Seguin and Anselmino it appears, that the amount of organic matter exhaled by the skin in the course of twenty-four hours is 107·47 grains. In commenting upon this, Dr. Bird writes, "It may be safely assumed, that when the skin is unable to perform its functions, the 107·47 grains of organic matter, which then lose their proper outlet, appear wholly or partly in the urine in the form of urate of ammonia."

It is, of course, a very rare circumstance for the whole skin to be incapable of performing in some degree its emunctory functions, and therefore the whole quantity of organic matter above referred to, would scarcely, in any case, be thrown upon the kidneys for elimination; neither could so large an amount of organic matter escape from the system wholly in the form of urate of ammonia, since the quantity of this urate contained in the urine is in general so small, and falls so very far short of what would be necessary, in order to bear out the remarks of Dr. Bird.

Chapters V., VI., VII., and VIII., are devoted to the consideration of "uric oxide, purpurine, cystine, and hippuric acid:" no additions have been made to any of these chapters; they remain as in the previous edition.

Chapter IX. is devoted to the consideration of oxalate and oxalurate (?) of lime.

This is the most original chapter in the work: Dr. Bird was the first to detect the frequent presence of oxalate of lime in the urine in a crystalline state, and to show its importance in a pathological point of view.

The views of Dr. Bird with respect to the pathology of oxalate of lime have met with a good deal of opposition, and it may therefore be well to bestow some consideration on this point, and to endeavour, from the facts already in our possession, to draw a definite conclusion. Dr. Bird, in the opening paragraphs of the chapter, thus refers to the diversity of opinion which exists as to the importance of deposits of oxalate of lime.*

"Oxalate of lime is so frequently present in the urine, is often a constituent of one of the most annoying forms of calculous concretions, and is generally so important in its pathological bearings, that it merits especial attention, and I am now particularly anxious to allude to the importance of carefully studying the relations of this form of deposit to certain states of health, because it seems now to run some risk of being tossed aside as a thing of no consequence. A curious revulsion of feeling seems to have taken place, amongst some at least, on this subject. When I first discovered oxalate of lime as a crystalline deposit, and announced its frequency, my observations were doubted by many, and whenever they were favoured by any attention, they were always distinctly stated to rest exclusively on my authority. Now that more extended observations have demonstrated the truth of my statements, we are told, both in this country and abroad, that oxalate of lime is of constant occurrence, and of no importance — a remark to which too many sufferers from this diathesis can give a melancholy denial."

Amongst those who have doubted the importance of the occurrence of oxalate of lime in the urine, may be mentioned more particularly, Lehmann and Bence Jones.

"Experience," observes Lehmann, "at the bed-side, teaches every unprejudiced observer that the appearance of oxalate of lime in the urine is by no means accompanied by the group of symptoms which certain English physicians describe as pertaining to what they call the oxalic diathesis."

And again, farther on, Lehmann remarks—

"It seems, moreover, unreasonable to set up such a diathesis, since the establishment of a special disease from a single symptom—that symptom being only the occurrence of oxalate of lime—is entirely opposed to the spirit of rational medicine."†

Dr. Bence Jones, in his seventh lecture, has the following remarks on the subject :

"Oxalate of lime is so frequently found in the urine of those who are in a good state of health, that I do not consider it as indicating any disease, but only a disorder of no serious importance. It scarcely indicates a more serious derangement of the general health than a deposit of urate of ammonia does. It may occasionally be found in the urine of all who lead sedentary lives, taking insufficient air and exercise, and more food than is requisite for the daily wants of the system. I have found it in the urine of those who are free from every complaint. Even in the urine of healthy children it may frequently be seen. I have met with it in every kind and stage of disease. In the fracture wards of St. George's Hospital I have very frequently found it. The most severe case I ever saw, was an artist, aged 30, dying of abdominal aneurism. In cases of indigestion, especially where flatulence occurs; in cases where no indigestion ever was felt; in skin-diseases; in cases where the skin never was affected; in cases of acute rheumatism, of acute gout, of fever; in sciatica in a gentleman 74 years old, with spermatorrhea; and in the diseases of women and children, octahedral crystals occur."‡

* Loc. cit. p. 207. † Translation, by Dr. Day. pp. 46, 47. ‡ Loc. cit. p. 63.

Because oxalate of lime is not unfrequently present in cases where no obvious or special disease exists, as we all know to be the case, it by no means follows, as a necessary consequence, that the occurrence of these deposits is not under any circumstances a matter of serious importance. Nothing is of more frequent occurrence than deposits of uric acid, the urates, triple phosphate, &c. ; yet it would scarcely be logical to conclude from that circumstance alone, overlooking a variety of other considerations, that these deposits were of no pathological importance. The detection of oxalate of lime in the urine, where no well-marked evidence of disease or derangement exists, shows simply we must not conclude, in all cases, that its presence is indicative of serious morbid derangement. It shows merely the necessity for carefully discriminating between those cases in which oxalate of lime is present from the nature of the food taken, and is merely temporary, and those in which it is connected with evident disordered function or disease, and in which also the deposit is persistent. Some of the constituents of the urine, the persistent presence of which in undue quantity is regarded by all as affording sufficient evidence of either disease or disordered action, are proper to healthy urine; now this can scarcely be said to be the case with oxalate of lime. Again, the condition of the urine, which, continuing for a lengthened period, ends in one of the worst forms of calculus,—viz., that of oxalic calculus,—cannot surely be regarded as a matter of no serious importance. Oxaluria, or the oxalic-acid diathesis, in the same way as are the other diatheses, is characterized by certain symptoms more or less peculiar, and these are well described by Dr. Golding Bird.

A few remarks upon certain modifications in the form of the crystals of oxalate of lime may next be offered. The ordinary form, as is well known, is that of the octahedron, but this presents certain peculiarities which have not been well described; thus the crystal is not an ordinary octahedron, with equal angles, but is a compressed or flattened octahedron. When the crystals are small and immersed in fluid, they lie, for the most part, with their summits more or less directed towards the observer (fig. 4, *A*); but when large, many of them frequently lie upon their sides, and then present the appearance of elongated rather than compressed octahedra (fig. 4, *B*), and as such they have been often described and figured. This appearance is, however, entirely deceptive, as is readily seen when the crystals are rolled over. The form of the octahedron of chloride of sodium is the very reverse; in place of being compressed, it is elongated, in consequence of which it always lies upon its side. This difference in shape affords a ready means of discriminating with the microscope alone these two salts from each other.

In another modification of the octahedron of oxalate of lime, the facets are excavated and the angles produced or thrown out a little, so that the crystals appear somewhat cruciform (fig. 4, *D*). This form of octahedron invariably occurs when oxalate of lime is artificially prepared by pouring a solution of oxalate of ammonia into lime-water.

A third, and one of the most remarkable varieties of the octahedron, is that in which the angles of the crystals run out, so as to form a four-rayed star (fig. 4, *C*); this variety is the largest and most beautiful of all the crystalline forms of oxalate of lime which have as yet occurred to the reviewer; it presented itself in the urine, together with other modifications, after a large dose of binoxalate of potash. There are still, however, other modifications of the octahedron. One of these consists of a quadrangular prism, terminated by the ordinary pyramidal summits (fig. 4, *E*); this variety, together with the preceding, was described by the reviewer in the 'Lancet' of Feb. 9th, 1850. The description there given of the prismatic variety is as follows:—"A second very distinct modification is that in which a short quadrangular prism is interposed between the quadrangular pyramids, of two of which each octahedron is composed."* The above form is sometimes further modified by the truncation of the pyramidal summits; indeed, the two modifications often occur in the same specimen. We have met with them in the urine both of the horse and of man.

The form of dumb-bell crystals of oxalate of lime is also not in general well understood or described. The enlargements forming the heads of the dumb-bells are not usually globular, but are flattened, so that the crystals seen on one surface appear narrow in the middle, and broad at each end; while in the side view they are altogether narrow, the neck even being, in some cases, the thickest part. Each dumb-bell formation, as was also pointed out in the communication above referred to, should therefore be studied under different views, front, side, and end views, otherwise no very clear or definite idea of its true shape can be obtained. In the same communication, the following modifications of dumb-bell crystals of oxalate of lime were thus briefly noticed:

"The dumb-bell form ordinarily met with is of a very regular shape and size, has a smooth and equal outline, is distinctly striated, and while its front surface is broad, its side is narrow and compressed, with a slight central enlargement, corresponding with the neck which joins the two halves of each dumb-bell together. (Fig. 4, *F.*)

"The first modification of the above typical form is of equal size with the former, is not flattened and compressed, but its side and end surfaces are nearly as broad as in the front view; the striæ are but just apparent. (Fig. 4, *G.*)

"A second variety, smaller than either of the above, has the side also nearly as broad as the front view; the neck of each dumb-bell is thick and protuberant, and the dilated extremities are furnished with short tail-like appendages. (Fig. 4, *H.*)

"A third form, rather smaller than the preceding, has the front surface of an oval shape; this surface appears excavated or indented, while the side surface is narrow, but presents an obvious dumb-bell outline. (Fig. 4, *I.*)

"A fourth variety is ellipsoidal, the dumb-bell formation being in many cases traceable within the ellipse; it is also in general of but small size. (Fig. 4, *K.*)

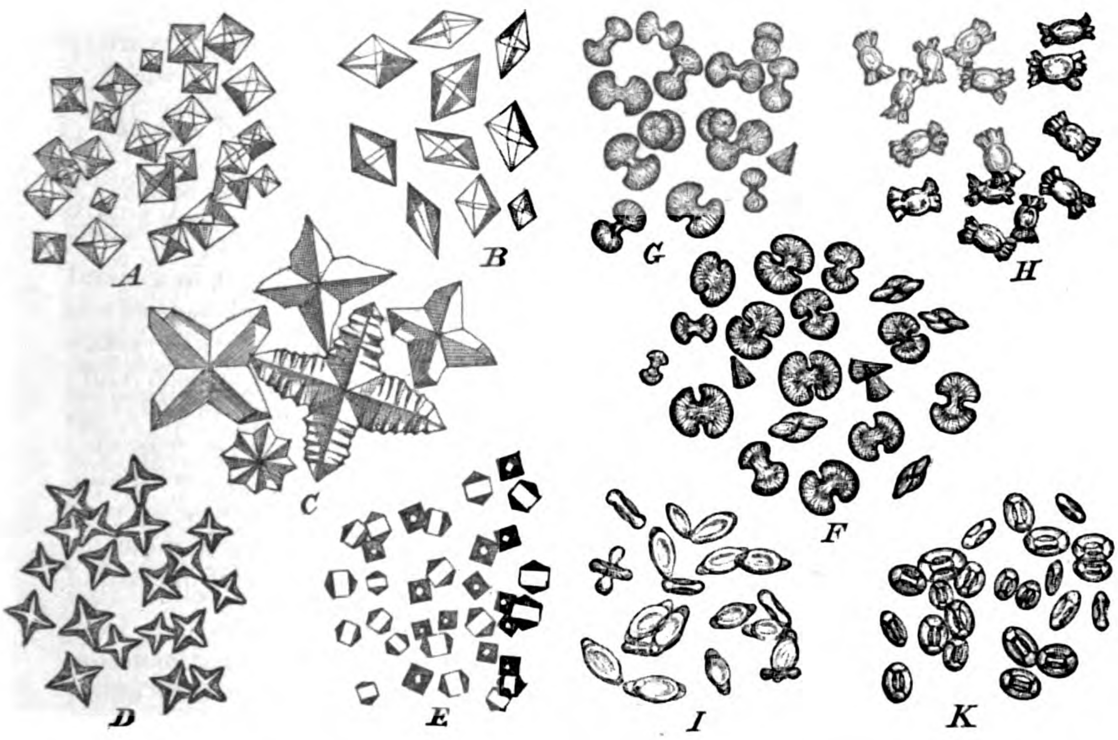
"This variety, which approaches very closely to the preceding, was first pointed out to me by Dr. Griffith, from whose preparation I have an accurate drawing; I have since observed it, presenting very nearly all the same characters." †

The modifications of the octahedron with the quadrangular prism, and also the oval variety of the dumb-bell, were figured for the first time in the third edition of 'Urinary Deposits,' published in 1851. Dr. Bird does not, however, refer to our previous descriptions of these varieties, nor to any of the other modifications then described. The whole of the above-noticed modifications are represented in the accompanying wood-engraving.

* Lancet, 1850, vol. i. p. 176. † Loc. cit. p. 176.

OXALATE OF LIME.

Fig. 4.



Modifications in the form of the crystals of oxalate of lime, all, with the exception of *D*, from human urine. *A*, Ordinary form of octahedra; *B*, Octahedra apparently elongated; *C*, Octahedra with the angles run out so as to form crosses and stars; *D*, Octahedra with the angles somewhat produced, formed artificially, magnified 420 diameters; *E*, Octahedra with a quadrangular prism interposed between the summits; *F*, Ordinary form of dumb-bell crystals, the crystals being flattened; *G*, Dumb-bells not flattened, having globular heads; *H*, Dumb-bells with tail-like appendages; *I*, Dumb-bells having a somewhat oval form, as seen on the front view; *K*, A somewhat similar modification of the dumb-bell crystals of oxalate of lime. Drawn with the camera lucida, and magnified 220 diameters.

Dr. Bird, as have other observers, refers to the fact, that oxalate of lime sometimes crystallizes in the urine after the lapse even of some hours, and this, also, where no crystals were present when first passed. This shows the necessity of not at once concluding, from the absence of crystals on the first examination, that oxalate of lime is not present. The most remarkable instance which we have met with, of the formation of these crystals after the urine was passed, is the following:—Two samples of urine, the one morning, and the other after-dinner urine, both being perfectly free from crystals of oxalate of lime, were set aside for some days; in the course of a week or so, a dense stratum of penicilium glaucum, in full fructification, formed on each urine; the underneath surface of this stratum, and the sides of the glass in which the urine was contained, was found to be covered with myriads of fine octahedral crystals of oxalate of lime, which adhered to and were entangled in the threads forming the thallus. Now this stratum must have taken, at the least, six or seven days for its growth, and consequently, nearly the same time must have elapsed before the crystals of oxalate of lime were formed.

At page 224 Dr. Bird refers to the occurrence of oxalate of ammonia, or of some other soluble oxalate, in urine containing oxalate of lime. This is a very important subject, and further investigations are much needed to determine the frequency with which, and the quantity in which, soluble oxalates are found in the urine. We, some time since, in connexion with the occurrence of certain dumb-bells, distinct from those of oxalate of lime, noticed the presence of soluble oxalates in the urine.*

Dr. Bird arrives at the conclusion, as the result of his examination of the urine in a large number of cases, that there is no necessary connexion between oxalate of lime and sugar; and that the occurrence of these together is to be regarded as the exception, and not the rule. While we agree with Dr. Bird in the opinion that no necessary connexion exists, and that the two substances occur in the majority of cases, quite independently of each other, we yet believe that in certain cases a relation between oxalate of lime and sugar does really exist Thus on several occasions we have detected sugar in small quantities, as well as oxalate of lime, in cases of extreme disorder of the digestive functions, and in which the presence of both might have been looked for from such disordered condition. Again, we have in some instances found both the dumb-bell and octahedral forms of oxalate of lime in cases of diabetes. We believe that the presence of sugar in the urine is very frequently overlooked, from reliance being placed chiefly upon the copper and potash tests, the action of which is very uncertain, and which will scarcely in any case afford evidence of the presence of sugar when contained in the urine in small quantities only.

Chapter X. treats of the "Chemical Pathology of the Earthy Salts."—This is one of the least perfect and satisfactory chapters in the whole book, no additions having been made to it since the last edition; the only alkaline and earthy phosphates described are the ammonio-phosphate of soda, phosphate of magnesia, and phosphate of lime.

The presence of ammonio-phosphate of soda, as a constant constituent of normal urine, is by no means well established; whereas, it has been clearly shown, that the neutral phosphate of soda and acid phosphate of soda are constant constituents of the healthy urine. These alkaline phosphates are not described or referred to by Dr. Bird. A very excellent account of these salts is given in the recent and voluminous work of MM. Robin and Verdeil: these authors give the following directions for obtaining the neutral phosphate from urine :†

"When we decant the fluid from highly concentrated urine, to separate the saline deposit and add to it absolute alcohol, there is deposited slowly upon the sides of the vessel, crystals of neutral phosphate of soda. These are plates derived from the rectangular or right rhombic prism with truncation of the edges.

"Sometimes these plates are irregular, and striated upon their surfaces, in different ways. They polarize light; the last forms, particularly, give colours most remarkable for their tint and intensity.

"It is easy, with a little practice, to distinguish these crystals from all other

* Lancet, 1850, vol. i. p. 177.

† Traité de Chim. Anatom. et Physiolog. vol. ii. p. 333.

kinds in the urine, and above all, from the acid phosphate, which we shall describe further on."

On the extraction of the acid phosphate of soda from urine MM. Robin and Verdeil have the following remarks :*

"The acid phosphate of soda may be obtained crystallized in the urine, by following the same course which we have indicated in treating of the neutral phosphate of soda. When this is crystallized, there is deposited, three or four days after, crystals much more soluble in water than the neutral salt; we may hasten the deposit by adding ether to the liquid already treated with absolute alcohol. These crystals, from the mode of truncation of their angles, or the sides of the base, appear to be derived from the rectangular or right rhombic prism. The truncation usually marks almost the whole of the base.

"The forms of these crystals vary but little, they are either prisms or plates. These crystals are very transparent, and their faces cannot be well observed, but as we see them turn upon themselves under the microscope, borne by a current of liquid. Moreover, as these crystals are generally deposited against the sides of the vessel, to which they adhere, it is rare to find them well formed; they are flat, and incompletely formed on the adherent side."

Figures are given showing the forms of the crystals of both these salts.

The varieties in the forms of the crystals of ammonio-magnesian phosphate are described by Dr. Bird, under the heads of prisms, stellæ, penniform crystals, and stellar and foliaceous crystals.

The penniform crystals described by Dr. Bird unquestionably consist, for the most part, of phosphate of lime, as was long since pointed out by the reviewer. This statement is founded upon an examination of the specimen from which the figure given by Dr. Bird was made, as well as upon repeated chemical analyses, both by Dr. Letheby and ourselves. The forms as well as the chemical characters of these crystals differ entirely from those of triple phosphate; viewed with a low power of the microscope, from being broad at one end and narrow at the other, they appear wedge-shaped; but under a higher power they are seen to be really six-sided prisms. Although they occur sometimes singly, they yet usually form more or less complete rosettes and stellæ, resulting from the union of several crystals, by their narrow, pointed extremities. They occur usually in urine which is somewhat acid, and not until after it has been voided some time: in several of the cases in which they have fallen under our own observation, the diabetic torula has become developed in the urine, showing the presence of sugar.†

Dr. Bird does not refer to the occurrence of phosphate of magnesia in the urine, except in combination with ammonia; nevertheless, there is no doubt that this salt does sometimes occur in a crystalline state, in a form and condition very different from that presented by any of the ordinary varieties of triple phosphate. It has been stated by MM. Robin and Verdeil to be commonly present in a crystalline condition in the urine of domestic rabbits, which owes its troubled aspect to this salt. We have met with phosphate of magnesia in human urine, alkaline from fixed alkali.‡

Neither does Dr. Bird describe phosphate of lime as occurring in the urine in a crystalline state, although it occasionally does so, as has been

* Loc. cit. vol. ii. p. 340.

† Proceedings of the Royal Medico-Chirurgical Society, Nov. 1852.

‡ Proceedings of the Medical Society, Jan. 1853; and also Lancet, April, 1853.

already shown. Moreover, MM. Robin and Verdeil describe an acid phosphate of lime differing from that which we have noticed, in the form of its crystals, as well as, probably, in its chemical characters.

"We have met with this principle," these authors state,* "in the urine of man, and in that of the dog. It is found in variable proportions, greater in the second than in the first, but we have not been able to determine accurately the quantity."

Farther on, they remark—

"The whole of the salt, of which we speak, contained in the urine, does not crystallize constantly. It forms on the surface of the liquid which one evaporates, an amorphous layer, which unites in groups the spherical blackish masses of urate of soda. When it crystallizes, which depends, no doubt, on the conditions of evaporation, it is always accompanied with this amorphous matter, which holds them united in groups with the urate of soda. The crystals have all the form of half-octahedra, elongated, being derived from the right prism with a rectangular base. Some are large, others very small. They may be isolated like the preceding, but the most part are united, one to the other, in different ways, either two and two, or in larger groups. They are soluble in acetic acid.

"In the dog especially, when the urine is very acid, we find a great quantity of these crystals, of the same size as the preceding. There exist, at the same time, many very small, either isolated or grouped, forms.

"We meet with, moreover, more frequently than in men, forms having the disposition of lamellæ, isolated or grouped in different manners, and forming very considerable masses; many of these lamellated groups present the very regular form of half-octahedra elongated.

"These crystals are colourless, transparent, their edges well marked; the dihedral angles are very sharp; they refract light but little; all these characters give them a hard aspect, almost mineral, which prevents them from being confounded with other principles."

The crystals of phosphate of magnesia, which we have described and figured, belong probably to neutral, or alkaline phosphate. We have reason to believe that the ordinary form in which phosphate of magnesia exists in the urine is that of acid phosphate, and that this, when the urine is evaporated, very readily crystallizes in prismatic triangular crystals. We once succeeded in obtaining them in sufficient quantity for a separate analysis; but before the actual composition of these crystals can be considered as finally determined, it will be proper that other analyses should be made.

The last earthy salt considered by Dr. Bird is carbonate of lime. The only remark we shall make in regard to this is, that it sometimes presents itself in the urine of the horse and rabbit in the form of dumb-bells. It is described and figured as occurring of the same form, by Dr. Otto Funke,† in human urine, who, in remarking upon them, somewhat strange to say, states that he has never seen crystals of dumb-bell oxalate of lime in human urine. The dumb-bells occur mixed up with the ordinary spherical striated masses.

Chapter XI., treating of "Deposits of Abnormal Blue and Black Colouring matters," remains the same as in former editions. The occurrence of these coloured deposits in urine is of the highest interest, and we believe that

* Loc. cit., tom. ii. pp. 303, 304.

† Atlas, der Phys. Chem., Table 1, fig. 2.

when they are rightly understood it will be found that they are no less important, both physiologically and pathologically.

The occurrence of indigo as a deposit in urine is by no means so rare as is ordinarily supposed. Hitherto but two or three well-authenticated cases have been recorded: we have records of several, the particulars of which will form the subject of a separate communication.

Chapter XII. is on "Non-crystalline Organic Deposits," as blood, hæmatosin, albumen, casts of tubuli, purulent and mucous urine, spermatic urine, torulæ, vibriones, sugar, kiestein, fat, urostealith, &c. This is a highly important chapter, but several of the subjects discussed are treated of much too briefly; it should have been divided into at least three or four other chapters. In some cases, sugar occurs in a semi-crystalline or granular state, and therefore it should certainly have been considered under a different head. The remarks upon the tests for albumen are complete and judicious.

After giving the characters of mucus and pus globules, Dr. Bird proceeds to describe two other forms of globules occasionally found in urine, and which he terms "organic globules," the characters of which are as follow:

The *large organic globule*, according to Dr. Bird, resembles the mucus particle or globule; being composed of a granular membrane investing a series of transparent nuclei, which become visible on the addition of acetic acid; two nuclei of a crescentic shape, with their concavities opposed, alone being seen in some of the globules.

"I know of no character," states Dr. Bird, "by which these bodies can be distinguished from mucus or pus, excepting that they are unaccompanied with the characteristic albuminous or glairy fluids in which the pus and mucus particles respectively float. The *large organic globules* seldom form a visible deposit, being free and floating in the urine, and are generally so scattered that not more than a dozen or two are visible at one time in the field of the microscope. They are abundant in the urine of pregnant women, especially in the latter months, when there is a frequent desire to empty the bladder. They have existed in every case of ardor urinæ I have examined, although irritability of bladder was not *necessarily present;* but when this does exist, they abound. The globule under consideration occurs in the greatest abundance in the albuminous urine of confirmed morbus Brightii: I have seen them so abundant as to cause a drop of the urine to resemble, when microscopically examined, diluted pus—a resemblance rendered more close by the albuminous character of the urine. Is it possible that these globules may here be indicative of sub-acute inflammatory action going on in the structure of the kidney? I am not aware whether they are quite identical with what have been termed the exudation or inflammatory globules of Glüge.

"In a most distressing class of cases, which occasionally occur in practice, where all the symptoms of stone in the bladder exist without any calculus being present, these globules are almost invariably present. This is more especially the case when a roughened state of the interior walls of the bladder can be detected by the sound. A more intractable and distressing ailment hardly exists." *

It will be observed that Dr. Bird gives no characters either as regards size, form, or structure, by which these bodies can be distinguished from either mucus or pus corpuscles; and hence it would appear that there is some danger of confounding them with either those of mucus or pus.

* Loc. cit., pp. 354, 355.

The *small organic globules* are described as being very beautiful microscopic objects, much smaller than pus or mucus particles, and essentially distinguished from them by the absolute smoothness of their exterior, no trace of granulation or nucleus being visible even with a high magnifying power. In hot acetic acid they are quite unchanged.

"These globules," according to Dr. Bird, "form a visible white deposit, resembling to the naked eye a sediment of oxalate of lime.

"So rare are these curious little bodies, that comparatively few examples of them have occurred to me; in two, the urine was passed by women during menstruation. It is just possible that they may really be nuclei of some larger nucleated cell, as pus or mucus, and have escaped by the bursting of the investing membrane, or sac of the cell." (p. 356.)

It is desirable that attention should be directed to these two forms of globules, because observers in general do not appear to be acquainted with them.

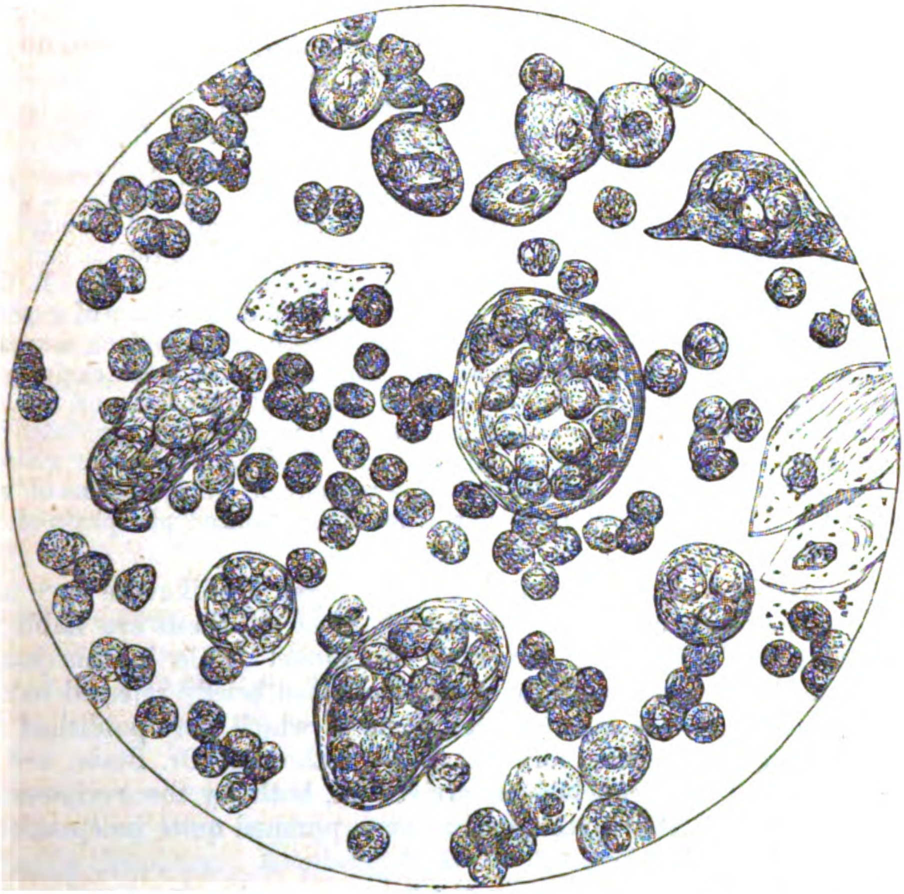
We have observed another kind of organic globule, differing entirely from either of the above, in several cases of catarrhus vesicæ, and also in simple irritation of the mucous membrane of the bladder, arising from enlarged prostate or other causes, and accompanied by an increased discharge of mucus. Although these globules vary in size, they are usually circular, and many times larger than pus or mucus corpuscles, and contain very distinct granular nuclei, clearly defined, without the aid of acetic acid or any reagent. The number of these nuclei is often very considerable, and they are themselves almost as large as ordinary mucus or pus corpuscles. (See fig. 5.)

These globules closely resemble the *parent cells*, which are considered to be characteristic of certain forms of malignant disease. There is no reason, however, for supposing that their presence in urine is indicative of any organic affection of the urino-genital organs; and it is highly important, in a practical point of view, that this fact should be borne in remembrance. An observer, not acquainted with these globules, might on first noticing them be led to form the erroneous conclusion, that some malignant or cancerous disease of those organs existed.

The remarks on *epithelium* are very meagre and unsatisfactory; no attempt is made to discriminate between the different forms of epithelium which occur in urine—as, for example, the epithelium of the tubules of the kidney, of the bladder, of the urethra—and that of the vagina; and yet the epithelia of these several parts differ greatly in their characters. These various forms occur in urine under very different circumstances, and their presence, therefore, very frequently affords practical information of high value. As one instance, but by no means the most important, showing the utility and even necessity of distinguishing between the different kinds of epithelium met with in urine, it may be mentioned that the urine of woman may, in nearly all cases, be known from that of man by the character of the vaginal epithelium, which is almost constantly present in it in considerable quantity.

The account of the development of the saccharine torula in urine is also very imperfect; and no description is given of penicilium glaucum. Dr. Bird does not refer to the investigations of the reviewer on the development of these fungi in the urine, which were brought before

Fig. 5.



Large compound globules, figured from a case of catarrhus vesicæ with enlarged prostate. Drawn with the camera lucida, and magnified 420 diameters.

the Royal Medico-Chirurgical Society, in November, 1852, and of which abstracts appeared in the different medical journals at the time. The principal conclusions arrived at as the results of these investigations were:—with respect to the development of *Penicilium glaucum,*

"That this fungus was frequently developed in urine. That the conditions necessary for its growth were, animal matter, especially, but not exclusively; albumen, an acid solution, and oxygen. That it may be developed at will in a variety of animal substances—as in solution of white of egg, acidified with acetic acid. That although the plant does not make its appearance in neutral or alkaline urine, no matter how much albumen may be present, yet that it will quickly become developed in these by acidifying such urines with acetic, phosphoric, or other acids; that its presence may, to a certain degree, be accepted as an indication of the acidity of the urine; that this fungus is no indication whatever of the presence of sugar, nor even of albumen, as a normal quantity of epithelium appears sufficient to induce its growth."

The conclusions with regard to the *diabetic torula* were—

"That in saccharine urine a distinct species of fungus was developed, which was identical with the yeast plant. That it passed through three distinct stages of growth, each of which is characteristic of the species. That as it was developed in urine in which the presence of sugar could not be detected by chemical reagents, it thus became an important means of diagnosing diabetes in its earliest stage. The conditions necessary for its development were—free exposure to the air, an acid liquid, nitrogenous matter, grape sugar or glucose, and a moderate temperature. By exclusion of air, the fungus was not developed, and a limited exposure insured an incomplete growth. Sugar might even be present in some cases, and yet, from defective acidity, the fungus would not be fully developed: and in such cases the growth of the fungus might be insured by the addition of phosphoric, or even of a solution containing carbonic acid. The presence of this fungus indicated the vinous fermentation, and this transformation of the sugar into alcohol was limited to the period of the sporule stage. The thallus and aërial fructification exercised little or no influence in promoting this change. In those cases in which the fungus was only partially developed in consequence of imperfect access of air, the sugar was converted chiefly into acetic, and a portion sometimes into oxalic acid: and when the fungus was not developed from exclusion of air, the sugar was converted into lactic, acetic, and butyric acids, and also, probably, aldehyde. The *Penicilium glaucum* was not unfrequently associated with the sugar fungus, as the conditions requisite for its development are generally present in saccharine urine. Lastly, that in very many of the specimens of urine in a case of incipient diabetes, large quantities of crystallized phosphate of lime were detected." *

The descriptions of the chemical tests for sugar are likewise very unsatisfactory; the numerous fallacies to which most of these are liable not being duly considered. The paper of Dr. Lionel Beale in this journal (vol. xi. p. 107), on the tests for sugar, is indeed briefly referred to; our own observations on the same subject, some of which were published previously to, and some contemporaneously with, those of Dr. Beale, are not even alluded to. The conclusions arrived at, both by the reviewer and Dr. Beale, although the investigations were pursued quite independently, were nearly identical.

The observations on vibriones in the urine are not more complete than those on the development of torulæ.

Our review of the work of Dr. Bird having already extended to a considerable length, we have no space left for any detailed remarks on the two concluding chapters. One of these, Chapter XIV., is on "Blood Depuration by the Kidneys as a Remedy in Disease." This chapter is entirely new. It contains, amongst other matter, original and important views in reference to the therapeutical action of the acetates, citrates, and tartrates of the alkalies.

We have dwelt thus long upon the work of Dr. Bird, because of the great and daily-increasing importance of the subject of the chemistry and pathology of the urine in the treatment of disease. In the course of our remarks, we have exercised the liberty which we reserved to ourselves at the outset of this review, to comment freely, and we trust impartially, upon such portions of the work as appeared to us to call for special observation. In particular, we have directed attention to those subjects which have been imperfectly, or rather, incompletely treated, partly in consequence of not being brought down to the present state of our knowledge. We have done so in the hope that these omissions will be supplied in any future editions of the work, and that such will be made to reflect fairly the state of the several subjects up to the periods of their publication.

* Abstract.

The advances made in any department of the science of medicine are surely slow enough; any work, therefore, professing to treat systematically of any branch of such science, should certainly give the most complete, as well as the latest, information on all subjects falling within its range; not to do so, is to render that progress, already slow, still slower; for information is thus withheld from the reader, of which he would no doubt, in many cases, make profitable use.

Regarding, then, the work of Dr. Golding Bird as a whole, we would say of it, on the one side, that it is less perfect and complete than could be desired, or than, with a little additional labour, it might very easily have been rendered; and, on the other, that it is clearly written; that the arrangement of the materials is good; that it contains many original views; and that it is eminently practical, and hence, well adapted as a guide to the student or practitioner. So excellent is the work in this respect, that its equal is not to be found up to the present time, either in this or in any other language.

Arthur Hill Hassall.

Review XI.

Die Rationalität der Molkencuren, eine Empfehlungsschrift für die Molkenanstalt zu Bad Rehburg. Von Dr. F. W. Beneke.—*Hannover, Helwing*, 1853. pp. 72.

The Rationale of Whey-cures. By Dr. Beneke.

Those who are conversant with German medical literature are aware that whey has long been employed as a curative agent in various ailments of a chronic character. Romberg, to mention a name now familiar to English readers, frequently recommends its employment as a mild restorative in nervous affections; and different methods in which it may be prepared have been admitted among the officinal directions of several of the German Pharmacopœias.* It is probable that the subject may soon attract the notice of the non-medical public of Great Britain, and the necessary consequence will be, the establishment of a "Whey-cure" institution in some lovely spot of Devonshire or Kent, on purely commercial grounds, with all those adjuncts of puffing and advertising which forebode an early death to the undertaking. We wish to save the whey-cure from the obloquy of becoming a mere

* The following is a translation of the directions given in the 'Pharmacopœia Borussica' of 1829, for the preparation of five different kinds of whey:

1. *Serum lactis dulce, sweet whey.*—Take an ounce of the dried stomach of a calf, infuse with six fluid ounces of cold water for ten or twelve hours, add an ounce of this liquor to nine pounds of fresh cow's milk, warm gently, and after coagulation is effected, decant and strain the liquid.

2. *Serum lactis dulcificatum, sweetened whey.*—Take three pounds of cow's milk, boil, and at the commencement of ebullition add one drachm of bitartrate of potash; when the coagulation is effected, and the whole has become cool, strain, and boil with a sufficient quantity of white of egg beaten up into a froth, until the albumen is coagulated; strain, and add as much prepared chalk (or shells) as is required to neutralize the acid, and filter.

3. *Serum lactis acidum, sour whey.*—The former without addition of the chalk.

4. *Serum lactis aluminatum, alum whey.* 5. *Serum lactis tamarindinatum, tamarind whey.*—In these respectively a drachm of crude powdered alum, or one ounce of the pulp of tamarinds, are employed instead of the bitartrate of potash.

Other pharmacopœias direct the preparation of a variety of whey with vinegar, or with Rhenish wine; in the latter case the proportions are eight ounces of wine to three pounds of milk.

empiricism; we desire that our professional brethren may be enabled to judge how far its systematic employment may be advisable and legitimate, in order that its real value may not be lost sight of in an exaggerated estimate of the advantages it undoubtedly offers. For this reason, we rejoice in the opportunity afforded by the publication of Dr. Beneke's little work, to consider the subject in its scientific and therapeutic relations.

Of late it has become much the fashion in Germany to combine the treatment by whey with a course of mineral waters; hence its preparation on a large scale has been introduced at many of the inland watering-places, to which our German cousins are so much in the habit of resorting in the summer months. This has been done at the picturesque brunnen of Rehburg, a small town in the kingdom of Hanover, the waters of which, though not very powerful, are, in conjunction with the delightful scenery, and the soothing influence of mental repose, considered admirable restoratives. They are of two kinds; there is a chalybeate for drinking, and a sulphurous spring, which supplies the bath, both at an average temperature of 54° Fahrenheit. Rehburg, we may add, is exempt from those sources of excitement which, to the disgrace of the German government, the gambling-tables offer in many other watering-places.

Dr. Beneke, of whose work on the aim and duties of scientific medicine an analysis is given in this number, has recently been appointed by the Hanoverian government, Physician to the Baths, and attends at Rehburg during the summer months. This has induced him to offer to the profession, in the memoir of which the title heads the present article, an explanation of what, upon chemical and physiological grounds, he conceives to be the *modus operandi* of whey in those morbid states in which it is commonly prescribed. He treats the subject in a manner which gives dignity to it, while it does not overrate the curative powers of the agent to which he especially draws the attention of his professional brethren. If a similar high-toned spirit pervaded all memoirs of the kind, the fate of quackery would be sealed, and the reproaches that the public sometimes deals out to the sons of Hygeia would be silenced.

The various writers on the employment of whey in disease have been content with vague statements of the effects it was found to produce, without regarding either the composition and character of the remedy in the proper light, or sufficiently discriminating between the influence of diet and regimen, and of the mineral waters that may have been used at the same time. The task that our author proposes for himself, is to establish, by the light of modern science, the true position of the subject. The first question which necessarily arises refers to the nature and composition of whey. Our author answers that—

"Whey is milk without its casein, and is prepared by the artificial removal of casein from milk; milk being one of our most important articles of diet, whey must be regarded as an article of diet artificially altered, while it offers a therapeutic agent for which we are unable to substitute an equivalent. The exact composition of the whey varies according to the milk used, and according to its mode of preparation. It appears that when milk is allowed to coagulate spontaneously, all the salts of the milk are retained by the whey, whereas a portion of the salts, and especially the phosphate of lime, are absorbed into the coagulum, when rennet is employed. The whey at Rehburg is prepared with rennet, and may therefore be stated to consist of water holding in solution the unnitrogeneous sugar of milk, and the lacteal salts, exclusive of a large portion of the phosphate of lime. The whey is more or less transparent according to the care with which it is prepared; any turbidity is attributable to small particles of casein suspended in it; unless intentionally acidulated, it ought to be perfectly neutral, and to have a specific gravity of 1034—1035. A knowledge of its composition enables us to arrive at a conclusion as to its effect when taken into the stomach. The large amount of water entering into its constitution renders it a diluent which promotes the secretions, and as the secretions cannot be increased without augmenting the quantity of solids removed from the system, it may be regarded as an agent which accelerates the metamorphosis of the tissues. The proportion of sugar consumed at the same time is very considerable; twenty-four to thirty-six ounces of cow's-milk whey, the average quantity taken by a patient in the course of one day, contains from $1\frac{3}{5}$ to $2\frac{2}{5}$ ounces of sugar; the introduction of this non-nitrogenous substance necessarily affects nutrition, while the acknowledged laxative influence of saccharine matter is also manifested in this instance, and accounts for the evacuant effect that patients experience while undergoing a course of whey."

It is unnecessary at present to enter into the author's views with regard to the physiological influence exerted by phosphate of lime introduced into the stomach. Suffice it to say, that he regards it as aiding materially in the process of cell-formation, and he therefore argues, that as it is shown to be excluded from the whey, it will be necessary to substitute it artificially whenever it is desirable that the whey should contain all the inorganic constituents necessary for building up the human fabric. It follows that—

"Whenever we wish to diminish the nitrogen of the diet, and therefore the nitrogenous constituents of the blood, without altering the quality and quantity of the inorganic compounds necessary for the healthy nutritive processes, we possess no more efficient agent than whey; and in all cases in which it is also desirable to promote the metamorphosis of the tissues, the large proportion of water entering into the composition of whey, indicates its exhibition, the more so if our patients can have the advantage of pure, fresh air, in a well-wooded, mountainous country."

The author briefly institutes a comparison between these theoretic arguments and the experience of the practical benefit to be derived from whey-cures accorded in the writings of other practitioners. Dr. Beneke shows, that while each observer has followed his own particular bias, by which much complexity has arisen, the main practical conclusions which others have arrived at, are supported by the views he expounds, and that whey may be regarded as alterative, nutritive, and laxative.

To promote the sanative influence of whey, a rigid attention to the dietary of the patient is indispensable; otherwise the dinner and evening meal may undo what has been begun in the morning. Dr. Beneke concludes from his experiments on numerous individuals, that the proportion of nitrogenized to non-nitrogenized aliment required to sustain a person in health, is as 1 : 4 or 5. If we wish to reduce the nitrogenous constituents of the organism, we must alter the ratio according to circumstances, to 1 : 6, 7, 8, or 9. This may be done by apportioning accurately the quantity and quality of the food taken during the day. A table, exhibiting the relative proportions of these two classes of constituents, in a list of the ordinary articles of diet, is appended, by which the physician is enabled to regulate his directions to the patient.

The constant supervision which in a small continental watering-place it is possible to exercise, and the almost filial veneration with which the medical man is often regarded, renders this a less arduous task than it might appear to most of our readers. And while we do not deny that there is a difficulty in determining these matters to a nicety under all circumstances, we shall certainly fail of any satisfactory result, unless guided by a leading principle, obtained by a careful analysis of the workings of natural laws, and more especially from those facts which the present state of bio-chemical knowledge enables us to deduce. We cannot treat a human being as we should turnips or a crop of barley, for which we can calculate to a fraction the amount of guano or superphosphate of lime necessary to supply the deficiency of a certain field, but we possess the indications that are to be followed, and by rigid observation and careful induction we may hope to arrive at a similar accuracy in adjusting the supplies to the roots of the human tree.

The influence of the treatment by whey, combined with a suitable dietary, is necessarily promoted by the exercise in mountain and forest scenery, by an emancipation from all mental labour and anxiety, and by the charms of social intercourse. But we must not expect to obtain very sudden or powerful changes by the procedure. Whey-cures are not adapted for patients whose system requires searching treatment. The watering-places suitable for persons of this class are altogether of a different character; the treatment by whey is adapted for individuals who cannot bear strong shocks, for delicate constitutions only. The exercise they take should not be carried to excess, and they should, above all, avoid violent emotions; such, for instance, as are excited by the gambling-table, against which, we are happy to see Dr. Beneke enters a lively protest.

"Nor should we," he adds, "estimate the contemplation of lovely scenery as of trifling influence in aiding the cure; it is one of the gentlest stimulants of the nervous system, and thus, in its turn, accelerates the metamorphosis of the tissues. Not less is every mental impression in some manner or somewhere reflected in the numerous physico-chemical processes which are the conditions of life; the more, therefore, we harmonize the objects of treatment with these processes, the more gratifying we shall find the result."

A point of considerable consequence to determine, before selecting a watering-place for a patient, is the climate of the locality; most of the whey-cure establishments that have hitherto enjoyed a reputation, as that of Gais in Switzerland, or Kreuth in the Tyrol, are situated at high elevations, with a much lower barometric pressure than patients from level countries are accustomed to. The average state of the barometer at Rehburg is between 28° and 29°, while that of the above-named places is between 24° and 25°. We are inclined to think that, except the residents in high mountain regions, those who would be benefited by resorting to a whey-cure in the climate of the Alps, would do as well without it.

A mere attention to the regimen of the patient who submits to a course of whey is not all that is required. Medicinal interference is often necessary, and more particularly the organs of digestion are apt to demand the aid of the physician.

The third question which Dr. Beneke attempts to answer, regards the morbid processes which are likely to be arrested by the whey-cure. As in healthy nutrition, it is necessary that the food should convey into the body non-nitrogenous, nitrogenous, and inorganic material, we find that—

"In every morbid process we have to deal with disturbances in these three groups, and if we reduce them to the first denomination of the groups (or the series of metamorphoses), we conclude that we have to deal with either a diminution or increase of the albuminates of the blood, with a diminution or increase of the non-nitrogenous compounds, and lastly with a diminution or increase of the inorganic compounds. . . . . . In by far the majority of morbid processes, we have to deal with an increase of the nitrogenous constituents of the blood, as in scrofula, tubercle, rheumatism, abdominal congestion and its sequelæ. The phenomena accompanying these processes differ much—whence comes this? It depends upon the great variability in the amount of the inorganic constituents which are associated with the pathological increase of the nitrogenous material; in one case there is a deficiency in the salts of iron; in another, of the phosphates; in another, the alkaline bases are in excess; in another, an excess of chlorides is associated with an increase of nitrogen; and according to this variation, the morbid process and its symptoms necessarily assume a different character."

We cannot follow the author farther in his argument, interesting as it is, but we have given a sufficient extract to show that his theory affords a clue to the results of daily experience, that the most opposite morbid phenomena are benefited by the same remedies, and that similar symptoms are successfully combated by different remedies.

In accordance with the rationale of the modus operandi of the whey, the diseased states in which its exhibition is indicated are analyzed. It is recommended, in the first instance, in those forms of scrofula and incipient pulmonary consumption which are characterized by a deficient supply of the phosphates, and of iron, and by an excess of the alkaline bases, the phosphates necessarily having to be artificially supplied in the treatment. The subjects peculiarly adapted for it are described as being in infancy affected with tumefaction of the glands and cutaneous eruptions of an impetiginous or ecthymatous character, by emaciation and defective development of their osseous system; or those who, later in life, are endowed with a very delicate constitution, a pale complexion, and a feeble nervous system, who possess a tendency to acid dyspepsia, to pulmonary congestion, and general excitability. Such patients, in addition to the employment of whey, commenced in moderate doses, and gradually augmented, would require the exhibition of the mineral acids, phosphate of lime, and exercise in fresh air.

The second great class of diseases to which Dr. Beneke considers the whey-cure well adapted, are certain rheumatic affections and the gouty diathesis; the forms which are described as likely to be particularly benefited are those affecting constitutions of a chlorotic and anæmic character.

The third class on the list is that catalogue of complaints which in Germany are set down to "Plethora abdominalis," and which the Englishman attributes to the "liver." The character of the patients is essentially the same as in the previous categories, only that their symptoms induce them to localize their malady in a different part of the body than their fellow-sufferers; they each would call their disease by a different name; the physician discerns essentially the same fons et origo mali, and decrees a similar system of treatment.

In a word, the scrofulous cachexia is to be regarded as the morbid process for which the treatment by whey is chiefly indicated. We have attempted briefly to sketch the outline of Dr. Beneke's views; those who wish to fill it up, we refer to the memoir in question, and to his other publications. Whether our readers agree with the doctrines promulgated or not, they will admit, with us, that the author treats his subject in a manner which savours of original thought and research, and indicates the path by which we may eventually thread the labyrinth of therapeutics. Every step in the right direction is of value. The modest manner in which he speaks of his specialty promises further results, which we shall be happy to receive: before we again meet him in the field of literature, some of our countrymen may have gone to see, and judge for themselves of, the real curative value of the whey-cure at the brunnen of Rehburg; their professional opinions we should also gladly welcome, as the daily increasing intercourse with the continent renders it imperatively necessary for the physician to be acquainted with those curative agents which are in vogue abroad, and are available for those fortunate unfortunates who, in illness, can exchange the anxiety and turmoil of active life for the calmness and repose of a rural holiday.

E. H. Sieveking.

Review XII.

1. *Etude Historique et Critique sur les Fonctions et les Maladies du Pancréas.* Par D. Moyse, Docteur en Médecine de la Faculté de Paris.—*Paris*, 1852. 4to, pp. 58.

An Historical and Critical Study in relation to the Functions and Diseases of the Pancreas. By D. Moyse.

2. *Cases and Observations connected with Disease of the Pancreas and Duodenum.* By Richard Bright, M.D., F.R.S., &c. ('Medico-Chirurgical Transactions,' vol. xviii.)

It is not often that we have occasion to review a memoir which has been twenty years before the public, and which may be presumed to have already taken its legitimate place in medical literature. Occasionally, however, it happens that observations or doctrines of real importance and permanent value, are jostled aside in the crowd of ephemeral productions which daily pass before the eyes of the cultivators of medical science; and when the course of modern investigation recalls our attention to facts which we believe to have been passed over too lightly in the first instance, we shall not hesitate to bring these again before our readers. We trust that in the present instance they will readily pardon us for an attempt to illustrate the physiology of the duodenal digestion, by a reference to a paper of Dr. Bright, read before the Medico-Chirurgical Society in November, 1832, and published in the succeeding year. The first memoir on our list—that of a devoted follower and partizan of M. Bernard—will serve to "point a moral," and will save this article from the charge of anachronism.

Some of our readers will probably remember, that at the date to which we have just referred, the attention of the Medico-Chirurgical Society was occupied by three papers, read successively by Dr. Bright, Mr. Lloyd, and Dr. Elliotson, and directed to the investigation of a peculiar and rare symptom connected with the digestive process.

"The symptom to which I refer," said Dr. Bright, who was the author of the first communication, "is a peculiar condition of the *alvine evacuation;* a portion more or less considerable assuming the character of an oily substance resembling fat, which either passes separately from the bowels, or soon divides itself from the general mass, and lies upon the surface, sometimes forming a thick crust, particularly about the edges of the vessel, if the fæces are of a semifluid consistence; sometimes floating like globules of tallow which have melted and become cold; and sometimes assuming the form of a thin fatty pellicle over the whole, or over the fluid parts in which the more solid figured fæces are deposited. This oily matter has generally a slight yellow tinge, and a most disgustingly fetid odour."

Of this affection three cases are related at length by Dr. Bright, as having come under his own observation; Mr. Lloyd contributed one similar case; while Dr. Elliotson, besides two cases observed by himself, and one by Dr. Prout, has referred to many others more or less carefully described in the literature of medicine, or in the catalogues of the London museums. None of these authors give any definite opinion as to the source of the oily matter; nor do they engage, except to a very limited extent, in pathological speculation as to the nature of the disease; but Dr. Bright's paper is distinguished from the others by an analysis of the phenomena found in connexion with this curious symptom, and whether we regard the severity of its logic, the importance of its conclusions, or the unassuming manner in which it is given to the world, we do not hesitate to refer to this investigation as a model of medical reasoning, and as eminently worthy of its author. The importance of Dr. Bright's views, as bearing upon the recent physiological researches to which we have alluded, will be seen in the following summary of them.

In referring to the three cases in which the symptom above described came under his notice, Dr. Bright remarks:

"When we draw a comparison between the three foregoing cases, a very close analogy, or even identity, in many circumstances, may be traced. In all of them chronic ailment terminated, sooner or later, in jaundice; and in all of them a great peculiarity in the character of the dejections existed. In the result of the examination after death we have likewise some circumstances which coincide in all—*obstructed biliary ducts; the liver gorged with bile; fungoid disease* attacking the *head of the pancreas;* and *malignant ulceration* on the surface of the *duodenum.* The question to be solved is, upon which of the conditions indicated or caused by these morbid changes, if upon either, the peculiarity of the alvine evacuations depended? That the obstruction of the biliary ducts, or even the total absence of all indication of biliary secretion, is not usually attended by the same peculiarity in the evacuations, many cases which have been cautiously detailed by various authors, and many which we have all observed, bear sufficient testimony; and I was therefore induced to ascribe it either to the existence of malignant disease, or to that disease being situated in the pancreas. That the simple fact of malignant disease existing is not *necessarily* productive of such appearances in the feculent matter, I infer from cases both of that form of disease and of melanosis in the liver to a very great extent being, within the scope of my experience, unaccompanied by any such discharge, though the evacuations were submitted to the most rigid observation. That simple ulceration in the bowels, to any known extent, is not attended by any such symptom, I am led to believe, from knowing that neither in the most extensive ulceration of the large intestines in cases of dysentery, nor in the worst cases of ulceration of the small intestines in fever, in diarrhœa, or in phthisis, does anything of the kind usually occur. Whether, however, malignant ulceration of the mucous membrane is accompanied by this symptom I cannot assert, though I have often seen most extensive ulcers of the pylorus and of the rectum, where, although the evacuations were attentively observed, such fatty matter was not detected. As, however, a malignant ulceration of the membrane did exist in each of the foregoing cases, it is not impossible that this was the cause of this symptom; but we must bear in mind that such ulcerations are *by no means* uncommon, and that the phenomenon of which I am speaking *is* uncommon; and that in each of the cases it was accompanied by another morbid appearance, which is not common—namely, the malignant disease of the pancreas. The fact of the intestinal ulceration having, in each case, occupied the duodenum, does, however, somewhat diminish the weight of this observation, for that certainly is not so frequent an occurrence."

Such is Dr. Bright's analysis of the cases adduced by him, in which the peculiar discharge above mentioned—of fat in the evacuations—existed as a positively ascertained symptom. Let the reader not omit to weigh well every assertion and every reservation here set down; we beg to assure him beforehand that not one clause of the above paragraph is superfluous. Dr. Bright then proceeds to analyze several other carefully observed cases, bearing upon the question by the greater or less similarity of their symptoms and morbid appearances, but differing in the absence of "the peculiar symptom." In two of these there were some of the recognised symptoms of pancreatic disease, and this diagnosis having been made, was negatived by Dr. Bright on the ground of the absence of fatty matter in the evacuations. The negative opinion was correct; but so far from maintaining the reasoning on which it was founded to be unassailable, Dr. Bright, on the ground of later experience, believes his arguments to have been in fact fallacious, and proceeds to relate three other cases in which pancreatic disease existed, even associated with a duodenal affection, without the occurrence of the fatty discharge. The first was one in which *the pancreas was completely involved in scirrhous disease, the duodenum thickened, contracted, and cancerous*, but not ulcerated, and *the common bile duct absolutely obliterated at its duodenal extremity.*—(Case 6, p. 34.) This case approaches very closely the first three; "the only point of distinction seems to be the condition of the duodenum, which, in all the other cases, had been affected by malignant ulceration, but in the present case was only united by the scirrhous disease to the pancreas." It is, however, *barely possible*, that *the fatty evacuation may have occasionally existed, and have escaped observation;* for the case, though carefully watched, was not specially watched in regard to this symptom during life. The next case related is one of *malignant disease limited to the middle third of the pancreas*, with some degree of *contraction of the duct* at the duodenal end; *contraction also of the bile-duct* (causing jaundice), and *incipient cancerous thickening of the duodenum.*—(Case 7, p. 36.) The alvine evacuations were "clay-coloured and yeast-like; but neither fatty matter nor mucus had been observed to be passed with them, and a few days before death they became very dark." (It is proper to mention that this observation was not made by Dr. Bright himself, nor are the number of examinations nor any of the details stated.) The last case was one of *cancerous disease of* *the liver, involving the capsule of Glisson, the common duct, and the duodenum, which was ulcerated opposite the disease;* the *pancreas* was questionably healthy, but *its duct much obstructed* and *distended*, behind the obstruction, to the size of " the largest swan's quill."—(Case 8, p. 44.) This case is earlier in date than all, except the first of those in which Dr. Bright observed the fatty evacuation, and therefore occurred before this symptom could have assumed the importance in his estimation which it afterwards acquired; but he assures us, from his own observation, that " on no occasion was the fatty matter detected, though the character of the stools was frequently noted." He also observes, that the occurrence of fatty evacuations in this case, without their having been observed, is "very improbable."

The facts of which we have given above a very brief summary, but which will be found carefully detailed in Dr. Bright's paper, are considered by him as sufficient to invalidate the conclusion that serious disease or derangement of the pancreas is *necessarily* or *constantly* accompanied by the peculiar symptom to which we have adverted. The strict inductive inference from the whole would be, that the occurrence of fatty evacuations is not necessarily determined by pancreatic disease, or by obstruction of the pancreatic duct, unless such disease or obstruction be such as completely to intercept the function of the organ; and that even in the case of very considerable disease of the entire gland, the fatty evacuation may be absent, provided the duodenum be sound or not much diseased. Taking these considerations in connexion with those in the paragraph which we have quoted above from Dr. Bright's paper, it will be seen that, as he remarks,—

" We bring the circumstances of the diseased structure in connexion with this symptom, within a narrow limit—*disease, probably malignant, of that part of the pancreas which is near to the duodenum; and ulceration of the duodenum itself.* These are the only two conditions which can be traced as being peculiar to all three cases."

We now propose to examine this conclusion by the light of modern physiology, and with the aid of the few additional pathological facts which have been recorded in such a form as to deserve attention. It was evidently the conviction of the great physician, to whose severe and conscientious study of morbid phenomena we owe, in a great measure, the knowledge we have of the diseases of the kidney, that in the present paper he had led the way to a somewhat similar generalization as regards the still more obscure diseases of the duodenal or pancreatic digestion.

" Should future experience and observation," says Dr. Bright, " serve in any way to connect the peculiar evacuation, which is at present under consideration, more decidedly with a diseased condition of the pancreas, or an imperfect action of the duodenum, which would, in all probability, be associated with pancreatic derangement, it will possess much interest, as at this moment our knowledge of the symptoms attendant on the derangement of the pancreas is very imperfect."

Very imperfect indeed! as the memoir of Claessen, published long after the date of this paper, and reviewed by one of our predecessors,* together with the numerous articles in our Cyclopædias and treatises on Practical Medicine, still testify, although there is scarcely one of them in which

* British and Foreign Medical Review, vol. xviii. p. 388.

the remarkable analysis of facts which we have presented above can be said to have received more than a very superficial, almost a depreciatory notice.

The reader who has followed us through the preceding paragraphs, and who is possessed of any knowledge, even the most cursory, of the experiments of Bernard on the pancreas, and the recent investigations into the physiology of the duodenal digestion by Lenz, Frerichs, Bidder and Schmidt, &c., will not fail to anticipate for the researches of Dr. Bright, in 1832, a position of the greatest historical importance. It is partly with the view of vindicating and establishing this position, that we have directed attention to them at present; for we consider ourselves bound, not merely in gratitude to their author, but in common justice to the cause of science, to claim for these researches the credit of originating an idea which will probably ere long assume a fixed position in pathological inquiries, and lay open to the physician much that even now lies hidden, "a spring shut up, a fountain sealed," waiting on the secure advance of physiological discovery. It is, indeed, apparent to us, that nothing but the rarity of cases such as Dr. Bright has here described, has prevented him from becoming the exclusive and generally acknowledged author of another *pathological discovery*, of a character at least as remarkable as the one with which his name is connected throughout Europe.*

It is not, however, wholly, or even chiefly, in this aspect of the matter that we claim the reader's attention to the pathological researches of Dr. Bright; but because we believe that the consideration of these cases, and the others which we shall presently mention, is calculated to influence the progress of the physiological inquiries to the exposition of which we have devoted an article in the present number. We are of opinion that the test of pathological facts may often be successfully applied to the solution of physiological doubts and difficulties; and that science will make more secure progress if the inductions from the study of normal structure and function are brought at every step face to face with the results of the analysis of morbid phenomena. It is in this spirit that we shall endeavour to point out the conclusions to which the researches of Dr. Bright appear to lead, when considered by the light of recent physiological investigations.

The experiments of Bernard appear to show the remarkable rapidity and power with which the normal pancreatic secretion acts upon the neutral fatty bodies of the aliment, converting them into an emulsion resembling chyle. We say this in the full knowledge that many of the ulterior conclusions of Bernard on this subject have not stood the test of further inquiry, and that the careful and varied experiments of several impartial and well-known physiologists have led to results which must very materially invalidate those to which our scientific neighbours in France have, by their authorized tribunals, given so hasty and unreserved a sanction. After careful consideration of the whole subject, however, we cannot but acquiesce in the conclusion of Lehmann, as well as of Bernard himself, that though

* To a sober and scientific mind the merit of this idea is all the greater, that it bears the form of a pure induction, carefully divested of everything like physiological hypothesis; although we believe one of Dr. Bright's anticipations, as incidentally expressed in the following sentence, will prove not far from the truth: "It may, indeed, at last, prove that the appearance to which I have been alluding is but the result of ill-digested aliment; but even in that case it will be very important to ascertain this fact." (pp. 52, 53.)

many other animal fluids possess the power, in a greater or less degree, of emulsionizing the neutral fats, the pancreatic juice is distinguished by this attribute in a high degree, and therefore is *probably* an important agent in the normal digestion of these bodies. It cannot, indeed, be admitted, as Bernard at first asserted, that the pancreas is the exclusive organ set apart for the decomposition and digestion of the fatty aliments; for the experiments of Bidder and Schmidt clearly prove that the decomposition and saponification of the fats is not a necessary part of the digestive process, and that the presence of the gastric fluid in the chyme tends to prevent that evolution of the fatty acids from their oils which takes place in contact with decomposing animal juices; and moreover, the emulsionizing of the fats and the absorption of milky chyle is proved not to be absolutely dependent on the pancreatic fluid, nor even on the pancreatic fluid and bile conjointly, but to be effected, in a certain measure, by the fluids of the intestine alone.* While, therefore, we must admit that the normal digestion of fat is a process to which several *factors*, as it were, contribute—viz., the intestinal juices, and especially those of the duodenum, the pancreatic juice, and the bile—it would appear highly probable that the pancreatic juice is among the most important and direct of the agents in the chylification of the fatty matters of the food; and that the bile, though not in itself possessed of much emulsionizing power, contributes powerfully, by an action (as yet imperfectly ascertained) upon the other juices above mentioned, to the digestion of the fatty matters in the duodenum and first portion of the small intestine.

Assuming for a moment the correctness of these propositions, it becomes, of course, of the greatest importance to science, to determine accurately to what extent the subtraction of any of the above *factors* influences the digestion of the fat. This branch of the physiological inquiry has not been neglected by those engaged in it: but the difficulties (as may readily be believed) of submitting some of these questions to the direct test of experiment are almost insuperable, and the results of such attempts appear, at first sight, hopelessly contradictory. Sir B. Brodie, in 1823, asserted that the fatty matters were not digested when the common bile duct was tied, and that the lacteals remained, in such cases, free of milky chyle. The observation was probably correct; but the experiments having been performed on cats, in which the ductus communis choledochus receives the pancreatic duct, as in man, the inference as to the effect of withdrawing the bile from the intestine cannot be admitted; and the subsequent experiments of Blondlot, Magendie, and others, together with numerous cases of jaundice, constantly under the observation of the physician, show that animals can exist for a protracted period without loss of flesh, although little or no bile reaches the intestine, the excrements being usually in such cases devoid of any marked excess of fatty matters, even when ordinary food, abounding in oil, is taken in nearly the usual quantity. Bernard's experiments appear to show, that the digestion of the fat is wholly arrested by the withdrawal of the pancreatic secretion. He has found that the pancreas may be entirely destroyed in dogs, by filling its ducts with oil (an ingenious procedure, capable of being turned to account in further experiments upon this subject);

* See especially the experiment of Frerichs, Wagner's Handwörterbuch, vol. iii. p. 849, par. 4.

and that the animals so treated remain for a time alive, with a voracious appetite, and capable of digesting food to a certain degree; but that they ultimately die emaciated, that they form no milky chyle, and discharge the fatty matters of the food unaltered *per anum*. We feel that, in the present state of the inquiry, we are not justified in accepting these statements to the full extent, especially as a quantitative analysis of the ingesta and egesta in these cases might have been easily performed, and would have exactly determined the extent to which the removal of the pancreas incapacitates an animal for the digestion of fat. It is well known that fatty substances, when given in sufficiently large quantity, are passed unaltered or imperfectly digested even in healthy persons; and hence the emaciation and the fatty evacuations of M. Bernard's dogs may have indicated only an impairment and not a suspension of the digestion of fat. This is still further rendered probable by the curious experiments of Brunner,* published in 1709; in which a large part, if not the whole, of the pancreas was excised or destroyed in the dog, the health and activity being maintained, and the functions regularly performed for a considerable time afterwards, to an extent which is scarcely consistent with the idea that the digestion of the fat was wholly or nearly suspended. We feel, therefore, that the solution of the question, as regards the pancreas, must depend on new experiments, after the manner of those above mentioned, and performed with strict regard to quantitative results: the animals should be weighed before and for some time after the operation, and its immediate effects; they should be kept alive as long as possible, and careful researches should be instituted into all the secretions and excretions. In the mean time, it is probable that the removal or destruction of the entire pancreas will be found—not, indeed, altogether to suspend the digestion of fat, but—to impair it so much as to be inconsistent with prolonged life; although it appears that a very small remaining portion of the organ is sufficient to enable an animal to live for at least several months.

Pending this inquiry, it appears to us that the facts of disease, in their bearing upon this subject, should be submitted to very close observation and careful criticism. It is not, indeed, improbable, that the most important contributions to the physiology of this subject may emanate from our hospitals, by an extension of the inquiry initiated by Dr. Bright. We have seen that, in the three cases of fatty alvine evacuation recorded by him, there was at once disorganization of the pancreas, disease of the duodenal mucous membrane, and some degree of functional derangement of the liver; in other words, it is clear that all the three *factors* probably concerned in the digestion of fat, were to a notable extent withdrawn from active service; and the performance of this function was accordingly almost entirely suspended. The other cases by which Dr. Bright illustrates his *negative* propositions appear to show—1st, that very great, almost complete, destruction of the pancreas with jaundice may exist, provided the duodenum be little affected, without any marked fatty evacuation (Case 6, referred to in a former paragraph); 2nd, that *partial*

* Experimenta nova circa pancreas. Lugd. Bat. 1709. These experiments, if not quite unexceptionable, are at least recorded with a degree of caution which renders them of unquestionable value.

disease of the pancreas, even with ulceration of the duodenum, and jaundice, with clay-coloured stools, is not necessarily followed by discharge of fat (Case 7); and 3rd, that all the three *factors* may be partially involved in disease, even when the appetite is "unusually great," and emaciation progressive, without this symptom becoming so marked as to arrest the attention, even of a most exact observer, who had already carefully studied this condition of the evacuations (Case 8).

We now see how eminently truthful, and how useful to science, has been the conscientious reserve with which Dr. Bright followed out his earlier impressions as to this remarkable condition. Had he come before the public with his three cases of fatty evacuation, and, with a self-satisfied philosophy, claimed the credit of a discovery in pancreatic symptomatology, the superficial reader could scarcely have failed to have been carried away by so inviting a novelty. How much of random assertion and one-sided observation would have followed, as is usual in such cases, we know not: we might even have been led, without any great stretch of imagination, to have anticipated Bernard's doctrine as regards the pancreas, and we should certainly have been disposed now-a-days to attribute to Dr. Bright the credit of that anticipation. But the inevitable result would have been disappointment; the hypothesis of the action of the pancreas would have disappeared under opposing instances, and would in six months have been consigned to the limbo of medical speculations in general. The unpleasant things which we, as reviewers, might have been tempted to utter of the parties concerned in such a controversy, we are now under the painful necessity of accumulating upon the head of M. Moyse, the author of the thin quarto pamphlet named at the head of this article.

With M. Moyse, the physiological investigations of Bernard, the experimental criticisms of these investigations by Frerichs, Lenz, &c., and the cases so carefully and accurately recorded by Dr. Bright and others, are evidently regarded chiefly as *grist* for his own peculiar *mill.* The product which he grinds out of this excellent grain it is not easy to describe in words which would appear otherwise than depreciatory of the scientific character and literary honesty of M. Moyse. Nevertheless, in consideration of his redeeming virtue of devotion to M. Bernard, we shall assume that M. Moyse, being a pupil and follower of that distinguished physiologist, felt called upon to vindicate his fame, and that the result of this disinterested admiration is the treatise before us. In the five final pages we find the sole novelty of the memoir; being an attempt of the author, conjointly with M. Robin, to discover a difference in the appearance of the salivary and pancreatic glands in animals, according as they are killed fasting or during digestion. In the latter case, the epithelium of the pancreas is a *little less* granular than in the fasting animal, while in the salivary glands it is *slightly more* granular in some cases, there being in others scarcely any appreciable difference. The period of digestion at which the observation was made, the number of experiments, and the other details necessary to give force to this observation, are not vouchsafed to us in the present treatise.

The first part of the work is devoted to an exposition of M. Bernard's theory of the pancreatic function, and of the physiological proofs which he has adduced in support of it in his various memoirs; and were its professed object only a defence and eulogium of M. Bernard, it might be admitted to be a creditable production; it even rises to a kind of eloquence in dealing with certain "accusateurs publics contre M. Bernard," although we never are informed who these accusers are, unless it be Lenz, the quotations from whose work do not appear to us to have the character attributed to them. M. Moyse, however, promises us an "historical and critical study" of the subject, and we cannot admit that he has in any respect fulfilled his promise. The whole of his history is dominated by his fixed idea, that M. Bernard and the digestion of fat are the only circumstances worthy to be commemorated. The criticism is equally one-sided; it consists in criticising the older physiologists by means of the observations of M. Bernard, and omitting to notice everything which demanded a little further investigation for its criticism. Reference is indeed made to the writings of Lenz, Frerichs, Bidder and Schmidt, but only in such a manner as shows that the author had given no attention to them, further than that he may have picked up at second-hand a few of M. Bernard's *answers* to their objections. Altogether, this first part is only useful to those who may have occasion to make themselves acquainted with M. Bernard's researches on this subject, and have not the opportunity of procuring his own very clear and excellently written exposition of them in the 'Comptes Rendus de l'Institut.'

The second part, or "partie pathologique," of M. Moyse's memoir, he commences by announcing, as a symptom of chronic affections of the pancreas, "this phenomenon: the patients digest the two other groups of substances, but not the fatty aliments." The next sentence we must give in the author's own words:

> "C'est là le point *nouveau;* jamais ceux qui se sont occupés des maladies du pancréas *avant moi* ne l'ont étudié avec cette liaison physiologique. . . . Je rends hommage aux travaux de ceux qui m'ont précédé, MM. Moudière, Schmackpfeffer, Percival, Bécourt, et tant d'autres, mais," &c. &c.

The author has clearly never heard of Dr. Bright's cases, as the reader has no doubt already observed. M. Moyse is very severe upon those respectable individuals who have presumed to record salivation as occurring in pancreatitis. "C'est dire," says he, "dans une phrase tout ce que la théorie abstraite peut enfanter." Considering the fact, that in some cases, and those within our own day, the salivation has been recorded as a matter of pure observation, this sentence is rather hard. But no doubt the author reposes upon an immense personal experience, or upon the most careful and exact researches into the cases of alleged salivation.

> "Je ne m'occuperai donc que de ce seul symptome pathologique, qui est *le véritable:* presence de matières grasses dans les fécès. . . . . Je diviserai les observations que *j'ai* pu recueillir en trois groupes."

We now begin to look with some degree of confidence to have the whole question of the pancreatic pathology and symptomatology settled by the researches of M. Moyse. But how earnest the devotion to his subject, how wide the experience, how close the observation, which must have been his in order to enable him to take this calm tone of superiority to all doubt or difficulty! Dr. Bright's cases, as we have seen, though most carefully observed, have not appeared to him consistent with the simple principle above enunciated. Some others that we have heard of are equally difficult to dispose of on these terms. M. Moyse has, however, doubtless brought his stores of original information successfully to bear on the subject!

Alas! alas! how shall we inform our confiding reader, that the cases of M. Moyse are precisely those of Bright, Lloyd, and Elliotson, to the most important of which we have already besought his attention, reinforced by *one other instance* from the Museum Catalogue of the Boston Society of Medical Improvement! How shall we confess that even these few instances present no indication of having been taken from the original sources, but are garbled extracts from the 'Archives Générales,' so full of errors and omissions, that we have marked three or four of the most important kind in almost every page! We have no space to go into details, but the following examples will show how little the work of M. Moyse can be trusted as the expositor of our medical literature. In p. 37, "le milieu de 1832" is put for the middle *of February*, 1832. In p. 38, "le duodénum avait contracté avec le conduit choledoque une si grande adhérence," replaces "the duodenum, towards its middle, including that part into which the ductus communis choledochus enters, was so contracted." In p. 40, "the quantity of oil became *inconsiderable*," is translated "la quantité de graisse rendue devint *énorme*." In pp. 42, 43, the fourth case is so related as to make the interval between March, 1827, and the 28th December, appear only a few days (the name of the latter month being omitted); and in the same case, an ulcer, "the size of a *silver* penny," is spoken of as being "pas plus large qu'une *pièce de deux sous*." With these and other errors of equal magnitude we need scarcely complain of such trifles as the substitution of "*Furradouy*" for "*Faraday*," "Gutterbluck" for "Clutterbuck," &c.; nor, perhaps, are we justified in dealing seriously with even the worst blunders of a writer manifestly so ignorant and incapable; but we are aware that among our neighbours across the Channel a little knowledge of our literature goes a long way, and we are anxious, if it be yet possible, to turn the attention of some among them to these valuable cases, through the medium of other channels than translations like the present.

Of M. Moyse we now take leave, hoping that, if we ever meet him again, he may in the meantime have learned to place scientific truth and literary candour even above the indulgence of his own self-esteem.

In conclusion, we must notice the few cases affecting this question which are known to us as having been published since Dr. Bright's paper. Among the cases of fatty alvine evacuations in Dr. Elliotson's communication to the 'Medico-Chirurgical Transactions,' there are only three (12, 13, 14) in which a post-mortem examination is recorded, and in all of these it is so superficially reported, that we cannot place much confidence in the negative statements. In one of these cases, however, "the pancreatic duct and the larger lateral branches were crammed with white calculi;" no jaundice is mentioned. In another, there was probably jaundice, as "the fæces were very pale, and almost destitute of fæcal odour;" but the details of the case are, as in the third instance, very short and imperfect. In three of the cases not examined after death, there was probably jaundice (6, 7, 16) coincidently with the fatty discharge. One of these cases ended in recovery, and another is not stated to have terminated fatally. Dr. Elliotson's paper is, on the whole, more interesting as a collection of historical curiosities than from any scientific or practical result.

In Mr. Lloyd's case, in the same volume of the 'Medico-Chirurgical Transactions,' there was jaundice, with obliteration of the common duct at its entrance into the duodenum, which was much contracted; the pancreas was nearly healthy, but its duct was of course obstructed at the duodenal end, and dilated in the course of the organ.

The case in the Boston Museum, referred to by M. Moyse, is one of considerable interest in relation to this question. The pancreas was much diseased, and obstruction of the bile-ducts seems, in this case, as in so many others, to have taken place simultaneously with the first attacks of fatty discharge. As the case may not be easily accessible to our readers, and is well reported, we give the following abridged extracts (*not* from the French edition):

"The patient was a labouring man, forty years of age. Thirteen years before death, he was greatly reduced by hæmorrhages from the bowels, and to this he always referred as the origin of his disease. Three years before death, he had a second attack, which was quite severe, and lasted several weeks. There was great tenderness in the epigastrium, with frequent returns of diarrhœa and discharges of blood, the functions of the stomach being, meanwhile, sufficiently well performed. In December, 1836, he was attacked with febrile symptoms, pain and obstinate constipation, followed by severe diarrhœa: *the discharges contained no bile*, but consisted in a great measure of blood, and the epigastric tenderness was extreme. In about a fortnight *a fatty substance was first noticed, and* from this time it was observed, more or less, in every discharge from the bowels, until the month of May, when it entirely ceased. The following report was made during the first attack:—'Patient declares that for six weeks he must have discharged, on an average, half a pound of this substance daily; *is also positive that he had no discharges of fat, except after eating meat or food cooked in fatty matters; that they would cease in twenty-four hours after abstaining from meat, and return again after resuming it; his wife, however, is confident there is no such connexion.*'"

After this there was a partial recovery: the appetite at first good, often craving, afterwards indifferent; the alvine evacuations uniformly deficient in bile after December; complete jaundice for five weeks before death, which took place on the 16th of September, a tumour having been detected in the epigastrium in August, and signs of advanced phthisis being known to exist.

"On opening the cavity of the abdomen, a large fluctuating tumour, of a regular oval form, was seen below the right lobe of the liver; the duodenum ran over, and almost around it. On incision, it was found to contain about 14 ounces of a bloody-looking serous fluid, without coagula, not viscid or greenish, *and without any appearance of fatty matter.* The tumour or cyst measured four inches by three in extent; it was evidently formed by a dilatation of the pancreas; two calculi adhered to its inner surface, near the orifice of the duct, which was entirely obliterated; they had the usual characters of pancreatic concretions, and were found to consist of carbonate of lime. The remainder of the pancreas, forming the left extremity of the organ, was about two inches in length, much contracted, and feeling very hard; the duct itself was considerably enlarged, and opened freely into the cavity of the sac. On slitting open the common duct, a portion of it, which formed part of the parietes of the cyst, was found in a very sloughy condition, it being evident that an opening was about to be formed, if it did not already exist, between the sac and the hepatic duct."*

This case we regard as one of the most important yet published of this disease. Whether we are to take the word of the patient or of his wife, in regard to the suppression of the fatty discharges when no fat was taken, it may be difficult to say; the discrepancy of evidence is certainly unsatisfactory, and the experiment ought, we think, to have been repeated under the observation of the physician. It, at all events, serves to point out the mode of investigating any case in which this disease may come under observation in future; from some of our hospitals we shall look for an observation of this disease, in which minute attention is paid to these circumstances; and the aid even of quantitative chemical analysis may, we think, be advantageously brought to bear upon the question. As regards the pathological significance of the case, it is to be observed, that the pancreas, although the only organ primarily diseased, was in all probability seriously disordered, and even disorganized, long before the fatty discharges were observed; and that the first attack of this kind noticed, manifestly coincided with such an enlargement of the tumour as caused it to press on the common bile-duct, while it is also exceedingly probable that pressure took place at this time also on the mucous membrane of the duodenum, which was "stretched" over the tumour, and probably more or less compressed by it. The condition of its mucous membrane is not reported, and was therefore possibly not accurately examined: but we may presume that there was no serious affection to be detected.

Mr. Alfred Clarke, of Twickenham, relates in the Lancet for 1851 (August 16th, page 152), a case very similar to the above, the pancreas being atrophied from obstruction of its duct, which formed a cystic tumour the size of a hen's egg, and the liver presenting more or less functional derangement and disease of structure. The patient, a woman 57 years of age, had been subject for some years to symptoms which were referred to hepatic derangement, and to gall-stones, but which are not particularly described. The fatty evacuation was similar to that noticed in other cases of this kind, but was supposed by the patient (erroneously) to have come from the bladder, thereby giving probability to the idea, that in a case recorded by Tulpius, and another by Dr. Elliotson, of fatty evacuation simultaneously from the bowels and bladder, a similar error of observation was committed. There does not appear to have been jaundice in this case, although the stools were deficient in bile; the duodenum is said to have been healthy; the gall-bladder and bile-ducts were empty; the pancreatic duct completely obstructed.

In the Museum of St. Bartholomew's Hospital (see Catalogue—Series XX. 2) there is a preparation, illustrating a disease of the pancreas, very similar to that in the foregoing case, and connected, like it, with fatty alvine discharges during life. "The patient was a very intemperate man, forty-eight years old. He had tuberculous disease of the lungs, cirrhosis of the liver, an ulcer in the stomach, and tuberculous ulcers of the intestines." (This case is referred to by Dr. Kirkes, in his 'Handbook of

* Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement. The case is also published by Dr. Gross, in his work on Pathological Anatomy; it was attended by Dr. A. A. Gould and Dr. Woodbridge.

Physiology,' p. 237, where it is stated, probably by an oversight, that "the liver *was healthy*, and there appeared nothing but the absence of the pancreatic fluid from the intestines to which the excretion or non-absorption of fatty matter could be ascribed.")

It is possible that other cases bearing on this subject may exist in the records of public or private practice in this country, and it is very desirable that they should, if accurately reported, be given to the public in some permanent form. We have given above all that have come to our knowledge up to the present time; and it will be seen, that if they add, on the whole, little to the evidence adduced by Dr. Bright, they strengthen his conclusion that the pancreas is probably the most important seat of the disorder in these cases of fatty alvine evacuation. On the other hand, the last three cases, and some of those insufficiently reported by Dr. Elliotson, appear to show that the participation of the duodenum, at least directly, in the disease is by no means invariable, and that simple as well as malignant disease may be attended by the symptom referred to. The importance of the hepatic derangement in determining the fatty evacuation seems to be still further borne out by the cases published since Dr. Bright's paper.

It should be remembered, however, in the further investigation of this question, that, as Dr. Bright pointed out, numerous cases of hepatic disease, and a considerable number of pancreatic disease, nay, even some cases of complete disorganization of the pancreas, combined with disorder of the liver, have been carefully examined and recorded, without the discovery of fatty evacuations at any period of the affection. Perhaps none of these cases may be quite unexceptionable; but they form a sufficiently strong body of evidence to make us pause in the interpretation of these pathological phenomena too absolutely. It is impossible at present to attach to the important symptom which we have been investigating any more exact significance than that it appears to be an indication of extreme imperfection of the duodenal digestion, and that it is, in a considerable proportion of cases, caused by organic disease of one or more of the secreting organs communicating with that part of the intestinal canal; the pancreas being especially indicated both by physiology and pathology as the one chiefly and most directly concerned in its production.* The great importance of faithful and minute anatomical and symptomatic details in future cases of this kind will, we trust, be now less apt to be overlooked than hitherto.

* Most of the authors who have mentioned Dr. Bright's cases have demurred to his conclusions, without, however, raising any difficulties, or proposing any theories, which he has not himself already encountered. Dr. Elliotson's paper exhibits a leaning towards the liver or the intestines as the source of the fatty discharges; but his conclusions are exceedingly *negative* and vague, and his facts, as we have seen, can scarcely be said either to oppose or to sustain Dr. Bright's views. Claessen, and other writers on pancreatic disease, have also treated the remarkable inquiry which we have brought before our readers in a manner that does not, we fear, redound to the credit of their appreciation of medical logic. The author of an article in the 'Medico-Chirurgical Review' (see vol. xx. p. 9), takes the strange position that "fatty substances in the evacuations, so far from being rare, are, on the contrary, a very common occurrence or symptom in disordered states of the alimentary canal." We scarcely think that general experience will bear out this observation; and it is plain from the sequel that those observed by the author were, in the majority of cases, mucous in character, and essentially different from those described by Dr. Bright. Nevertheless, it is not mprobable that the frequency of fatty evacuations has been underrated.

Review XIII.

1. *Die Verdauungssaefte und der Stoffwechsel. Eine Physiologisch-chemische Untersuchung.* Von Dr. F. Bidder und Dr. C. Schmidt, Professoren in Dorpat.—*Mitau und Leipzig*, 1852.

The Digestive Fluids and the Metamorphosis of Tissue. A Physiologico-chemical Investigation. By Professors Bidder and Schmidt.

2. *Of Food and the Digestive Process. Being Chapter* VII. *of* Carpenter's *Principles of Human Physiology.* 4th Edition.—*London*, 1853.

We purpose in the present article to lay before the readers of this review a succinct sketch of the physiologico-chemical investigations to which the digestive fluids have recently been submitted. Adopting Bidder and Schmidt's mode of arrangement, we shall successively consider the physico-chemical characters and the physiological relations of the saliva, the gastric juice, the bile, the pancreatic fluid, and the intestinal juice.

Human *saliva*, in the ordinary acceptation of the term, is a mixture of the secretions of the three pairs of salivary glands and of the buccal mucus. It is a colourless, or very faintly blue, turbid, viscid, inodorous, and tasteless fluid, which, after standing for some time, separates into an upper transparent layer, and a lower, opaque, yellowish-white portion. It is always alkaline, but the degree of alkalinity varies, and is found to increase during and after meals; while after prolonged fasting the secretion is almost neutral. Its specific gravity was determined by Jacubowitsch* at 1·0026; and after the deposition of the sediment, at 1·0023. Lehmann† states that its usual variations in man are between 1·004 and 1·006, its extreme normal limits being 1·002 and 1·009; while Wright, our principal English experimenter on the saliva, asserts that, in numerous experiments made on 200 healthy persons, the specific gravity varied from 1·0069 to 1·0089. Lehmann may probably be regarded as giving the safest estimate.

In the following table we give the chemical composition of healthy human saliva as determined by Frerichs‡ and Jacubowitsch:

Frerichs.		*Jacubowitsch.*	
Water	994·10	Water	995·16
Solid constituents	5·90	Solid constituents	4·34
Epithelium and mucus	2·13	Epithelium	1·82
Fat	0·07	Soluble organic matter	1·34
Ptyalin, with a little alcohol extract	1·41	Sulphocyanide of potassium	0·06
Sulphocyanide of potassium	0·10	Fixed salts	1·82
Fixed salts	2·19		

The 1·82 parts of salts in Jacubowitsch's analysis consisted of phosphate of soda 0·94, lime 0·03, magnesia 0·01, and the chlorides of sodium and potassium 0·84.

* De Saliva. Diss. Inaug. Dorpati, 1848.

† Physiological Chemistry (printed for the Cavendish Society), vol. ii. p. 10.

‡ Wagner's Handwörterbuch der Physiologie, vol. iii. part 1, p. 766.

Two of the substances in the above analyses require a brief notice—viz., the ptyalin, and the sulphocyanide of potassium. The ptyalin—or as it is termed in Jacubowitsch's analysis, the soluble organic matter—exists in the saliva in combination with potash, soda, and lime; and although occurring in small quantity, is the most important constituent of this fluid, being that apparently on which its metamorphic action on starch depends. In many respects it is very similar to albuminate of soda and to casein, but it must by no means be confounded with them. Many of its reactions are noticed by Lehmann in the second volume of his 'Physiological Chemistry' (pp. 19, 20, of the English translation). Bidder and Schmidt are of opinion that "the soluble organic matter" is not altogether identical with the ptyalin or salivary diastase of certain authors (Mialhe, and others), which they regard as a mere admixture of various organic and inorganic substances: it occurs, they very correctly observe, in too small quantities to admit of a very accurate chemical examination. It must be apparent to every one versed in the history of animal chemistry, that Berzelius, Tiedemann and Gmelin, and Wright, have used the term *ptyalin* in very different senses. The presence of sulphocyanide of potassium is interesting, insomuch as the saliva is the only fluid in which sulphocyanogen seems normally to occur. It would be out of place in the present article to enter into the chemical details connected with the quantitative determinations of this substance. (For information on this point we may refer to the second volume of Lehmann's 'Physiological Chemistry,' p. 22; or to Bidder and Schmidt, p. 10.) This salt only occurs in very small quantities, as is obvious from the above analyses. Lehmann found it to vary, in his own saliva from 0·0046 to 0·0089 per cent.

So little is known regarding the variations in the amount of the sulphocyanogen, that we make no apology for introducing in this place the conclusions at which Kletzinsky* has recently arrived, in reference to this subject. His determinations were merely relative, and perhaps not of a nature to be very exact, his method being to make the saliva to be tested drop from the mouth into one and the same small white porcelain basin, and then to add *guttatim* a solution of neutral per-chloride of iron (1 part of $Fe_2\ Cl_3$ to 10 parts of water), and to stir with a glass rod till the maximum degree of redness was induced.

1. Taking the morning reaction as the normal type, the sulphocyanogen is most abundant after meals, and most deficient towards night.

2. On fasting it diminishes more rapidly towards evening, and hardly attains its average quantity in the morning.

3. Its quantity is diminished by alcoholic drinks, and is increased by coffee, pepper, salt, and spices; still more so by mustard, garlic, and radishes.

4. Peruvian balsam always causes an augmentation; and musk, in a half-grain dose, always produces a very great increase.

5. The use of iodine always diminishes it.

6. In true ptyalorrhœa the sulphocyanogen is always very much diminished, or actually disappears; but hydrosulphate of ammonia is always present as a product of its decomposition.

* Arch. f. Chemie und Mikroskopie, 1852, p. 40.

7. In almost all chronic exhausting diseases the sulphocyanogen is diminished, while during convalescence it is usually a little above the average.

8. It is relatively deficient in infancy and old age, and in the later months of pregnancy.

9. In all conditions of excitement, whether psychical or somatic, it is always somewhat increased till depression begins to supervene. It was very much augmented in a male lunatic addicted to onanism, and in an insane woman with nymphomania.

The only conclusion at which we can at present arrive is, that the quantity of sulphocyanogen may, in some measure, be regarded as proportional to the intensity of the vital processes.

We offer no conjectures regarding the manner in which the sulphocyanogen is produced within the system. Kletzinsky,* in a subsequent paper to that to which we have already referred, points out certain ways in which it may *possibly* be formed. Whether it is *actually* formed in these ways is a very different question.

A number of experiments have recently been made in the Dorpat laboratory (by Jacubowitsch, under the directions of Bidder and Schmidt) on the salivary secretion of dogs, with the special view of ascertaining the chemical characters and the physiological function of the different fluids entering into the ordinary or mixed saliva. He determined the special characters of

- α. Their ordinary or mixed saliva;
- β. Their saliva, excluding the parotid secretion;
- γ. Their saliva, excluding the submaxillary secretion;
- δ. Their saliva, excluding the parotid and submaxillary secretions;
- ϵ. Their parotid saliva; and
- ζ. Their submaxillary saliva.

We may remind our readers, that the reason why no reference is made to the sublingual glands is, that they are so slightly developed in the dog that their secretion would scarcely affect the general result. The submaxillaries, on the other hand, are as large as the parotids.

The following are the chemical results yielded by the fluids α, β, γ, δ, ϵ :

	α.	β.	γ.	δ.		ϵ.
Water	989·63	990·48	988·1	996·04	991·45	995·3
Solid residue	10·37	9·52	11·9	3·96	8·55	4·7
Epithelium	3·58	4·25	2·24	1·51	2·89	1·4
Soluble organic matter			5·04			
Phosphate of soda	0·82					
Chloride of potassium		4·08	4·20		4·50	2.1
Chloride of sodium	5·82					
Sulphocyanide of potassium		—	—			
Phosphate of lime						—
Phosphate of magnesia	0·15	1·19	0·42		1·16	—
Carbonate of lime	—	—	—			1·2

* Arch. f. Chemie und Mikroskopie, 1852, p. 176.

The specific gravity of α was 1·0071; of β, 1·0042; of γ, 1·0067; that of δ and ϵ varied respectively from 1·0026 to 1·0041, and from 1·004 to 1·007.

We cannot enter at any length into the differences which are presented by these varieties. The subject is pretty fully discussed in the chapter on the Saliva in the second volume of Lehmann. It is sufficient to observe, that the parotid, submaxillary, and mixed salivary fluids *coincide* in being unaffected by nitric, hydrochloric, sulphuric, phosphoric, and acetic acids, and by solutions of ammonia and alum; in being only rendered slightly turbid by ferrocyanide of potassium, after previous acidulation with acetic acid; and finally, in being very strongly precipitated by alcohol, tannin, and acetate of lead: whilst they *differ* in the following respects—parotid saliva, when exposed to the action of the atmosphere, becomes covered with a crystalline coating of carbonate of lime, and it becomes turbid on the addition of caustic potash, whilst neither of the other secretions are thus affected; and, on the other hand, the latter secretions become turbid at the temperature of boiling water, assume an orange tint when treated with nitric acid and subsequently with ammonia, and are precipitable in red flakes by perchloride of iron; while parotid saliva undergoes no such changes. (The non-occurrence of *red flakes* on the addition of perchloride of iron is not to be taken as an evidence of the non-existence of a sulphocyanide in this secretion, for a *red colour* is induced on the addition of this reagent.)

These fluids obviously contain no albumen, since they are unaffected by a boiling heat, by nitric acid, and by ferrocyanide of potassium after previous acidulation; and no casein, since they are not covered with a film during evaporation, and since acetic acid throws down no precipitate; and that no chondrin or pyin is present, is obvious from there being no precipitate on the addition of acetic acid or of alum.

In concluding our notice of the chemistry of the saliva, we must say a few words regarding the buccal mucus. This fluid, in a state of purity, was obtained from a dog, whose parotid and submaxillary ducts had been previously tied.

In 1000 parts of buccal mucus Jacubowitsch found:

Water	990·02
Solid residue	9·98
Organic matter soluble in alcohol	1·67
,, ,, insoluble ,, ,, ...	2·18
Fixed salts	6·13

This fluid was very viscid and frothy, so that its specific gravity could not be determined with certainty; it was extremely turbid, from the presence of an enormous number of epithelial cells, which would not deposit themselves on allowing the liquid to stand; and it always exhibited a strong alkaline reaction.

There is great diversity of opinion regarding the average amount of the salivary secretion. The views of Mitscherlich, Valentin, Donné, and Thomson, on this point, may be found in the second volume of Lehmann's 'Physiological Chemistry,' where a reference is also made to the experiments of Jacubowitsch, whose most important conclusion was, that to whatever extent the quantities of the saliva (or rather of the water) secreted by the different organs may vary, the solid constituents—both the organic and the inorganic substances—amount to very nearly the same from all three sets of glands; the solid constituents from each pair of glands averaging, in one hour, about 0·232 of a gramme,* of which 0·080 is organic, and 0·152 inorganic matter. These experiments, however, afford us no means of determining the average amount of the salivary secretion, because nothing is known of the size or weight of the different animals (dogs) which he employed. The most trustworthy observations on this subject are probably those just published by Bidder and Schmidt. Experimenting on a dog weighing 16 kilogrammes (about 35 pounds), they obtained in one hour 5·640 grammes of saliva from one of the ducts of Wharton, so that the two submaxillary glands yielded in that time 11·28 grammes. From one of the ducts of Steno they obtained in an equal time 8·79 grammes of a clear, limpid secretion; hence the saliva yielded by the two parotids was 17·58 grammes. The flow of saliva was stimulated by the occasional application of a feather moistened with acetic acid to the buccal mucous membrane. Assuming that on an average a man weighs four times as much as the above dog (namely, about 140 pounds), and that these secretions vary directly with the weight, the horary discharge from the Whartonian ducts would be 45 grammes, and from the Stenonian 70 grammes. If the parotid and submaxillary glands were constantly secreting at this rate, their daily saliva would average rather more than six pounds. Bidder and Schmidt express their belief that the actual quantity of the collective saliva in all probability averages about half that quantity. They arrive at this conclusion partly from the above experiment, and partly from a direct observation of the quantity that a man discharged in one hour. This quantity ranged in that time from 100 to 120 grammes, care being taken that none was swallowed. Assuming that no secretion, or very little, took place during sleep, we should obtain about three pounds as the result of our calculation.

All such determinations must, however, be looked upon as merely approximative, since the activity of the salivary glands is dependent on numerous influences and conditions. The ordinary exciting cause of a copious flow of saliva is the act of mastication; it depends, however, very much upon the nature of the food, whether much or little saliva is effused into the cavity of the mouth, dry and hard food inducing a much more copious flow of saliva than food which is soft and moist. From experiments which have been made by Bernard and others on horses (in which the œsophagus was exposed and opened, and the food which the animals had swallowed was intercepted and removed), it appears that straw and hay, as they pass down the œsophagus, are mixed with four or five times their weight of saliva; while highly amylaceous seeds, as, for instance, oats, are mixed with an equal quantity, or perhaps with one and a half times as much, of saliva, and fresh green fodder with only half its weight; and that food mixed with water seems to take up scarcely any saliva.

The physiological value or the function of the saliva now claims our attention. It is now established beyond all question, that the principal use of the salivary secretion is to promote the conversion of the *amylacea* into dextrine, sugar, and lactic acid, and thus to facilitate the absorption of this class of foods.

* French measures are now so universally used in chemistry, that we have deemed it expedient to retain them in this article. The gramme is equal to about fifteen grains and a half.

The first point we shall notice is the time required for the metamorphosis of starch by human saliva (the mixed secretion). If we take a fresh decoction of starch, prepared with distilled water, and proved by Trommer's test to be free from sugar, and if we mix it with an equal quantity of fresh saliva, and agitate the mixture, it will instantly lose its viscid character, and become thin and watery; on testing a small quantity of it for starch, we find that iodine no longer induces the well-known reaction, while, on the other hand, the rapid reduction of oxide of copper (in Trommer's test) affords indisputable evidence of the presence of sugar.

The almost instantaneous induction of this action is a point which must not be overlooked, in considering the question whether this is a special property of the saliva, or whether it is shared by other animal fluids. There can be no doubt, as we shall presently show, that in this respect the pancreatic and the intestinal juices exactly coincide with the saliva; but when we find stress laid upon the circumstance that many other organic substances—as, for instance, nasal mucus, pieces of kidney, putrefying serum, &c.—produce similar changes in eight or twelve hours, at 100° or upwards, we must recollect that at such a temperature, and after so long an interval, changes may be spontaneously set up in a solution of starch. There are, however, a number of animal substances which occasion the appearance of sugar in a solution of starch, in so short a period as altogether to exclude, in such cases, the suspicion of spontaneous metamorphosis; but the action induced by the saliva is incomparably more rapid even than that of any of these substances.

The following table contains the results of a large number of experiments made by Bidder and Schmidt, with the view of determining the metamorphic action of various animal substances on a solution of starch.

Substances which were mixed with the solution of starch.	Period when the formation of sugar commenced.
1. Saliva of adult men	The formation of sugar began instantaneously, but it was only in No. 1 that the whole of the starch was so changed that iodine induced no blue tint; in the other experiments the complete change was effected in various times, the longest being one hour.
2. Nasal mucus of ditto	
3. Saliva of a child aged four months	
4. Saliva of dogs	
5. Pancreatic juice of ditto	
6. Pancreatic tissue of ditto	
7. Parotid tissue of adult pig	
8. Pancreatic tissue of ditto	
9. Gastric juice of dogs which was rendered neutral by their swallowing the saliva	
10. Mucus from the urinary bladder of the pig	30 minutes.
11. Saliva of dog, the parotid secretion being excluded	20 minutes.
12. Pancreatic tissue of a dog ten days old	40 minutes.
13. Tissue of submaxillary gland of adult pig	1 hour.
14. Hepatic tissue of the same animal	1 hour 20 minutes.
15. Muscular coat of bladder of the same animal	1 hour 30 minutes.
16. Acid gastric juice of dogs in which there were no epithelial cells from the buccal mucous membrane	1 hour 30 minutes.
17. Tissue of submaxillary gland of dog ten days old	2 hours 15 minutes.
18. Parotid tissue of ditto	3 hours.

Substances which were mixed with the solution of starch.	Period when the formation of sugar commenced.
19. Mucus from the mouth of a dog whose salivary ducts had been tied a fortnight previously 20. Aqueous extract of the detached buccal mucous membrane of ditto . . . . . 21. Parotid tissue of ditto . . . . 22. Tissue of submaxillary gland of ditto . . .	After 3 or 4 hours, traces of sugar appeared, but the solution of starch remained thick and viscid.
23. Parotid secretion of the dog . . . . . . 24. Submaxillary secretion of ditto	Traces of sugar appeared after 8 hours.
25. Orbital gland secretion of ditto	No trace after 7 hours.
26. Saliva after the exclusion of the submaxillary secretion	No sugar after 2 hours.
27. Acid gastric juice of a dog whose parotid and submaxillary ducts had been tied . . .	No trace of sugar after 15 hours.

There are two points to be noticed in regard to the second of these experiments: in the first place, the facility with which a little saliva may become mixed with the nasal mucus, in sneezing and similar movements; and in the second, the circumstance that in other similar experiments with nasal mucus, the change did not commence until after a quarter of an hour, or even a longer space of time, had elapsed.

The question regarding the part which the various secretions entering into the composition of the saliva take in this action, is a comparatively recent one, and much light has been thrown upon this subject by the investigations of Bernard and others. The first step in this direction seems to have been made by Magendie and Rayer, who ascertained that the parotid secretion of the horse exerted no metamorphic action on starch; very shortly after this, Bernard demonstrated the same thing with both the parotid and submaxillary secretions of the dog. To the parotids he applied the term *glandes aquipares*, and to the submaxillaries that of *glandes mucipares*, the aqueous secretion of the former tending to liquefy the food in the process of mastication, while the viscid character of the latter is to facilitate deglutition. That the conversion of the starch into sugar depends solely upon the secretion of the buccal mucous membrane is further demonstrated, according to Bernard, by the circumstance that the fluid obtained by macerating this membrane in water possesses, after filtration, the power of effecting this change. Since, however, it might be objected that some of the salivary secretion might adhere to the mucous membrane, Bidder and Schmidt performed the experiments marked 19 and 20 in the above table, and obtained results by no means in accordance with Bernard's views. As the accuracy of this statement, as originally promulgated by their pupil, Jacubowitsch, has been impugned by Frerichs,* they have instituted new experiments.

"We have consequently," they observe, "obtained not only the pure secretions of the parotid and submaxillary glands, but also the pure secretion of the buccal mucous membrane of dogs. We have further obtained the parotid secretion of the ox, although we did not succeed in procuring the pure submaxillary secretion. The secretions thus obtained were individually mixed with a solution of starch, and exposed to a temperature of 104° F. In no case was sugar detected sooner than in eight hours by the application of Trommer's test, and then only in mere traces; and hence we must most decidedly repeat, that the ferment on which the conver-

* Op. cit. p. 773.

sion of starch into sugar depends is not contained in any single one of the secretions by whose admixture the ordinary saliva is formed, and that it has its source solely in the admixture of these secretions." (Bidder u. Schmidt, p. 19.)

The next question is—Are all the three secretions (those of the parotids, the submaxillaries, and the buccal mucous membrane, for that of the sublingual glands is neglected as unworthy of notice) of equal importance in producing the final result, or would the admixture of two of them be sufficient? Jacubowitsch performed some admirable experiments, which in themselves seem quite sufficient to settle the question definitely. He convinced himself, by preventing the flow of the parotid and submaxillary secretions from entering into the mouth of a dog, that the mere secretion of the mucous membrane of the mouth (contrary to Bernard's assertion) was unable to convert starch into sugar. But when he tied the ducts of only a single pair of glands (either of the two parotid or the two submaxillary glands), and suffered the dog to recover from the effects of the operation, and then, according to Bernard's method, digested starch with the saliva that exuded from the mouth of the dog, some of the starch was converted into sugar in the course of five minutes. Starch was also quickly changed, when brought in contact with an artificial admixture of either of the above-named glandular secretions and buccal mucus. A mixture of the parotid and submaxillary glands, without any secretion from the mucous membrane, was entirely deficient in this property.

These experiments have been subsequently repeated by Bidder and Schmidt, and the only important point in which their results differ from those of Jacubowitsch is, that, according to them, parotid saliva, mixed with pure buccal mucus, exerts no marked action on the conversion of starch into sugar. They are unable to account for this difference in any very satisfactory manner. In the hands of these admirable experimenters, a mixture of the secretion of the submaxillary glands with pure buccal mucus exerted as rapid and perfect a metamorphosis of starch as the ordinary saliva.

The following are the two principal conclusions at which Bidder and Schmidt have arrived in connexion with these points:

1. They agree with Bernard in regarding the parotids as *glandes aquipares;* in short, as yielding a secretion which is unquestionably intended to moisten and saturate the dry food, but whose principal object is connected with the general metamorphosis of the fluids within the body, and which is devoid of any marked action on starch.

2. By the union of the submaxillary secretion and that of the buccal mucous membrane, there is formed that peculiar ferment which almost instantaneously converts starch into sugar. This active principle is not contained in the cells or other solid particles suspended in the saliva; for the filtered fluid exhibits an undiminished force; and, indeed, this property is not destroyed when, by the addition of a little alcohol, we precipitate the mucus and (entangled in it) these solid particles.

Another subject worthy of notice, to which these authors have referred, is the condition of the salivary secretion during the period when the infant or the young animal continues sucking. Although to all appearance there is no special retardation of the development of the tissues of these glands, yet at this time they yield no secretion whatever. This was demonstrated in the following way. (1) On establishing fistulous openings in connexion with Steno's ducts in calves, no fluid escaped through the canula; (2) Starch was converted into sugar in the presence of portions of the parotid or submaxillary glands of adults (see experiments 7 and 13 in the table) in a far shorter period than when the corresponding parts of sucking animals were used (see experiments 17 and 18); (3) when the saliva of an adult man and that of a child at the breast were respectively mixed with equal parts of a thick solution of starch, the metamorphic action *commenced* in equally short spaces of time (that is to say, almost instantly); but there was this striking difference, that in the former case, the action was *completed* almost as soon as it was begun, whilst in the latter the process occupied fully an hour.

Formerly much importance was attached to the alkalinity of the saliva (which, by the way, seems rather to be due to the presence of the alkaline phosphate of soda than to that of a free alkali), and it was regarded as an undoubted fact (determined by *à priori* reasoning), that the special functions of this secretion must be altogether suspended when it entered the stomach and was mixed with the gastric juice. There is still considerable obscurity and diversity of opinion regarding the question, whether the action of the saliva on starch is continued in the stomach. Jacubowitsch, Frerichs, and Bence Jones believe that the metamorphic process is continued in the stomach, and Lehmann and Carpenter support this view, while the experiments of Bidder and Schmidt lead to an opposite result. Seeing no way in which this discrepancy can be satisfactorily explained, we shall lay before our readers, as briefly as possible, an abstract of the experiments on which each view is based. We may premise that there seems in this case to be some unaccountable difference between the actual process that goes on within the stomach, and imitations of it carried on in the laboratory. With respect to even the latter class of experiments, we may observe, that observers are by no means in accordance.

The external relations of the gastric juice to the saccharifying power of the saliva were tested by Jacubowitsch in the following manner:

Pure filtered gastric juice (obtained from a dog with an artificially induced gastric fistula) with a strong acid reaction, and presenting no histological elements under the microscope, was neutralized with strongly alkaline filtered human saliva, and mixed with a fresh decoction of starch.

Another portion of the same saliva was treated with a sufficient quantity of the same gastric juice, to give the mixture a decided acid reaction; and this was also mixed with a fresh decoction of starch.

Both mixtures, after standing for two hours at a temperature of about 100°, plainly indicated the presence of sugar on the application of Trommer's test.

The following mixtures were also made, and treated with a fresh decoction of starch:

(*a*) Pure filtered gastric juice was neutralized with human saliva; (*b*) In another instance it was rendered alkaline by an excess of saliva; (*c*) In a third case it was rendered alkaline by soda; (*d*) Saliva was rendered acid by gastric juice; (*e*) Pure gastric juice was finally mixed with starch.

All these mixtures, excepting *c* and *e*, being kept at a temperature varying from 86° to 104°, in the course of fifteen minutes exhibited distinct traces of sugar.

These experiments show, as Jacubowitsch observes, that the gastric juice does not in itself impede the metamorphic action of saliva on starch; and further, that the former secretion, even when neutralized with an alkali, possesses no such property.

More decisive experiments have been subsequently published by Bidder and Schmidt.

> "New experiments," they observe, "have convinced us that the metamorphic action of the saliva, even in the presence of free acids, exhibits itself with quite the ordinary rapidity. We have added to fresh human saliva enough acid gastric juice obtained from dogs, or enough hydrochloric acid, to render the mixture neutral or acid, and have found that this mixture caused as immediate a conversion of starch into sugar, as when pure alkaline saliva had alone been added."

Now, since the gastric fluid may be regarded as a natural mixture of the swallowed alkaline saliva with the acid gastric juice, in which, however, the acid character normally prevails, it might be expected that it would exert the same metamorphic action on starch as the artificially acidulated saliva. In reference to this point, the experiments of Bidder and Schmidt seem to indicate, what indeed might naturally be expected, that the action of the gastric fluid on starch is directly proportioned to the quantity of saliva that has been swallowed. Thus, when the gastric fluid is alkaline, in consequence of the quantity of saliva that has been swallowed, as is commonly the case in animals that have been kept fasting for some time, or when it exhibits large patches of frothy saliva, it occasions an immediate metamorphosis of the starch; but when it is strongly acid, and contains only little or no buccal epithelium, and is therefore mixed with only a little saliva, it does not begin to affect a solution of starch in less than an hour and a half, and then only slightly. In the latter case, the action of the saliva is not neutralized, but merely impeded by the extreme dilution.

From these experiments it might be concluded that starch would be rapidly converted into sugar in the stomach; but numerous observations instituted by Bidder and Schmidt, upon the lower animals (dogs, sheep, and rabbits), in which the starch was introduced into the stomach, either by an elastic tube passed down the œsophagus, or by means of a gastric fistula, and was re-obtained, after a certain interval, for examination by a corresponding process, seem to afford a conclusive demonstration that no conversion into sugar takes place within this organ. It may be objected that a solution of starch usually passes very rapidly from the stomach into the small intestine; but even when starch was found abundantly in the stomach several hours after meal-time, no trace of sugar could be detected.

Now, seeing, in the first place, that the formation of sugar commences in the mouth immediately upon the starch coming in contact with the saliva, and further bearing in mind that no sugar can be usually detected in the stomach, it follows that one of two things must occur—either that the newly-formed sugar is at once absorbed, or that it is converted into lactic acid, and in this form mixes with and is lost in the gastric juice. In the latter case, we must ascribe to the gastric mucous membrane the power of instantaneously converting sugar into lactic acid—a power which as we learn by experiments, is possessed neither by the stomachs of herbivora nor carnivora; in the former case we can only say, that so rapid an absorption is perhaps the least improbable mode of explanation.

We are bound to remark, that the above opinion of Bidder and Schmidt, regarding the absence of sugar in the gastric contents of animals fed upon starch, has not met with universal acceptation. It is true that it is in accordance with views previously expressed by Blondlot,* and by Bouchardat and Sandras,† but it is opposed to the results obtained by Frerichs, who "in at least fifty experiments" constantly found sugar in the filtered contents of the stomachs of men, carnivorous and omnivorous mammalia, and birds, and to those of Jacubowitsch. Indeed, the experiments of the last-named physiologist are so well devised, and their results are apparently so conclusive, that we shall record one or two of them, and allow our readers to weigh for themselves the evidence on both sides. A dog with an artificial gastric fistula was kept fasting for twelve hours and then fed with boiled starch. On examining the contents of the stomach four or five hours afterwards, by the aid of the fistulous opening, it was invariably found that the whole of the starch which the dog had taken was converted into sugar; the evidence of this fact being afforded by the application of Trommer's test, and of the microscope. To complete the demonstration, he introduced freshly-boiled starch through a tube into the stomach of another dog, which had been similarly prepared for digestive experiments, and whose salivary ducts had been likewise tied. This animal had also previously fasted for twelve hours; on removing some of the contents of the stomach, at intervals of about an hour, for nine and a half hours afterwards, it was demonstrated by Trommer's test, by the microscope, and by the application of iodine, that there had been no conversion of the starch into sugar, or even into dextrine. This experiment was several times repeated with precisely similar results.

Lehmann (in noticing the principal results of Bidder and Schmidt, in the third volume of his 'Physiological Chemistry') confirms the views he had independently expressed in his second volume‡ prior to the publication of their work, and observes, that in the numerous experiments he has made, he has always found sugar in the stomach, although often in small quantities, in animals fed upon starch. The presence of the sugar in these cases was determined, not merely by Trommer's test, but also by fermentation and other means.

The saliva exerts no definite metamorphic action on any of the carbohydrates except starch: according to Lehmann, cane-sugar, gum, vegetable mucus (bassorin), and cellulose, remain unchanged in the saliva; it is only in certain species of sugar, and after long-continued digestion, at

* Traité de la Digestion. p. 107. † Comptes Rendus, tom. xx. pp. 143—148.

‡ "It can no longer be doubted that the saliva in the normal condition in which it is mixed with the food, possesses the property of converting starch into sugar; and this being the physiological function of the saliva, its action must not be confined to the short time in which the food remains in the mouth, but must also be continued in the stomach and intestines. Now we may readily convince ourselves that this is really the case by observing what occurs in an animal in whom a gastric fistula has been established; for while pure gastric juice exerts no action on starch, sugar may be detected by the ordinary means in the stomach of the animal in ten or fifteen minutes after it has swallowed balls of starch, or after they have been introduced through the fistula. Hence it cannot be doubted that the saliva, after it has been mixed with the other animal secretions, continues to exert its action on the *amylacea* in the digestive canal" (Vol. ii. pp. 31, 32.)

a high temperature, that we observe the formation of lactic and subsequently of butyric acid. We shall revert to this subject in treating of the digestion of the carbo-hydrates generally.

The saliva exerts no action whatever on albuminous and gelatigenous foods; its utmost effect being to relax their tissues like pure water, and thus to render them more accessible to the action of the special secretion prepared for their transformation, the gastric juice.

Kletzinsky has attempted to determine the importance of the sulphocyanide of potassium in connexion with the digestive process.

He found that amylaceous and albuminous bodies, when digested at a temperature of from 100° to 110°, with very dilute solutions of this salt, underwent no change—that no sugar was formed from the starch, and that coagulated albumen was not dissolved; a solution of sugar with which yeast had been mixed *did not ferment* when sulphocyanide of potassium was added; and the experimenter's saliva exerted the same effect, while the same secretion in a case of mercurial ptyalism did not impede fermentation. Those low fungous growths, the Oidium auranticum and Penicilium glaucum, were destroyed by sulphocyanide of potassium; and in certain cases of fungous formations in the mouths of children, this salt was found to be scarcely detectible in the saliva. Kletzinsky's opinion obviously is, that it is the function of the sulphocyanide of potassium to check too rapid decomposition and the noxious development of fungous growths within the system. The subject, however, requires further investigation.

There is one additional point in connexion with the chemistry of the saliva to which allusion should perhaps be made—namely, the intensely poisonous character which it assumes in rabies. Our knowledge on this point is altogether negative. That the active agent in these cases is the sulphocyanide of potassium, which is assumed to be increased in rabies, is improbable, because this salt has been given to dogs in doses of 2 and even 4 grammes (31 to 63 grains), without producing any decided poisonous effects; and as it was re-discovered in the urine, it must have passed unchanged through the blood. An analysis of the secretion of the poison-glands of some of the venomous serpents might possibly throw some light on this subject.

The secretion of the *glandular apparatus* embedded in and forming the chief bulk of the mucous coat of the *stomach* next claims our attention. Many of our best recent observers (amongst whom we may especially mention Hübbenet,* a pupil of Bidder and Schmidt's) believe that two perfectly distinct secretions are found in the stomach—namely, the true *gastric juice* and *gastric mucus*. Almost all observers since Beaumont† have agreed in describing the former as a clear aqueous or only slightly viscid fluid with a strong acid reaction, and devoid of morphological elements—that is to say, of *essential* morphological elements; for on a microscopic examination, a few solid particles, consisting partly of unchanged cells of the gastric glands, partly of the nuclei of these cells, and partly of

* De Succo Gastrico, 1850.

† Experiments and Observations on the Gastric Juice and the Physiology of Digestion. United States, 1834.

fine molecular matter, which is produced by the disintegration of these elements, may usually be observed: it is only secreted during the period when digestion is actually going on, and it is this fluid alone which possesses any true digestive power. All that is known of the latter (the mucus) is, that it accumulates in the empty stomach, that it is neutral (unless when slightly alkaline from the commixture of saliva), and that it exerts no solvent action on the protein-compounds. Frerichs,* however, whose views on the Chemistry of Digestion are entitled to the highest respect, holds a different view, and maintains that at the commencement of each act of digestion the round cells in the interior of the glands escape in excessive quantities, and form a stratum of about a line in thickness, which has been hitherto regarded as mucus, and which either invests the interior of the stomach, or surrounds the contents in the form of a white membrane, the latter being especially the case when dry food is taken. The gastric cells become gradually disintegrated during the continuance of the digestive process, and thus afford a continuous source of pepsin or ferment. The digestive act being accomplished, the gastric glands collapse, and in that state no nuclei or cells, and only a few scattered granules, escape from them. During abstinence from food, the morphological elements are again perfectly formed, and the tubes become filled with cells, which probably in very prolonged fasting again become disintegrated.

There are two points in connexion with the secretions of the gastric mucous membrane to which Kölliker† has directed special attention, and which we shall now lay in a condensed form before our readers.

The first is, that *in many of the mammalia the gastric mucous membrane is covered with a more or less thick coat of mucus during digestion.* This fact was originally noticed by Eberle‡ in the stomachs of rabbits, and he clearly regarded the mucus as essential to digestion. The next writer who referred to it was Bischoff,§ who described it as the ejected contents of the gastric glands. Henle‖ also maintains that the cells which are produced from within the gastric glands form a tolerably thick layer, which invests the contents of the stomach during digestion. After referring more fully than we have done to these authorities, Kölliker observes that he has found this mucus constantly present in the stomachs of certain animals examined immediately after death, as, for instance, in rabbits, pigs, oxen (in the fourth stomach), and horses; whilst in other animals, as in dogs and sheep, it is altogether absent, or only present in very small quantity. When present, it by no means consists solely of the roundish cells from the gastric glands (gastric cells), and, indeed, often contains no trace of them. In the pig and in the rabbit it chiefly consisted of desquamated cylindrical epithelium. Taking all these facts into consideration, and recollecting that, altogether independently of digestion, a small quantity of mucus is found in the stomach, we hardly feel inclined to adopt the view that the gastric mucus should be regarded as necessary and essential to digestion, although it possibly may exert a peculiar influence on the digestive process in certain parts of the stomach, or in certain animals, in so far as it actually contains the contents of the gastric glands.

* Op. cit., p. 749.
† Mikroskopische Anatomie, vol. ii. p. 144.
‡ Physiologie der Verdauung, 1838, p. 74
§ Müller's Arch., 1838, p. 513.
‖ Allgem. Anatomie, p. 910.

The second point to which Kölliker especially adverts is, that *in many animals the glands of the stomach occur under two distinct forms, and yield two different kinds of secretion.* This is a subject which we shall enter into somewhat fully in a subsequent article on the microscopic anatomy of the digestive canal. It will be sufficient here to state that Kölliker holds that the gastric glands present differences in different classes of animals; thus, in dogs there are glands with cylindrical epithelium in the pyloric region, and glands with roundish cells in the remaining parts of the stomach, and the same is the case in the ruminants (in their fourth or true stomach) and in rabbits; whilst in swine it is only in the central part of the stomach, especially in the greater curvature, that the former glands are situated. Goll, who, with the assistance of Kölliker, has recently made a series of experiments on artificial digestion with fluids prepared from the stomach of the pig, regards it as an established fact, that the digestive powers exercised by the secretions of these glands are altogether different, and that acidified mixtures prepared from the glands with round cells very rapidly dissolve coagulated protein-compounds, while those prepared from the glands with cylindrical epithelium either exert no action whatever, or only a very slight action after a long period. The following remarks give the substance of Kölliker's views on this point. A very acid reaction is invariably observed on that part of the gastric mucous membrane in which the complicated glands lie, and the so-called pepsin must necessarily be situated here, since the mucous membrane of this part yields an energetic digestive fluid: indeed, it could hardly be presumed that pepsin was secreted by the glands lined with cylindrical epithelium, and the very slight effects which were occasionally observed to be slowly produced by the digestive fluid prepared from these glandular structures were probably due to the presence of the nitrogenous matters which occur here, for all mucous membranes yield with distilled water and a little acid a fluid possessing *some* solvent power. The acid reaction exists in the whole stomach, and Kölliker observes that the more simple glands, which do not secrete pepsin, may possibly yield an acid juice; he adds, however, that the reaction is always weakest where merely the simple glands are situated. With regard to the histological elements yielded by these structures, he states that the glands with cylindrical epithelium give forth a secretion containing no visible objects excepting a few epithelium cells from their upper part, while the true gastric glands, at all events in certain animals, as for instance rabbits, &c., yield not only cylindrical, but also roundish cells. These latter cells, however, do not seem to be invariably present in the gastric juice, and in many animals this secretion apparently contains no visible particles. There can be no question that it is these comparatively large, round cells which secrete the pepsin, which either oozes through their walls, or is liberated by their solution.

Various methods have been devised for obtaining the gastric juice for the purpose of analysis. Formerly, the only means of obtaining it in any quantity was to feed animals which had been for a long time kept fasting, and to kill them in from ten to thirty minutes afterwards. The gastric juice used by Tiedemann and Gmelin, in their celebrated experiments, was all collected in this manner, except that, in place of feeding the animals, they used irritant and insoluble substances, such as peppercorns and pebbles. Spallanzani, Braconnot, and Leuret and Lassaigne, obtained gastric juice without killing the animals, by making them swallow sponges attached to a string, and after some time withdrawing them from the stomach. Both these methods presented many drawbacks; the former required the death of a large number of animals, the latter yielded only very small quantities of an impure gastric juice. The best method of obtaining this secretion in the greatest possible purity is by the establishment of artificial gastric fistulæ. This method of procedure was doubtless suggested by Dr. Beaumont's admirable description of the case of Alexis St. Martin. We are indebted to Blondlot* for first producing such fistulous openings in dogs, and his method has been much improved and simplified by Bardeleben.† Finally, Hübbenet‡ has tied the salivary glands before establishing the gastric fistula, and has thus got rid of one great source of error, the admixture of saliva.

Our description of the physical and chemical characters of the gastric juice is principally taken from Hübbenet's thesis, and from the work of Bidder and Schmidt. We may premise that these experiments were made upon two dogs and upon a sheep. Fresh gastric juice, even when the animals have been kept for twenty-four hours or longer without food, is never perfectly pure, always containing remnants of food, swallowed hairs, sand, &c. The fluid, as it escapes from the canula inserted into the fistulous opening, often appears for a short time in large drops as a perfectly clear and limpid secretion, but the above-named extraneous admixtures soon re-appear, and give more or less colour to it. In the dog this colour was ash-grey or brownish, while in the sheep it was grass or olive green, from the remains of the food. There was always a much greater admixture of remains of food in the gastric juice of the sheep than in that of the dog, in consequence of the compound stomach and the slower digestion of ruminating animals. We need hardly remark that the fistulous opening was made into the fourth stomach of the sheep. The filtered fluid was always perfectly clear and transparent, being limpid or of a pale yellow colour in dogs, and of a light brownish tint in sheep. On examining the substances left on the filter with the microscope, there were found fragments of partially-digested muscular fibre and areolar tissue, fat-globules, either solid from cooling or still fluid, starch-granules, and fragments of vegetable tissues. (These latter form the chief bulk of the insoluble constituents of the gastric juice of the sheep.) Epithelial scales from the mucous membrane of the mouth were rare, and cylindrical epithelium from the stomach still rarer and quite exceptional; but there were constantly found accumulations of roundish corpuscles, about one three-hundredth of a line in diameter, which were unaffected by acetic acid, and were held together by, and imbedded in, a tough transparent mass. That all the above elements are unessential, and merely incidental, is, we think, sufficiently clear from the fact mentioned by Bidder and Schmidt, that the perfectly clear, filtered gastric juice is as efficient a digestive agent as the fluid in its original unfiltered state.

The gastric juice always presents an acid reaction; if it is apparently

* Traité analytique de la Digestion, considérée particulièrement dans l'homme et dans es animaux vertebrés. Paris, 1813.

† Arch. f. phys. Heilk., vol. viii. p. 1.

‡ De Succo Gastrico. Dorpati Livon. 1850.

neutral or alkaline, this must be owing either to the presence of an excess of mucus, or to the admixture of a large quantity of swallowed saliva. The method employed by Hübbenet, and by Bidder and Schmidt, to determine the strength of the acid reaction, was to treat a weighed quantity of fresh filtered juice with a solution containing one per cent. of potash, till litmus paper ceased to be affected. From nine experiments made on a dog whose salivary ducts had been tied, it appeared that, on an average, 100 parts of gastric juice were neutralized by 0·356 of potash; while from eleven experiments on a dog whose salivary ducts had not been tied, it appeared that 100 parts of the gastric juice were neutralized by 0·390 of potash. The apparent singularity of there being an excess of acid in the gastric juice into which the alkaline saliva had been allowed to flow uninterruptedly, may probably be explained by the fact that the first dog was kept solely on animal, and the second exclusively on vegetable food, and that the starch in the latter kind of diet was converted by the saliva into lactic acid, which thus contributed to the acid reaction. This explanation might at first sight seem hardly consistent with the fact that the same observers found that (taking the average of ten experiments) 100 parts of the gastric juice of the sheep was neutralized by only 0·264 of potash; but this difficulty vanishes when we consider the great excess of water in the gastric juice of this animal, the small quantity of amylaceous matter in its food (hay), and further, when we bear in mind that any changes which the starch has to undergo are probably undergone, and the products thereof are absorbed, before it could reach the fourth stomach. It is, moreover, worthy of notice that the gastric juice of the same animal, living on precisely similar diet, presents great fluctuations in the strength of the acid reaction: thus, the extreme quantities of potash required to neutralize the gastric juice of the dog whose salivary ducts were tied, and who lived solely on flesh, were 0·260 and 0·426; while in the dog whose salivary ducts were not tied, and who lived on vegetable diet, they were 0·286 and 0·570; and in the sheep they were 0·187 and 0·355.

The gastric juice contains two essential elements—a free acid, and a highly nitrogenous matter allied to the albuminates, commonly known as pepsin, but to which Hübbenet, and Bidder and Schmidt have given the name "ferment-substance." There is great diversity of opinion regarding both these substances. Prout* maintained that the acid in question was hydrochloric acid; and Tiedemann and Gmelin, and several other writers, have adopted his view. Lehmann,† on the other hand, asserts that lactic acid (either alone, or associated with hydrochloric acid) is the main acidifying agent; and his experiments have been repeated and distinctly confirmed by Heintz,‡ and are more or less supported by the observations of Bernard and Barreswill,§ Pelouze,|| Thomson,¶ and others. Blondlot, again, maintains that the acid reaction is solely due to the presence of acid phosphate of lime—an opinion utterly at variance with the results obtained by all trustworthy chemists, and undeserving of further notice. The

* Philosophical Transactions, 1824, p. 45.

† Physiological Chemistry (printed for the Cavendish Society), vol. i. p. 93, and vol. ii. p. 44: his original memoir is published in the Bericht d. Gesellschaft d. Wiss. zu Leipz., vol. i. p. 100.

‡ Jenaische Annalen für Physiologie und Medicin, 1849, vol. i. p. 222.

§ Journ. de Pharm., Jan. 1845, p. 49.

|| Comptes Rendus, vol. xix. p. 1227.

¶ Philosophical Magazine, vol. xxvi. p. 420.

question—*Does lactic acid exist as a normal constituent of the gastric juice?*—requires some investigation. Chemists and physiologists were, as far as we know, generally of opinion that Lehmann* had definitely proved, that "the acid of the gastric juice is perfectly identical with lactic acid," until Hübbenet (in his Thesis), and Bidder and Schmidt (in their comprehensive treatise on the Digestive Fluids), distinctly asserted that they could find no traces of this acid, and that the reaction was due solely to the presence of hydrochloric acid. Lehmann refers, in the third volume of his 'Physiological Chemistry' (now in the course of translation), to these experiments, in the following terms:

"One result of Schmidt's analyses is, that this admirable observer, in entire opposition to my investigations, not only discovered free hydrochloric acid in the gastric juice of dogs, but also demonstrated, beyond all doubt, the absence of lactic acid in all the cases which he observed. Although I had been long ago informed of this result, I have as yet been unable to ascertain the conditions on which the presence or absence of lactic acid in the gastric juice depends. Being unable, from circumstances, to experiment on a dog with a fistulous opening, I collected the gastric juice of fourteen dogs, which, from eight to sixteen hours previously to their death, were fed on horseflesh, and a quarter of an hour before their death on broken fatty bones. (The stomachs of most of the dogs did not contain even any fragments of flesh, but merely the pieces of bones.) In this gastric juice, lactic acid was most distinctly (*auf das evidenteste*) recognised by the form and the properties of its salts, and by its saturating capacity (as determined from the magnesian salt)."

The ferment-substance, and its action on albuminates when a free acid is present, have been carefully studied by Schwann, Wasmann, Lehmann, Frerichs, Hübbenet, and Bidder and Schmidt. It is precipitated by bichloride of mercury and acetate of lead, and is decomposed by a boiling heat and by anhydrous alcohol; it may be separated from the mercury and lead compounds by sulphuretted hydrogen, and on the addition of a few drops of dilute hydrochloric acid, possesses a great solvent power over coagulated albumen. Schmidt some years ago suggested, and still adheres to the ingenious view, that the gastric acid should be regarded as a compound of the ferment-substance (Wasmann's pepsin) with hydrochloric acid. This theory, and the objections to it, are so clearly described by Lehmann, that we need offer no apology for introducing his remarks on the subject:

"Schmidt regards the digestive principle as a conjugated acid, whose negative constituent is hydrochloric acid, with Wasmann's non-acid or coagulated pepsin as an adjunct; and assumes that it possesses the property of entering into soluble combinations with albumen, gluten, chondrin, &c.; according to him it more nearly resembles ligno-sulphuric acid than any other conjugated acid, and as this becomes disintegrated into dextrine and sulphuric acid, so the *pepsin-hydrochloric acid* becomes separated at 212° F. into Wasmann's coagulated pepsin and hydrochloric acid: and in either case it is equally impossible to reproduce the conjugated acid from its proximate elements, after their separation. On bringing the complex acid in contact with an alkali, the adjunct—the substance which has been in combination with the hydrochloric acid—is precipitated. Schmidt believes that he has ascertained that an artificial digestive mixture, which has expended its solvent and digestive powers, regains them on the addition of free acid; and that, when hydrochloric acid is added, the pepsin-hydrochloric acid is expelled from its

* Op. cit. vol. i. p. 93.

combinations with albumen, &c., and thus regains its former properties, whilst the newly-added hydrochloric acid enters into its well-known soluble combinations with albumen, &c. By the repeated addition of hydrochloric acid, a digestive fluid or this pepsin-hydrochloric acid might preserve its digestive powers for ever, unless the fluid became saturated with the dissolved substances, or the conjugated acid underwent decomposition.

"Ingenious as this view of Schmidt's undoubtedly is, and singularly as it seems to harmonize with certain facts, there are other and very important facts which appear to render its correctness doubtful. The existence of the pepsin-hydrochloric acid has not been recognised by any analysis of a combination of it with a mineral base, or with an albuminous substance. Although I have instituted numerous experiments regarding the quantitative relations between the digestive fluid and the substances to be digested, I cannot ascertain that there are any such proportions between the acid and the digested substance as at all accord with the ordinary acid or basic combinations of acid and base; and further, the digested substances separated by the acid are altogether different from the original albumen, fibrin, casein, &c., which, however, according to Schmidt, combine in a simple manner with this complex acid, and then directly undergo solution." ('Physiol. Chem.' ii. pp. 49, 50.)

We must therefore regard Schmidt's view as untenable, and consider the free acid (lactic or hydrochloric, as the case may be) and the pepsin or ferment, as independent although essential constituents of the gastric juice. According to Schmidt the ultimate composition of the ferment is, C 53·0, H 6·7, N 17·8, and O 22·5.

The mean composition of the gastric juice is given in the following table, which is drawn up from nine analyses of the fluid of the dog A (whose salivary ducts were tied), from three analyses of the fluid of the dog B (whose salivary ducts were not tied), and from two analyses of the fluid of a sheep (whose salivary ducts were not tied). These analyses were performed by Hübbenet under Schmidt's direction.

	Gastric juice (without saliva) of dog A.	Ditto (mixed with saliva) of dog B.	Ditto of sheep.
Water	973·062	971·171	986·147
Solid residue	26·938	28·829	13·853
Ferment or pepsin	17·127	17·336	4·055
Hydrochloric acid	3·050	2·377	1·234
Chloride of potassium	1·125	1·073	1·518
Chloride of sodium	2·507	3·147	4·369
Chloride of calcium	0·624	1·661	0·114
Chloride of ammonium	0·468	0·537	0·473
Phosphate of lime	1·729	2·294	1·182
Phosphate of magnesia	0·226	0·323	0·577
Phosphate of iron	0·082	0·121	0·331

If our space permitted, we should extract at length the tables in which Bidder and Schmidt have recorded their numerous experiments on the digestive power of the gastric juice upon coagulated albumen. Some idea of the extent of their labours may be formed, when we mention that they have recorded *forty-five series* of independent experimental inquiries, embracing no less than *one hundred and forty* individual experiments. These experiments are divided into two great classes: those performed externally to the organism, and those actually performed within the stomachs of living animals. The following are the principal points which they have investigated:

1. The question, whether the saliva takes any part in the gastric digestion of nitrogenous bodies.
2. The influence of a greater or less quantity of free acid in the gastric juice.
3. The action of neutralized or alkaline gastric juice.
4. The difference in the action of filtered and unfiltered gastric juice, with the view of ascertaining the influence of the organic histological elements suspended in it.
5. The influence of greater or less concentration and dilution of the gastric juice on its digestive power.
6. The influence of heat, the addition of bile, &c., on the digestive power of the fluids of the stomach.

Their experiments lead to the following conclusions:

The admixture of saliva with the gastric juice is of no service, in so far as the solution of albuminous bodies is concerned. It appears from the tables to which we have referred (Bidder and Schmidt, pp. 81—83), that in experiments with gastric juice containing saliva, the number expressing the dissolved per-centage of dry albumen was not so high as in corresponding experiments (both sets of experiments being made within the stomachs of dogs) when the saliva was excluded. Whilst in the latter the averages of nine experiments of two hours', nine of four hours', and nine of six hours' duration were 29, 62, and 76 per cent., in the former they were only 26, 45, and 65 per cent. This difference is unquestionably dependent upon the partial neutralization of the free acid of the gastric juice by the saliva.

Both classes of experiments agree in showing that the acid gastric juice possesses a specific and peculiar power of dissolving coagulated albumen; and if this solution proceeds with more rapidity in the stomach than externally to the organism, the reason is, that in the former the albumen is kept in constant motion, and is being always brought in contact with fresh quantities of gastric juice. Several of Bidder and Schmidt's experiments show, that provided a sufficient quantity of pepsin be present, the digestive power of the gastric juice varies in a direct ratio with the amount of free acid that it contains. That the solvent power of the gastric juice is not solely due to the presence of hydrochloric acid is, however, evident from the results of several experiments, which showed that this acid, in a diluted state, could only, at the most, exert one-fourth part of the solvent action on coagulated albumen which was exerted by the gastric juice itself. The neutral or alkaline gastric mucus of fasting animals dissolved only very slight quantities of albumen, but on adding hydrochloric acid the activity of the fluid was at once augmented. On neutralizing the free acid of the gastric juice with potash, its digestive power is destroyed.

With regard to the effects of filtration, they observe that repeated experiments have convinced them that gastric juice from which all undissolved organic matters have been separated acts with the same efficiency as that which has not been filtered. This is obviously at variance with the view held by Frerichs, that new ferment continues to be developed from the cells contained in the gastric juice by the action of the free acid.

The experiments on concentrated and diluted gastric juice led to no definite result: indeed, no experiments seem to have been made with the concentrated fluid till it had been re-diluted with water.

It was proved by several experiments that the solvent action of the gastric juice is destroyed by rapid ebullition, or by evaporation at a boiling heat—an additional illustration of the well-known fact, that ferments are decomposed at an elevated temperature. This may possibly be the basis of the universally recognised dietetic rule, which declares all very hot foods or drinks to be injurious. On the other hand, the solidification of the gastric juice by artificial cold did not destroy its solvent powers—a circumstance which, as our authors suggest, may be consolatory to those who are addicted to ices.

It was distinctly shown by three experiments, that the admixture of bile with the gastric juice—even when the quantity is so small that the mixture retains a distinctly acid reaction—completely destroys the solvent power of the latter secretion upon coagulated albumen.

The quantity of the gastric juice secreted in twenty-four hours was determined by Bidder and Schmidt, in the case of dogs, to be about one-tenth of the whole weight of the body. Assuming the same ratio to be true for men, a man of ordinary weight (say from ten to twelve stone) would daily secrete no less than *from fourteen to about seventeen pounds of this fluid.* This is a very different estimate from that formed by Lehmann, whose calculations were based on the following data:

100 grammes of the fresh gastric juice of a dog cannot, according to Lehmann's experiments, effect the solution of more than 5 grammes of coagulated albumen (calculated as dry).

An adult man receives into the stomach about 100 grammes of dry albuminous matter in twenty-four hours.

Hence, to digest this quantity, there must be secreted 2000 grammes, or four pounds of gastric juice.

Bidder and Schmidt differ very considerably from Lehmann in their estimate of the quantity of albumen which 100 grammes of the fresh gastric juice of a dog can dissolve: as a mean of twenty-seven experiments, they found that the quantity was only 2·2 grammes, their maximum being 3·95; whilst in eight experiments made by Lehmann the maximum was 6·14, and the minimum 4·317. Can this discrepancy be owing, as Lehmann hints, to the fact that his gastric juice contained lactic acid, while Bidder and Schmidt's was destitute of that acid?

The last-named investigators attach great weight to the extraordinary amount of this very aqueous secretion: it dilutes the dissolved nutriment in such a manner that it is enabled to enter into endosmotic relations with the blood circulating in the intestinal walls, and, indeed, even with the chyle within the lacteals. Hence, within certain limits, the quantity of secreted gastric juice must be greater the less fluid is directly introduced into the stomach as drink. Dogs fed solely on flesh drink little or nothing, and hence it is far from impossible that in other animals which drink freely (as for instance man), the daily quantity of gastric juice is relatively less. To what extent this consideration may affect Bidder and Schmidt's estimate of the quantity of gastric juice secreted daily by the human stomach, it is impossible to say.

We now proceed to the consideration of the *bile*—a subject to which Bidder and Schmidt have devoted much experimental labour.

The first question they consider is the following—Is the bile to be regarded simply as an excretion? This point they have attempted to decide by observations on dogs in whom biliary fistulæ had been established.

A dog on whom this operation was performed lost two-fifths of its weight in thirty-four days, although its appetite was increased rather than diminished. The emaciation was especially marked both in the loss of fat and muscle. The hair fell off at many spots, or was loosened by the slightest touch. Digestion appeared to go on in an undisturbed manner, although the animal was in the habit of licking the bile as it escaped. The excrements had a greyish-yellow, and sometimes a partially green colour; but during the last few days of life regained their normal colour and odour. There were constant eructations of a bad odour, and the breath likewise had a very unpleasant smell. The urine was occasionally so concentrated, that its specific gravity was 1·050. The animal at length (on the thirty-fourth day) died from general exhaustion. Dissection showed that a minute new ductus choledochus had become formed, probably between the twenty-first and twenty-fifth day after the operation, at which time the fistulous opening presented a tendency to contract. The right half of the stomach presented numerous ecchymoses, both of older and of more recent standing, although there were no symptoms of disturbed gastric digestion during life— that is to say, there was no vomiting, and the appetite continued unimpaired. The rest of the intestinal tract presented nothing abnormal, except that it was of a somewhat deeper red tint than usual.

A second dog, similarly treated, did not present a voracious appetite, or exhibit the same amount of flatulence, or such a tendency to putrefactive decomposition as the former animal; but in the course of twenty-seven days (when it died) it had lost about one-half of its weight. Except that the fat and muscles had almost disappeared, none of the organs were found to present any marked peculiarity.

Since the quantity of bile that escaped from the system in these two cases was not compensated for by the additional food that was taken, the idea suggested itself that the emaciation and the disturbed condition of the metamorphosis going on within the tissues might be due to the continuous drain upon the system. This view was confirmed by the two following experiments, in which this disturbing cause was eliminated:

A dog, whose spleen had been extirpated, and in whom a biliary fistula had been established fourteen days afterwards, took such large quantities of food as more than to compensate for what was lost by the escape of bile. Its bodily weight, under these circumstances, did not materially diminish during the space of two months, at the end of which it was killed. The normal development and power of the muscles were retained, but the fat had gradually disappeared. Putrefactive decomposition of the excrements, and abundant flatus of a disgusting odour, were likewise observed in this case.

A very emaciated dog, whose spleen had not been extirpated, and in whom a biliary fistula had been established, regained its healthy appearance and its previous normal weight nineteen days after the operation. During this period it took more meat and bread than were requisite for it in its perfectly healthy condition. The ductus choledochus subsequently became re-established, and the weight then again materially increased.

From these experiments, it seems clear that the question which they attempt to solve cannot be directly and positively answered either in the affirmative or the negative. By an artificial interference with the organism (by the establishment of a biliary fistula) the bile may be reduced, in a certain sense, to the condition of an excretion, if the loss thus occasioned to the system at large can be suitably covered. If this compensation be impossible, the derivation of the bile from the intestinal canal and its direct external discharge in a comparatively short time induce fatal emaciation and debility. For the full comprehension of the question in all its bearings, we should know the quantity of bile secreted in a given time and the average amount of solid constituents which it contains, the share which it takes in the process of digestion, the changes which it undergoes during and after its resorption into the blood, and lastly, the form in which it is finally eliminated from the organism.

Their investigations regarding the amount of the hepatic secretion in different animals in a given time were very extensive. Experiments were made on cats, dogs, sheep, rabbits, geese, and rooks; and the details, which are pretty fully given, occupy nearly one hundred pages of their work. We have only space for a brief notice of their most important results.

We may incidentally mention that our knowledge upon this subject has been, until lately, so vague, that while some physiologists have calculated the amount of bile secreted in the human subject in twenty-four hours at only one ounce, others have considered that it amounted to twenty-four. The absolute number of grammes of bile secreted in twenty-four hours by the following animals, for one kilogramme of weight,* is given in the annexed table:

	Cats.	Dogs.	Sheep.	Rabbits.	Geese.	Rooks.
Fresh bile ...	14·500 ...	19·990 ...	25·416 ...	136·84 ...	11·784 ...	72·096
Dried residue ...	0·816 ...	0·988 ...	1·344 ...	2·47 ...	0·816 ...	5·256

The quantity of bile corresponding to a given bodily weight varies so much in different animals, as is obvious from this table, that we can draw no safe inference from it regarding the quantity daily secreted by man. They think it most probable, however, that an adult man secretes daily about a kilogramme and a half of this fluid (54 ounces), containing 5 per cent. of solid constituents.

They have devoted much attention to the determination of the differences in the amount of the biliary secretion at different times after a meal. They found that the secretion of bile is continuous, but that it is augmented or diminished according to the stage of digestion. It reaches its maximum thirteen or fourteen hours after a copious meal, and from then till twenty-

* That is to say, if a cat weighing one kilogramme (or about 2·2 pounds) secreted daily 14·5 grammes of bile, a cat weighing two kilogrammes (or about 4·4 pounds) secretes 29 grammes.

four hours after the meal, it gradually diminishes, till it reaches the same quantity that is secreted one or two hours after eating. In prolonged starvation, the quantity of this secretion gradually and progressively diminishes.

Their experiments, as likewise those of Nasse,* show that *the nature of the food* exerts a considerable influence on the amount of the biliary secretion. A flesh-diet induces a far more abundant secretion of bile than vegetable, amylaceous food. Thus, for instance, a dog on which Nasse experimented, when fed on bread and potatoes, secreted daily 171·8 grammes of bile (in which there were 6·252 grammes of solid matters); and when kept on a flesh-diet, secreted in the same period, 208·5 grammes of bile, in which the solid constituents amounted to 7·06 grammes: and Bidder and Schmidt found in their experiments on cats, that during a purely fatty diet, the secretion of bile diminished as much as if the animals had fasted for an equal period. Many of their experiments show, that after the abundant ingestion of water, not only is the actual quantity of bile increased, but also the quantity of its solid constituents.

Nasse found that large doses of carbonate of soda considerably diminished the secretion of bile, and especially of its solid constituents, while calomel augmented the quantity of fluid bile, but caused a diminution of its solid constituents. It was found both by Bidder and Schmidt, and by Nasse, that during febrile excitement, the amount of this secretion was always diminished.

Pure bile, as it escapes from a biliary fistula, is always a clear fluid, without any admixture of undissolved or insoluble matters. It was only when the animals used in their experiments were in a catarrhal state that there were present patches of desquamated cylindrical epithelium from the gall-bladder. Pure yellow bile (such as occurs in carnivorous animals, as cats, dogs, and rooks) may be rendered green by oxygen, and green bile (as that of herbivorous animals, as rabbits, geese, and sheep) may be rendered yellow by de-oxidation. When the bile continues for a long time in the gall-bladder, it undergoes a change of colour, which appears to be dependent on oxidation; it likewise becomes poorer in water, and receives an accession of mucus. The increased concentration of the bile consequent on its retention in the gall-bladder is very clearly demonstrated both by Bidder and Schmidt, and by Nasse. The former experimenters found that the fresh bile of cats, dogs, and sheep contained on an average 5 per cent. of solid constituents; in the case of dogs and cats, this might rise to 10 or even 20 per cent., according to the time it was retained in the bladder, while in the sheep it rose to only 8 per cent.; in rabbits, whose fresh bile contains 2 per cent., the quantity may rise to 15; and in geese and rooks, whose fresh bile contains about 7 per cent., it may rise to 20, or in the latter birds, to 25 per cent.

The reaction of pure bile is always neutral; if, however, the secretion be mixed with the mucus of the gall-bladder, it may appear to be alkaline; even then, on the precipitation of the mucus, the neutral reaction returns. Decomposed bile occasionally presents an acid reaction.

As Bidder and Schmidt express their entire concurrence in Strecker's views regarding the chemical constitution of the bile, they do not enter at any length into that subject, and we shall follow their example. We will

* Commentatio de bilis quotidie a cane secreta copia et indole Progr. 1851.

merely remind our readers, that every kind of bile contains two essential constituents—namely, a resinous and a colouring constituent.

The resinous constituent is the soda or potash salt of one of the conjugated acids, glycocholic or taurocholic acid, whose respective adjuncts are glycine (or glycocoll) and taurine.

The colouring matter exists in combination with an alkali. (For a full notice of these substances, we may refer to Lehmann's 'Physiological Chemistry,' vol. i. p. 222, and vol. ii. p. 62.)

We shall now select for notice some of the most interesting physiological points which have been elucidated by Bidder and Schmidt's experiments.

In order to decide whether the constituents of the bile are resorbed into the intestine, or whether the greater portion of them is removed with the fæces, they fed a dog first with black bread, then with flesh, and then again with black bread. The external appearance of the fæces differed so strikingly with the difference of the diet, that they could tell with precision which fæces pertained to the flesh-diet. The analysis of the fæces passed during the period of this diet, showed that only very little of the bile effused into the intestine was contained in them; and this view was further confirmed by a comparison of the quantity of sulphur contained in these fæces with the quantity of that element which must have been contained in the bile. They are hence led to the conviction, in opposition to the opinions of Mulder and Frerichs, that by far the greatest portion of the solid biliary constituents, probably seven-eighths, is returned into the mass of the juices, in order to undergo further metamorphoses, and finally to be eliminated from the body in some other way.

The antiseptic properties of the bile are next considered—a subject to which Hoffmann* has lately directed special attention. When allowed to escape externally, its absence from the bowels occasioned a strong putrid decomposition of fleshy diet, and an excessive acid fermentation of vegetable food. Our authors confirm Hünefeld's statement, that while bile freed from mucus dissolves the blood-corpuscles, the natural bile does not possess the power. They also show, by numerous experiments, that the bile exerts no influence on the solution or digestion of albuminous or amylaceous bodies.

The action of the bile on the digestion and resorption of fat is next considered. As, however, the subject of *absorption* generally will probably be discussed in an early number of this Review, we will content ourselves with stating, that the general result at which Bidder and Schmidt arrive in connexion with this subject is, that fats may be digested and resorbed when no bile is effused into the intestine; but that the quantity of resorbed fat is greater when the secretion is not interfered with. This influence of the bile is perceptible in the composition of the chyle which, after the ductus choledochus has been tied, is found to be relatively poor in fatty matters.† The bile seems to render the intestinal mucous membrane more permeable for fats. Oil rises higher in a capillary tube moistened with bile, than in a similar tube not wetted with this secretion. It is very probable that the voracious appetite of dogs with biliary fistulæ is owing, not merely to the waste of bile, but also to the circumstance that the

* Häser's Arch., vol. vi. p. 157.

† See Bidder and Schmidt, p. 226.

animals must take in a sufficient quantity of albuminous and amylaceous matters to supply the carbon that is deficient in consequence of the diminished resorption of fat.

It will be gathered from the preceding remarks, that, in the strict sense of the word, we can hardly regard the bile as a digestive fluid. There is no special class of substances whose solution is dependent upon it. If, as certain experiments show, animals with biliary fistula can live even for years,* the function of this fluid in digestion must, at all events, be a limited, and probably only an indirect one; and this is confirmed by the fact, that the secretion of bile does not attain its maximum till twelve or fifteen hours after food has been taken, when by far the greatest part of the ingesta must have passed beyond the duodenum; hence the greatest part of this fluid enters the small intestine at much too late a period to exert in it any great influence on the metamorphosis of the chyme.

If our space permitted, we should gladly enter into Bidder and Schmidt's general views regarding the main object of the biliary secretion in the animal economy, and the arguments with which they ingeniously, and we think (with some limitations) satisfactorily, support those views. We must, however, content ourselves with their final result, which is, that its principal object is "to prolong the series of changes to which animal matter is submitted within the organism, and thus to render it for a longer time efficient in the discharge of vital processes," in short, to economise animal tissue.

The *pancreatic juice* has recently been made the subject of special investigation by many able observers, amongst whom we must especially mention Bernard,† Frerichs,‡ and Bidder and Schmidt. The most complete analyses of this fluid are those of Frerichs, who examined this secretion in an ass; and of Schmidt, who examined the pancreatic juice of the dog. We shall chiefly follow Schmidt's description.

The pancreatic juice of the dog is a perfectly clear, limpid, colourless, transparent, viscid, and ropy fluid, in which no particles of any kind can be detected by the microscope. It has a strong alkaline reaction, and on the addition of alcohol (of 85 per cent.) becomes coagulated into a milky mass, which deposits thick white flakes, above which there is a clear, colourless, strongly alkaline alcoholic solution. (According to Frerichs, the addition of alcohol only renders it slightly turbid.) The flakes, when collected by filtration, washed with alcohol, and dried, may be for the most part redissolved in water, and then constitute a fluid resembling in consistence the original secretion. When exposed to a strong heat they leave a slight residue of carbonate of lime. The alcoholic solution leaves, on evaporation, a colourless albuminate.

Its specific gravity, according to Schmidt, is about 1·031; whereas, according to Lehmann,§ it ranges from 1·008 to 1·009. It is so prone (according to Frerichs) to decomposition, that after exposure to the air for

* At the meeting of the French Academy, on the 23rd of June, 1851, Blondlot gave the history and post-mortem appearances of a dog that lived for five years without bile passing into the intestines. (Quoted by Dr. Bence Jones, in his excellent Lectures on Digestion, Respiration, and Secretion: Medical Times, new series, vol. iii. p. 3.)

† Arch. Gén. de Méd., quatrième série, vol. xix. pp. 68—87.

‡ Article *Digestion*, in Wagner's Handworterbüch der Physiologie, vol. iii. Part I. pp. 842—849.

§ Phys. Chem., vol. ii. p. 106.

a few hours it develops a distinct odour of putrefaction; Schmidt, however, as we shall presently show, differs from him in this respect. While Frerichs found only 1·36 per cent. of solid constituents in the pancreatic juice of the ass, and 1·62 per cent. in that of the dog, Schmidt found no less than 9·924 per cent. in that of the latter animal.

The following tables give the composition of the fluid:

Pancreatic juice of Ass (Frerichs).	
Water	986·40
Solid residue	13·60
Fat	0·26
Alcohol-extract	0·15
Water-extract	3·09
Soluble salts	8·90
Insoluble salts	1·20

Ditto of Dog (Schmidt).	
Water	900·76
Solid residue	99·24
Organic matter	90·38
Inorganic constituents	8·86

In another case Schmidt found that the solid constituents in 1000 parts actually amounted to 115·6.

What the water-extract of Frerichs and the organic matter of Schmidt actually is, is not known. Closely resembling albumen and casein in many of its properties, it is clearly not perfectly identical with albuminate of soda, with casein, or with ptyalin. It is to this substance that the pancreatic fluid especially owes its principal chemical and physiological properties.

We have no very certain information regarding the quantitative relations of this secretion, since the injury which is caused by the operation that is necessary for collecting the secretion must obviously derange the normal condition of the animal. The results obtained by Frerichs and Bernard* are of comparatively little value, because the weights of the animals are not given; and the same objection applies to the investigations on this point which have been recently instituted by Colin.† Perhaps the most satisfactory experiment is that of Bidder and Schmidt, and even that is sufficiently vague. For the full details of the method by which they arrived at their result we must refer to page 243 of their work. They believe that a dog yields 0·10 of a gramme in one hour for each kilogramme of weight. According to this calculation an adult man, weighing 64 kilogrammes (or about 140 lbs.), would secrete in twenty-four hours 150 grammes of pancreatic juice, containing 15 grammes of solid residue. They confirm (as also does Colin) the statement of Frerichs, that in fasting animals the gland is pale and anæmic, and that it is only during digestion, when it becomes highly vascular and turgescent, that it yields a copious secretion.

In relation to the physiological function of this secretion, they have demonstrated that the pancreatic fluid of the dog exerts no solvent action on the albuminous bodies, but that it acts with extreme power in converting starch into dextrine and sugar, as also does the tissue of the pancreas both of carnivora and herbivora. The sugar-forming power of the pancreatic juice was, we believe, first noticed by Valentin; but the proof that this power far exceeds that of the saliva seems due to our authors. The substance on which this influence depends pre-exists in the pancreatic

* See Lehmann's Phys. Chem., vol. ii. p. 114.

† Comptes Rendus, vol. xxxii. p. 374; and vol. xxxiii. p. 85.

fluid, and retains its energy even after the fluid has stood for twenty-four hours at a temperature of 64°. This action is not arrested by the presence of gastric juice or bile.

Bernard, about four years ago, discovered that remarkable fact, that, externally to the organism, the pancreatic juice possesses the property of decomposing the neutral fats into their base and acid. Our authors have convinced themselves of the accuracy of this statement in the case of butter, from which, in the course of two or three hours, they got unquestionable proof of the formation of butyric acid. Bernard, however, left it undecided how far this action takes place in the living body—a blank in this department of physiology which has been filled up by Lenz (a pupil of Bidder and Schmidt's). He fed healthy cats with fresh butter, or, if necessary, injected it into their stomachs, and killed them in from six to fourteen hours afterwards. Although all the lacteals and the thoracic duct were distended with milky chyle, no trace of butyric acid could be detected in the stomach and in the intestinal canal, or in the thoracic duct, the portal vein, or in the gall-bladder. By further experimental investigation, he found that the metamorphic action was hindered by the acid gastric juice in proportion to the amount of free acid that was present, that a similar action might be artificially induced by other acids, and that it might be overcome by neutralizing the free acid by bile or by an alkali. Hence it may be concluded, that it is only in exceptional cases that the pancreatic fluid decomposes the neutral fats into acids and bases in the living body. We shall enter no further at present into the action of the pancreatic fluid on fats, as it will be fully treated of in the future article on *Absorption*, to which we have already alluded.

Frerichs ascribes to the pancreatic juice a peculiar power of hastening the conversion of the bile into insoluble products, and of thus contributing to the more perfect elimination of this secretion. This view is, we think, completely overthrown by the experiments of Bidder and Schmidt, who have shown (see p. 190), first, that the greater part of the bile is not thrown off with the fæces, as Frerichs believes; and secondly, that the lime to which Frerichs' specially ascribes this power, only exists in very subordinate quantity in the pancreatic fluid.

From the preceding remarks, it follows that the only unquestionable action of the pancreatic fluid is that which it exerts on starch; but since the relative size of the pancreas is greater in carnivorous than in herbivorous animals (the weight of this gland being $\frac{1}{300}$th that of the whole body in dogs and cats, and only $\frac{1}{600}$th in that of rabbits), and since, further (as has been shown by Bidder and Schmidt's experiments), the greater part of the amylaceous food of the sheep is converted into sugar before it enters the duodenum, we may fairly conclude that the principal uses of this secretion are still unknown.

There are far greater difficulties in obtaining pure *intestinal juice* than in procuring any of the secretions we have previously considered. The most recent investigations on this subject are those of Frerichs (contained in his article on Digestion), and of Zander* (a pupil of Bidder and

* De succo enterico. Diss. Inaug. Dorpati, 1850.

Schmidt's, who have themselves continued and extended his researches), and their conclusions are, upon the whole, very discordant.

All that we can say with certainty with regard to the physico-chemical properties of the fluid is, that after filtration it is a tenacious, ropy, colourless, and strongly alkaline substance.

According to Frerichs, the intestinal juice exerts no change on any of the ordinary elements of the food. Protein bodies and gelatigenous substances remained perfectly unchanged; fat became disintegrated in the same manner as in all other viscid fluids; neither, according to Frerichs, does it exert any special action on starch. Hence he denies to the intestinal fluid any action as a direct digestive agent. Lehmann, on the other hand,* found that the intestinal juice possessed, in a high degree, the power of converting starch into sugar; but protein bodies and fats were so little affected by it, that he doubts whether it exerts any digestive action on these substances, and the more so, since cubes of coagulated albumen and pieces of flesh, when introduced into the gut of a hospital patient through a fistulous opening, were expelled from the rectum almost entirely unchanged. The fistula was, however, in the lower part of the ileum, and probably near the cæcum. Finally, the results of experiments on nineteen cats and two dogs have convinced Zander and Bidder and Schmidt, that the intestinal juice exerts a solvent action on albuminous bodies, scarcely inferior to that of the gastric juice, and a sugar-forming power on starch scarcely inferior to that of saliva or of pancreatic juice.

The following table, taken from Bidder and Schmidt, shows at a glance how active a part the so-called digestive fluids take in the general metamorphosis of matter within the animal body.

An adult man weighs about 64 kilogrammes [one kilogramme being equal to about 2·2 lbs.], about 20 kilogrammes being solid constituents, and 44 kilogrammes water.

The different digestive fluids secreted in twenty-four hours occur in about the following quantities:

1·5 kilog. of saliva,		with 1 per cent. of solid constituents,		contains	15 gram.	of solid constituents.		
1·5	,,	bile	,, 5	,,	,,	80	,,	,,
6·4	,,	gastric juice	,, 3	,,	,,	192	,,	,,
0·2	,,	pancreatic juice	10	,,	,,	20	,,	,,
0 2	,,	intestinal juice	1·5	,,	,,	3	,,	,,

We shall conclude the present article with a brief reference to the manner in which the carbo-hydrates, of which the most important are cellulose, gum, starch, and certain kinds of sugar, are digested, and the changes which they undergo in the system.

Cellulose defies the solvent power of all the digestive fluids, and hence we find that all vegetable tissues which consist essentially of this substance pass unchanged into the excrements, in the case both of herbivorous and carnivorous animals. Certain curious observations recently made by E. H. Weber,† regarding the power that is probably possessed by the beaver of effecting the solution of woody fibre, do not materially affect the truth of this general statement. Little is known with certainty regarding the changes which gum undergoes within the body. Any of the three following processes are conceivable. It may be converted into sugar

* Phys. Chem., vol. ii. p. 120.

* Ber. d. königl. sächs. Gesellschaft. d. Wissensch., vol. ii. p. 192.

previously to its absorption; or it may be absorbed directly, and without change; or, finally, it may be not at all absorbed, and thrown off with the excrements. For an excellent abstract of the experiments bearing upon this question, we may refer to the third volume of Lehmann's 'Lehrbuch der physiologischen Chemie,' pp. 285—289 (of which the translation is not yet published). The conclusion to which they lead is, that if gum be at all absorbed, it can only pass into the animal juices very slowly, and in very small quantity. Hence the use of the *potiones gummosæ*, which are so much employed in acute diseases by a large class of physicians, must be an extremely limited one.

So much has been already said regarding the effects of the different fluids on amylaceous matters, that we shall pass on to the digestion of sugar. The first question we shall notice in reference to this point is, Does grape-sugar (or glucose) become absorbed in an unchanged state, or does it undergo any previous change? and we shall answer it in the words of Lehmann:

"When we find no sugar in the blood which circulates in the walls of the intestines during digestion, and only traces of it in the chyle, even after highly saccharine food—when we find that even dilute solutions of sugar (as well as concentrated ones) are distributed in an unresorbed state over the whole intestinal tract, even to the cæcum, and that the saccharine fluid is everywhere accompanied by a free acid—when, even after highly saccharine food, the most soluble sugar may be recognised as far as the ileum—when there is no other source from which the lactic acid in the intestine can be developed than from the sugar or starch of the food—when, finally, physical experiments on diffusion and endosmosis teach us that sugar does not readily penetrate animal membranes—when injections into the blood show us that the smallest quantities of sugar reappear in the urine, whilst lactic acid is rapidly consumed in the blood, and does not pass away unchanged by the kidneys—when we actually see with our own eyes starch in contact with the alkaline intestinal juice in closed loops of intestine becoming acid,—we should be carrying scepticism too far if from such a number of coincident facts we did not draw the conclusion, that a great part of the sugar in the intestine there undergoes a further change into lactic acid. We cannot, however, refrain from admitting that there are several equally undoubted facts which do not accord with the above assumptions. Starch and sugar must surely be applied in the organism to some other purpose than to the formation of lactic acid and the promotion of combustion. The undoubted (although still unexplained) formation of sugar in the liver, the production of fat in the organisms of the herbivora, and many other facts, indicate the necessity that a considerable part of the sugar should be removed unchanged from the intestine, and that only a fraction of it is converted into lactic acid."

The consideration of the mode in which fat is digested is, as has been already mentioned, deferred to a future article; and the extent to which the present paper has run precludes us from noticing many important facts regarding the digestion of the albuminous bodies. We leave the subject in this imperfect state with the less regret from the knowledge that the third volume of Lehmann's 'Physiological Chemistry,' in which the subject of digestion is very fully discussed, will shortly be accessible to the English reader. At present, Dr. Carpenter's chapter on Food and the Digestive Process, which we have placed at the head of this article, may be referred to as affording the most accurate summary of this subject, in all its bearings, which is to be found in our own language.

G. E. Day.

Review XIV.

Over den Oorsprong en de Vorming van Tube·cula Pulmonum. Door J. L. C. Schroeder van der Kolk. Overgedrukt uit her 'Nederlandsch Lancet.' 3rd Serie, 2de Jaarg, No. 1 en 2. 1852.

On the Origin and Formation of Tubercles of the Lungs. By J. L. C. Schroeder van der Kolk. Reprinted from the 'Netherlands Lancet.'*

A considerable portion of this essay is occupied with criticism and disapproval of Rokitansky's doctrine of a tuberculous dyscrasia; but we pass by this, feeling sure that English readers will prefer to know what the distinguished Professor of Utrecht has ascertained, for himself, by the minute study of tuberculous structures—a study in which, as is well known, he has been long occupied, and of which some of the fruits have been already published.

He describes, first, the epithelial lining of the air-vesicles, by the increase and morbid conditions of which he holds that pulmonary tubercles are formed:†

> "The wall [of each healthy air-vesicle] is everywhere covered with flat epithelial cells, of which some are smaller, and appear like only nuclei; others are larger; all have a nucleus, with more or less granular matter."

And among them are certain very large cells filled with granular matter and some oil-drops, which are very easily detached from the walls of the air-vesicles.

> "If, however, we examine, in a very thin section, an air-vesicle at the border of a tubercle, where, for instance, the bloodvessels may be still filled with injection, we find no longer a single layer of these cells, but they are remarkably increased in number, and form sometimes two, three, or more superjacent layers, till, in the tubercle itself, the whole air-vesicle is filled with them."

Illustrations are given, from drawings by Schroeder van der Kolk himself, of the progressive changes from healthy epithelium to the complete formation of a tubercle—changes which may sometimes be traced in adjacent air-vesicles. In one air-vesicle, the healthy portion of the wall is shown covered with epithelial cells of various sizes; in the parts of the same air-vesicle, nearer to a tubercle, there are some larger cells, of darker colour, no longer flat, but swollen and spherical; these are epithelial cells, more or less distended with fluid, detached, and constantly enlarging. In the next adjacent air-vesicle, in which the formation of the tubercle is nearly complete, the detached and distended epithelial cells are much larger, and closely adherent together; and it is observable, that the largest of them commonly lie in the middle of the cavity of the air-vesicle, and are, for the most part, filled with numerous nuclei, while the smaller usually contain but one.

> "It is hence evident that these cells, which fill the air-vesicles, and make up the tubercles, are nothing but epithelial cells, which swell by imbibition of plastic

* The continuation of the review of Mr. Ancell's and Dr. Cotton's works has been unavoidably postponed till the following number.

† Numerous admirable observations, by Schroeder van der Kolk, on the natural structures of the lungs, are contained in an essay by one of his pupils, which is too little known in this country: 'Adriani: De subtiliori pulmonum structura.' Utrecht, 1847.

matter exuded in the cavity of the air-vesicle, enlarge, and are cast off from the wall. The cells which are placed in the middle of the air-vesicle are thus the oldest—i.e., they are farthest removed from its walls, longest exposed to the influence of the surrounding fluid, and thus, also, the largest. They are all filled with granular matter and minute oil-globules, while, in the larger, an increase of nuclei takes place. If the tubercles be examined in a somewhat further advanced stage, when they present more tendency to softening, the larger cells are found in much less quantity, and in their place the air-vesicle is again filled with smaller cells. Among these, however, there are some larger cells, in which several nuclei or smaller cells are inclosed, which completely agree with the smaller free cells; so that no doubt can remain but that, in this state, the larger cells are dissolved or burst, and the smaller ones set free."

The smaller cells or nuclei here referred to are the bodies usually described as tubercle-corpuscles; and Schroeder van der Kolk's description of them agrees very well with that which is current, except in that he assigns to them (as Rokitansky also does) a frequent occurrence of nuclei. He traces similar cells as characteristic of tuberculous sputa.

Supported by these observations of the epithelial cells being, with their contents, the constituents of pulmonary tubercles, Schroeder van der Kolk holds that the formation of tubercles should be regarded as analogous to the process of glandular secretion; and to show how the exudation of albuminous matter, or plastic lymph, in the air-vesicles, may thus lead to the changes in the epithelial cells imbibing it, he compares, with the formation of tubercles, the process observable in hepatization of the lungs. The likeness, he says, is complete; there are the same cell-forms in the air-vesicles, the same alteration of the epithelial cells, the same absence of transition to higher organization. In the less advanced stages of hepatization, indeed, numerous blood-cells are found among the altered and detached epithelial cells in the air-vesicles; but in the more advanced stages these are absent, and the likeness to tubercle is by so much the more complete.

Moreover, he compared with these appearances in tubercles and hepatization, the structures in the air-vesicles of a child born dead, which had been in contact, not with air, but with the liquor amnii. Here, also, he found the air-vesicles lined with flat, nucleated, epithelial cells.

"Not only, however, at the margins, but in the middle, of the air-vesicle, he saw larger cells, filled with fluid, laid on the other cells, and completely agreeing with the cells found in the morbid state in peripneumonia and tuberculosis. Many air-vesicles appeared wholly filled with these cells."

The chief conclusion deduced from these observations is, that serous fluid or liquor sanguinis, being effused in the air-vesicles, will be imbibed by the cells of their epithelial lining; and that successions of cells thus filled and enlarged are apt to be cast off into the cavity of the air-vesicles, and therein to constitute the morbid structures of tubercle and hepatization. So far as the description of the elementary structures is concerned, the observations of Schroeder van der Kolk are consistent with those of Virchow, reported in the January number of this journal; but in his interpretation of them, he maintains (as he has long done) the inflammatory nature of tuberculosis, which the posthumous works of Reinhardt, reported in the same number, have brought into new repute. He says, indeed, that the formation of tubercles may be paralleled exactly with the process of glandular secretion; but he herein, evidently, assumes a wider meaning for secretion than it has in the instances from which Dr. Carpenter, whom he follows, derives his law of secretion through cells. The process of forming tubercles which he describes is like that of a morbid desquamation rather than a secretion; it is a separation, not through or by the cells, but of the cells of an epithelium. Regarded as a process analogous to, if not identical with, inflammation, it may be best compared with that described by Virchow* as the result of the imbibition of inflammatory products by the cells or other elements of parenchymatous organs.

The likeness between the formation of pulmonary tubercle and the process of hepatization, in their early state, is already described; in their later progress they may cease to be parallel.

"In the slow progress and duration of the tubercles, the bloodvessels in the walls of the air-vesicles become closely compressed; the epithelial cells, in their increase and enlargement, fill the vesicles and the whole infundibulum [intercellular passage] into which the bronchial tube expands after its entrance into the lobule; for want of circulation in the walls of the air-vesicles, the local exudation within them ceases, the mass becomes firmer through loss of fluid and through pressure, and can no longer be discharged from the wider space of the infundibulum, through the narrow tube of the bronchus; till, by fatty metamorphosis, the material becomes again more liquid and more puriform, and now, after the destruction of the walls of the air-vesicles, is coughed up with the elastic fibres by which those walls were traversed."

"In pneumonia and hepatization the course is more acute. Here the bloodvessels are not so speedily closed; the contents [of the air-vesicles] remain more liquid, and are discharged by coughing without destruction of the pulmonary tissue. If, however, the hepatization has passed into a further stage, and especially into grey hepatization, then the bloodvessels waste from the air-vesicles, and only larger branches are spread upon the lobules."

In these cases of hepatization the morbid products are, equally with those of tubercle, incapable of being organized into higher forms of tissue; not, the author maintains, because of any defect or peculiarity in their albuminous or fibrinous constituent, but because they are in, or derived through, epithelial cells, and are therefore in conditions such as higher organization never ensues in. Such development does, however, ensue in the same material, exuded at the same time, in the interlobular tissue; for here, whether in the neighbourhood of tubercle, or in that of hepatization, rows of cells are found in all stages of elongation and attenuation into filaments. Similar development may be traced in part of the tuberculous elements exuded in tuberculous peritonitis—a fact which Rokitansky observing, has referred to the mingling of tuberculous with organizable inflammatory deposits.

James Paget.

* Essay on Parenchymatous Inflammations: Archiv. für pathol. Anatomie, Band iv. Heft 1.

PART SECOND.

Bibliographical Record.

ART. I.—*The Progress of Improvement in the Treatment of Consumption, and other Pulmonary and Laryngeal Diseases*, &c. By JAMES TURNBULL, M.D., Physician to the Liverpool Royal Infirmary.—*London*, 1853. 8vo, pp. 74.

DR. TURNBULL published some two or three years ago an inquiry into the curability of phthisis, by the use especially of cod-liver oil. This was an useful and practical volume, without any pretensions to originality. The present work is a continuation of it, and in some respects is superior. Dr. Turnbull starts with the allowable assumption, that the progress of tubercular phthisis is often arrested, and after citing some cases illustrative of this fact, puts the important question:

How far is recovery permanent when consumption is arrested?

In answer to this he cites seventeen cases, and concludes—

"In the *first* case, the disease was in the early stage before any softening and disorganization of the lung had occurred, recovery seems to be complete, and there is, therefore, good reason to expect that it may be permanent. In the *second*, a degree of dulness, indicating a very considerable amount of consolidation, has been almost completely removed, and the general health perfectly restored for nearly two years and a half; and as this has occurred at an early period of life, when tubercular disease does not commonly attack the lungs, there is sufficient ground to believe that the judicious use of hygienic means and prophylactic treatment may enable the child to outgrow the tubercular tendency entirely. In the *third*, the disease had not gone beyond the first stage; but remaining dulness shows that there is pulmonary induration, or partially absorbed tubercular matter, which may have undergone cretaceous transformation. In other respects recovery is most perfect; and there is reason to hope that, with due care on the part of the patient, it may prove permanent. In the *fourth*, the tubercular matter is probably cretaceous, recovery is very good, and, as the period of life of the patient—above fifty years of age—lessens the tubercular tendency, there is reason to expect that due care may prevent the activity of the disease being renewed. In the *fifth*, the disease was in the first stage; and a very perfect recovery has been effected. The *sixth* is a case where the slow progress of the disease, and the fact of it being arrested in the first stage, are grounds for hope; whilst, on the other hand, the natural delicacy of constitution is a reason to fear that slight exciting causes might rekindle the activity of the tubercular deposit. The *seventh* and *eighth* are cases where the amount of tubercular deposit has been small; and it is not improbable that it has reached a condition similar to what was described as found in the lung of the gentleman who died of aneurism (page 3). If so, a similar degree of permanence may be reasonably looked for. In the *ninth*, the disease had reached the second stage; recovery being very perfect, there is some ground to hope that it may also be permanent. In the *tenth* case, the disease had arrived at the third stage; but its extent was more limited than in most cases so far advanced, and the general health had never sunk to a low point. These circumstances, and the perfect restoration of the general health, with almost complete removal of the local signs, lead us to believe that the small cavity, if not already obliterated, may ultimately be completely healed. The fatal result in the *eleventh* case, where a good recovery of very considerable duration had been made, shows that there is uncertainty as to the result of all those cases where a cavity of considerable size has once been known to exist, however perfectly the disease may seem to be arrested. The termination in the *twelfth* would lead to a similar observation: but in this case it is satisfactory to add, that though the original extent of the disease precluded any favourable anticipations, reparation was carried forward to a much greater extent than was expected, so that the disease did not prove fatal in the ordinary way, but by an accidental inflammatory complication. In the *thirteenth* and *fourteenth* cases, it is hoped that, as the constitutional powers are good, the alterative influence of a complete change of climate may enable them to maintain the ascendancy over the local disease; and if the opinion advanced as to the cause of the dulness in the *fifteenth* be correct, it may be hoped that the good state of the general health may effect a similar result. The *sixteenth* is undoubtedly one of the most interesting cases on record. It furnishes a most important link in the chain of evidence in favour of the curability of consumption, and also of its curability by treatment. Reparation was not only carried so far forward by absorption and elimination of the tubercular deposit, which was almost wholly removed, and the two remaining cavities so nearly healed, that no injurious effects could afterwards have been produced by them on the health of the patient, but the tubercular diathesis itself had been so completely eradicated, that another—the cancerous, usually considered to be almost incompatible with the tubercular—had sprung up in its place." (pp. 25—27.)

It is to be regretted, however, that the majority of these cases are of much too short duration to make us feel complete confidence in the conclusions; the earliest date is December, 1849, only three and a half years ago.

Dr. Turnbull then alludes to the effect of cod-liver oil, a remedy in which he still puts great confidence. He discusses briefly its mode of action; but we do not think that he has succeeded better in this than those who have gone before him. We have little liking for hasty explanations of the action of medicines. As we are scarcely yet able, or indeed are still unable, to tell what are the changes through which a particle of albumen or of chloride of sodium passes in its transit through the body, we are still less likely to be able to determine the metamorphoses and varying combinations of a medicinal agent, which does not appear in the excretions in the same form in which it entered the system. We do not underrate the advantage of conjecture and hypothesis; but these are, to use Bacon's phrase, merely questions which are put to nature, and cannot be final and satisfactory.

Dr. Turnbull then alludes to the effect of some other medicines. He has tried tar-water, and found it useful (a fact which must delight the shade of Bishop Berkeley), naphtha, naphthaline, creosote, pyroxylic spirit, &c.; he has found creosote beneficial in gangrene of the lung, chronic pneumonia, and phthisis with fœtid expectoration, and in chronic bronchitis with copious sputa.

Dr. Turnbull has also employed the alcoholic extract and the tincture of the seeds of the œnanthe phellandrium, an indigenous umbelliferous plant, which formerly enjoyed some little reputation. It is slightly narcotic and diuretic, but evidently has not much power.

The treatment of ulceration of the larynx and pharyngeal follicular disease by topical application of nitrate of silver is next referred to. Dr. Turnbull has used injections by a syringe, with benefit. There is nothing to detain us in this chapter, or in the following one on "Inhalations in Phthisis."

The last chapter in this little work is on hygienic means of treatment, and especially on the use of sugar of milk as an article of diet. In our review of the "Whey-cure," we have somewhat anticipated this chapter. Dr. Turnbull does not appear to have tried on a large scale the effect of lactine, but his views of its action are very much in accordance with those of Dr. Beneke already alluded to. There is no doubt that it is a highly valuable article of diet, and we quite agree with him in his regret, when he quotes the remark made by Liebig in his 'Familiar Letters,' that "in the cheese dairies of England thousands of cwts. of this valuable respiratory matter are annually lost in the whey." We have little doubt, however, that medical men will soon perceive the great use of this dietetic, and that whey will be a profitable article of agricultural produce.

We recommend Dr. Turnbull's work: on some subjects he has touched too superficially; but altogether, it is both a practical and a suggestive work, and is, on both accounts, an useful treatise.

Art. II.—*A Text-book of Physiology.* By Dr. G. Valentin, Professor of Physiology in the University of Berne. Translated and Edited from the third German Edition by William Brinton, M.D., Physician to the Royal Free Hospital. Part I. 8vo, pp. 320.

What with original and translated works, England is at present rich in physiological treatises. Each has its peculiar merit, and is superior to its contemporaries in some points, and inferior in others. Professor Valentin has long been celebrated as one of the most learned and laborious physiologists of Germany, a character not easy to earn in that land of labour and experiment. This work is characterized by great fulness of detail and accuracy of description. It appears to have been very well translated by Dr. Brinton, although, as we purpose a full review of it when the concluding Part is issued, we have not looked at it in this respect with a very critical eye. We cordially recommend the work as one of great utility, and we hope that the able translator will soon enable us to take a more lengthened notice of it.

Art. III.—*A Clinical Phrase Book; in English and German, &c. &c.* By Montgomery Johns, M.D.—*Philadelphia*, 1853. pp. 308.

The utility of books of phrases, as aids for acquiring the knowledge of a foreign language, or at least the habit of speaking it, can scarcely be doubted; and especially for a medical man who is obliged to communicate with patients of another nation in their own language, before he has made himself thoroughly master of it, it is highly desirable to possess a guide in which the phrases used at the bedside are correctly interpreted. To furnish this, being the design of the little volume before us, it may become useful to those who are in such a situation. But we could have wished to have found a greater accuracy in the translation, and as brevity must be a main object in a book designed to be a pocket-volume, several chapters might have been omitted without injuring the completeness of the book. We cannot see the good of giving in a "*clinical* phrase book," any space to the conversation "in a shop" (p. 60), or "on change of money" (p. 58), or "on lodgings;" nor can we consider it necessary to have an extract of a German grammar, and a rather copious English-German and German-English lexicon, in such a book.

Concerning the part especially devoted to clinical phrases, we find sometimes phrases in the column for the one language, which we miss in that for the other language; in other places the German phrase has not the same meaning with the corresponding English. For the questions—"Has anything struck you or fallen on you?" and again, "Was this burnt black or only blistered?" (p. 20), we find no translation in the German column. In the same page we see, "Do you chew tobacco? do you smoke?" interpreted by "Kauen sie; rauchen sie?" No one, however, could easily guess that "Kauen sie" is meant for "Kauen sie tabak?" In the examination about delirium, "wandering" is translated by "unruhig," while this word in German means simply—unquiet, restless. In asking about gonorrhœa, "discharge" is interpreted by "abführung" (p. 40), instead of by "ausfluss," a mistake which gives a totally different meaning to the phrase, as "abführung" is in Germany merely used for an alvine discharge, principally an increased one. In asking about the commencement of labour in parturition, we find the word "show" translated by "aussatz," which, in the language of the working people, conveys the meaning of a chronic eruption, and is the German scriptural term for leprosy. We could point out several other mistakes of a similar nature, which it would be desirable to avoid in a second edition. On the whole, however, we may say that both the English and the German practitioner, who is not well conversant with the foreign tongue, will derive some profit from the 'Clinical Phrase Book.'

ART. IV.—*Inflammation of the Breast, and Milk Abscess.* By THOMAS W. NUNN, Surgeon to the Western Dispensary.—*London,* 1853. 8vo, pp. 53.

THIS is a sensible little treatise, with no pretensions to novelty. The symptoms are detailed as usual. The period of occurrence of inflammation of the breast after delivery, as noted in 37 cases, was found to be during the first 8 weeks in 22 cases; from the 8th week to the 44th in 7 cases; and beyond the 44th week in 8 cases. The inference would appear to be, that for some weeks after delivery, and again from over-lactation at a time when weaning should commence, are the periods of danger. In the intervening months, when the secretion is fully established and proceeds equably, there is less chance of inflammation. The causes of the disease are enumerated as "secretional congestion; irritation of sore nipples; mechanical irritation; exposure to cold; the condition of tissues remaining from a previous attack; irritation from vitiation of the secretion; irritation from increased arterial supply in lactation; over-lactation." (p. 12.) In respect of treatment, Mr. Nunn draws a distinction between the cases commencing within the first 8 weeks, and those occurring from over-lactation after the 44th. In the first class of cases, the ordinary treatment by rest, horizontal position, and active purging, is recommended, with cold or hot applications to the breast, according to the amount of relief respectively given. In the second class of cases, the treatment is different; the patient is in a debilitated and asthenic condition, and should be treated with tonics, and anodynes for the pain. Suckling should be discontinued, and if suppuration be not imminent, the breast (often large, pendulous, and doughy) must be strapped. In neither period are leeches recommended, except occasionally in the first. Mr. Nunn does not enter at sufficient length into the treatment of the abscess after opening, or of the obstinate sinuses, which are frequently so troublesome to the accoucheur. If this subject had been fully treated, the work would have been more useful.

ART. V. — *A Pathological and Practical Treatise on Epidemic Cholera.* By O. B. MAHONY, L.F.P. & S., L.S.A. — *London,* 1853. 8vo, pp. 190.

THIS book contains nothing new, and nothing old but what has been already better said a hundred times before. The author states "that neither pains nor industry are spared to render it [the work], though condensed, yet he hopes not very incomplete." We are sorry to say that medical literature would have lost nothing had Mr. Mahony employed his industry in some other direction.

ART. VI.—*On Diseases of Women, and Ovarian Inflammation, &c.* By EDWARD JOHN TILT, M.D. Second Edition.—*London.* pp. 276.

To judge by the number of second editions which lie on our table, English medical men have no reason to complain of want of patronage. Dr. Tilt's first edition was published in 1850; the second (with the title a little altered) follows in little over two years. We reviewed the first edition at considerable length, and we do not find anything in the present issue which need now detain us, although there are some parts of the work to which we shall return hereafter.

ART. VII.—*On Prophylactic and Curative Syphilization.* By VICTOR DE MÉRIC, Esq., M.R.C.S.E.—*London.* Foolscap 8vo, pp. 24.

IN this little pamphlet (a reprint from the 'Lancet') is contained a very good summary of the singular discussion on syphilization, which has now received its *coup-de-grace* from the Academie de Médecine.

Syphilization is buried, and we trust that even in this singular age, when everything most revolting to common sense seems attractive to the public mind, no one will be bold enough to disinter it. If any one dreams of doing so, we beg him first of all to read carefully M. de Méric's pamphlet.

ART. VIII.—*Moral Sanatory Reform.* By HENRY M'CORMAC, M.D., Consulting Physician to the Belfast General Hospital, &c.—*London,* 1853. Small 8vo, pp. 149.

WE have read this little work with much interest. It is written with force and sometimes with eloquence. It contains twelve chapters, every one of them on a theme of the most vital interest to us all, and which are headed, Female Education, Employment, Education, Household Culture, Criminal Management, Physical Training, Clothing, Food, Drink, Air, Drainage, and Prevention of Disease. Nothing is said, indeed, on these points which can be novel to any of *our* readers, but it is well to see such topics handled with vigour, and in a way which will impress the public. There are two chapters—viz., on Female Degradation, and on Physical Training—which are to us at present of particular interest, and to these we shall direct attention at a future period.

ART. IX.—*The Treatment of Obstinate Ulcers and Cutaneous Eruptions on the Leg, without Confinement.* By HENRY J. CHAPMAN, F.R.C.S. Second Edition.—*London,* 1853. pp. 156.

WHEN we noticed the former edition of this work (vol. iii. p. 198), we described briefly the method of treatment employed by Mr. Chapman. He now states that three years' additional experience have convinced him of its efficacy. The book is an useful one.

ART. X.—*On Near Sight, Aged Sight, Impaired Vision, and the Means of Assisting Sight.* By WILLIAM WHITE COOPER, F.R.C.S., Ophthalmic Surgeon to St. Mary's Hospital, &c. Second Edition.—*London.* 8vo, pp. 320.

THIS useful work treats, in as many chapters, of Myopia, Presbyopia, Impaired Vision, Achromatopsy, Glasses, Eye Protectors, and Artificial Light. A very good and readable account is given of myopia and presbyopia, and the chapter on glasses will be found useful both by professional men and others. The work is particularly valuable from the number of illustrative cases, and has throughout a practical aim.

ART. XI.—*Medical Lexicon. A Dictionary of Medical Science, &c.* By ROBLEY DUNGLISON, M.D., LL.D., Professor of the Institutes of Medicine, &c., in Jefferson Medical College, Philadelphia. Ninth Edition.—*London,* 1853. 8vo, pp. 927.

WE are happy to see that this well-known and learned work has passed into its ninth edition. As, however, the eighth edition was published in 1851, and as the present issue bears only the name of the London publisher, it is not impossible that the work before us may be merely a reprint. However this may be, we welcome it cordially—it is an admirable work, and indispensable for all literary medical men. The labour which has been bestowed upon it has been something prodigious; well may Dr. Dunglison quote the words of Scaliger:

> "Si quelqu'un a commis quelque crime odieux,
> S'il a tué son père, ou blasphémé les Dieux,
> Qu'il fasse un Lexicon; s'il est supplice au monde
> Qui le punisse mieux, je veux que l'on me tonde."

The work, however, has been now done, and we are happy in the thought that no human being will have again to undertake the same gigantic task. Revised and corrected from time to time, Dr. Dunglison's 'Medical Lexicon' will last for centuries.

ART. XII.—*The Medication of the Larynx and Trachea.* By S. SCOTT ALISON, M.D.—*London*, 1853. pp. 49.

THIS is a little book, but contains much matter, not overlaid with unnecessary commentary. If every author said what he had to say as simply as Dr. Scott Alison, the task of the critic would be easier.

The application of remedies to the larynx and trachea has lately attracted much attention. There cannot be a doubt of the great utility of local treatment; and if some of the followers of Dr. Horace Green have pushed their opinions to extremities, others, as the author before us, have brought to bear on the subject a correct and discriminating judgment, which sees the advantage without extravagantly over-praising it. Imitating the brevity of the author, we proceed to give a brief analysis of this little treatise.

Dr. Alison has used, of course, the nitrate of silver, after the manner of Green (but in weaker solution—grs. v. ad ℥j. of water), in suspected ulceration, and in cases of chronic hoarseness and œdema. He found, however, that even this weak solution was too stimulating in acute cases, and he therefore determined to try some other agent. *Olive oil* suggested itself to him, and he has tried, in several instances of more or less acute laryngeal attacks, the introduction of the sponge soaked in oil into the larynx. He found the sense of dryness and weight very much relieved, and in some cases the voice has at once greatly improved. *Glycerine* has been substituted for olive oil, and answers the same purposes; *cod-liver oil* leaves a disagreeable taste behind it, and is not better than olive oil; *mucilage* has been also employed. Glycerine and mucilage have been also used by Dr. Alison as vehicles for the local application of morphia, atropine, and conia. The morphia is used in doses of $\frac{1}{8}$th of a grain, and has been found to allay the irritation and coughing. Atropine (gr. $\frac{1}{30}$) appeared useful in a case of hysterical cough, and its employment is suggested in epilepsy. Some few remarks on the method of applying the sponge, of no novelty, close this little work.

As we are at present unable to speak from experience of the value of these suggestions, we cannot do more than put Dr. Scott Alison's statements before our readers. At the same time, the undoubted efficacy of the local application of nitrate of silver, justifies an inquiry into the effects of other agents similarly applied, and Dr. Scott Alison has done service by commencing it.

ART. XIII.—*Memorandums made in Ireland, in the Autumn of* 1852. By JOHN FORBES, M.D., F.R.S., &c.—*London.* Two vols. 8vo.

THIS is a book to be reviewed rather by the general than by the professional critic; yet written as it is by one of the most esteemed physicians of the day, we cannot pass it by without a word of comment. We doubt not that the exclamation which rose to our lips, "This is a most valuable book!" when we had finished its perusal, will be re-echoed by all those who read it, and who, unblinded by prejudice and partisanship, desire to form a correct idea of that country in which every Englishman must feel the profoundest interest. In Dr. Forbes's book we have exactly what we wanted: a statement of facts, carefully collected by a practised and truthful observer. There are few men who possess the same power of seeing things as they are, and of describing them as they are seen. The book bears internal evidence of truth and accuracy; and the confidence of the reader, soon given to the author, augments from page to page. There are doubtless, in the book, many opinions and conclusions from which we may demur, but this very circumstance proves its value, for in the work itself is the material from which an adverse judgment may be formed.

Many of the statements strike at the very root of English prejudices about Ireland and Irishmen. Dr. Forbes appears as the apologist, or we should rather say, the justifier, of catholic Ireland, and as the vindicator of the fundamental goodness and beauty of the Celtic mind. In his sympathy with the Irish character we most heartily concur; in his admiration of Irish catholicism we confess we are not so much in accord. And we cannot avoid surmising, that in his surprise to find the priesthood not the incubus it is represented to be by ill-informed periodical writers in this country, he has, by an inevitable re-action, kept his eyes fixed too steadfastly on the golden side of the shield. Certainly, nothing in the book astonished us more than to find that Dr. Forbes even passes a marked, though temperate, encomium on the practice of confession. He certainly, in his sturdy individualism (if we may use the word), is the last person we should have supposed could have advocated the practice of a ceremony, in which a man foregoes his independence and inherent dignity, and delivers himself body and soul to another man, equally frail, tempted, sinning, as himself. The ground that Dr. Forbes has taken up on this question does not appear to us to be tenable; surely he only can successfully defend confession, who believes that the pope is Christ's vicegerent, and that the priests are his appointed messengers. To carry our most sacred thoughts to a mere man, who has received no delegated powers, and to accept from him a judgment without appeal, must be a ceremony alike dangerous to the confessor and degrading to the confessed.

If differences of opinion will prevail, and be expressed on this and analogous points, no difference of opinion can exist as to other parts of the work. Ireland, or at least the most important part of it, is as it were daguerreotyped. We are convinced that succeeding writers will refer to Dr. Forbes's work as the most authentic and masterly description of Ireland, as it is now, after its great trial, and in the midst of its regeneration. We see the last throes of that gigantic convulsion, which bids fair to displace a nation, if not to obliterate a race. No blue book, no impassioned oration, could tell us what the careful traveller with his ready note-book has been able to tell us. We can truly say that for the first time in our life we seem to comprehend distinctly the social problem which Ireland has presented, and to recognise the means by which it may be solved. And not the least pleasing feature about Dr. Forbes's book is, that as he unfolds before us the condition of Ireland, he gradually infuses also a hope for the future, that grows as symptom after symptom of regeneration is brought to light. At the close of the book we seem to see that day is at hand, though thick darkness may still cover the land. If it be so, happy is every man who has been permitted to aid in the work of regeneration and reform.

ART. XIV.—*Observations on the Nature and Treatment of the Asiatic Cholera.* By WILLIAM STEVENS, M.D. &c.—*London*, 1853. 8vo, pp. 475.

THIS book should have been entitled, 'Observations on the Saline Treatment of Cholera, and on those who have, and those who have not, used it,' for both parties fall equally under Dr. Stevens's lash. The greater part of the volume is controversial, and is chiefly made up of attacks on the late Sir David Barry, and on two or three gentlemen, whose fate it was in 1832 to co-operate with Dr. Stevens, and not to agree with him. We cannot perceive the necessity of such a publication, and do not believe that the interests of science can be in the least benefited thereby. If Dr. Stevens thinks that the merits of his saline treatment are not appreciated, let him publish his evidence without unnecessary comment, and then leave the profession to form its opinion.

ART. XV.—*Monographie des Eaux Minerales de Wiesbaden.* Par M. CHARLES BRAUN, M.D.—*Wiesbaden.* 8vo. Cahier I. und II., pp. 105 & 117.

The Mineral Waters of Wiesbaden. By Dr. BRAUN.

IN the first part of his work the author gives an elaborate and, we should suppose, an accurate account of the geological features of the land of warm springs, and of the composition of the springs themselves. In the second part, the effect of the water taken internally and used as baths is related at great length. There are already some English works, with more or less full and accurate description of the waters, but none which can compare with the work before us.

The effect of a moderate internal use of the waters is chiefly evidenced on the urine. Dr. Braun has found, from many experiments, that the quantity of uric acid and urea increases considerably; the quantity of chloride of sodium in the urine is also of course increased, as this salt is contained in large quantity in the waters of all the springs. When the water is taken in larger quantity, the same effects are perceptible in the urine; but in addition, the secretions of the salivary glands, liver, and mucous membranes are augmented, and a moderate purgative effect is produced. The quantity of chloride of sodium in the stools also increases. The catamenia become more abundant; and if the patient be suckling, the milk is more copious, and richer in chloride of sodium. If the water be taken in the largest dose usually given, the purgative effects follow sooner, and are more marked; and the quantity of urine is diminished instead of being increased. In all three cases the appetite is usually augmented two or three hours after the use of the water; digestion is active, and provided the waters are not used in excess, nutrition is well performed, and the body speedily gains in weight.

The physiological action of the water is, then, an eliminating one; the metamorphosis of the tissues is accelerated, and the excreta are increased in amount. If this action is carried to excess, it debilitates, and hence care is necessary in the cases of persons who are easily reduced. The therapeutical uses of the waters are very numerous, especially in gouty and rheumatic affections, and in most chronic disorders of digestion and nutrition. Those who require more minute details on these points we may refer with confidence to the work before us.

ART. XVI.—*Iconographie Ophthalmologique, ou Description avec Figures Coloriées des Maladies de l'Organe de la Vue.* Par J. SICHEL. Livraison 1—4. *Paris*, 1853.

Ophthalmic Iconography. By J. SICHEL.

THE author informs us, in his preface, that the materials of this work have been prepared for twelve years, but that he has delayed the publication during the whole of that time, for the purpose of improving the delineations. The greatest labour and expense have evidently been bestowed upon these, and, we must in justice say, with entire success. Some of the figures are most beautiful, and several are unequalled by any previously published. Mr. Dalrymple's plates may certainly be placed in juxtaposition with these, but, on the whole, they are not better done. The letterpress consists of the description of each plate, and of a short account of the several diseases of the eye. We shall delay any consideration of this part till the work is completed. Each part (four being now published) contains four plates, and each plate contains six to eight figures. The whole work is to consist of twenty parts, and will cost about £6 10*s.* —a large price, certainly, but not more than the merit of the plates warrants.

PART THIRD.

Original Communications.

Art. I.

Considerations on the Causes of Dilatation of the Heart, with an Analysis of Evidence bearing on the Connexion of that Affection with Disease of the Lung. By W. T. Gairdner, M.D., Superintendent of Morbid Anatomy and Assistant-Physician to the Royal Infirmary of Edinburgh.*

Dilatation of the heart, accompanied, as it usually is, by hypertrophy, and by all the symptoms of cardiac disease in their most aggravated form, is a condition which has at all times attracted the attention of pathological anatomists; and by Morgagni, Senac, Corvisart, and almost all subsequent writers, the causes of dilatation, and of the concomitant hypertrophy, have been discussed with a fulness which may appear at first sight to have exhausted the subject. Yet it will not be denied by any one much in the habit of observing diseases of the heart, that there occur many cases of dilatation and hypertrophy which cannot be adequately explained upon any clearly ascertained principles; and which even the best-informed and most careful theorists are obliged to dismiss into the region of pure speculation. Thus Rokitansky, after enumerating at great length the individual causes which lead to these diseased conditions,—first, by mechanical obstruction of the circulation, and secondly, by disease of the heart's structure,—concludes, that "in all those cases in which cardiac disease cannot be referred to any of the above enumerated causes, it may originate in *excessive innervation* of the heart." In like manner Hope, after exhausting the known and clearly intelligible causes of hypertrophy of the heart, and adding a few others not so generally known or so clearly intelligible (*e. g.*, the influence of "pedestrian tours among the Swiss and Scotch mountains" in producing disease of the heart), takes refuge at last in the theory of "inflammation" to explain (*obscurum per obscurius*) the residual cases. It is unnecessary to refer to the numerous fanciful theories of cardiac hypertrophy and dilatation advanced by the older authors. Most of them repose on too slender a basis of induction to entitle them to the smallest consideration in modern science.

The causes of dilatation, so far as they can be distinctly traced, and are generally acknowledged, may be referred to two heads. In the first place, everything that leads to obstruction of the circulation is conceived to exer-

* This paper was read before the Medico-Chirurgical Society of Edinburgh, at the second meeting of the present session. See Monthly Journal, Feb. 1853, p. 178.

cise a mechanical effect in overloading the cavities of the heart; and from this over-distension, when permanent, springs organic dilatation. In the second place, all circumstances which diminish the power of the heart relatively to the amount of blood which it has to propel, tend to cause accumulation of blood in the organ, and overloading of its cavities; dilatation being produced in this case as in the former, but not so decidedly, or so frequently in combination with hypertrophy.

These alleged causes of dilatation of the heart are little open to objection. Of the first, the valvular lesions afford satisfactory examples, and the great constancy with which dilatation and hypertrophy attend on these affections will not be disputed. The fatty degeneration of the heart is an example of the latter; and I believe with Rokitansky and others, that observation will bear out, to a certain extent at least, the idea that this and other causes of diminution of the heart's activity and power may be found concerned in the production of some cases of dilatation. The coincidence of degenerated muscular fibre with dilatation of the heart, especially in those forms in which dilatation is partial and irregular, and associated with a comparatively small degree of hypertrophy, is in truth extremely frequent; though, on the other hand, it must be confessed that the most exaggerated forms of cardiac dilatation and hypertrophy not unfrequently occur apart from any such combination. I think I might even say with truth, that it is quite common, among hypertrophied hearts, to find what in the ordinary forms of chronic fatal disease is rather an unusual occurrence—an organ presenting throughout a normal condition of the muscular fibre.

The most indisputable of all the causes of dilatation of the heart is valvular disease; and the relation of the different valvular lesions to hypertrophy with dilatation of particular cavities, constitutes perhaps the only really unquestionable evidence of the influence of obstruction to the circulation in producing these conditions. Thus, it is well known that extreme contraction of the mitral orifice usually gives rise to great hypertrophy and dilatation of the right side of the heart, and of the left auricle, while the left ventricle often remains small and contracted. Mitral regurgitation, on the other hand, without considerable contraction, leads to hypertrophy of the right side, and enlargement of the corresponding cavities and left auricle, with a minor degree of hypertrophy and dilatation of the left ventricle; and aortic valvular lesions almost invariably determine great hypertrophy and dilatation of the left ventricle, followed by a lesser degree of affection of the right side.

But even in the case of valvular disease, there is often an amount of *capriciousness* (so to speak) displayed as to the occurrence of dilatation and hypertrophy for which it is not easy to account. Nothing is more common than to find the extent to which the heart is altered in size and weight out of all proportion to the amount of valvular obstruction or regurgitation. The most remarkably dilated heart which I ever saw, either with or without valvular disease, was one in which very slight rigidity and possibly slight incompetency of the aortic and mitral orifices existed without contraction of either. The heart lay quite across the thorax, and could not have been less than ten or eleven inches in length, with corresponding increase of breadth. The cavities were enormous. In the private house at which the dissection was performed, I had no means of appreciating accurately the weight of the organ. Similar instances are recorded by Morgagni and others. I sent to the Edinburgh University Museum, some time ago, a heart in which, the ventricles being normal, and the mitral valve alone slightly rigid, the auricles had undergone a dilatation quite enormous. The left auricle was capable of containing a moderate-sized cocoa-nut; the right would have held a billiard-ball; the walls of both were slightly hypertrophied. Unfortunately, the condition of the lungs and other organs was not very accurately noted in either of these cases. As a counterpart to them, I have not unfrequently seen extreme valvular disease with but little hypertrophy and dilatation. In the case of mitral contraction, I have even seen the size of the heart small, and the relation of its cavities all but normal.

These facts show that some important truths are still wanting to complete our knowledge of the causes of cardiac dilatation. But this fact becomes still more evident in considering the cases of dilatation and hypertrophy without valve-disease. For here we constantly find, that without any appreciable mechanical cause of dilatation—at least without any cause of obstruction to the circulation at all commensurate with the effect produced—dilatation arises to a degree almost unexampled in cases of valvular disease, often accompanied by corresponding hypertrophy; and in these cases, as I have already observed, often without the concomitant of diseased and degenerated muscular fibre. Nothing can be more puzzling than many of these cases of simple hypertrophy and dilatation, or more calculated to seduce the mind into vague and ill-founded speculation; yet it is undoubtedly to the careful observation and analysis of such cases of disease of the heart that we must look for information as to their obscure or unknown causes. I trust, therefore, it may prove neither uninteresting nor unprofitable to give an account of the results deducible by analysis from a considerable recorded experience of these affections, and bearing particularly upon the cases unconnected with valvular disease.

The organic causes adduced by authors as most commonly engaged in the production of dilatation and hypertrophy, apart from valvular disease and congenital malformation, are diseases of the lungs, liver, and kidneys, together with atheroma and aneurism of the great arterial trunks, and especially of the aorta.

Disease of the lungs is commonly supposed to act by producing obstruction to the circulation in the capillaries of these organs, and hence occur distension, and subsequently permanent dilatation and hypertrophy of the right side of the heart.* This explanation, though plausible enough, and probably correct to some extent, is not, as I shall presently show, sufficient to account for the peculiar influence of the state of the lung over the volume of the heart—an influence so generally observed by all authors, and so consonant with daily experience, that it can scarcely be doubted.

Chronic diseases of the liver and kidney have been supposed to exercise

* This opinion is as old as Senac, who states it in most distinct terms, and says, 'I have observed that in pleurisies the right auricle and its ventricle are very often dilated: Willis confirms this observation. . . . . . Some have ascribed the same consequences to phthisis pulmonalis, and apparently with reason." He observes, however, that many phthisical hearts are not dilated, and are even flaccid and small. In asthma, dilatation is more frequent, but it is often difficult to determine whether the asthma or the heart-disease is the primary lesion.

an influence on the volume of the heart, and to tend to its dilatation. But, further than the undoubtedly correct observation, that these diseases are frequently connected with diseased heart of all kinds, I know of no good evidence on this subject, nor of any rational theoretical explanation of this supposed influence. It seems much more probable, and more consonant with experience as well as theory, to suppose that the affections of the liver and kidney are in general secondary diseases, resulting, like the well-known secondary diseases of the lungs and serous membranes, from disorder induced in the circulation by heart-disease.

With regard to disease of the vessels, and particularly aneurisms of the aorta, there seems to be good reason to accord to these affections some degree of influence over the size of the heart and its cavities—an influence probably due to their interference with the normal reaction of the arterial system upon the circulating blood: as it is a tolerably well-established fact in physiology, that the elastic, and probably also the vital or tonic, contraction of the arteries, holds an important relation to the due propulsion of the blood into the capillaries. So far as my own limited experience goes, however, I should incline to ascribe to disease of the arterial system a much smaller influence in causing hypertrophy and dilatation of the heart than many authors have contended for; because I have repeatedly seen extreme disease of the aorta in its whole length, and even large aneurisms of its descending portions, without the slightest appreciable hypertrophy of the heart. The only aneurisms which have appeared to me to be invariably, or even generally, productive of disease of the heart, are those of the ascending portion of the arch of the aorta or of the transverse portion when very large, and producing great dyspnœa by interference with the lungs. The facts in my possession are not sufficiently numerous, however, to be reduced to a statistical form upon this subject, or to warrant any very precise theoretical conclusions.

Passing from these vague statements of opinion, which I have merely thought it right to indicate, because all previous statements have been almost equally vague, I pass to the more precise examination of the facts bearing on the most frequent class of dilatations without valvular disease; those, namely, dependent on affections of the lungs. I have already observed that the influence of pulmonary disease on the heart has been recognised in pathology from the middle of the last century; and probably there is no doctrine in relation to this subject which has remained so unvaried, or has been so universally admitted, as that which ascribes the connexion of hypertrophy of the heart with pulmonary disease to the obstruction of the capillary circulation in the lungs, occurring in all diseases accompanied by dyspnœa, and especially in emphysema and bronchitis. If I call attention again to the connexion of pulmonary and cardiac disease, it is not for the purpose either of re-asserting this doctrine, or of directly opposing it. But theoretical considerations having some months ago led me to reflect upon another view that might be taken of the connexion between primary disease of the lung and cardiac dilatation, I lately undertook a review, and a tolerably searching analysis of the documents which I possessed on this subject, and which were collected before any other theory than the one generally received was present to my mind. The result was, a conviction that the main link in the chain of cause and effect in this case has been entirely overlooked by pathological writers, and that obstruction of the pulmonic capillaries plays a very subordinate part in the dilatation of the heart which results from disease of the lung. I shall, in the first place, endeavour to make good this point, and then state the theory to which I have been led by observation and analysis, as well as by theoretical considerations.

The first circumstance to which I think it necessary to call attention, as demonstrating the insufficiency of the current theory, is, that in a considerable proportion of cases, and in all the extreme cases of cardiac dilatation and hypertrophy, combined with pulmonary disease, and not accounted for by any other organic cause, the left cavities are dilated as well as the right. This is clearly demonstrated by the first table of observations. No doubt the received opinion as to the connexion of pulmonary disease with hypertrophy and dilatation of the *right* ventricle, is borne out by numerous and well-attested facts, which are every day observed and reported; but the existence of a considerable minority of cases in which marked dilatation of the left ventricle may be observed to accompany the hypertrophy of the right with pulmonic disease, has been to a great extent overlooked; or perhaps such cases have been permitted to swell the lists of cases of what has been called "simple hypertrophy," in the absence of any sufficient theoretical explanation. It is nevertheless true that I do not remember to have seen a single case of *the more considerable* degrees of dilatation of the right side, presumably connected with pulmonary disease, in which the left auricle, and even the left ventricle, did not participate in the affection.

Almost all the numerous instances of pure hypertrophy of the right side are attended by very slight dilatation, and even in these it is scarcely ever to be remarked that the left ventricle and auricle are either diminished in volume or in capacity, as might naturally be expected in dilatation from obstruction of the pulmonic capillaries. On the contrary, the dilatation of the left side has always appeared to me to follow, not indeed always *pari passu*, but at no long interval, that of the right; and undoubtedly, whenever the latter has assumed such a degree of prominence as to affect seriously the weight of the heart, the left ventricle and auricle may be observed to participate in the abnormal condition.

Now why this absence of contraction of the left cavities? Still more, why should they ever dilate under the influence of obstruction of the pulmonic capillaries? The appearances in hypertrophy and dilatation from disease of the lung differ remarkably in this respect from those in extreme contraction of the mitral orifice, in which it may often be observed, that the left ventricle is almost as remarkably diminished in volume and power as the corresponding auricle, and the right cavities are increased in these respects. M. Cruveilhier, the only author who, so far as I know, has fairly met this difficulty,—and who boldly and correctly states that the "influence of bronchitis upon the heart exercises itself at once upon the left and upon the right cavities,"*—ascribes this fact to the strictly harmonious action (l'étroite solidarité) which exists between all the cavities. But this explanation, to be valid, *ought* to extend to the facts I have just alluded to of mitral contraction, while, in reality, these are entirely opposed to this view.

* Anatomie Pathologique Générale, vol. ii. p. 717.

The next fact to which I have to direct the attention of the reader is, that it is only a limited number of affections of the lung which exercise the influence I have been alluding to on the cavities of the heart. Of these, emphysema and bronchitis have been long recognised as having the first place; a circumstance which has been naturally enough ascribed to the very great dyspnœa and great amount of obstruction to the pulmonary circulation which occurs in these cases. It is equally a recognised fact in pathology, that phthisis pulmonalis does not, as a rule, lead to hypertrophy of the heart, and, in fact, that this disease is usually attended by a positive diminution in the bulk of all the cavities of the organ. I would add, that while a very considerable proportion of cases of phthisis in a state of progressive advancement show the atrophy I have mentioned, an equally considerable proportion of cases of tubercle in a retrograde condition are accompanied by hypertrophy of the heart, and dilatation, especially of its right cavities. These facts are not difficult of explanation on the obstruction theory; for it is commonly and correctly held that phthisis is a disease in which the constitutional vice, the destruction and waste of the entire system, and especially of the blood itself, predominate so much over the local disease, that in the majority of cases death occurs, not from want of breathing-power, but from emaciation and deficient assimilation. Still I might urge it as remarkable enough, that in a disease where the obstruction of the pulmonary capillary circulation is perhaps greater than in any other without exception, and where pathological anatomists have even attempted to show, with some appearance of truth, that the obliterated pulmonary circulation is replaced, in the diseased parts of the lung, by an increased flow through the bronchial arteries and veins (furnished by the systemic vessels and left side of the heart), there should not in the majority of cases be the slightest departure from the normal relations of the two sides of the heart to one another.

I leave, however, this argument, and the more precise statistical detail of the observations I have recorded, as to the connexion of cardiac dilatation with several other pulmonary diseases. It is sufficient to state that, according to the analysis of Sections I. *b*, II. *b*, of the appendix, neither phthisis nor pneumonia appear to exercise any material influence upon the heart, in the way of producing either hypertrophy or dilatation. I cannot, however, pass over so lightly another class of cases, which have been often supposed, theoretically, to determine cardiac hypertrophy by obstruction; and which certainly fulfil, in a remarkable degree, all the requirements of the obstruction-theory.

When one or both pleuræ are full of fluid, and lung carnified on one or both sides, the pathological conditions of obstruction to the pulmonic capillaries arise in a remarkable degree. This is usually clearly manifested also in the symptoms. If ascites or some cause of abdominal distension be added, perhaps no cases could be named which are in so high a degree attended with cyanosis, distension of the veins of the neck, and all the symptoms of obstructed circulation on the right side of the heart. Yet it is a result of the most careful analysis of my experience of these affections, that they do not in the slightest appreciable degree tend to produce cardiac hypertrophy and dilatation. This will be clearly seen by a reference to Section III. of the appendix to this paper.

I am now in a position to state the theory which has all along guided me in these researches, but in support of which, it will be observed, I have appealed to evidence collected without the slightest reference to any such theory, or indeed any theory on the subject. Until the last few months, it had never occurred to me to doubt or modify the prevailing opinion, and I trust that the reader will accord me the credit of not having allowed preconceptions to prevail with me in the statement of the above facts.

Led by fact and observation, as well as by theory, I am now well assured that it is chiefly, if not exclusively, those diseases of the lung that lead to diminution of its volume, which have any appreciable influence in dilating the heart. Nothing can be more certain than that in emphysema and the collateral affections, dilatation of the heart is a secondary result of extreme frequency. It would probably not be too much to state, that nine-tenths of the cases of cardiac hypertrophy not referrible to valvular disease, are found to be connected either with emphysema of the lungs or with some of the atrophic affections which I have already, in another department of this journal, shown to be its congeners.* Now in these cases I have endeavoured to show that emphysema is not the primary, but in all cases the secondary lesion; and that it depends upon the dilatation of the chest in inspiration, acting upon lungs some portions of which are diminished in bulk by disease. Not to retrace these arguments, I shall content myself with stating, as a corollary of the doctrine I have maintained with regard to emphysema, that the same organic causes which tend to the abnormal dilatation of the air-vesicles of the lung, tend also to the abnormal dilatation of the cavities of the heart: that this affection, therefore, accompanies diseases of the lung, not so much in virtue of the existence of obstruction of the capillary system, *as in consequence of partial atrophy or collapse of the air-cells;* the special forms of disease with which cardiac hypertrophy is connected being bronchitis, bronchial obstruction, some forms of chronic pneumonia, retrograde and healing tubercles, and chronic pleurisy or empyema, *when accompanied by contraction of the chest.* If this view be correct, it will follow that emphysema (*per se*) is in no material degree *the cause* of cardiac dilatation; but rather that cardiac dilatation and emphysema have one common cause—viz., partial atrophy of the lung; and that emphysema owes its acknowledged frequent coincidence with dilatation of the heart to their arising in many cases out of the same conditions. Further, if the justice of my views in general be admitted, it will follow, that a certain amount of atrophic disease of the lung being given, it will react with even greater power upon the heart, when, from the extended character or peculiar disposition of the lesion, the lung is enabled to resist the dilating power of the chest; and, therefore, that we may look for a certain number of cases where emphysema is insignificant in amount, and where collapse or atrophy of the lung and the concomitant dilatation of the heart exist in the most extreme form. Such cases, I believe, are not unfrequent, but they have hitherto been generally misinterpreted, the disease of the lung being supposed to be the result, and not the cause, of disease of the heart. The occurrence of dropsy of the pleura and of hæmorrhage of the lung in many cases of heart-disease,

* British and Foreign Med.-Chir. Review for April, 1853, p. 452: article on Bronchitis, Pulmonary Collapse, and Emphysema.

has a peculiar tendency to mask the original affection; and I believe that I have myself observed many cases of cardiac disease so accompanied, and have erroneously supposed the entire pulmonary disease to be a complication, when I might, with at least equal probability, have looked to it as the real source of the more prominent cardiac affection. I am therefore led to hope, that by more careful observation in future, the law I have now attempted to elucidate may be more extensively confirmed, and may even be brought to explain a large class of cardiac diseases which are at present inexplicable, or have fallen to be accounted for by extremely vague pathological theories.

APPENDIX,

Containing an Investigation, founded on Pathological Anatomy, into the Causes of Hypertrophy of the Heart, apart from Valvular Disease.

I. *State of the most important organs of the Thorax and Abdomen in 24 cases of Hypertrophy of the Heart, not accompanied by Valvular Deformity.*

Case 1. (Pathological Register,* xiv. 3.)—A female, æt. 41. General dropsy; hypertrophy of right ventricle, not great; obsolete tubercle of lungs; extensive atrophy, with emphysema in early stage; mucous obstruction of bronchi; large fibrous tumour of uterus; liver, spleen, kidneys, normal; aorta very slightly opaque.

Case 2. (P. R. xiv. 10.)—A male, æt. 60. Died by coma, without organic lesion of brain; hypertrophy chiefly of right side. Obsolete tubercle (?), atrophy, and marked emphysema of lungs; muco-purulent obstruction of bronchi; liver, kidneys, and spleen normal.

Case 3. (P. R. xiv. 28.)—A female, æt. 25. Hypertrophy, chiefly of right side; tubercle of lungs, with contracting cavities, atrophy, induration, and emphysema; waxy liver, kidneys, and spleen.

Case 4. (P. R. xiv. 55.)—A male, æt. 60. Heart dilated with comparatively slight hypertrophy; its tissue pale and flabby; aortic valves *scarcely* competent, but no material deformity; adhesion of pericardium over part of left ventricle; large aneurism of ascending aorta; lungs normal, except a few tubercles at apex; liver, spleen, and kidneys normal; aorta very atheromatous throughout.

Case 5. (P. R. xiv. 64.)—A male, æt. 33. Hypertrophy chiefly of right side, not great; weight of heart, 12½ oz.; chronic tubercle, with atrophy in upper lobes of both lungs; secondary development of miliary tubercle, with no large cavities; indistinct "nutmeg" appearance of liver; spleen and kidneys normal; aorta quite normal throughout.

Case 6. (P. R. xiv. 78.)—A male adult, age not stated. Dilatation of right side, slight; hepatization of upper lobe of right lung, with contraction; fibrinous plugs in bronchi, and emphysema, to a slight extent; waxy liver, with great hypertrophy; waxy spleen; tuberculated kidney (Bright's disease).

Case 7. (P. R. xiv. 82.)—A female, æt. 54. Slight hypertrophy of right side; bronchitic collapse and emphysema; liver, spleen, and kidneys normal.

Case 8. (P. R. xiv. 93.)—A female, æt. 40. General dropsy; considerable hypertrophy of both sides; weight of heart, 22½ oz.; bronchial obstruction, collapse, atrophy, concretions, and hepatization of lungs; fluid in pleuræ; incipient

* The references in brackets are to the Register of Dissections in the Royal Infirmary. The Roman numerals refer to the volumes, the Arabic figures to the numbering of the cases.

cirrhosis of liver; spleen firm; kidneys normal; aorta very atheromatous throughout.

CASE 9. (P. R. xiv. 99.)—A male, æt. 70. Slight hypertrophy of both sides; collapse and emphysema of lungs; cystic atrophy of kidneys; atheroma of aorta; liver and spleen not mentioned.

CASE 10. (P. R. xiv. 109.)—A male, æt. 40. Considerable hypertrophy and dilatation of both sides, especially right; atrophy and induration of lungs, with emphysema and recent hepatization; liver, kidneys, and spleen normal; slight atheroma in aorta and pulmonary artery.

CASE 11. (P. R. xiv. 111.)—A female, æt. 51. Considerable dilatation, with comparatively slight hypertrophy of both sides; retrograde tubercle, atrophy, and induration of lungs; recent pleuritic effusion; liver and spleen normal; Bright's disease of kidneys; slight atheroma of aorta.

CASE 12. (P. R. xiv. 122.)—A male, æt. 49. General dropsy; hypertrophy, especially of right side; weight of heart, 16 oz.; polypus in right auricular appendage; atrophy, induration, emphysema, and great œdema of lungs; hydrothorax; liver, spleen (?), and kidneys normal.

CASE 13. (P. R. xiv. 126.)—A male, æt. 69. Extreme corpulence and general anasarca; great hypertrophy and dilatation of both sides; disease of coronary arteries; weight of heart, 35 oz.; collapse, with bronchial obstruction, atrophy, and moderate emphysema of lungs; hydrothorax; incipient cirrhosis of liver; firm spleen; kidneys congested, otherwise normal (albuminuria); dilatation and atheroma of ascending aorta; descending aorta atheromatous, but not dilated.

CASE 14. (P. R. xiv. 140.)—A male, æt. 30. Slight hypertrophy; weight of heart, 15 oz.; tubercle of lungs, in great part obsolete; small excavations, and emphysema; liver, spleen, and kidneys normal; slight atheroma of aorta.

CASE 15. (P. R. xiv. 150.)—A male, æt. 55. Death from cerebral softening; great hypertrophy and dilatation of all the cavities; adherent *true* polypi of left ventricle (recent); double hydrothorax; hæmorrhage of lungs (imperfect description).* Incipient cirrhosis of liver; spleen and kidneys (?) normal; slight atheroma of aorta.

CASE 16. (P. R. xiv. 159.)—A male, æt. 45. Death from cerebral softening; moderate hypertrophy, chiefly of right side; weight of heart, $15\frac{1}{2}$ oz.; fatty degeneration of fibre; extreme hæmorrhagic condensation of lungs; a puckering, with cretaceous concretion, in right lung; liver, spleen, kidneys, mostly normal, but containing secondary limited deposits; aorta slightly atheromatous.

CASE 17. (P. R. xv. 18.)—A male, æt. 35. Slight hypertrophy; tubercle, atrophy, induration, and emphysema of lungs; liver, spleen (?), and kidneys normal.

CASE 18. (P. R. xv. 29.)—A female, æt. 35. Hypertrophy of both sides, especially right; weight of heart, $15\frac{1}{2}$ oz.; very great collapse, atrophy, and emphysema (history of chest affection dating from measles); liver, spleen, and kidneys (?) normal; aorta normal.

CASE 19. (P. R. xv. 33.)—A male, æt. 55. Hypertrophy of all the cavities; weight, 18 oz.; bronchial obstruction, collapse of lungs, and emphysema; liver, kidneys, and spleen normal; extremely rigid aorta, with calcareous deposit.

CASE 20. (P. R. xv. 35.)—A male, æt. 48. Hypertrophy of both sides; weight of heart, 14 oz.; adhesions of pericardium; retrograde tubercle, cavities, atrophy, induration, and hepatization of lungs; liver, spleen (?), and kidneys (?) normal; aorta normal.

* The cause of the hypertrophy in this case and the following is by no means clear. The hæmorrhage of the lungs and the hydrothorax are evidently secondary lesions; and it will scarcely be contended that the cirrhosis of the liver and the adherent polypus of the left ventricle in the one case, the fatty degeneration of the heart in the other, or the slight atheromatous disease of the aorta, are adequate causes of the cardiac hypertrophy. I am inclined to suppose that an extensive atrophic disease of the lung has been overlooked in both cases, being masked by the existence of recent hydrothorax and hæmorrhage. In case 16, indeed, partial atrophy is noted, but not as being considerable in amount.

Case 21. (P. R. xv. 45.)—A female, æt. 24. Slight hypertrophy, chiefly of right side; anasarca; retrograde tubercle of lungs, atrophy, induration, emphysema; liver, spleen, and kidneys normal; aorta normal.

Case 22. (P. R. xv. 55.)—A female, æt. 36. Slight hypertrophy, chiefly of right side; atrophy and emphysema of lung, grey hepatization, &c.; liver, kidneys, and spleen normal.

Case 23. (P. R. xv. 56.)—A female, adult, age about 35. Slight hypertrophy, chiefly of right side; contracted left lung, with dilated bronchi; recent bronchitis, with bronchial abscesses of right lung; fatty liver; waxy kidney (extreme Bright's disease); waxy spleen.

Case 24. (P. R. xv. 73.)—A female, æt. 29. Slight hypertrophy, especially of right side; obsolete tubercle, with atrophy and emphysema of lungs; liver, spleen, and kidneys normal.

Analysis of the above Cases.

(*a*) *The Heart.*—The word *hypertrophy* is here used to signify increase of external size and weight. In this sense it almost always corresponds to a certain increase of muscular substance, accompanied by more or less dilatation of the cavities. It is not pretended that any special means were taken to ensure accuracy upon the subject, beyond the use of an experienced eye, aided, in regard to doubtful or peculiarly interesting points, by the rule and balance. Exact measurements were certainly not made in all the cases; but I can nevertheless state that all doubtful cases are excluded from the above return. Nor has any case of the so-called hypertrophy with contraction, or even without dilatation of the cavities, been included. Such cases are, according to my experience, quite exceptional; and I believe, with Dr. Joy and others, that they are mostly pseudo-morbid appearances.

It may also be assumed as a fact, that where hypertrophy existed at all, the right side was to some extent involved. Indeed, I have never seen an instance of hypertrophy affecting the *left side alone*.

On the other hand, the *right side* was *alone* or almost exclusively affected in a considerable number of the above cases. It would be very difficult to determine the proportion of cases in which hypertrophy of the right side *alone* existed, because the lesion is in these cases always incipient, and it is impossible to pronounce upon the absolute existence or non-existence of a certain degree of hypertrophy on the left side. It is commonly stated that pulmonary disease gives rise to hypertrophy of the right side exclusively. A careful study of these cases individually will, however, show that even in hypertrophy evidently connected with pulmonary disease, the left side of the heart is always involved when the hypertrophy becomes considerable. I have never seen a heart, under any circumstances (except, perhaps, those of congenital malformation), exceed its normal weight by more than a few ounces, without the left cavities and their walls participating in the lesion. In cases 8, 10, 11, 13, 15, 19, will be found the most marked instances of hypertrophy and dilatation extending to both sides of the organ; and in by far the greater number of cases the left cavities were appreciably, though slightly, affected.

In 2 cases, organized polypi of fibrinous matter existed in the heart (12, 15); but in neither was it probable from their position that they had influenced the progress of the hypertrophy; indeed, they had the appearance of being recent formations.

In 2 cases there were adhesions of the pericardium, possibly of clinical importance (4, 20). I have elsewhere shown that this lesion is, in chronic disease at least, not invariably, or even usually, connected with hypertrophy of the heart;* but it may nevertheless be admitted as having possibly influenced the progress of these cases, especially when combined with other causes of hypertrophy or dilatation. In one of the cases (4) of adherent pericardium, the tissue of the heart was pale and flabby, probably fatty.

* Monthly Journal of Medical Science.

In 1 case (13) there was disease of the coronary arteries of the heart.

(*b*) *The Lungs.*—In all the cases, *without exception*, the lungs and pleuræ were more or less abnormal. In 1 case only (4) were the pulmonary lesions so slight as to be considered unimportant in reference to the history of the fatal disease; in this case a few tubercles existed at the apices, and the cause of the hypertrophy was manifestly a large aneurism of the ascending aorta. In 2 other cases (15, 16) the greater part of the disease found in the lung was recent, and manifestly of secondary occurrence, considered in relation to the cardiac affection; but in one of these cases there were certainly, in the other not improbably, traces of older disease, partially hidden by the recent affection.

In 21 of the 24 cases (being all those remaining after the above deductions), there were manifest and extensive *old atrophic lesions* of the lungs, with or without accompanying emphysema, which is recorded as having existed in 17 of the cases. The atrophy was in the majority of cases accompanied by induration, and in a large proportion by retrograde tubercle; in several, however, it was mostly what I have called elsewhere "simple atrophy;" in 4 cases (6, 7, 9, 19) it presented the appearance of being *comparatively* recent, and in all of these the hypertrophy of the heart was slight, except the last, in which it was probably caused in part by an extremely diseased aorta.

In 10, or possibly in 11 cases, there were tubercular lesions, but in no case were these in the rapidly advancing form; on the contrary, they were associated universally with the induration and atrophy above mentioned, and usually presented the appearance of cicatrices and concretions. In the instances in which cavities were present they were small and manifestly contracting.

Hepatization or œdema of the lung existed in 6 cases; hydrothorax or pleurisy in 5 cases; hæmorrhage of the lung only in the 2 cases (15, 16) above referred to as being doubtful instances of atrophic disease. The whole of these lesions may be dismissed from consideration in considering the causes of the cardiac affection, being evidently recent and secondary.

(*c*) *The Liver.*—In 1 case of the 24, not mentioned; in 15, normal. Of the remaining 8 cases, 5 may be thrown out of consideration, as being instances of lesions either slight or doubtful, and therefore not calculated to influence the heart. The 3 instances of decided disease are as follow: 2 cases (3, 6) of waxy liver, associated with Bright's disease of kidney and retrograde tubercle; 1 case (23) of fatty degeneration, with dilated bronchi and complete atrophy of one lung, as well as with extreme Bright's disease.

(*d*) *The Spleen.*—Disease of this organ accompanied that of the liver in the above cases of decided disease, and in several of the doubtful cases; normal in 14 cases.

(*e*) *The Kidneys.*—Normal in 14 cases; doubtfully or slightly diseased in 5 cases. Of the remaining 5, 4 were cases of Bright's disease (3, 6, 11, 23), all of them being associated with chronic disease of lung, and 3 with disease of liver, as already mentioned; 1 was a case of cystic atrophy of the kidney (9), also with marked disease of lung.

(*f*) *The Pancreas.*—Not regularly noted; I therefore abstain from numerical statements. I believe, however, that it was examined in a large proportion of the cases, and that no instance of disease presented itself.

(*g*) *The Aorta.*—In 7 cases, not mentioned in the report, though probably examined. Of the remaining 17 cases, normal in 7; affected with slight atheroma, not amounting to rigidity or thickening, in 5; considerably diseased in 5, among which there is found 1 case of aneurism (4), and 1 of dilatation of the arch (13). In all the cases except four there was considerable atrophic disease of lungs, but there is reason, from the details, to think that in Nos. 4, 8, 13, 19, the state of the aorta may have exercised some influence.

(*h*)—A review of the above cases, and of the analysis of them here presented, affords grounds for believing that in estimating the effect of organic diseases of the viscera on the heart, we may dismiss from consideration the liver, spleen, and pancreas, (we might also add the brain.) Disease of these organs accompanies cardiac hypertrophy only in exceptional cases; and in the majority of these it is almost certainly a secondary condition, associated with other organic disease, and of later date than the cardiac affection. The influence of renal disease on the heart is scarcely more decided in the above cases than that of disease of the liver; in the cases in which it occurs it is frequently secondary, and constantly associated with other forms of disease, especially with retrograde pulmonary tubercle. There is somewhat more reason to attribute to disease of the aorta, and especially of the arch, an influence in the production of cardiac hypertrophy; but even this is a comparatively rare coincidence. Diseases of the lung, on the contrary, and especially those forms of atrophic disease which are usually connected with emphysema, coincide with hypertrophy of the heart in the vast majority of cases in which that affection is not dependent on valvular disease.

In the following sections I shall endeavour to show the nature of the connexion which thus manifestly exists between disease of the lung and hypertrophy of the heart.

II. *Numerical Analysis of miscellaneous cases of fatal disease, examined in the Royal Infirmary of Edinburgh; showing the connexion between Cardiac and Pulmonary Disease.*

The following numbers are founded principally on a series of 84 cases, the post-mortem appearances in which were recorded by myself in the winter of 1851-2; the most scrupulous care being taken at the time to verify the individual facts, and to transfer to a classified index every lesion of importance discovered in the parts examined. The results, therefore, obtained from this series have, besides their relation to the present inquiry, an ***absolute*** numerical value; or at least, they approach as near to absolute accuracy as the nature of the investigation permits. In other words, it may be assumed, with regard to this series, that a lesion, not stated to exist, *was looked for, and not found;* and, therefore, that the proportion of cases in which any morbid condition was found, represents very nearly the actual proportion in which it existed.

I have, however, in one important branch of the inquiry, referred to a larger series of 414 mixed cases, collected at a considerably earlier period. This series has by no means the same value as the other, no attempt having been made to secure numerically complete results; but it furnishes corroborative data of some importance, as they were recorded long before any of the theoretical views embraced in this paper had begun to occupy the mind of its author.

(*a*) *Connexion of Hypertrophy of the Heart with Emphysema of the Lung.*—In the series of 84 mixed cases, hypertrophy of the heart occurs 13 times (excluding 6 doubtful instances) = 15·5 per cent. of the whole number. Of these, 6 cases were from valvular disease = 7·1 per cent. of the entire number of cases.

In the same series, there were 26 cases of emphysema of the lung = 30·9 per cent.

Among the 26 cases of emphysema, there were 6 instances of hypertrophy of the heart = 23·1 per cent. *of the emphysematous cases.* Of these 6 cases, 2 were instances of valvular disease. This number is too small for an accurate numerical indication; but the proportion of cases of valvular lesion among the emphysematous cases may be fixed arbitrarily at *under* 8 per cent.

Combining these results, it appears that—

	Of mixed cases.		Of emphysema cases.
Hypertrophy of heart occurs in ...	15·5 per cent.	...	23 per cent.
Valvular lesion with hypertrophy in	7·1 „	...	Under 8 „

In the larger and less exact series, there are recorded only 40 cases of considerable emphysema, and 47 of cardiac hypertrophy; evidently none but the more marked examples of both lesions. The latter has been, however, obviously more accurately noted than the former, which, from its extreme frequency, is apt to escape being recorded.

Applying the same process of analysis to these cases, and arranging the results as above, the result is as follows:

	Of mixed cases.		Of emphysema cases.
Hypertrophy of heart occurs in ...	11·4 per cent.	...	30 per cent. *nearly.*
Valvular lesion with hypertrophy in	5·0 „	...	5·3 „

From both these series of cases it appears, that while hypertrophy of the heart is associated with emphysema of the lung in a much larger proportion than with other forms of disease in general, the excess of its frequency in emphysematous cases is entirely due to the cases of hypertrophy without valvular disease. Hypertrophy from disease of the valves, notwithstanding its almost invariable association with secondary affections of the lung, has, in fact, no great tendency to produce, or to be connected with, emphysema; its usual sequelæ being dropsy, hydrothorax, œdema of the lung, &c., conditions almost exclusive of, and inconsistent with, dilatation of the air-cells. In regard to the connexion of emphysema with hypertrophy of the heart, I believe that theory and experience will be found to agree in the explanation of the above numerical statements. It is sufficiently established by them, as well as by the facts adduced in Section I., that *in cases where emphysema of the lung is associated with cardiac hypertrophy, the former is the primary, and the latter the secondary lesion.*

But, as I have elsewhere shown that emphysema of the lung is itself invariably a secondary affection, and that its cause is to be found in partial atrophic affections of the pulmonary tissue, it becomes infinitely probable that the latter are the real antecedents of hypertrophy of the heart in emphysematous cases, and that they are the common causes of these two diseases. This view is borne out by the facts in Section I., which show that pulmonary atrophy, from a variety of causes, may be connected with hypertrophy of the heart.

The following paragraphs show, by numerical data, that neither pneumonic consolidation nor tubercular disease of the lung have, considered apart from collateral circumstances, any marked tendency to occur in connexion with hypertrophy of the heart.

(*b*) *Connexion of Hypertrophy of the Heart with Tubercle, and with Hepatization of the Lung.*—In the smaller series of 84 cases, there occur 29 cases of tubercle, 14 of hepatization of the lung, and, as before mentioned, 13 of hypertrophy of the heart. Among the 29 tubercular cases, hypertrophy of the heart occurs in 3; among the 14 cases of pneumonia, hypertrophy is found to have been present in 1 case certainly, and doubtfully (i. e., to a scarcely ascertainable extent) in 2 others. From these numbers, it follows that hypertrophy of the heart occurs in about 10·3 per cent. of the tubercular cases, while in the miscellaneous cases it bears even a larger proportion to the whole—viz., 15·5 per cent., or *vice versa;* that among the cases of hypertrophy, tubercle occurs in the proportion of 23 per cent., while in the miscellaneous cases it presents a proportion of 34·5 per cent. It is thus evident that the relation between tubercle and hypertrophy of the heart is not of a very intimate character.*

In the case of pulmonary hepatization, the numbers are not sufficiently large to afford a definite proportion; but they certainly give little countenance to the idea of any very intimate relation between pneumonia and hypertrophy of the heart.

The general result to which the facts in this section, as well as in the last, appear to lead is, that *while diseases of the lung which merely obstruct or obliterate the circulation in its capillaries have no well-marked tendency to be associated with hypertrophy of the heart, those which produce atrophy of its tissue,*

* A further analysis of the tubercular cases would show, what is, however, sufficiently demonstrated by the details in Sect. I., that while advancing tubercle has even less tendency than most other forms of disease to be associated with hypertrophy of the heart, retrograde tubercle—i. e., tubercle connected with pulmonary atrophy—is a very frequent concomitant of that form of cardiac hypertrophy not dependent on valvular disease.

and secondarily, emphysema, have an obvious influence on the heart, and are frequently the causes of its hypertrophy.

It remains to examine into those cases in which the function of the lung is impeded, and its tissue compressed and altered, by causes acting from without. This will form the subject of Section III.

III. *Examination of cases bearing on the question — Have Effusions into the Pleuræ and Peritoneum, or other causes compressing the Lungs, and giving rise to severe Dyspnœa and Pulmonary Obstruction, any influence in determining Hypertrophy of the Heart?*

Case 1. (P. R. xii. 51.)—General dropsy, hydrothorax; simple dilatation and hypertrophy of heart.

Case 2. (P. R. xii. 126.)—Empyema, perforating the pleura *inwards;* compressed lung; pneumothorax; waxy liver and kidney; heart normal.

Case 3. (P. R. xii. 141.)—Empyema and pneumothorax from tubercle; perforation of pleura *outwards;* Bright's disease of kidney, general dropsy; heart normal.

Case 4. (P. R. xii. 143.)—Contraction of right side of chest from absorbed empyema; hypertrophy of right ventricle of heart.

Case 5. (P. R. xii. 210.)—Chronic empyema of three months' standing, perforating the pleura *inwards;* compressed lung; pneumothorax; heart normal.

Case 6. (P. R. xii. 239.) — Chronic empyema, perforating *inwards;* compressed lung; pneumothorax; recent lymph on endocardium; heart otherwise normal.

Case 7. (P. R. xii. 279.) — Chronic empyema, perforating *inwards;* compressed lung; pneumothorax; heart normal.

Case 8. (P. R. xii. 287.)—Clear sero-fibrinous effusion (probably recent) in right pleura; compressed lung; hypertrophy and dilatation of heart, without valve-disease.

Case 9. (P. R. xii. 372.)—Chronic pleuritic effusion on one side; tubercle of lung; compression of lung; heart normal.

Case 10. (P. R. xiii. 51.)—Large serous effusions in both pleuræ and in peritoneum; cirrhosis of liver; heart small, soft, and flabby.

Case 11. (P. R. xiii. 51.)—General dropsy, double hydrothorax (extreme), ascites; cirrhosis of liver; slightly enlarged spleen; normal kidney; atheroma of aorta; great collapse of lungs; heart small, its fibre highly granular.

Case 12. (P. R. xiv. 14.)—Extreme general dropsy, ascites, double hydrothorax, carnified lungs; liver, spleen, and kidneys normal; large fibrous tumour of left ovary; aorta normal; right side of heart very much distended, doubtfully hypertrophied, all the orifices apparently normal; weight of heart 10½ ounces.

Case 13. (P. R. xiv. 117.)—General dropsy from Bright's disease of kidney; double hydrothorax; great compression of lungs; perforation of pleura *inwards* on right side; slight atheroma of aorta; "heart *small,* normal."

Case 14. (P. R. xiv. 153.)—General dropsy, without apparent organic cause; double hydrothorax; ascites; extreme compression of lungs; slight atheroma of aorta; heart (examined with extreme care) free from hypertrophy or dilatation.*

Case 15. (P. R. xv. 8.)—Bright's disease of kidney, with granulations; obsolete tubercle in lungs and intestines; adhesions (universal) of pericardium by cellular tissue; effusion into right pleura (one quart), with partial collapse of corresponding lung, which contained a gangrenous abscess; liver normal; spleen large, waxy;

* Cases 11 to 14 deserve great attention. The hydrothorax being double, and in some instances accompanied by ascites, was the cause of extreme dyspnœa and impediment to the pulmonary function, probably of considerable standing, yet in no case was there appreciable hypertrophy of the heart. In case 14, the cause of the dropsy being obscure, the heart was under suspicion, and was accordingly accurately weighed and measured in all its parts.

kidney (see above); ***heart weighed 6 ounces, perfectly free from disease or hypertrophy.****

CASE 16. (P. R. xv. 11.)—Great sero-purulent effusion (chronic pleurisy) in left pleura; the corresponding lung compressed; lungs otherwise normal, except slight tuberculous atrophy; liver and kidneys normal; spleen not examined; heart quite normal.

CASE 17. (P. R. xv. 66.)—A female, æt. 60. Moderate double hydrothorax; great distension of abdomen from large fibrous tumours of uterus, and ascites from cancer of peritoneum; atrophic disease of right, and partially of left kidney; liver and spleen mostly normal; heart of small size, quite normal. The great veins of chest and the coronary veins considerably distended.

CASE 18. (P. R. xv. 74.)—A female, æt. 35. Extreme ascites from cirrhosis of liver with atrophy (29 ounces), repeatedly requiring paracentesis on account of abdominal distension; partial collapse of lungs; spleen enlarged (11 ounces); kidneys normal to naked eye, fatty granular deposit in tubuli; much distension of venous system; heart small.

Summary of the above cases.

In order to obtain a sufficient basis for induction, it was necessary to make available the records of cases extending over several years, and not elaborated with a view to statistical results. The summary of these cases must be, therefore, attempted on a different plan. In collecting the above data, I have thought it right to omit, as merely complicating the details, on the one hand, all cases of *valvular* disease of heart, with manifestly secondary dropsical effusions; on the other hand, all instances of effusions, dropsical or inflammatory, which were manifestly too recent to be reasonably suspected of a tendency to cause cardiac hypertrophy. This office of selection has been exercised with great caution; and the reader has thus before him all the really valid evidence in relation to this inquiry which I have been able to procure, by a search through about 800 miscellaneous cases.

(*a*) *Sources of the Compression of the Lungs in these cases.—Hydrothorax*, generally with anasarca and general dropsy, but in two cases accompanied only by ascites, occurred in Nos. 1, 10, 11, 12, 13, 14, 15, 17. In case 14, the pulmonary pleura of one side was perforated from without from the extreme distension.

Ascites occurred in Nos. 10, 11, 12, 14, 17, and 18. In the first two, and in the last, it was determined, at least in part, by cirrhosis of the liver; in No. 17 it was the consequence of a cancerous affection of the peritoneum; in the others it was an accompaniment of general dropsy.

A large fibrous tumour of the uterus combined with ascites to produce distension of the abdomen in No. 17.

Empyema of one side occurred in Nos. 2, 3, 5, 6, 7, 9, 16. In all these cases, except the last two, it was accompanied by perforation of the pulmonary pleura, generally with pneumothorax. In cases 2, 5, 6, and 7, the perforation was evidently from without inwards, and was therefore clear evidence of long-continued and extreme distension; in one case (3) the perforation was tubercular, and in this case the pneumothorax and distension of the pleura *may* have been recent. In cases 9 and 16 there existed a distinctly chronic effusion, with considerable distension, but without perforation.

* This case is in many respects very remarkable. Notwithstanding the condition of the lungs, kidney, and pericardium, each of them of a kind supposed to have a tendency to produce hypertrophy; and notwithstanding a considerable effusion into the pleura, the heart remained small, and was not found even distended with blood. I believe the solution of these difficulties to be as follows: The tuberculous tendency had only been recently extinguished, and from emaciation and impoverished blood no material dyspnœa had been experienced until the supervention of the pleuritic effusion under the influence of disease of the kidney and local inflammation. I have elsewhere (*loc. cit.*) shown the immunity of the heart under pericardial adhesion in the majority of chronic cases.

In No. 4 the chest was *contracted on one side, with the remains of an absorbed empyema;* the lung remaining considerably collapsed on the diseased side.

In No. 8, pleuritic effusion existed on one side, of a character not distinctly defined. It was probably recent; in which case, it can have had no influence on the cardiac affection. This case might perhaps have been justly excluded from the series; but as the origin of the cardiac affection is obscure, it was thought expedient to retain it.

(*b*) *The cases of hypertrophy of the heart* in the above series are only four in number—viz., Nos. 1, 4, 8, 12; of these the last is doubtful, the organ weighing not more than $10\frac{1}{2}$ ounces.

In No. 1, the cause of the cardiac hypertrophy is not clearly intelligible; but the hydrothorax and general dropsy must have been, in all probability, secondary on the disease of the heart, as no affection of the kidney is recorded. The same remark applies to Nos. 8 and 12.

In No. 4, a slight degree of hypertrophy of the heart accompanied an old inflammatory affection of the pleura; the effusion being here absorbed, the side retracted, and the lung atrophied, all the conditions are present for the development of hypertrophy of the heart, according to the law which I have attempted to develop at page 215. This fact is the more striking from the circumstance, that in the numerous cases of empyema with large effusion, the heart was uniformly normal.

(*c*) *The cases in which the heart was not hypertrophied or dilated* are Nos. 2, 3, 5, 6, 7, 9, 10, 11, 13, 14, 15, 16, 17, 18. Among these the organ is noted as *normal* or *not hypertrophied* in 8 cases; as *small* in 6 cases, in one of which (15) it weighed only six ounces.

The general conclusions from the observations in this section are as follow:—1st. In *all* the cases of *empyema* (except No. 4, in which the effusion was absorbed, the thorax retracted, and the lung atrophied), the heart was either normal or small; 2nd. In *all* the cases of hydrothorax, ascites, or abdominal distension, in which was an evident organic cause for these affections, independent of the heart, that organ was either normal or small; 3rd. In 2 (*possibly* 3) cases there was hypertrophy of the heart, accompanied by thoracic effusion; but in these it is doubtful which was the primary and which the secondary lesion.

There is therefore no proof, nor even probability, that thoracic and abdominal effusions have any power to induce hypertrophy of the heart.

IV. *General Summary of the preceding Investigation of Facts.*

The following conclusions seem to be fairly deducible from this inquiry, as its ultimate and most important results:

1. It is *possible* (judging from other cases which I have observed, I should say, *not improbable*), that disease of the aorta and of the kidney may have an influence in determining cardiac hypertrophy. (Section I., *e*, *g*.) In the case of the kidney, however, this influence is exceptional; in the case of the aorta, it is only exercised in extreme cases of disease, and chiefly in the case of aneurism or dilatation of the arch.

2. There is no good reason to ascribe to disease of the liver, pancreas, spleen, or brain, any considerable influence in determining disease of the heart. (Section I., *c*, *d*, *f*.)

3. Chronic disease of the lung has a most important influence in determining hypertrophy and dilatation of the heart; and the very great majority of cases of cardiac disease, not caused by deformity of the valves, owe their origin to pulmonary affections. (Section I., *b;* and *Cases*, passim.)

4. Hypertrophy and dilatation of the heart, arising from pulmonary disease, affect in the first instance the right cavities; but when the hypertrophy exceeds a few ounces, the left cavities also become invariably involved in the disease. (Section I., *a;* and *Cases*, passim.)

5. It is *possible* that part of the influence of pulmonary disease on the heart may be due to obstruction of the circulation in the pulmonic capillaries (as is commonly supposed); nevertheless, it appears, that under various circumstances producing serious pulmonary obstruction, hypertrophy of the heart does not occur in a considerable proportion of cases. (Sections II., III.)

6. Tubercular disease of the lung produces hypertrophy of the heart only when combined with pulmonary atrophy and induration. Retrograde or obsolete tubercle, with contracting or obliterated cavities, concretions, cicatrices, &c., is very generally associated with secondary disease of the heart; while advancing tubercle has no appreciable influence (or rather, is frequently the cause of cardiac atrophy). (Sections I., *b;* II., *b;* and *Cases*, passim.)

7. It is not ascertained that pneumonic consolidation, considered *per se*, has any tendency to produce cardiac hypertrophy; but in some cases it appears to do so when accompanied by atrophy or collapse of the lung and emphysema. (Sections I., *b;* II., *b;* and *Cases*, especially Section I., No. 6.)

8. The great majority of the pulmonary lesions which give rise to hypertrophy and dilatation of the heart are accompanied by partial atrophy of the lung; and usually also by emphysema, which (as I have elsewhere shown) is the almost invariable consequence of such atrophy. (Section I., *b*, and *Cases*, passim; Section II.)

9. Atrophy of the lung tends, in an equal degree, to produce cardiac disease, whether it proceed from bronchitis, pneumonia, or tubercle; whether it be "simple atrophy," or accompanied by induration; whether it be seated at the base, apex, anterior or posterior parts of the lung. Generally speaking, a given amount of pulmonary atrophy may be expected to develop a degree of hypertrophy of the heart proportionate to the dyspnœa which it entails, and the amount of contraction produced by it in the affected tissue. (*Cases*, passim.)

10. The well-known concurrence of emphysema with hypertrophy of the heart is, in all probability, due to their common origin in atrophic lesions of the lung; the law of production of the one affection being also that of the other. It is reasonable, therefore, to suspect that pulmonary emphysema and cardiac hypertrophy may be found to be, to some extent, alternating as well as collateral affections, in cases where atrophy of the lung gives rise to the conditions necessary for the production of either.

11. It is rendered by this inquiry extremely probable, that dilatation and hypertrophy of the heart are never otherwise than secondary affections, and that they are dependent, in the very great majority of cases, 1st, on valvular deformity and other obstacles to the circulation in the heart or great vessels (dilatation *from within*); 2nd, on the expansion of the thorax under abnormal conditions (dilatation *from without*). The consequence of either of these forms of dilatation, or even of the tendency to either of them, may be hypertrophy of the muscular substance, due to the effort of the organ to act effectively under an increased resistance to its contraction. In the case of dilatation *from within*, an increased power is required to overcome an obstruction in the circulating system itself; in the case of dilatation *from without*, hypertrophy takes place, because the expansion of the thorax in inspiration tends constantly to overload the heart, and this tendency can only be resisted by increased muscular force. The increase of dilatation without corresponding hypertrophy (the *aneurisme passif* of Corvisart) is always the signal of disaster; because it indicates that the balance of the circulating forces is finally destroyed.

Art. II.

Notes of the Chemical Composition and Microscopical Characters of the Liver and Kidney in some cases of Diabetes, more particularly with reference to the amount of Fatty Matter present in these Glands. By Lionel Beale, M.B.

In a clinical lecture delivered at King's College Hospital, in 1848, Dr. Todd drew attention to the remarkable condition of the kidneys of a patient who died of diabetes. These kidneys were in a state of well-marked fatty degeneration, as the following observations clearly indicate.

They were examined microscopically by Dr. Johnson, and the following is his report:—"The kidneys were slightly above the natural size, vascular; in the cortical portion there were numerous small tubercles. On a microscopic examination, the uriniferous tubes were found of a yellowish colour, from an abundance of granular matter and oil in the epithelial-cells. The appearance of the renal-cells contrasted remarkably with that of the liver-cells, the latter being very transparent and colourless, and containing scarcely any biliary particles or oil; they had the appearance of being starved and emaciated, while the kidney-cells were overfed and plethoric."

The following is my analysis of the kidney of this patient:

	I.		100 parts of solid matter.
Water	76·800		
Animal matter	18·792		
Fatty matter	3·016	...	16·04
Fixed salts	1·392		
	100·000		

Not a trace of sugar could be discovered in the substance of the kidney.

Dr. Todd says, "I may here remark, that this is not the first case of diabetes in which I have found fat in the tubes of the kidney. The kidneys of a woman who died here last year of diabetes, contained fat in considerable quantity."*

Since the above remarks were made, so far as I have been able to ascertain, no further observations have been published upon this subject, which leads me to hope that the following analyses of the kidney and liver in cases of diabetes may prove somewhat interesting. It appears to me that any well-ascertained facts with reference to the particular condition of any of the secreting organs in this obscure disease are worth recording, and although in themselves these observations cannot be said to enlighten us much with reference to the pathology of diabetes, it is more than probable, that by carrying our investigations still further with reference to the state of the glandular organs, and by carefully endeavouring to ascertain the exact manner in which these organs depart from a healthy condition, some information may be ultimately arrived at with respect to the real nature of the affection. In the present communication, therefore, I shall give the results of some analyses which I have made since the delivery of Dr. Todd's clinical lecture, and shall compare them with analyses of organs which, there is every reason to believe, were in a healthy state.

* From the report of the lecture by S. J. A. Salter, Esq., in the Provincial Medical and Surgical Journal for 1848, p. 343.

The cases that furnished the glands were all well-marked cases of diabetes, which have occurred in King's College Hospital, and, with the exception of one, have formed the subject of clinical lectures by Dr. Todd, which either have been published, or are in course of publication. For details of the symptoms, and other particulars connected with these patients, I must therefore refer to the reports of the lectures in the journals.

It may be here remarked, that the most interesting facts ascertained in the following inquiries are, that the quantity of fatty matter present in a given weight of the cortical substance of the diabetic kidney, exceeds that present in the same quantity of liver; while in a state of health the converse is the case, and that the kidney contains a much greater proportion of fatty matter than is present in the healthy organ, while in the diabetic liver the quantity of fat is less than in health.

The following analyses of the liver and kidney are presumed to represent the composition of these organs in a state of health. No. II. represents the chemical composition of a liver which I analyzed for Dr. Budd.* It was taken from the body of a gentleman 31 years of age, well formed and muscular, and above the middle stature, who had led a temperate life, and who, while in perfect health, was killed by falling from a second-floor window. No. III. was the liver of a schoolmistress, 40 years of age, who had lived well, but very temperately, and had enjoyed good health, until an attack of cerebral hæmorrhage, of which she died. No. IV. is an analysis of a kidney removed from the same body as the liver No. II.

Healthy Liver.

	II.	100 parts of solid matter.	III.	100 parts of solid matter.
Water	68·58		72·05	
Solid matter	31·42		27·95	
Fatty matter	3·82	12·15	4·28	15·31
Extractive, soluble in water and alcohol; Extractive, soluble in water only, and Albumen	10·07	32·04	10·40	37·20
Alkaline and earthy salts	1·50	4·77	1·19	4·25
Matter insoluble in water, alcohol, and ether	16·03	51·01	12·08	43·22

Healthy Kidney.—199·9 grains were operated on.

	IV.	100 parts of solid matter.
Water	76·450	
Solid matter	23·550	
Fatty matter, containing much cholesterine	·939	3·98
Extractive matter, soluble in water	5·840	24·79
Fixed alkaline salts	1·010	4·28
Earthy salts	·396	1·68
Albumen, vessels, &c.	15·365	65·24

* Diseases of the Liver, second edition, 1852, p. 283.

The liver and kidney forming analyses V. & VI. were taken from the body of a girl, æt. 19, who died in the hospital, of diabetes, in March, 1851. This patient (Catherine Keyworth) was admitted into the hospital, under the care of Dr. Todd, about a fortnight before her death. She appeared to have been suffering from diabetes about a twelvemonth or rather more. At the time of her admission she was passing about ten pints of urine per diem, varying in specific gravity from 1035 to 1045, and was suffering from slight cough, unaccompanied by expectoration. The emaciation increased, and she gradually sank; but her death was not preceded by convulsions, nor was there any tendency to coma.

Upon examination, the right lung was found to contain tubercles in its upper lobe, and in this portion of the lung there was a small cavity about the size of a walnut. The left lung contained a few chalky masses in its upper lobe.

Liver. Catharine Keyworth, æt. 19.—The liver weighed 2 lbs. 14 oz.; the colour was natural; it was not acid; of hard and firm consistence. Upon microscopical examination the cells were noticed to be large, numerous, and well formed; rather pale, and filled with granular matter. In some instances, however, cells containing a little oil were observed. A certain number of free oil-globules were seen floating in the surrounding fluid.

200 grains were subjected to examination, and the per-centage calculated:

	V.		100 parts of solid matter.
Water	71·60		
Solid matter	28·40		
Extractive matter	4·54	...	15·98
Fatty matter	1·32	...	4·64
Alkaline salts	·86	...	3·02
Earthy salts	·67	...	2·35
Albumen, vessels, &c.	21·01	...	73·97

No cholesterine could be detected in the fatty matter by dissolving it in alcohol, and allowing the alcoholic solution to evaporate spontaneously, and subsequent microscopical examination of the residue.

The alkaline salts contained alkaline and earthy phosphates, chlorides, and sulphates, in considerable quantity.

Kidney. Catharine Keyworth.—The kidneys were larger than natural, weighing 7½ oz. each. The cortical part was pale, presenting the appearance of fatty kidney, but the medullary portion was congested. Upon microscopical examination the tubes appeared large, and contained well-formed epithelial cells, which did not vary much in size, and were circular in form. Much free oil was present between the tubes and on their surface, preventing the epithelium in them from being detected without great pressure. Acetic acid rendered the mass more transparent, so that the condition was easily made out. No *cells* containing fat could anywhere be detected, and in all parts of the kidney could the renal epithelium be seen.

200 grains were operated on, and the results reduced to 100 parts:

	VI.		100 parts of solid matter.
Water	75·53		
Solid matter	24·47		
Extractive matter	7·13	...	29·14
Fatty matter	3·51	...	14·34
Alkaline salts	·92	...	3·75
Earthy salts	traces		
Albumen and renal tissue	12·91	...	52·75

The alkaline salts contained chlorides, a little alkaline phosphate, and traces of sulphates.

Upon comparing these analyses with those of the healthy organs, we notice, in the case of the liver, a diminution in the quantity of the extractive matters, compared with health, and we also find, that while the healthy liver contains upwards of 4 per cent. of fatty matter, from the diabetic liver less than one-third only of this quantity could be extracted. No great variation is noticed in the proportion of fixed salts.

Analyses VII. & VIII. represent the composition of the liver and kidney of a woman, æt. 28, who died in King's College Hospital, in December, 1851, of diabetes; and IX. shows the composition of the medullary portion of the same kidney. This patient was admitted into the hospital in November, 1851. She had been suffering from symptoms of diabetes for about twelve months. At the time of her admission she was very emaciated, and was passing about twelve or thirteen pints of urine a day, which contained much sugar. A specimen of urine passed on December 4th had a specific gravity of 1027, and contained 41·4 grains of sugar per 1000.

The patient died December 9th. Cavities were found in the upper parts of both lungs, and the lower portion of the upper lobe of the right lung was found to be in a condition much resembling hepatization, depending probably upon a change of a very similar nature to tubercular infiltration.

The liver was considerably larger than in a state of health, of a natural colour and consistence. Interspersed at various intervals throughout its substance were many small cysts, varying in size from a hemp-seed to that of a nut, and filled with a viscid, pasty fluid. No oil-globules could be detected in this fluid upon microscopical examination. The cells of the liver were unusually pale, and presented a homogeneous appearance; they contained no oil. The bile found in the gall-bladder was unusually pale.

Liver. Eliza Atkins.—1500 grains were subjected to examination:

	VII.		100 parts of solid matter.
Water	77·360		
Solid matter	22·640		
Extractive matter	2·952	...	13·03
Fatty matter	1·779	...	7·85
Alkaline fixed salts	·936	...	4·13
Earthy salts	·295	...	1·30
Vessels, cells, &c.	16·678	...	73·66

Upon comparing analyses VII. with that of the healthy liver, it will be observed that the relative quantities of extractive matter and fatty matter exhibit striking variations. In 100 grains of the healthy liver is contained a quantity of extractive matter, or animal matter soluble in water, much greater than is present in the diabetic liver; and upon comparing the figures representing the amount of fatty matter in the two cases, it is found that the healthy liver contains more than twice the quantity present in the diabetic organ.

The kidneys were about half as large again as natural; hard, and of rather a brittle nature, but otherwise they appeared healthy. The cortical portion was well developed, and the capsule was not more readily stripped off than in a healthy kidney. Upon microscopical examination the tubes were found to be clear and well defined, containing much epithelium, for the most part of a circular form, and each cell exhibited a well-developed nucleus. The matrix was not unusually developed. The Malpighian bodies were large, and their vessels contained blood, but were not distended with it. *No fat could anywhere be detected,* and the different structures of the kidney were well marked. The tubes were easily broken off in long pieces, which exhibited the epithelium very distinctly.

Kidney. Eliza Atkins.—250 grains were submitted to analysis.

Cortical Portion.

	VIII.	100 parts of solid matter.
Water	81·52	
Solid matter	18·48	
Fatty matter, with much Cholesterine	2·21	11·95
Fixed salts	·99	5·35

Medullary Portion.

175 grains submitted to analysis.

	IX.	100 parts of solid matter.
Water	83·55	
Solid matter	16·45	
Fatty matter, with much Cholesterine	1·48	8·99
Fixed salts	·88	5·34

The circumstance of the fat present in this instance not being recognised in the usual minute globules so characteristic of fatty matter, cannot but be regarded as interesting; and it is important in connexion with the presence of fatty matter in tissues generally, for this analysis clearly shows that fatty matter may exist even in considerable quantity, and may yet not be recognised by the microscope, probably in consequence of being in an exceedingly minute state of division. In fatty degeneration of muscular fibre, the oil may often be observed in the form of minute granules, which can only be proved to be of this nature by the addition of ether, by which they are immediately dissolved; and as the ether evaporates, the fatty matter may be seen to form globules. The quantity of fat which may exist in this granular or molecular state is often considerable. In some specimens of chylous urine a large quantity of fatty matter is present in this form. In an instance which came under my notice, through the kindness of my friend Mr. Cubitt, of Norwich, I found nearly 14 grains of fatty matter in 1000 grains of urine, or upwards of a fourth part of the total quantity of solid matter of the urine was found to consist of fat in a minutely granular form, diffused through the specimen.

The following analysis represents the per-centage composition of the thyroid gland, which was somewhat enlarged, each lobe measuring upwards of two inches in length by one inch in width, and about half an inch in thickness:

	X.		100 parts of solid matter.
Water	75·460		
Solid matter	24·540		
Fatty matter	1·160	...	4·72
Fixed salts	·873	...	3·55

In connexion with the subject of a fatty condition of the kidney in diabetes, I may here remark, that in several cases of this affection, my friend, Dr. Hyde Salter, has observed an unusual quantity of fatty matter present in the follicles of the pancreas. In some cases the secreting cells, upon microscopical examination, appeared completely gorged with oil-globules.

Kidney. M. W. O., female, æt. 27.—This patient was in the hospital, suffering from diabetes, in March, 1851, under the care of Dr. Budd. The kidneys were of the natural size and colour. Consistence, soft. Upon microscopical examination the tubes were found to be filled with epithelium, which caused them to appear as if choked-up with granular matter and oil-particles. Some of the tubes contained very much oil. The cells were pale, containing much fatty matter, and they were very large. Much free granular matter and oil-globules were also observed.

136·7 grains were subjected to examination, and the results calculated to 100 parts:

	XI.		100 parts of solid matter.
Water	77·11		
Solid matter	22·89		
Fatty matter	4·617	...	20·17
Fixed salts	1·136	..	4·96

No cholesterine could be detected in the fatty matter. The fixed salts contained sulphates and phosphates, but not a trace of chloride could be detected.

In this case the liver presented a natural appearance. Upon microscopical examination the cells were found to be unusually pale, large, well defined, and containing as many fat-globules as in health. There were also a good many free oil-globules. It was not chemically examined.

It may be interesting here to give the results of the analysis of a kidney from a confirmed case of fatty degeneration.

The kidney containing the largest quantity of fatty matter of any I have yet examined, is that of Ann White, a patient who was in the hospital, under Dr. Budd; and the history of this case is given by Dr. George Johnson, at page 415 in his work on Kidney-Diseases.

The kidneys were much enlarged, and the capsule was stripped off very readily. The cortical substance was unusually pale.

200 grains were operated, and from the results the per-centage composition was calculated.

	XII.		100 parts of solid matter.
Water	79·652		
Solid matter	20·35		
Fatty matter	5·49	...	26·97
Fixed salts	1·42	...	6·97

I have examined two or three other fatty kidneys, which contained almost as large a quantity of fat as the one referred to. In such a kidney as the present the proportion of fatty matter is so large, that it may surely be looked upon as characteristic. Upon comparing the numbers representing the quantity of fat in 100 grains of the *solid matter* of the healthy organ, and of this fatty kidney, the difference becomes still more striking. In health we find not quite 4 per cent., while here we find upwards of 25 per cent. Of the diabetic kidneys, in analysis I. there is 16 per cent.; in analysis VI. there is upwards of 14 per cent.; in VIII., 11·95; and in the last analysis (XI.) there is as much as 20·17 per cent.

Of the four diabetic kidneys subjected to examination, then, we find that one contained five times the quantity of fatty matter present in the healthy organ; another, four times as much; a third, nearly the same proportion; and the fourth contained three times as much as was found in the healthy kidney. The fatty kidney contained rather more than six times the quantity of fat obtained in the healthy specimen.

Upon referring to the per centage of fatty matter in the solid matter of the two diabetic livers subjected to examination, we find in one only 4·64, and in the other specimen, 7·85 per cent.; while in the two healthy livers we observe the numbers 12·15 and 15·31, respectively representing the per-centage of fat in the solid matter.

The results of the foregoing analyses cannot be looked upon as conclusive; and from these alone I shall not offer to make any deduction, but must, for the present, content myself with simply recording the facts above detailed, and must postpone any remarks until the number of observations may be multiplied. At present it is difficult to state with any certainty the average chemical composition of a healthy liver or kidney, and opportunities seldom occur of obtaining these glands from persons who can be said to have been in a healthy state immediately before death. Nevertheless, I have every reason to believe that the analyses given at the commencement of this article will be found to give a fair representation of the chemical composition of these glands in health. My chief object in this communication is to draw the attention of observers to the comparative amount of fatty matter in the liver and kidney, in some of the cases of diabetes which happen to have fallen under my own observation; and they have been submitted to examination, in the hope, that by still further multiplying observations, the condition of these organs, in a considerable number of cases of diabetes, may be ascertained. The few results that I have obtained, however, are quite sufficient to show that in these cases the kidney, in its chemical composition, approaches to that of fatty degeneration; and that, in these same cases, the liver appears to be, as it were, starved, and its secreting cells seem to manifest a tendency opposite to that of fatty degeneration. I should here remark, that the analyses in this communication have not been selected for the purpose of illustrating any views on the subject, but are all that I have yet made; and the chemical examination was instituted quite independent of the microscopical characters.

Art. III.

Observations on Excision of the Os Calcis; with Cases.
By H. Martineau Greenhow, Esq.

If the history of conservative surgery be examined, it will be found to have gained ground immensely during the last few years;—now, indeed, the skill of the surgeon is shown rather by preserving a limb than by performing a brilliant amputation, or being able to show a fair and round stump.

A hundred years ago, resection of joints was not thought of; and though the Moreaus introduced the operation of removal of the bones of the ankle as a substitute for amputation, and practised it in two cases with success, yet their example remained unfollowed for many years, and a sort of prejudice appeared to exist against the operation—a prejudice now fast vanishing away, based, as it is proved to have been, on very insufficient grounds.

The Moreaus were great advocates of resection of joints, but it never appeared to them feasible to excise, or extirpate singly, any of the larger bones of the tarsus. Thus Moreau says, "Caries of the os calcis does not present the same facilities, with respect to excision, as is the case with the other tarsal bones. If you take off the inferior surface of the calcaneum, the heel can no longer bear the weight of the body, &c. . . . . If the surgeon were obliged to destroy the attachment of the tendo-Achillis, I think it would be better to remove the limb." He then goes on to say, that he has gouged out the greater part of a diseased os calcis, leaving, however, the insertion of the tendo-Achillis untouched, and that the case did well.

Lisfranc performed the same operation with success; and the idea of a nearly complete excision of the bone appears to have occurred to him, for he asks—"Would it not be feasible to remove a great portion of the posterior aspect of the os calcis, in cutting through the insertion of the tendo-Achillis? Would not this tendon, after this section, form adhesions which would counteract the action of the flexor muscles?" Yet, as he adds that amputation "is indispensable when the os calcis is too extensively carious," it is plain that he placed little value on the plan he proposes, and was far from considering the removal of the heel-bone a desirable proceeding.

Mr. Syme does not refer in direct terms to excision of the whole of the os calcis; and, indeed, the idea of such a thing probably never occurred to him, as he says—"When the os calcis alone is affected, the disease may be extirpated by making a crucial incision on the fibular side, and then digging out the carious part with the gouge." He adds, "If the disease extend to any of the other tarsal or metatarsal bones, there is hardly any remedy but amputation; and if either the astragalus or os calcis be affected, of course the whole foot must be removed." Experience shows that little, in so advancing a science as surgery, can be set down as "of course;" and nothing proves it more forcibly than the present cases.

Other modern surgeons seem of much the same opinion; for even Liston and Fergusson do not encourage the practice of excision of the ankle bones. Thus we find that Mr. Liston never excised any of these bones; and his opinion of excision generally is, that "when the soft parts are much diseased, when the disease is not limited to the articulating surfaces, or when the patient is reduced to a low state by hectic," they are not admissible. Again, it appears, that though Mr. Fergusson has performed partial operations on the os calcis with success, yet he has never removed the entire bone; and he says of excisions, that "such operations are, under any circumstances, extremely difficult, and, in most instances, more dangerous to the patient than amputation at the ankle or in the leg."

But it is now proved that these opinions have been, to a great extent, based on *à priori* reasoning; and though at first there appeared to be many reasons against the success of excision, it is now evident that these have been over-rated, and that difficulties have been conjured up which have really no existence. In December, 1847, Mr. Thomas H. Wakley performed his operation of excision of the os calcis and astragalus, and with success. In August, 1848, my father, Mr. Greenhow, successfully performed excision of the os calcis, as he believed for the first time. Mr. Hancock, however, soon after the publication of Mr. Greenhow's case, made known a case in which he had operated in June, 1848, though unsuccessfully; he therefore may be said to be the author of the operation, though perhaps Mr. Greenhow has an equal claim to the honour, inasmuch as he performed it without having heard of Mr. Hancock's case. The operation has been repeated many times, with good results generally.

Mr. Greenhow has operated, at the Newcastle Infirmary, three times since his first case—twice successfully. Mr. Potter, also at the Newcastle Infirmary, has operated twice. Mr. Page, at the Cumberland Infirmary, operated in October, 1848, successfully. Mr. Gay, at the Royal Free Hospital, in March, 1851, successfully. Mr. Simon, at St. Thomas's Hospital, in April, 1851, successfully. Mr. Lowe, of Congleton, in December, 1851, successfully. Mr. Field, at the Royal Sea-bathing Infirmary, Margate, in July, 1852, successfully. Besides these, several other cases have occurred, but have not been published.

Before entering into a discussion of the merits of this operation, it becomes necessary to cite the cases, at more or less length, in which it has been performed; and afterwards, it will be easy to examine the different modes of operating, and the nature of those cases in which operative procedure seems to be demanded. It may be remarked, that though many of these cases have appeared in the journals,* their practical use is greatly increased by collecting them together, and deducing general inferences from their accurate analysis *en masse.*

Case I.

R. W——, æt. 24, butcher, of scrofulous diathesis, admitted into the Charing Cross Hospital, under the care of Mr. Hancock, May 23rd, 1848. He was suffering from caries of the os calcis of the right foot, with abscess; the bone was rough, but not loose.

June 2nd.—Mr. Hancock removed the bone.†

For four days all did well, but then erysipelas attacked the wound, and, recurring at intervals, at last rendered amputation of the foot necessary.

Case II.

Henry H——, æt. 20, pitman, of scrofulous diathesis, admitted into the Newcastle Infirmary, under the care of Mr. Greenhow, June 15th, 1848. Eight weeks previously he had received a wound from a nail running into the left heel. Abscesses had formed and been opened, one of which continued to discharge. The integuments were much swollen and indurated, and fluctuation was felt below the outer ankle. The joint admitted of easy motion without pain. Opiates &c. ordered. August 15th the operation was performed. The os calcis was found to be extensively carious; the disease even extended across the articulating surface to the astragalus, a portion of which bone was also removed. Erysipelas attacked the limb repeatedly, and great care was required to get him through.

December 1st, the wounds were all healed, and he could bear some weight on the heel. A piece of cork was fitted to fill up the space in the shoe, and on the 29th he left the hospital quite well, and able to walk with only a slight halt.

Case III.

Thomas B——, æt. 29, pitman, of scrofulous diathesis, admitted into the Newcastle Infirmary, under the care of Mr. Greenhow, August 10, 1848, with disease of the left foot, principally affecting the os calcis, which could be felt through two fistulous ulcers. The disease commenced, two years and a half ago, with inflammation and abscess. He was a patient in the hospital some months ago, and underwent an operation for the partial removal of the bone, which proved of little use.

August 15th, a partial operation was performed.

In about a fortnight the wound was nearly healed; but erysipelas attacking it, excision of the whole os calcis was deemed necessary, and performed. The bone was carious in every part, except at one or two points of its articulating surfaces.

October 17th.—After the operation slight sloughing of the integuments took place, but in a few weeks the wound healed over.

February 10th, 1849, he left the hospital, the heel being quite sound. A piece of cork filled up the vacancy in the shoe, and he walked freely about on crutches.

Case IV.

John R——, æt. 16, a country-lad, of delicate and scrofulous appearance, admitted into the Newcastle Infirmary, under the care of Mr. Greenhow, November 30th, 1848, with disease of the foot, principally affecting the os calcis, which was

* Three of them are now published for the first time.

† For a description of this and the other operations, see the *Synopsis.*

greatly enlarged and carious. One or two sinuses at the apex of the heel were found to penetrate deep into the bone; ankle-joint moveable without much pain. The disease began eighteen weeks ago, from excoriation of the heel, occasioned by the friction of his shoe.

December 5th, excision of the os calcis was performed, and diseased portions of the cuboid bone were also sawn off. The calcaneum was one mass of diseased bone; its shape was hardly to be recognised, so far had caries destroyed it.

June 15th, 1849, he left the hospital, the wound being nearly healed; yet he had had many severe attacks of erysipelas before he was brought into a satisfactory state.

Subsequently he walked freely about on his foot; and when seen, about two years ago, was using it as perfectly as could be desired. He is lately dead of phthisis.

Case V.

Alexander L——, æt. 29, glassmaker, of scrofulous diathesis, admitted into the Newcastle Infirmary, under the care of Mr. Greenhow, May 13, 1852, with disease of the right anke, of seven months' duration; it followed an attack of erysipelas. An ulcer on the outer side leads to diseased bone, whether confined to the os calcis or not seems doubtful. Joint generally swollen, and but partially moveable, yet pretty free from pain. He has lost much flesh of late. Pulse quick; tongue furred; bowels regular; slight perspirations; no cough. Sleeps ill in consequence of pain in ankle at night.

May 18th.—The os calcis was this morning removed; it was found to be in an advanced stage of caries. The other bones were healthy. There was considerable hæmorrhage from the posterior tibial artery, which was divided during the operation.

21st.—Foot dressed this morning. The wound looks well. A pledget of lint, with a compress and bandage, were applied. Ordered a chop and tea daily.

22nd.—Foot dressed again this morning; wound healthy; suppuration has commenced.

27th.—Wound has been dressed daily; much discharge of unhealthy, flaky pus; yet the wound looks healthy. To have a pint of beer daily.

July 27th.—The wound now is almost healed, and the foot is in a very satisfactory state; it is easily moveable at the ankle, and though he cannot yet lean much weight on it, yet he has got about on crutches, and finds it gain strength daily. His general health is greatly improved, though he has been unable to conquer a strong dislike to cod-liver oil, the use of which would probably have been beneficial to him. His appetite is now excellent; he requires his anodyne occasionally at night, because of twinges of pain which occur now and then in the foot. He was now sent home.

Sept. 20th.—From exposure to cold, and over-exertion in attempting to walk, the wound has broken out again; the other bones are probably diseased.

Nov. 2nd.—Amputation was performed to-day. The astragalus, cuboid, scaphoid, and cuneiform bones, were all carious—the soft parts being in a state of unhealthy suppuration.

Case VI.

William G——, æt. 16, an unhealthy, ill-nourished, scrofulous boy, admitted into the Cumberland Infirmary, under the care of Mr. Page, July 29th, 1848, with disease of the right tarsus, the result of a slight injury he had received several years before. Suppuration and ulceration commenced only six months ago, since which time he has been unable to put the foot to the ground. The disease appeared to be confined to the os calcis, into which a probe could be passed at two distinct points.

Oct. 9th, the operation was performed. The astragalus and cuboid were quite healthy. Inflammation of the tarsal joints retarded the cure; but on January 7th, 1849, he left the hospital. He was enjoined not to use the foot, and was very careful with it for some months; but at the end of a year, he found it, "for all the uses of a foot, as serviceable to him as the other."

Case VII.

Thomas C——, æt. 15, tailor, admitted into the Newcastle Infirmary, under the care of Mr. Potter, May 31st, 1849. Had been in bad health for two years, and had, he supposed, injured his heel by a tight shoe. The right foot at the ankle was much swollen and indurated. There were several sinuses leading down to diseased bone. Poultices were applied, iodide of iron given internally, and good diet ordered. No improvement took place, though every attention was paid to the constitutional as well as local disease. It was therefore desirable to remove the diseased bone, and endeavour to save the foot.

July 3rd, Mr. Potter removed the bone; the posterior tibial artery was not wounded, and no ligatures were required. It was found necessary to use the scoop to the astragalus, as a portion of it had become soft and diseased. The wound healed by the first intention, leaving a minute orifice by which discharge escaped. No bad symptoms followed, and the patient left the infirmary in about two months, cured.

He was seen in October, 1850, and had then walked six miles, without inconvenience.

Case VIII.

Hugh C——, æt. 15, of scrofulous constitution, admitted into the Newcastle Infirmary, under the care of Mr. Potter, in March, 1851. About five years ago he sprained his foot by slipping off a stone, since which time he has had occasionally a good deal of pain. Three years ago, abscesses, followed by sinuses, formed around the ankle. He is very anxious to have an operation performed on the foot.

March 11th, Mr. Potter performed the operation. The astragalus was affected as well as the os calcis, and a portion of it was therefore removed. The wound healed favourably, with the exception of a sinus, which remained open, and continued to discharge unhealthy pus.

The boy was seen some months after the operation, but the foot was not at that time fit to walk upon.

Case IX.

———, æt. 22, house-painter, of a pallid aspect, light complexion, and presenting a few of the characteristics of the scrofulous diathesis, admitted into the Royal Free Hospital, under the care of Mr. Gay, in March, 1851. For more than ten years pain in the heel and foot had existed, but he had been enabled to attend to the duties of his calling up to this time, appropriate means having been used to keep the disease at bay. Now, however, it was found that the os calcis was extensively diseased, the probe, passed through fistulous openings, coming in contact with dead or carious bone.

March 20th, the operation was performed. "On being examined, the articular surfaces were found quite eroded, and the body of the bone was in various places deeply carious." The tibio-tarsal and astragalo-cuboid articulations were healthy.

The case went on favourably, so that at the end of six months he began to bear the whole weight of the body on the foot, and was able to throw away his stick. Subsequently, it was found that his facility of progression was very little impaired.

Case X.

William C——, æt. 10, with impairment of general health, admitted into St. Thomas's Hospital, under the care of Mr. Simon, April 18th, 1851, with severe disease of the left foot. Four months back, without any known cause, the foot became swollen, hot, red, and very painful, and openings soon formed, from which there was a profuse discharge of pus. The posterior half of the foot was now

[*Continued on p. 240*

Name of Operator.	Name of Patient.	Predisposing causes of Disease.				Exciting causes of Disease.	Duration of Disease.
		Sex of Patient.	Age of Patient	Constitution of Patient.	Occupation of Patient.		
Mr. Hancock.	R. W. . .	Male .	24	Scrofulous .	Butcher .	. . . . .	. . . .
Mr. Greenhow	Henry H. .	Male .	20	Scrofulous .	Pitman .	. . Nail running into foot.	8 weeks .
Mr. Greenhow	Thomas B. .	Male .	29	Scrofulous .	Pitman .	. . Inflammation and abscess.	$2\frac{1}{2}$ years .
Mr. Greenhow	John R. .	Male .	16	Scrofulous .	. . None; had lived in country.	. . Friction of shoe.	$4\frac{1}{2}$ years .
Mr. Greenhow	Alex. L. .	Male .	29	Scrofulous .	. . Glass-maker.	. . Erysipelatous inflammation.	7 months
Mr. Page . .	Wm. G. .	Male .	16	Scrofulous .	. . .	. . Slight injury.	. . Several years.
Mr. Potter .	Thos. C. .	Male .	15	Scrofulous .	Sailor .	. . Friction of tight shoe.	2 years .
Mr. Potter .	Hugh C. .	Male .	15	Scrofulous .	. . . .	. . Sprain from slipping his foot.	5 years .
Mr. Gay . .	. . . .	Male .	22	Scrofulous .	. . House-painter.	. . . . .	10 years .
Mr. Simon .	Wm. C. . .	Male .	10	Scrofulous .	. . . .	. . Inflammation from unknown cause.	4 months
Mr. Lowe . .	M. A. I. .	Female	16	Scrofulous .	Mill-girl .	. . Inflammation.	3 years .
Mr. Field . .	Edw. E. .	Male .	17	Scrofulous .	. . . .	. . . . .	. . . .

OF EXCISION OF OS CALCIS.

Date of Operation.	Description of Operation.	Result of Operation.	Pathology of the Disease.
June 2nd, 1848 .	. . A single flap was formed in the sole, with the convexity looking forwards, by an incision from one malleolus to the other.	Unsuccessful	. . Caries, with abscess.
Aug. 15th, 1848 .	. . In the 1st, 2nd, and 3rd cases—1st, incisions were made from the inner and outer ankles, meeting at the apex of the heel; and then, 2nd, others extending along the sides of the foot, the flaps being dissected back so as to expose the bone and its connexions. A wedge of integument was removed in the first case, but it was found better to avoid this in the other cases. In the 4th case, an incision was commenced at the apex of the heel, and carried slightly upwards towards the inner malleolus, and then again downwards and forwards; and the same being repeated on the external side, two curved flaps were formed, which were dissected upwards and downwards.	Successful .	. . Caries, extending to astragalus.
Aug. 15th, 1848 .		Successful .	Caries.
Dec. 5th, 1848 . .		Successful .	. . Caries, extending to the cuboid bones.
May 18th, 1852 .		Unsuccessful	. . Extensive caries.
Oct. 9th, 1848 . .	. . 1st, an incision from half an inch below the inner ankle, directly across the sole of the foot, to just below the fibula; 2nd, two incisions on either side of the foot, commencing at the junction of the os calcis with the os cuboides, and ending at the extremities of the first or transverse incision; flaps were then dissected back.	Successful .	Caries.
July 3rd, 1849 . .	. . Two oblique incisions in the sole of the foot—one a little towards the outer, the other towards the inner, side of the heel, the two meeting at one point in the centre of the sole; the flaps were then dissected back.	Successful .	. . Caries, involving the astragalus.
March 11th, 1851 .		Successful .	. . Caries, extending to astragalus.
March 20th, 1851 .	. . Two flaps, one on the posterior part of the heel, the other on the plantar aspect of the foot.	Successful .	Caries.
April 28th, 1851 .	. . A longitudinal incision in the axis of the bone, beginning just above the heel, and extending to the centre of the sole; and from the end of this incision a second one, extending at right angles directly outwards, passing round the outer margin of the foot to its dorsum—the two cuts together forming the outline of a rectangular flap.	Successful .	. . Necrosis (new bone having been thrown out.)
Dec. 12th, 1851 .	. . 1st, an incision on the outer side of the foot, from a point opposite the external malleolus, and a quarter of an inch below, downwards and backwards to the heel; 2nd, a semilunar incision on the inside—the latter being more superficial than the former, which was made at once down to the bone; 3rd, an incision from the centre of the first, along the sole of the foot, to a point opposite the astragalo-cuboid articulation; 4th, another on the inside, similar to but shorter than that on the outer; next, a flap was dissected from the sole, and the integuments turned off from the heel for another, thus laying bare the entire calcaneum.	Successful .	Caries.
July 26th, 1852 .	. . 1st, an incision from an inch behind the inner malleolus downwards and forwards, across the sole, and backwards and upwards to an inch behind the outer malleolus; 2nd, an incision of about two inches in extent made to pass from the convexity of this directly forwards on quite the outer part of the sole, over the calcaneo-cuboid articulation; two flaps were then dissected back.	Successful .	Caries.

swollen to at least double its proper bulk, and was of a bright red colour. There were several orifices at different parts of the swollen integuments, from all of which sinuses ran towards the os calcis; and this bone could be felt with the probe, exposed to a considerable extent. He had exquisite pain when the foot was moved, or even touched. After he had been in hospital some little time, it was found that on the inner side of the foot the skin was healthy, and here pressure could be made without causing pain.

April 28th, the operation was performed. It was found that the whole of the calcaneum was necrosed, "with the exception of that part which enters into articulation with the cuboid, and which was sound to a small depth from the joint;" and that some little new bone had been thrown out. "The articulating surfaces of the cuboid and the astragalus were perfectly healthy."

May 30th, he was discharged cured, having been about the ward on his crutches for some time previously, and being able to press his foot firmly to the ground without pain.

The absence of the heel-bone was not so apparent as might have been expected.

Case XI.

M. A. J——, æt. 16, a milk-girl, a red-haired strumous girl, applied to Mr. Lowe, of Congleton, in October, 1851, with disease of the left foot. The disease was of three years' duration, and commenced by inflammation and swelling, which recurred, in attacks at intervals, up to the present time. Now the foot generally was much swollen, the swelling being, however, greatest on the outer side of the ankle and heel; here, also, were two discharging sinuses. The integuments had a purplish, unhealthy hue, and were tense and shining. Pressure did not give much pain, but there was a deep-seated and constant aching. The discharge from the fistulous openings was profuse, thin, and unhealthy. A probe, introduced into either, passed into the interior of the os calcis, but not either towards the astragalus, nor yet forwards to the other tarsal bones. The motion of the tibio-astragaloid articulation was tolerably good, but the girl could not bear much weight on the heel.

December 12th, the operation was performed. The operator observes, "Nothing short of excision could have effected a cure, as the bone, now lying by me, proves, the whole of it, with the exception of a small portion on its outer side, being in a carious condition."

The case did well, and on February 3rd, 1842, the wound was completely healed.

The final report says—"The foot is now soundly healed, and capable of bearing the weight of the body with ease and comfort; she can walk moderate distances without any difficulty, and with but slight alteration in her natural gait. The contour of the foot is but little altered."

Case XII.

Ed. E——, æt. 17, admitted into the Royal Sea-bathing Infirmary, Margate, under the care of Mr. Field. There was an ulcer, about an inch behind each malleolus, as large as the tip of the finger; through these a probe could be passed directly on, to so large an extent of diseased bone as to leave little doubt that the whole was affected.

July 26th, 1852, the operation was performed. The disease was carious.

"On the fourth day the dressings were removed, and almost complete union was found to have taken place between the cut surfaces."

The 12 cases here given do not, perhaps, include every instance of excision of the os calcis; yet they are all that I have been able to collect, and are sufficient to form a basis of inquiry into the merits of the operation.

The frequent occurrence of disease of the bones composing the ankle-joint—for, says Mr. Syme, "next to the knee-joint, the ankle is the most common seat of white swelling"—and the importance of preserving so useful a member as the foot, render this operation a valuable addition to surgery.

All the patients were males, excepting one. This is no peculiarity; for diseases of bones are much more common, for obvious reasons, in males than in females. All the patients were under 30 years, and the majority under 20 years of age; so that caries of the os calcis is a disease, as far as these observations show, of youth. All the patients, without exception, were of a scrofulous diathesis; and, it may be added, for the most part had lived in ill-drained situations, and on unnutritious and scanty food. Their occupations in some cases were such as to favour the development of scrofulous disease; while others were probably of a healthy nature. It may be remarked, however, that in those cases where the occupation was healthy, some accidental circumstance produced the disease; thus, in the pitman, in the sailor, and in the country-lad, the entrance of a nail into the heel, and the friction of a shoe, were thought to have originated the mischief in the calcaneum. And, on the other hand, where the employment was, in all probability, unhealthy, as in the cases of the milk-girl, the painter, and the glass-maker, no such accidental cause was needed to produce the disease; inflammation, either simple or erysipelatous, was sufficient to call it forth. Possibly, however, the inflammation in these cases was rather the external manifestation of the internal disease than the cause of that disease.

In whatever way the causation of the disease be looked at, it cannot be doubted, that with it the scrofulous constitution of the patient was intimately concerned; and it is probable that the so-called *causes* were only such in so far as they called forth, at a particular point, the local manifestation of a general or constitutional disorder.

As regards the duration of the disease: it varied from eight weeks to ten years. The latter instance shows how long it may be kept at bay, yet also proves its incurability without operative interference.

The morbid anatomy of those diseases of the calcaneum which call for its removal, appears to be a simple matter, for, on looking at the list of cases, it will be seen that caries is the almost universal disease.

The question arises, Where does this caries originate? Does it commence in the bone, in the cartilage of articulation, or in the synovial membrane? It must be remembered that the heel-bone is much exposed to the influences of cold, damp, and accident, and that its synovial membranes may thus be inflamed, and the inflammation spread to the cartilages and the bone; yet it is not often so; more frequently the cartilage itself is the original seat of the ulceration, and particularly is this the case between the astragalus and os calcis; and oftener still is the disease situated, *ab origine*, in the bone. Then it is that excision is peculiarly applicable, because, as Mr. Syme says, "the extent of this cavity (i.e. a carious cavity) seldom, or rather never, exceeds the bounds of the epiphyses, except sometimes in young subjects, where the bone has been widely altered by scrofulous action, previous to suffering the inflammation, which more immediately occasions the caries."

The excision, then, should be performed early, before the articulating surfaces are involved; for if the disease extend so far as the cartilages, it presently erodes them, and, rapidly spreading, in a short time places all interference short of amputation out of the question. Yet many of the cases prove that portions of the astragalus and of the cuboid bone may be removed, and still the case do well; on the whole, however, it may be laid down, that the disease, when originating in the bone itself, is, for a considerable time at least, limited by the articulating cartilages; and hence, probably, the success of the operation.

The calcanea which Mr. Greenhow removed were all so extensively carious as to resemble a very porous sponge, while the neighbouring bones were either unaffected, or so slightly so as to require only a little gouging or scraping to secure success to the operation.

On looking at the list of cases, and their results, the success which has attended this operation is rather striking; more especially as it is one which an *à priori* reasoning would show to be of a dangerous nature in itself, and not likely to answer the end for which it is undertaken. But though the dangers are really considerable, and the difficulties to be contended with not a few, yet, as the cases prove, they had been exaggerated; and experience now makes manifest what theory never could have shown—viz., that excision of the os calcis is a practicable and successful operation.

And now to compare the advantages and disadvantages of this operation with those of amputation.

The great advantage of amputation is, that the diseased limb is quickly and effectually removed. This, no doubt, is an important matter, if the constitution be already much shattered, the age of the patient advanced, and his powers of life feeble; but in the cases in which the excision has been required, the patients have been tolerably vigorous, in the prime of youth, and, after judicious constitutional treatment, sufficiently strong to bear the operation.

Amputation, however, has its disadvantages. There is the important consideration of the loss of the foot, of which it is needless to say anything. As regards the actual effects of excision on the system, they are probably less severe than those of amputation; for there is less loss of blood than in that operation, and the shock to the nervous system is slighter. Nor is there such danger of destructive inflammation (which was feared by some surgeons sufficiently to deter them from the operation), as in a case (for instance) of wound of a joint; for, it must be remembered, the parts around the ankle where excision is demanded, have long been exposed to the contact of the air, and are not so very liable to inflammation. Lastly, as in Mr. Hancock's, and Mr. Greenhow's last case, amputation may be had recourse to in the end, if the removal of the bone prove not to include the extirpation of the disease. Thus one of the supposed disadvantages of excision turns out to be no disadvantage.

It has also been supposed that the limb would never be fit for anything; that the incisions would not heal; that the joint would be stiff; and that the tendo-Achillis, being detached, would not fix itself to the neighbouring parts. To this it may be answered, Experience shows that the incisions do heal, in some cases even by the first intention; that the joint is generally very moveable; that the tendo-Achillis is found to attach itself firmly to the soft parts, and to adapt itself to the new state of things in the ankle;* that the patient is freed from pain; that his foot is at once put into a comfortable state; and that at last he has a useful limb preserved to him—a limb which is almost as serviceable as its fellow.

With these important advantages on its side, the operation can hardly fail to overcome the other minor objections that may be urged against it—that it is not easy to diagnose the exact amount of disease; that the operation itself is difficult to perform, &c. No surgeon of any enterprise would be deterred by such considerations; and especially as to the first it may be answered, that it is tolerably easy, with care, to know how far the caries has gone, and how much the neighbouring bones are implicated; and to the second, that the operation is, on the whole, not very arduous, the principal difficulty consisting in disentangling the os calcis from its connexions.

The fact that the posterior tibial artery was wounded in several of the cases, and that the circumstance did not appear in the least to retard the progress of the cure, proves that the fear of it need be no obstacle to the performance of the operation. The cases for excision of the calcaneum have hitherto been well chosen, and, as is proved by their success, many unnecessary amputations have been avoided; but it may be said, that a more partial operation than extirpation of the whole bone would have answered the purpose. This I believe to be a mistake; in several of the cases the disease involved the neighbouring bones—a circumstance which would probably have been overlooked had a less extensive operation been attempted; in one or two a more partial excision had been tried, and had failed; while in all, the os calcis itself was very completely diseased, being in some instances reduced to a cribriform, shapeless mass.

It is desirable, as was said before, to remove the bone before its articulations are much affected;—their condition may be judged of by the degree of mobility of the ankle, and by actual examination with the probe.

While claiming for this operation a considerable share of merit, it is far from my intention to deny that more partial excisions and gougings of the calcaneum are of great service; indeed, it would be impossible to do so, whilst such operations are being practised almost daily by the most eminent surgeons; but I believe, and the cases adduced are in favour of the belief, that there are instances in which partial excisions are inadmissible, yet in which amputation can be successfully avoided, by the adoption of this operation of excision of the whole os calcis.

Nor is the deformity very great after this operation; in nearly every case the patient was able to walk with only a very slight halt; while the shape of the foot was not materially disfigured.

In regard to the manner of performing the operation, it will be observed, that every operator had his own method. The great point of difference is, that some preserved the sole of the foot entire, while others did not hesitate to form a flap or flaps from it.

* In Mr. Greenhow's fourth case, where it became necessary to amputate the foot, the tendo-Achillis was firmly connected with the muscles and integuments of the heel.

In Mr. Hancock's case, in which the sole was wounded, sloughing of the flap occurred; but whether or not it attacked that flap the more readily because it was formed from the sole, appears doubtful. It seems natural to suppose that a cicatrix in the sole would be irritable and liable to inflame, if much used in walking, and therefore it may be desirable to avoid wounding that part; but the direction of the incisions must be often, in a great degree, determined by the position of pre-existing sinuses and ulcers.

Art. IV.

Decennium Pathologicum; or, Contributions to the History of Chronic Disease, from the St. George's Hospital Records of Fatal Cases during Ten Years. By Thomas K. Chambers, Physician to St. Mary's Hospital, London.

Part III. — Malignant Disease.

At St. George's Hospital, from January 1, 1841, to December 31, 1850, in the 2539 fatal cases there were 199, or 7·8 per cent., which appeared to the curators to be instances of "malignant tumour." By this term I believe I and they, and the majority of the profession, mean "*a morbid cell-growth, dependent on a specific state of system, and therefore liable to affect all parts of the body, without direct communication with the place where it is primarily manifested.*" It does not seem wise to rest much confidence in any one of the various touchstones which have been proposed as means of distinguishing these formations from others said to closely resemble them. For the statistician, at any rate, the best test is the judgment of a well-educated person who makes the autopsy, checked and corrected, as it is in a public institution, by the discussion which ensues among the spectators before the final report is made. I shall therefore take it for granted that the numbers are unaffected by any important fallacy of observation.

Of these 199 there were 20 examined so far only as to identify the disease in the particular organ chiefly affected, and not the sympathy of the other viscera; that is to say, that 5·2 per cent. of the 378 cases not examined, from the interference of their friends or other causes, were obviously cases of malignant disease. The parts implicated, capable thus of being described without full dissection, were of course usually external, or partially external. Of the 20 there were 4 males, in whom the localities attacked were, twice the Femur, once the Tongue, and once the Penis; 16 females, in whom the localities attacked were, 6 times the Genital Organs, 4 times the Mamma, twice the Neck and Head, once the Stomach, once the Upper Jaw, and once the Rectum. The numbers are here enumerated, in order that they may, for any special purposes, be added to those elicited from the cases more fully examined.

In the 2161 cases examined there were 179, or 8·2 per cent., of malignant disease.

The following tables afford a conspectus of the ages and sexes of these cases, the first containing the actual numbers, the second the per-centage in the totals of all diseases together.

Number of Cases of Malignant Disease in cases examined.	Males	Females	Both sexes.	in cases not examined.	Males	Females	in total cases examined and not examined.
In 94 males, and in 60 females, from birth to 15, inclusive	—	—	—	In 43 males, and in 29 females, from birth to 15, inclusive	—	—	—
In 377 males, and in 259 females, from 15 to 30 inclusive	9	14	23	In 58 males, and in 30 females, from 15 to 30 inclusive	—	2	25
In 472 males, and in 179 females, from 30 to 45 inclusive	24	36	60	In 59 males, and in 30 females, from 30 to 45 inclusive	2	3	65
In 299 males, and in 139 females, from 45 to 60 inclusive	30	37	67	In 46 males, and in 31 females, from 45 to 60 inclusive	1	8	76
In 109 males, and in 58 females, above 60	12	5	17	In 17 males, and in 12 females, above 60	1	2	20
In 74 males, and in 37 females, of unknown age	4	8	12	In 10 males, and in 7 females, of unknown age	—	1	13
In total of 1425 males, 732 females, and 4 of unknown sex, of all ages	79	100	179	In total of 233 males, 139 females, and 6 of unknown sex, of all ages	4	16	199
Column	A.	B.	C.		D.	E.	F.

Per-centage in cases examined.	Males	Females	Both sexes.	Per-centage in total deaths.	Males	Females	Both sexes.
In 94 males, and in 60 females, from birth to 15, inclusive	—	—	—	In 137 males, and in 89 females, from birth to 15, inclusive	—	—	—
In 377 males, and in 259 females, from 15 to 30 inclusive	2·4	5·4	3 6	In 435 males, and in 209 females, from 15 to 30 inclusive	2·0	5·5	3·4
In 472 males, and in 179 females, from 30 to 45 inclusive	5·0	20·1	9·2	In 531 males, and in 289 females, from 30 to 45 inclusive	4·9	18·6	8·7
In 299 males, and in 139 females, from 45 to 60 inclusive	10·0	26·6	15·3	In 345 males, and in 170 females, from 45 to 60 inclusive	8·9	26·4	14·7
In 109 males, and in 58 females, above 60	11·0	8·6	10·1	In 126 males, and in 70 females, above 60	10·3	10·0	10·2
In 74 males, and in 37 females, of unknown age	5·4	21·6	10·8	In 84 males, and in 44 females, of unknown age	4·7	20·4	10·1
In total of 1425 males, 732 females, and 4 of unknown sex, of all ages	5·5	13·6	8·2	In total of 1658 males, 871 females, and 10 of unknown sex, of all ages	5·0	13·3	7·8
Column	G.	H.	I.		J.	K.	L.

Deduction from columns I and L.

Though the actual numbers do not show it very clearly, yet it is evident from the per-centages how much rarer malignant disease is in the earlier and prime periods of life than in the years of bodily decline. It does not appear, however, by these tables, to be common in a direct ratio to the age, for the proportionate numbers are greater between 45 and 60 than after that time. Whether this is a fallacy dependent on the restricted field of observation, or whether it represents the real truth, there are no data to judge by, but such is at any rate the inference from the facts before us. The entire absence of instances under 15 is doubtless to be classed under the first category: for few, probably, have gone through the same number of years of private practice, without a case of malignant disease in childhood, affecting most commonly (if it is fair in a statistical work to speak so roughly) either the kidneys or some other abdominal organ.

Comparison of Male and Female columns.

The great preponderance of females, not only in the per-centages, but even in the actual numbers, is very striking, and is distinctly shown in the totals. It is not, however, the same at all ages, being most decided between 30 and 45, and above 60 not existing at all.

This preponderance is partly due to the tendency exhibited by the female generative organs to become affected with the disease, and to the fatal nature of the maladies thereon consequent, even when it does not extend beyond the parts immediately adjoining.

To form an idea of this, let us subtract from our totals those cases where the uterus and ovaria were the causes of death, without other parts being malignantly affected, and on the other hand those males where the penis and testicles alone were implicated. We thus get in the cases examined 80 females (or 10·9 per cent.) to 77 males (or 5·4 per cent.)

To look at the matter in another way—let us deduct the instances where the differential organs (including the breasts) were *at all* affected, and compare the *general system* of the two sexes. This reduces the females to 51, and the males to 73; the former to 6·9, and the latter to 5·1 per cent., in the cases examined.

The inferences from these facts are:

1st. That females are to a certain extent more disposed than males to malignant disease originating even in parts of the body common to both sexes, being affected in the proportion of nearly 7 to 5.

2nd. That the female organs provided for the continuation of the species are in that sex more prone to the disease than any other locality, and in one-fifth of the fatal female cases are the cause of death without other organs being affected at all.

3rd. The whole risk of women being affected with malignant disease is to that of men as 13 to 5.

Development of Malignant Disease in various localities.

The frequency with which malignant disease is developed in different parts of the body, either alone or in complication with other parts, may be seen by the following list of the numbers of times each organ was affected in the 199 cases:

	Alone.	With other parts of body.	Not examined.	Total.
DIGESTIVE CANAL:				
(*One part only affected—viz.*)				
Pharynx and œsophagus	7	6	—	13
Stomach*	12	17	1	30
Duodenum	—	1	—	1
Ileum	—	2	—	2
Cæcum	—	1	—	1
Colon	—	1	—	1
Rectum	7	3	1	11
(*Several parts affected—viz.,*)				
Stomach* and cæcum	1	—	—	1
Stomach and colon	1	—	—	1
Stomach and (?)	1	—	—	1
Stomach, colon, and rectum	—	1	—	1
Ileum and colon	—	2	—	2
Colon and rectum	—	1	—	1
Total of digestive canal	29	35	2	66
PERITONEUM (including subperitoneal cellular tissue) without affection of the contained viscera	5	2	—	7
PANCREAS	1	4	—	5
LIVER AND GALL-BLADDER	2	45	—	47
KIDNEYS AND SUPRA-RENAL CAPSULES	—	10	—	10
BLADDER	4	11	—	15
FEMALE DIFFERENTIAL ORGANS—viz.,				
Breasts without genitals	1	4	2 (?)	7
Vagina without the rest of genitals	—	1	—	1
Uterus " "	19	7	3	29
Ovaries " "	6	6	—	12
Ovaries and uterus	1	3	—	4
Uterine ligaments	—	1	—	1
Total of female differential organs	27	22	5	54
Breasts and genitals both affected (cases repeated from above)	—	6	—	6

* Of the 34 times in which the stomach was affected, in 17 the pylorus is specially mentioned as principally diseased, in 2 only was the morbid growth confined to the cardiac end.

	Alone.	With other parts of body.	Not examined.	Total.
MALE DIFFERENTIAL ORGANS—viz.,				
Testicles	—	3	—	3
Penis	1	—	—	1
Prostate gland	—	1	—	1
Epididymis	—	1	—	1
Total of male differential organs	1	5	—	6
RESPIRATORY ORGANS—viz.,				
Pulmonary tissue, &c.	—	23	—	23
Bronchial glands without pulmonary tissue	1	2	—	3
Pleura " "	1	2	—	3
Trachea " "	—	2	—	2
Total of respiratory organs	2	29	—	31
BRAIN	8	4	—	12
BONES—viz.,				
Antrum, without other bones	3	—	—	3
Feet, bones of "	—	1	—	1
Femur "	3	—	1	4
Humerus "	—	1	—	1
Ribs "	—	1	—	1
Scapula "	—	1	—	1
Sternum "	—	1	—	1
Tibia "	1	—	—	1
Vertebræ "	—	2	—	2
Total of bones	7	7	1	15
MUSCLES—viz.,				
Diaphragm	—	2	—	2
Heart	—	3	—	3
Heart and recti	—	1	—	1
Thigh	—	1	1	2
Tongue	2	—	1	3
Total of muscles	2	7	2	11

PART FOURTH.

Chronicle of Medical Science.

ANNALS OF PHYSIOLOGY.

By Henry Gray, F.R.S.,
Demonstrator of Anatomy at St. George's Hospital, and Surgeon to the St. George's and St. James's Dispensary.

In commencing a series of systematic reviews of the progress of physiological inquiry, the writer deems it necessary to state briefly the plan he intends to follow. His aim will be, not to occupy space by noticing every physiological paper, whatever may be its value, but to make such a selection of the materials before him that in a short space every important fact in the progress of physiology may be presented to his readers. It is not intended either to include systematic treatises, which are more properly analyzed in the review department, or to repeat statements which have already appeared in the reviews. The 'Annals of Physiology' will appear every six months, alternately with the 'Annals of Micrology;' and, as far as possible, it is the intention of the author, in future numbers, to test by independent experiment the statements made in the papers under review. He has to apologize for the short notice some important papers have received from him on the present occasion; but the limited space which could be assigned to him must be his excuse.

BLOOD.

Dr. Thomas Williams* shows, in the invertebrate animals, that there exists two distinct nutritious fluids, dissimilar in their anatomical relations, and in their chemical and vital compositions.

Below the Echinodermata the blood proper is wholly supplanted by the chylaqueous fluid; above the Annelida, this latter fluid, in the adult animal, is superseded by the true blood; whilst in the Echinodermata and Annelida these two systems of nutrient fluid exist, bearing always to each other in the same individual the same quantitative proportion.

The author also shows, that in the *containing system of the blood proper*, excepting the Echinodermata, there is an entire absence of an internal lining of vibratile cilia, the contents being propelled by the contraction of the muscular parietes; whilst, on the contrary, *the containing system of the chylaqueous fluid* is circulated by ciliary vibration.

The main differences between the chylaqueous fluid and the true blood appear to be these:

1st. The former is contained in channels, the walls of which are lined with vibratile cilia, by means of which the fluid is circulated.

2nd. In the Zoophytes and Medusæ this fluid is distinguished from the blood proper by the fact of its having mixed with it sea water, in large quantities, more or less directly, and with which it vitally assimilates.

* On the Blood Proper and Chylaqueous Fluid of Invertebrate Animals. By Thomas Williams, M.D. (Philosophical Transactions, part ii. 1853.)

3rd. In all classes in which this fluid exists, the corpuscles vary in size with the variations in the size of the body of the animal. In this respect they are diametrically opposite to the corpuscles of the true blood in the vertebrate animals, the corpuscles of which bear no proportion to the size of the animal from which they are taken.

The author commences his researches on this fluid in the lowest classes in the animal scale.

In the *Sponge* the chylaqueous fluid is contained partly in the interior, and in part between the cells of the gelatinous cortex. It is composed of a mixture of *salt water and albumen.*

In all *Polypes* it is principally composed of sea water, which, in passing through the stomach, is mixed with the secretions of this organ, and then enters the visceral cavity. In the Tubularia indivisa it is charged with minute granules, consisting of solid spherules from the solidification of albumen.

In the *Molluscoid Polypes* this fluid presents a higher organic composition than in the former class. It is true blood in its composition and function; its cells are corpusculated, many bear globules of oleine, although comprising several individual forms of cell, which are constant in their microscopic characters in the same species.

In the *Medusæ* the chylaqueous fluid is contained in a system of canals, which are in direct communication with the stomach, and which are lined with a vibratile epithelium. The contained fluid is composed of corpuscles which never contain a distinct nucleus, suspended in a fluid consisting chiefly of salt water, presenting in the Rhizostomidæ a yellowish hue, which has its seat in the fluid: in the Vilella it is bluish. The floating cells, which contain secondary oleaginous cells, and a molecular base, exhibit great irritability, the minute molecules being mutually repulsive.

In the *Echinodermata,* instead of the three distinct systems of fluids which physiologists usually describe—1st, that of the general cavity of the body; 2nd, that of the feet and water canals; and that, 3rdly, of the blood proper—the author is led to believe that they constitute a system of separate cavities containing a similar fluid, from the fluid contents of the blood-vascular system being chemically and morphologically identical with those of the water-vascular system, and with those again of the visceral cavity; the blood-vascular system not being an independent and closed system of conduits, but is imperfect in its peripheral portions, whilst the internal lining of its channels is ciliated: a peculiarity which separates the blood-system of this class from that of every other in which it is known to exist.

The Blood proper, in the Echinidæ and Asteriadæ is colourless, containing irregular organized particles, which cannot be distinguished from those found in the peritoneal fluid or in the water-vascular system of the same individual. In the Sipunculidæ the corpuscles in this fluid present a pink tinge; each corpuscle is flat, irregularly oblong, and contains a small bright refractive nucleus: in some cells a second may be seen. The colour is dissolved in the fluid between the nucleus and involucrum. The blood proper is contained in the Uraster in a circular vessel, with radial trunks, the walls of which are everywhere lined with ciliated epithelium. In the Sipunculidan genera there is only one vessel, extending back from the œsophageal ring, no capillary system being traceable at its end. In these animals the blood proper has acquired scarcely any distinctive and independent characters, the system of channels in which it moves being so rudimentarily organized as to receive its contents directly from the fluid occupying the visceral cavity; the blood-vascular apparatus, developed only in its central segments, is designed to concentrate the nutritive force of the chylaqueous fluid upon the more important viscera: the nutrition of the peripheral structures—such as the muscular, calcareous, and integumentary—being sustained under the agency of the chylaqueous fluid.

The Chylaqueous fluid in the Crinoidea, Asteriadæ, and Echinidæ, is far more voluminous than the blood proper. In the Sipunculidæ it has assumed a greater relative development. In the Holothuridan genera it exhibits the most advanced condition under which it is known to exist in the echinodermal series. In all echinoderms it is contained in the peritoneal cavity and its hollow prolongations, the boundaries of which are universally ciliated. These prolongations constitute the true respiratory organs, exposing the chylaqueous fluid to the external air. This fluid, which is of a pure water-like appearance, is charged with cells which are less organized and fewer in number than those of the chylaqueous fluid of the superior genera. In the Echinus it contains flattened corpuscles, the largest of which are provided with an involucrum bearing particles of a limpid oleine, the involucrum projecting out like a cilium, and when these are numerous it is easy to mistake such an appearance for the characteristic of a sperm-cell. It is really due *to the fibrinous contents coagulating in lines* on escaping. The fluid tested by nitric acid contains albumen. In the Sipunculidæ the fluid is opalescent, the great bulk being salt water holding in suspension numerous flattened irregularly oval cells, of a pink hue, like those of the blood proper. In the Asterias rubens, the corpuscles in the water-vascular system, and the contents of the digestive cæca, are identical with those of the peritoneal fluid. For these reasons the author concludes, that the bulk of the fluid contained in the peritoneal cavity of the Asterias and in the Echinus, is derived from that which enters through the mouth into the digestive cæca, in which the first phase of the digestive process is performed; its subsequent changes, by which it is raised to a higher grade of oaganic composition, occurs during its sojourn in the peritoneal space into which it passes by exosmosis from the digestive cæca, penetrates the hollow axis of all the membranous processes of the shell which constitute the true organs of breathing, and where it experiences the change of oxygenation, conveys the results of this change to the blood proper, and replenishes the water-system or ambulacral feet.

In the ***Entozoa***, the blood-proper system is very inferiorily developed, and the blood itself is colourless, and perfectly fluid, holding no cells in suspension.

In the Trematoid and Nematoid Entozoa, the space intervening between the intestine and the integument is filled with the chylaqueous fluid, remarkable for its viscidity, and the molecule-like size of its corpuscles.

In the Cestoid Entozoa, where the alimentary canal is intimately adherent to the integument, the chylaqueous fluid, which is much reduced in volume, is contained in the recesses of the alimentary canal. In those cases where the fluid exists external to the digestive canal, it is limpid and non-corpuscular. In other cases, where the chylaqueous fluid entirely disappears, it is compensated for, as in the earthworm and leech, by a corresponding greater devevelopment of the true blood system.

In the ***Annelida***, the chylaqueous fluid which is contained in the peritoneal cavity is of specific gravity, 1·032 to 1·034, sea-water being 1·028. On standing, a coagulum is formed, which proves the presence of fibrin; albumen also exists, as seen by the addition of nitric acid. The contained corpuscles, which differ in different species, are usually of an orbicular form, bearing a nucleus, and filled with minute granules. It is colourless, excepting in the Clymene, Arenicordia, and Glycera alba, where the colourless fluid contains blood-red corpuscles.

Contrary to the statements of Milne Edwards and Wharton Jones, the author states, that the red blood of the Annelida contains *no corpuscles of any description*, it being a limpid fluid, variously coloured in different species, owing, probably, to the salts contained in it—the red to iron, the green to copper, and in those where the fluid is colourless, the author supposes these salts may exist under colourless combinations.

In the ***Embryonic condition*** of the Myriapoda, Insecta, Arachnida, and Crustacea, the circulating fluid presents all the characters of the chylaqueous system. On the contrary, in their mature state, the blood-proper system is the only one observed, the chylaqueous fluid and the blood proper having in no instance a contemporaneous existence in the same individual. In all the articulated animals, the corpuscles of the true blood conform to one type of structure and figure; the blood proper of insects is colourless, and charged with colourless floating-cells, impelled by a dorsal vessel. There is a nucleus in each cell, surrounded by minute, pellucid, very slight refracting granules, which, on bursting, fibrillate.

The author also differs from Mr. Wharton Jones as regards the structure and form of the blood-corpuscles in the Crustacea, stating that the corpuscles in the blood occur in three discernible varieties: 1st, simple non-granular, non-nucleated, pellucid, spherical globules; 2nd, more or less orbicular bodies, of which the bright nucleus is prominently visible, and a mass of slightly-refractive molecules; 3rd, the fact, characteristic of all blood-cells falling under the denomination of the articulate type, of the apparent suppression of the cell-capsule.

In the *Arachnida*, the corpuscles of the true blood occur under the character of minutely granular bodies, varying between the spindle-shaped and orbicular in figure, differing from those in the Crustacea in the position and invisibility of the nucleus, which is seated in the centre of the body, and therefore indetectable, because surrounded by molecules, which are the counterpart of those formerly described in the blood-cells of the Crustacea.

In the *Mollusca* there is but one system of fluids, which unites in itself the separate characteristics of the blood-proper system and the chylaqueous, and is in every physiological property intermediate between that of the vertebrate animal and the chylaqueous fluid of the Annelid. It is colourless, like dilute milk, and more coagulable than any kind of chylaqueous fluid, containing corpuscles, the amount of which varies in different orders.

Development of the Blood-globules.—Moleschott* has lately been conducting a series of observations on the splenic and cardiac blood of frogs after excision of the liver. The first effect of excision of the liver is a striking diminution in the quantity of the blood, inducing a sort of chlorosis. The colourless corpuscles are much increased in relative quantity—the proportion in the cardiac blood being (average of many observations) 1 white to 2·24 coloured; while in healthy frogs it is 1 to 8. In the blood of the liver, the proportion was 1 to 5·88. The same diminution of the coloured corpuscles, after ablation of the liver, was observed also in the abdominal blood and in that of the "fat body." In the spleen, the quantity of the coloured corpuscles was found reduced by more than half, so that, in consequence of the smaller number (only about one-sixth) of coloured corpuscles naturally present, the quantity of colourless corpuscles under these circumstances exceeds that of the coloured.

From these experiments, it follows that the Liver is an organ in which the conversion of colourless into coloured corpuscles goes on to a great extent.

Frogs deprived of the spleen show a slight increase in the relative proportion of the coloured to the colourless corpuscles. Frogs deprived of both liver and spleen present, in proportion to the colourless, four times fewer coloured corpuscles than in the natural state.

In the *conversion of colourless cells into coloured*, the author states that the nuclei separate into two or three smaller ones, and these into granules; the granules become coloured, and dissolve; and thus coloured cells without nuclei are produced. At the same time, the round form of the colourless is gradually converted into the elliptical one of the coloured. This change of form takes place sometimes before, sometimes after, the cleavage of the nucleus.

Chemical Composition of Blood.—Magendie† has lately instituted a series of experiments on the blood of four horses, differently fed, but no distinct results were arrived at, the quantity of all the principal elements of the blood varying

* Müller's Archiv., 1853, i. 73.

† Ueber den Ein fluss der Nahrungsnittel auf das Blut. (Schmidt's Jahr., i. 1853.)

irregularly. The most constant phenomenon was, that the blood-corpuscles and fibrin were in inverse proportion to each other.

Enderlin,* from a series of researches which he has lately made, has shown that cholic acid, in combination with soda (choleate of soda), is a normal element of the blood, but, under ordinary circumstances, is soon eliminated from it. The alcoholic extract of dried blood, tested with Baryta water and tribasic acetate of lead, shows similar reactions to the extract of dried bile. Its ash contains much carbonate of soda, which is a product of the combustion of choleate of soda, together with tribasic phosphate of soda and sulphates. It also answers to the test with sulphuric acid.

Temperature of the Blood.—The results of former investigators on the difference in temperature between arterial and venous blood, have shown that there is usually a difference of about ·5° C. in favour of arterial blood, comparing generally the jugular and the carotid, or the opposite ventricles of the heart. A series of extensive and very accurate researches made by Liebig,† son of the renowned Munich Professor, have shown, on the contrary, that venous blood is warmer than arterial under all circumstances, the average of the maxima and minima being ·07° to ·19° C. The experiments which were made on living or recently-killed animals were conducted in the following way:—A dog was killed, and a ligature tied tightly round the neck, to prevent the lungs from collapsing. In this way, as the pulsations of the heart lasted a short time longer, some of the blood would be arterialized by the air in the lungs, and some arterialized blood would therefore always be found in the left side of the heart. The thorax was then opened, by as small an opening as possible, the aorta tied at its bend, and the vena cava superior close to the auricle; then, the dog being raised, and the ligatures drawn upwards, in order to make the opening in the vessels the highest point, thermometers were passed into the two ventricles down the artery and vein. After the introduction of the thermometers, cotton-wool was placed in the opening of the thorax, in order to prevent any real difference between the temperature of the blood occurring from the difference in the thickness of the two ventricles. In the experiments conducted on living animals, thermometers were introduced by the carotid artery and jugular vein into the heart.

The conclusions that may be drawn from these experiments may be divided into three heads:

1st. The difference of temperature between the two kinds of blood.
2nd. The temperature at different parts of the same system.
3rd. The changes of temperature at one and the same level of the arterial or venous system.

1st. As to the difference of temperature between the two kinds of blood. In the experiments on the recently-dead animal, the temperature of the blood in the right ventricle was found once higher, and twice equal, to that of the left. The temperature of the blood in the ascending cava was found ·72° C. higher than in the carotid. In the experiments on the living animal, in which the temperature was constant, that of the venous blood was found constantly higher than that of the arterial by ·07° to ·19° C. on an average of the minima and maxima.

2ndly. As to the differences in temperature of the venous blood at different parts of the circulation. The temperature of the blood in the large vessels coming from the head and extremities, rises as we approach the vena cava inferior, in which the blood reaches its highest temperature. This change does not reach a high value for small distances in the vena cava superior, but is rapid in the auricle, where the mixture of abdominal blood is greater. A similar rise is observed in the iliac vein. The temperature of the venous blood of the extremities is far

* Enderlin: Chlosaures Natron im Blute. (Schmidt's Jahr., i. 1853.)

† Liebig Georg: Ueber die Temperaturunterschiede des Venösen und arteriellen Blutes. Giessen, 1853.

lower than that of the vena cava inferior, or the right heart. The differences in the arterial blood are far smaller; they are not even appreciable at less than 6 cn. from the heart.

3rdly. As to the changes of temperature at one and the same level of the arterial or venous system. The temperature in the veins varies regularly in inspiration. This is observable in the larger venous trunks of the chest and belly, except the vena cava inferior. In the superior cava, the rise takes place at the end of inspiration; it reaches its highest point in the interval, falls at the end of expiration, and is at its lowest point after expiration.

Maximum. Minimum. Maximum. Minimum.
Inspiration — Expiration — Inspiration — Expiration — Inspiration.

The oscillations in regular breathing are from ·07° to ·10° C. These oscillations are thus explained. In inspiration the pressure of the diaphragm and abdominal viscera causes an increased flow of blood from the vena cava inferior into the right ventricle, which overpowers the stream from the vena cava superior; while in expiration, the flow of blood occurs towards the vena cava inferior, and its exit is impeded. The exit of blood from the vena cava inferior is also impeded, but not to so great an extent. Now the blood of the inferior cava being warmer than that of the superior, it follows that the blood in the right ventricle will be found warmest just after inspiration. Their maxima and minima will be found at different periods of inspiration, in different parts of the circulation, according to the period at which the stream of blood reached them.

VASCULAR SYSTEM.

On the Structure of the Endocardium.—The researches of Luschka on the structure of the endocardium add but little to the account already given by Thiele, Purkinje, and Raeuschel. He describes the epithelium of the endocardium as consisting of two forms of plates. 1st. Lancet-shaped cells placed close together, with sharply-defined nuclei, and one or two nucleoli. The second, which is more seldom met with, is composed of irregularly polygonal cells, like the epithelium of most serous membranes. The epithelium is sometimes met with detached. This degenerates into fat-corpuscles, whereby it is hindered from being deposited by passage into the smaller vessels. Under the epithelium are longitudinal fibres, not (like the fibrous coat of the vessels) united by interstitial substance, but running quite isolated, in a straight direction, frequently bifurcating and crossing each other at acute angles. The layer under that of the longitudinal fibres is analogous to the contractile coat of the vessels; it is composed of simple elastic fibres, and of other fibrillæ united into nets, the interspaces being filled up with a thin, fenestrated, structureless lamella, which, in the endocardium of the auricle, is granular and non-nucleated. This layer, though everywhere very thin, is thicker near the insertion of the valves. Lastly, beneath the elastic coat is the cellular coat, analogous to the "tunica adventitia" of the vessels; it is the conductor of the vessels, and may easily be separated as an independent membrane. He gives the following description of the arrangement of the vessels between the layers of the endocardium on the valves. In the auriculo-ventricular valves they run between the two layers of the endocardium; they are separated at the insertion of the valves by a thick cellular layer, and at their edge by a thin one; the vessels are most numerous where the valve is thickest, and they ramify towards its free edge. The results of the author's researches on the arrangement of the blood-vessels between the layers of the valve, have led him to conclude, that the exudations or fibrinous vegetations observed on the surface of the valves in endocarditis, is dependent on a hyperæmic condition of these vessels, accompanied by exudation of lymph from them, and do not arise as fibrinous precipitations from the blood flowing through the heart, as Simon and others have supposed.*

* Luschka: Archiv. für Pathol. Anat., iv. 2.

[With regard to the vascularity of the valves of the human heart, we have been completely unable to verify the statements of Luschka. In a minute injection of the heart of a young child which we lately made, we could trace no blood-vessels between the layers of the valves; the capillary vessels of the muscular substance of the heart stopped at the point where the valves were attached, having a loop-like termination.]

Nega* has added some contributions to our knowledge of the function of the auriculo-ventricular valves of the heart, their tones and sounds, and the signification thereof. His conclusions are founded on five vivisections of sheep and calves, on experiments on the human heart after death, and on clinical experience. He believes, first, that the closure of the auriculo-ventricular valves is not induced by the contraction of the ventricles, but by that of the auricles. To prove this (on the dead heart), he fills the ventricles through the venous sinuses with water, slowly, until the flaps of the valve are raised, and their still floating edges made slightly tense, then he injects rapidly a stream through a small syringe, pointed directly towards the valvular orifice. This induces such a tension and closure of the valves, that the heart may be turned over without the water running out. In life, the contraction of the auricle acts in the same way as this syringe; thus the closure of the valves occurs at the end of the auricular, and before the ventricular contraction.

The second point he has assured himself of by vivisection. The tense valvular flaps are drawn down by the contraction of the papillary muscles, which, according to Kürschner, almost disappear into the substance of the heart during the systole. Thus the valve acts not only as a valve to the auricular opening, but as a forcing pump on the ventricular, and a suction pump on the auricular side. Further, it may be concluded from this that the first sound of the heart is caused, not by the closure of the valves (for these have closed before the beginning of the ventricular systole), but by the tension of the valves and their tendinous fibres, caused, on the one side, by contraction of the musculi papillares, and on the other, by the hydrostatic pressure of the blood on the flaps.

These conclusions, which he announces in opposition to those of Skoda, do not seem to affect the diagnosis of heart-affections.

Muscularity of the Valve which closes the Foramen Ovale, and Cause of the Closure of the Foramen after birth.—In a series of preparations lately made by Dr. Peacock, is shown the valve which closes the foramen ovale, which presents a peculiarity of structure which he thinks powerfully contributes to the permanent adhesion of this membrane, and the consequent completion of the auricular septum. The muscular character of this valve was first pointed out by Senac, but denied by Haller, who remarked that its structure was purely fibro-cellular, the presence of muscular fibres being accidental and unusual. The researches of Dr. Peacock have enabled him to refute the assertion of Haller, and established the truth of Senac's doctrine. Heretofore, the explanation afforded of the closure of the foramen ovale was purely mechanical. After birth it is said the pressure of the blood in each auricle becomes equal, and no excess of force existing on either side of the foramen ovale, the valve is kept in contact with the edges of that aperture, to which, in process of time, it becomes solidly united. The author, on the contrary, believes that muscular action is called into force for the purpose of bringing the valve in contact with the margins of the foramen ovale, and a series of examinations have shown him, that the drawing up of the cornua and fold of the valve above the isthmus takes place by the action of muscular fibres, derived from the walls of the left auricle, assisted by its general dilatation; whilst the approximation of the cornua of the fold takes place by the contraction of either muscular or contractile fibres. Without muscular fibre it is difficult to explain the closure of the orifice, for after birth the pressure of the blood in both auricles is not so

* Nega: Beitrage zur Kenntniss der Function der Atrioventricularklappen des Herzens. (Schmidt's Jahr., i. 1853.)

certainly equalized, for there are instances in which the walls of the left auricle are scarcely stronger than those of the right, while the cavity of the right auricle remains the larger; and hence superiority of force on the right side of the valve exists, which, but for some counteracting agent, would drive the flapping membrane into the left auricle, and prevent the completion of the septum.

Hypertrophy of the Nerves of the Heart in Hypertrophy of the Heart.—Cloetta* has repeated Dr. Lee's dissections of the nerves of the heart with similar results, but thinks that the flattened swellings which the nerves present in calves and oxen, at the places where they cross the vessels, are not ganglia.

In order to discover whether Dr. Lee was right in his description of the increased size of the nerves in hypertrophied hearts, he dissected them in a case of the kind, and found, as Dr. Lee has figured, the nervous cords much enlarged, the gangliform swellings well marked, and the nerves of the left side more developed (as they are in the natural state) than those of the right. Whether this excess in the size of the nervous cords depended on increase in the nervous substance or hypertrophy of the fibrous tissue, he cannot decide.

On some Forces which influence the Speed of the Circulation.—Hering's† experiments on the forces which influence the circulation of the blood, are a continuation of those made as early as 1828, published in Tiedemann and Treviranus's 'Zeitschrift für Physiologie.' He then showed (and these experiments he has confirmed) that the time required for the passage of a solution of ferrocyanate of potash, which is mixed with the blood, from one jugular vein (through the right side of the heart, the pulmonary circulation, left cavities of the heart, and the general capillary circulation) to the jugular vein of the opposite side, varies from twenty to thirty seconds.

To these experiments it has been objected, that the rapidity of the circulation is increased by the circumstances of the experiment—viz., by opening the jugular vein in which the salt is detected at the same period as that in which the solution is injected into the blood. Under these circumstances the author performed a series of experiments.

(A.) *Experiments on the influence of opening the two jugular veins on the rapidity of the circulation.*—These consist in comparing the time at which the salt is detected under the above circumstances, and when the blood is *not drawn from the opposite jugular till the salt may be expected to be found.* He concludes that the acceleration cannot be more than five seconds.

(B.) *In order to find the time occupied in passing through the systemic capillaries.*—He opens the metatarsal artery on one foot and the corresponding vein on the other, and notes the difference in the time at which the solution can be detected in them. He finds the time consumed to be, *at most*, five seconds.

(C.) *On the influence of phlebotomy on the rapidity of the circulation.*—The same experiment as that last mentioned was performed before and after bleedings of from eight to ten pounds. From these he concludes, that moderate bleeding has no appreciable effect on the rate of the circulation; but if so great as to weaken the animal considerably, the circulation is retarded to a variable extent.

(D.) *As to the influence of the rapidity of the pulse on the circulation.*—He finds that increased rapidity of the pulse has no constant influence on the time of circulation.

(E.) *On the influence of the frequency of respiration on the rapidity of the circulation.*—To test this without including the influence of the pulse, he chooses horses suffering under two diseases in which the respiration is affected while the pulse remains natural—viz., tetanus and broken wind, in both of which the

* Hypertrophie der Herznerven bei Hypertrophie der Herz substanz. Von A. Cloatta. (Virchow's Archiv., 1853, Band v. 2.)

† Versuche über einige Momente, die auf die Schnelligkeit des Blutlaufs Einfluss haben. Von E. Hering. (Vierordt, Archiv., 1853.)

respirations are increased in number. He finds the rate of the circulation not materially affected, even when the respiration was from four to seven times the natural frequency.

In one case of very slow respiration he found the circulation quickened, in another the reverse.

On the Velocity and Pressure of the Blood in the Pulmonary Artery and Vein.—The results to which Beutner* arrives from his investigations are as follow:

The average height to which the column was raised in a tube inserted into the *pulmonary artery* was 29·6^{mm} in a dog, 17·6^{mm} in cats, 12·07^{mm} in rabbits (measured by the "kymographion"). The pressure in the left pulmonary artery, as compared with that in the carotids, he finds as 1·3 in dogs to 1·5 in cats. The actual values seem to vary in each individual case.

The average height to which the column was raised in a tube inserted into the pulmonary vein of a cat (only one experiment) gave a value of 10·5^{mm}.

The following are the results which he announces:

1. The capillaries of the lung have a very small lateral pressure to restrain.
2. They oppose very slight resistance to the velocity of the blood—the difference of speed in the artery and vein being 9^{mm}.

RESPIRATION.

Donders,† in a series of observations on the movement of the lungs and heart in respiration, made on living rabbits or on the dead subject, has shown that the expansion of the lungs takes place principally from above downwards, and from behind forwards; the apex and posterior part of the lungs may be regarded as fixed points. In ordinary respiration the lungs do not descend below the sixth or seventh rib. In forced inspiration they may reach the eleventh. The movement from behind forwards is most important: the edges of the lungs come into contact in front during forced inspiration, covering the pericardium so that the dull sound of the heart on percussion is quite lost.

With regard to the movement of the heart during respiration, the author has shown that during expiration the heart lies in the anterior part of the chest. In inspiration, on the contrary, the heart falls back, and the folded part of the pericardium lies against the lungs,—this change of position being due to the forward movement of the thoracic parietes. The impulse of the heart is produced by the contact of its apex with the walls of the chest, and partly to the shock communicated to the parietes of the chest by the change of shape of the heart. Kiwisch found that a needle passed in exactly at the place where the impulse is most distinct, hits, not the apex of the heart, but some point in the right ventricle. The apex of the heart lies loosely against the intercostal space in diastole, and is pressed against it in the systole. This is a consequence of the direction of the axis of the heart, which, during the diastole, forms an obtuse angle, directed backwards with the plane of its base, which is then elliptical, and in contraction becomes vertical to its base, which becomes circular. The change in the diameter of the heart from before backwards may give an impulse to the chest, but cannot raise the finger. That the impulse is produced by the systole is easily proved by observing the movement of an empty heart, or one containing any substance put into it. The movement of the apex downwards which takes place in systole, has no influence in producing the impulse.

* Schmidt: Jahr., ii. 1853.

† Die Bewegung der Lungen und des Herzens bei der Respiration. (Zeitschrift für Rat. Med., von Henle und Pfeufer, 1852, Heft 1.)

NERVOUS SYSTEM.

Brain.—Dr. Brinton,* in a paper on the structure of the Dentate Body of the Cerebellum, after mentioning the varying and somewhat conflicting accounts given by many anatomists, and more particularly by Reil, attempts to explain its structure by the physical characters of the tissues investigated, and by the condition of the specimens examined in a fresh state, or after immersion in alcohol.

He determines the form and situation of the corpus dentatum by uniting numerous and successive sections made in the three directions of space. By this method he is led to conclude that each corpus dentatum forms a tubular investment to the extremity of the processus e cerebello ad testem, open towards the fourth ventricle, and connected with the opposite body by a commissure of grey matter in the median line. Its arrangement with regard to the fibres of the cerebellum, cerebrum, medulla oblongata, and medulla spinalis, is chiefly deduced from examinations of specimens hardened in spirit. By this means he is led to believe that its interior exclusively receives the fibres of the processus e cerebello, its exterior radiating fibres to the various lobes of the cerebellum, which fibres, at the bottom of each lobe-stem, become inseparably mixed with a bundle from the restiform body, and with another from the pons Varolii. The author then makes some observations on its comparative anatomy, and concludes by assigning to it the name of the "cerebro-cerebellar ganglion."

Phrenic Nerve.—In a monograph by Luschka,† on the Phrenic Nerve (which will be specially reviewed in our next number), the author arrives at the following conclusions:

1. The phrenic is not merely a motor nerve, but a mixed nerve, containing sensory filaments distributed to the pleura, pericardium, and the peritoneum, covering the diaphragm, and on the anterior wall of the belly. It is also distributed to the coronary and suspensory ligaments of the liver.

2. It brings about a double interchange of fibres between the sympathetic and spinal nerves, since organic nerve-fibres go to it from the inferior and occasionally the middle cervical ganglion, and it gives, by its abdominal portion, fibres to the solar plexus.

3. In the majority of cases the phrenic arises but from one cervical nerve—the fourth.

4. The diaphragmatic branches he traces to the tendinous centre, the inferior vena cava, the right auricle, and the liver.

5. In its course over the pericardium it appears to be endangered in diseases of the pleura and lungs, especially tubercular. Hence, probably, some of the disturbances of respiration in these complaints.

Contributions to the Histology of the Nerves.—Marcussus‡ describes, from independent observations on the arrangement of the nerves in the torpedo, that after the bundles have broken-up into fibres, the latter divide: each double outlined fibre being broken-up into two, also double outlined fibres. The fibre not being always contracted at the point of division, the division is sometimes visible in the interior of the tubular membrane before the branches are separated. The division is always dichotomous, except in the very smallest branches, where it is sometimes triple. Here the double outline is lost, nuclei make their appearance: further still these disappear, and the nerve is seen to end in a free extremity, not quite sharp, generally smaller than the tube. Thus all the divisions of one primitive nerve-fibre form an independent system.

[These independent observations fully bear out the earlier investigations of Savi, who was the first to discover that the elementary nerve-tubules in the electrical

* On the Dentate Body of the Cerebellum, by William Brinton, M.D. (Proceedings of Royal Society, June 17, 1852.)

† Schmidt's Jahrbuch, iii. 1853.

‡ Froricps.

apparatus of the torpedo divide dichotomously. Wagner, also, who has more lately examined this structure, recognises the division of the tubular fibres. Robin, also, and Sharpey, have both seen the division of the primitive nerve-tubules in an organ in the tail of the common rays, and which, in respect of its structure, resembles the electrical apparatus of the torpedo. It would also appear that this division of the primitive nerve-tubules takes place in other organs. Kolliker has described the nerves of the spleen in the calf as having this arrangement.]

Pacchionian Bodies.—The investigations of Luschka* have completely elucidated the structure of the Pacchionian bodies. He states that they are found to be connected both with the cerebral and parietal arachnoid along the course of the longitudinal sinus; that they are normal structures, as might be supposed from their being found in all persons of all ages, although they may readily be confounded with actual inflammatory exudation under or upon the free surface of the membrane. They are found only near the mesian line, and are unconnected with disease of the subjacent pia mater, which is not found to adhere more closely in their vicinity than elsewhere to the arachnoid, from which they cannot be torn, and of which they are indeed regularly formed offsets.

The Pacchionian Bodies of the Cerebral Surface appear under a lens as shaggy projections of the arachnoid of various forms and size, as well as of very different number in different skulls. They are usually smaller and less distinct the younger the individual, their size varying from that of a poppy-seed to that of a millet-seed. Their colour is grey or whitish, and their structure usually firm and fibrous; they are covered with a scanty epithelium, and contain no vessels.

The Pacchionian Bodies of the Parietal Surface.—The fibrous web of the dura mater, where it forms the longitudinal sinus, presents many irregular interstices, over which the arachnoid is stretched, and into which it partly sinks. From these points, where the serous membrane is unsupported by the fibrous tissue, spring the Pacchionian bodies, presenting various shapes, pressing backwards through the dividing fibres of the dura mater upon the skull, and even into the canal of the sinus, and projecting forwards upon the surface of the brain, so as to come into contact with the Pacchionian bodies already described as springing from the cerebral arachnoid.

In the young they are less prominent, and of simple shape, but especially in old subjects they assume a more complex form, and interlace so closely with those springing from the cerebral arachnoid, that on removing the dura mater, both folds of serous membrane come away together, an occurrence often attributed to pathological adhesions. Their use is unknown. It can only be guessed that they in some way protect or strengthen the veins, as they enter the longitudinal sinus. They cannot be detected in animals.

ORGANS OF SENSE.

Eye.—Dr. Bush† concludes, from his experiments, that the superior oblique muscle of the eye possesses a double action—1st. That of rotating the eye round its antero-posterior axis, an opinion previously advocated by Hüik; 2nd. That it serves to direct the pupil inwards and upwards, as was asserted by Dieffenbach. The experiments of the author were made on rabbits, immediately after death, by removing the brain, isolating the fourth nerve, and passing a galvanic current through it. At each application of the wires, the ball was always directed *upwards and inwards*. To determine whether the ball was rotated round the antero-posterior axis by the muscle, a needle bent at right angles was introduced, the outer part hanging perpendicularly downwards. When the nerve was irritated,

* Müller's Archiv., 1852, p. 101.

† Eini ges über die Wirkung des Musculus Obliquus Superior Oculi. (Müller's Archiv., 1852, Heft iv.)

this perpendicular limb of the needle was moved towards the outer side, showing that the globe is ***rotated by the action of the muscle to the inner side.***

Vitreous Body.—Virchow* has made some observations on the development of the histological elements of the vitreous body. In an embryo swine, four inches in length, he found this structure already formed, the greater mass presenting all the qualities of mucus, and consisting of a homogeneous intercellular substance, at some places presenting a slightly fibrous appearance, and in which was scattered at tolerably regular intervals, round granular nucleated cells, of the same nature as those found in the gelatinous matter of the umbilical cord and of colloid. At the circumference was found a fine membranous layer, composed of a fine network of vessels, and an areolar network of fine fibres, containing nuclei at their points of interlacement, the meshes being filled with a muco-gelatinous substance. The author remarks here upon the analogy which exists between its structure and that of colloid or cartilaginous tissue. Its further formation appears to be, that the cells disappear, and the intercellular substance alone remains. Thus the cells, he says, must be regarded as the formative organs of the intercellular substance, as he before asserted in the case of the umbilical cord.

On the Structure of the Crystalline Body.—The researches of Gross† on the structure of the lens have led him to adopt the following conclusions:

The capsule is most intimately united to the lens, and can only be separated by tearing.

The inner wall of the capsule is provided with a cellular network, which is the peculiar matrix of the lens.

The humor Morgagni has no existence.

The cells by multiplication form layers, which become flattened, and applied to each other form polyhedral fibres.

These fibres, which are being continually renewed, are formed from all the cells lying on the same level, and in the circle which passes from a particular vertex to the opposite pole.

If the cellular layer remains undestroyed, and adhering to the capsule, fibres may be formed even after the extraction of the lens.

The removal of used-up fibrous elements of the lens appears to be accomplished through ***a central triangular canal*** directed from before backwards.

In the fresh state, the cellular germinal layer is perfectly transparent.

Ear.—The latest researches of Mr. Toynbee on the Anatomy of the Ear, relate to the muscles which open the Eustachian tube. In this paper,‡ the author, after having alluded to the opinion generally held, that the guttural orifice of the Eustachian tube is always open, and that the air in the tympanum is constantly continuous with that in the cavity of the fauces, states, as the result of his examinations, that the guttural orifice of the tube in man and other animals is ***always closed,*** excepting during muscular action, and that the tympanum forms a cavity distinct and isolated from the outer air. The muscles which open the tube in man are the tensor and levator palati, called into action during the process of deglutition. That the act of swallowing is the means whereby the Eustachian tubes are opened, is shown by the following experiments:—If the mouth and nose are closed during the act of swallowing the saliva, a sensation of fulness is produced in the ears; this sensation arises from the air which is slightly compressed in the fauces passing into and distending the tympanic cavities. Upon removing the hand from the nose, it will be observed that this feeling of pressure in the ears does not disappear, but it remains until the act of deglutition is again performed while the nose is not closed. In this experiment the Eustachian tubes were opened during each act of

* Virchow's Archiv., iv. 3. † Archiv. Générales, v. 1852.

‡ On the Muscles which open the Eustachian Tubes, by Joseph Toynbee, Esq., F.R.S. (Read before the Royal Society, Feb. 17, 1853.)

deglutition; during the first act, while they were open, air was forced into the cavity of the tympanum by the contraction of the muscles of the fauces and pharynx, and the guttural orifices of the tubes remained closed until the second act of swallowing, which opened the tubes and allowed the air to escape. That the act of deglutition opens the Eustachian tubes was inferred also from the custom usually adopted of swallowing while descending in a diving-bell. By this act the condensed air is allowed to enter the tympanum, and the sensation of pain and pressure in the ears is removed or entirely avoided. The author then enters into a detailed account of the Eustachian tube and its muscles in Mammalia, Birds, and Reptiles. In some mammalia, the muscles opening the tubes appertain, as in man, to the palate; in others, this function is performed by the superior constrictor muscles of the pharynx. In birds, it is shown that there is a single membranous tube into which the two osseous tubes open. This membranous tube is situated between, and is intimately adherent to, the inner surface of each pterygoid muscle, and by these muscles the tube is opened.

The conclusion at which the author arrives respecting the influence of the closed Eustachian tubes is, that the function of hearing is best carried on while the tympanum is a closed cavity, and that the analogy usually cited as existing between the ordinary musical instrument, the drum, and the tympanum, to the effect, that in each it is requisite for the air within to communicate freely with the outer air, is not correct. On the contrary, the author shows that no displacement of the air is requisite for the propagation of sonorous undulations, and that were the Eustachian tubes constantly open, these undulations would extend into the cavity of the fauces, there to be absorbed by the thick and soft mucous membrane, instead of being confined to the tympanic cavity, the walls of which are so peculiarly well adapted for the production of resonance, in order that they should be concentrated upon the labyrinth. In corroboration of these views, the author states, that in cases of deafness, dependent simply upon an aperture in the membrana tympani, whereby the sonorous undulations are permitted to escape into the external meatus, the power of hearing is greatly improved by the use of an artificial membrana tympani, made of thin vulcanized india rubber or gutta percha, so applied as again to render the tympanum a closed cavity.

DIGESTION.*

Influence of the Pneumogastric Nerves on Digestion.—Bidder and Schmidt† have instituted a series of experiments (four in number) to determine the influence of the pneumogastric nerves on digestion, two of which were performed on dogs in whom gastric fistulæ had been formed. They found that—

1. Section of the vagi did not diminish the sensation of hunger, and increased that of thirst. *The œsophagus being paralyzed, however, in its lower part, by the section of the vagi* in the middle of the neck, the food or saliva swallowed could not pass into the stomach, and was again rejected. The absence of the usual endosmose of fluid and saliva into the blood explained the thirst.

2. *The motions of the stomach were not impeded:* they could be felt by the finger introduced into the fistula, and food passed in by the fistula *was forwarded in the usual way, and did not return with the vomiting which was going on.* Thus the vagus does not possess (in the neck) those fibres by which the muscular coat of the stomach is connected with the centre of its regular actions. Nevertheless, irritation of the vagus in the neck excites the action of the stomach.

3. The secretion of the gastric juice *was little diminished.* When any diminution occurred it was to be attributed to the lack of the stimulus of food from paralysis of the œsophagus. In consequence of the continued excretions of the animal, a lack of the necessary water for the preparation of the gastric secretion

* The review on the 'Chemistry of Digestion' already given, has anticipated the details into which we should have entered under this head.

† Ill. Med. Zeitung, Heft viii. p. 112, 1832.

failed, and its elimination diminished considerably, but rose again as soon as the surface of the alimentary canal was artificially moistened. The gastric juice formed under a general deficiency of fluid was far less acid than that furnished after a new supply of water. This, however, which in one case was observed to rise continuously till the time of death, differed little from the reaction before the operation, being, on an average, neutralized by ·413 gr. of potash per cent. *Thus the chemical constitution of the gastric fluid* is not remarkably altered by section of the vagi.

4. Finally, it is proved that *the quantity of albumen digested is materially diminished* by section of the vagi (though the function is *not* destroyed): an effect which must apparently be attributed to the alteration in quantity of the secretion—the only known change in the action of the stomach caused by the operation.

[The results obtained from these experiments confirm in a most marked manner those of Dr. Reid, and also those made by Müller and Dickhoff on geese. It is, however, somewhat remarkable, that in a series of experiments performed by Bernard,* exactly opposite results were arrived at to those mentioned above. It is stated by this distinguished physiologist, that after division of the pneumogastric nerves in the middle of the neck, the movements of the stomach ceased, the secretion of gastric juice was instantaneously put a stop to, and in no case did any part of the food pass through the peculiar changes of chymification.

For a detailed review of the chemistry of the digestive process, I beg to refer my readers to the article on the 'Chemistry of Digestion,' p. 167, where the late very elaborate investigations of Bernard, Bidder and Schmidt, and their pupils, are succinctly set forth.]

The Secretion of the Pancreatic Fluid.†—Dr. Weinmann, at the suggestion of Ludwig, has conducted an inquiry into the amount of pancreatic secretion, and the circumstances influencing it. Pancreatic fistulæ were made in large dogs; the method of making the fistulæ differs in some particulars from that employed by Lassaigne, Frerichs, Bidder, and Bernard. For these details we refer to the paper.

(*a.*) For three days after the fistula was made there was a great diminution in the amount of secretion. Afterwards, in a dog (weight not given), there were excreted 0·301 gramme in a minute; 18·06 grammes in an hour; and 433·44 grammes (nearly 14 ounces) in a day. In a second dog (weight 30 kilogrammes) there were secreted 1055·52 grammes (about 33 ounces) in 24 hours, or 35·184 grammes to each kilogramme of weight.

[If the same numbers hold good for men, a man of 12 stone weight would secrete no less than 184½ ounces = 11½ pounds, in 24 hours!]

(*b.*) The secretion always proceeds, but more rapidly at some times than at others; it is at its minimum after long fasting, during vomiting, and after operative proceedings; and is at its maximum after food, and especially after water. Of this last point accurate tables are given.

(*c.*) Amount of solids in the pancreatic fluid. The amount of solids does not appear subject to the same changes as the quantity of the secretion: the more copious the fluid, the less the amount of solids. But the experiments at present lead to no definite law.

(*d.*) Influence of the nerves. All experiments through electrical excitement of the cœliac ganglion, &c., have led, at present, to no certain result.

SECRETION.

Mr. Baxter,‡ in an experimental inquiry, undertaken with the view of ascertaining whether any and what signs of current force are manifested during the organic process of secretion in living animals—a continuation of a series of experiments

* Gazette Médicale, Juin 1, 1844.

† Henle's Zeitschrift, 1853, vol. ii. p. 247. This paper has appeared since the article on the 'Chemistry of Digestion' was in type.

‡ Philosophical Transactions, part ii. 1852.

published in the 'Phil. Trans.' for 1848—shows, that the changes which occur during the organic process of secretion in living animals are accompanied with the manifestation of current force during biliary, urinary, and mammary secretion, and also when a circuit is formed between the mucous membrane of the lungs and arterial blood in the left ventricle of the heart.

With regard to the manifestation of current force during the biliary secretion, four experiments are detailed, two on rabbits and two on cats, the animals being destroyed by prussic acid; one electrode was inserted into the gall-bladder, the other in contact with the blood flowing from the vena cava inferior. In all the experiments the latter *was positive,* and by breaking and making contact, made to increase. The electrodes inserted respectively in the gall-bladder and vena porta, the latter was *slightly* positive. When other circuits were formed between blood in the chest and the various contents of the abdomen, it was generally found that the electrode in contact with the blood was positive, but not always. The fact proved by these experiments is, *that when the electrodes of a galvanometer are brought into contact with the bile flowing from the liver, and with the venous blood flowing from the liver, we obtain evidence of the secreted product and the blood being in opposite electric states.*

On Current Force during Urinary Secretion.—In six experiments, three on rabbits and three on cats, the electrodes of a galvanometer being in each experiment respectively inserted into the *urinary bladder* (in two cases containing highly acid urine), and the *blood in the renal veins,* the latter was always positive to a greater or less extent, varying from 3° to 4°.

The conclusion arrived at from these experiments was, that *during urinary secretion the blood and urine are in opposite electric states.*

On Current Force during Mammary Secretion.—The author here only relates one experiment, in which, on the application of one of the electrodes of a galvanometer to one of the mammary glands of a cat during suckling, the other electrode placed in contact with the blood flowing from the mammary vein, the blood was positive 8°.

On Current Force during Respiratory Actions.—Without giving any definite opinion as to the lungs performing the office of a secreting organ, it was found that the results of seven experiments performed on the rabbit and cat was, *that when the mucous membrane of the lungs, and the arterial blood flowing from the same part, are formed into a circuit, the arterial blood is positive.*

This fact affords some explanation of the failures of Müller, Pouillet, and of the author, in his early attempts to obtain evidence of current force being manifested when a circuit was formed between an artery and vein in a living animal. The author also considers whether these effects can be referred to any known actions. The effects could not be referred to the heterogeneity of the fluids without assuming that the blood was acid, and combined with the secreted product; nor could it be referred entirely to thermo-electric effects, inasmuch as the current varied in each organ, and was capable of traversing a liquid conductor. The effects, however, may be partly due to catalytic actions on the combining power of platinum, which supposition tends to confirm the opinion originally entertained by Wollaston, that the changes which occur during secretion are analogous to those which take place in the decomposing cell of a voltaic circle.

DUCTLESS GLANDS.

Thyroid Gland.—Kohlrausch,* in a paper on the structure of the thyroid gland, in addition to the structures usually mentioned by most anatomists, describes the frequent existence of nucleated cells, which are found lying on

* Müller's Archiv., 1853, p. 142.

the inner wall of the acini. They are spherical, pale, and finely outlined, with a light reddish membrane, especially in specimens full of blood. They vary in diameter from $\frac{1}{224}'''$ to $\frac{1}{160}'''$, the average being $\frac{1}{188}'''$. The nucleus is excentric, and measures $\frac{1}{560}'''$. They lie either singly or in groups, something like epithelium, and appear very easily destructible.

According to the description given by the author, the identity of these cells with embryo blood-discs seems indubitable.

Spleen.—There are still numerous opinions almost constantly being advanced on the structure and function of this complicated organ. Buk* supposes that the colourless corpuscles of the blood which are to be changed into the coloured ones, are formed in it. This takes place by the passage out of the twigs of arteries ramifying on the Malpighian vesicles of an organizable lymph, which thus gets into the lymphatics. (He believes the Malpighian corpuscles to communicate with the lymphatics.) Here the first developed elements of the blood, the colourless corpuscles, are formed; part being transferred to the lymphatic vessels, and part to the veins. Thus the venous blood contains an important component not found in the arterial.

Tigri,† in a paper reprinted from an Italian medical journal, and written in order to vindicate his claim to priority over Asson and Kolliker in their researches on the function of the spleen, reasserts, as the results of his microscopic and other investigations on the spleen of men and animals, the following conclusions:

The spleen is an organ which nature has destined to preside over the material composition of the blood. It receives into its vessels blood loaded with solid matters for elimination; these are the used-up epithelial cells and red globules, which are assimilated in it, and reduced into new principles of nutriment.

1. The anatomical elements of the spleen are blood and lymphatic vessels, to which are united the Malpighian corpuscles, the fibrous structure, the microscopic web, and the splenic fluid.

2. That it is not credible that the vessels of the spleen (looking at their size as compared with that of the organ) are destined only for its nutrition.

3. This is confirmed by observing, that in other organs in which the blood has to undergo a modification, there exist two orders of afferent vessels—i. e., the pulmonary and bronchial arteries for the lung, the hepatic artery and portal vein for the liver.

4. Similarly, these two orders of vessels must be recognised in the spleen: the first comprises the nutrient vessels; the second, those which carry into the venous system the blood loaded with eliminable materials.

5. The special conformation of the splenic venous canal of ruminants, visible from the point of their entrance into the organ, has reference not only to the form of the canal, but also to the structure of its walls.

6. To the form, which is cylindrical, but irregular from hollows and projections, to which he gives the name of splenic productions.

7. To the structure, inasmuch as the parietes of the veins are formed by the red substance of the organ, together with a most subtle and transparent membrane, which divides it from immediate contact with the blood.

8. This membrane, organized like that of the capillaries, performs the office of a filter, and gives passage to the red globules of the blood, which are rendered inactive as well as the epithelial bodies.

9. This structure, so visible in the large venous trunks of the spleen of ruminants, is verified also in that of the horse, pig, and lastly, in the human spleen.

10. The communication between the arteries and veins of the second category, by the intervention of a capillary system, is effected by channels so ample as to permit the easy passage of bodies as large as the one-third of a millimeter.

* Illust. Med. Zeitung, Munich, 1852, vol. ii. No. 8.

† Schiarimenti sulla struttura e sulla funzione della milza del Prof. A. Tigri. (Est. dalla Gazzetta Med. Ital. Tuscana, tom. iii. ser. 2.)

11. The spleen pulp otherwise is not a dense liquid, but an assemblage of fusiform nucleated cells, involuted or folded on themselves, isolated nuclei, and red blood-globules, which elements are contained in a most delicate areolar web.

12. The presence of the Malpighian bodies is undoubted.

13. The structure of the spleen presents no resemblance to the cavernous bodies.

14. The microscopic web, with its areolæ, is in direct communication with the venous cavity, by the porosities of the stratum limiting the isolated or confluent splenic productions.

15. The same web is in communication with the lymphatics.

16. The epithelial bodies!! detached from the walls of the vascular system, and mixed with the circulating blood, are brought by the artery into the splenic tissue, in which there is every reason to believe that they are arrested.

17. The same happens to the worn-out blood-globules.

GENITAL ORGANS.

Dr. Leydig* states, that the uterine glands of swine, over their whole internal surface, are lined with vibratile cilia. They may be prepared by taking a vertical section of the lining membrane of the uterus; the glands are then visible to the unaided eye as winding canals, which divide into branches, and terminate by broad ends in the cavity of the uterus. They differ in form and appearance as taken from a pregnant or unimpregnated uterus; in the latter they appear like the perspiratory glands; in the pregnant uterus they are more stretched out. The gland-cells, when examined fresh, are seen to consist of cylindrical epithelium with cilia. The contents of the cells are pale granular. The ciliæ are slender, but plainly visible, and reach to the cæcal end of the gland canal. The motion is tolerably energetic, and has been seen persisting a day and a half after the removal of the uterus, in a few of the canals. The author concludes by stating, that it is highly probable that the uterine glands of other animals, and of the human female, contain cilia.

Procreation.—Hirsch† has arrived at some practical conclusions in contradiction to the prevailing theories of Procreation. He concludes that menstruation in the human female has no analogy with the heat of beasts; that fructification can be accomplished at any time, not at the period of menstruation only, as in the lower animals at heat.

This he proves from analogy with the male, who is capable of fruitful coition at any time; from the fact of females feeling the instinct of copulation equally at one time as at another; from that of the Jewish females, who are obliged by their customs to abstain from coitus for five days before and seven days after the first appearance of menstruation, being, nevertheless, very fruitful; and from a case which came before him, where a woman was found to be impregnated twenty-two days after healthy menstruation. He states, also, that the ovary produces continually mature ova, which are either fructified during coition, and then being discharged, roll down in the uterus; or if unfructified, are dissolved and washed away at the monthly period. This he supports by observing, that menstruation may be replaced by vicarious hæmorrhage, and that women who are incapable of conception still go on menstruating. The human ovum is fructified in the ovarium, not in the oviduct.

The stay of the ovum in the oviduct must be very short, and contributes nothing to the further development of the ovum. In many animals the ovum may be fructified in the tubes, or even in the uterus; but in the human female the decidua is already formed if the ovum has got to the uterus.

The follicle (Graafian) leaves a far larger scar after impregnation than after menstruation.

* Ueber Flimmerbewegung in den Uterin drüsen des Schweines. (Müller's Archiv., No. 4, 1852.)

† Schmidt's Jahrbuch, 1853, No. 2.

Menstruation in the human female has a double object—to purge the body of blood, which is superfluous when impregnation does not take place, and to clear away the mature ova when no fructification has taken place.

Impregnation.—In Mr. Newport's preceding papers on the Impregnation of the Ovum in the Amphibia, which have been previously reviewed,* he has shown that the spermatozoon is the impregnating agent, the liquor seminis taking no part in effecting impregnation; and he believed, from the results of his observations, that neither before nor at the time of impregnation does the spermatozoon penetrate into, or lie in contact with, the coverings of the egg.

The results of his later observations† have shown, that the spermatozoon does penetrate into the substance of these coverings, and is sometimes partially imbedded in the vitelline membrane beneath them; but there is no evidence that it enters the vitelline cavity.

He first proceeds to show the relative duration of vitality in the spermatozoon and the egg; that in the former it is shorter than is usually supposed, being usually lost in from three to four hours after removal from the body into water at the temperature of 55° Fah., retaining its vitality longer at a lower temperature. When the spermatic fluid has contained many undeveloped cells, and has been preserved in a temperature of 51° Fah., it has fertilized at the end of twenty-four hours. The egg loses its fitness for impregnation very soon after it has passed into water; but when retained in the body of the dead frog, its vitality is preserved for twenty-four hours, and at a low temperature for forty-eight hours.

The results produced by the application of spermatozoa to the dead egg are similar to those produced on the living one by a solution of potash, the yelk becoming shrivelled and contracted; the same result ensues if decomposing spermatic fluid is applied to it.

A series of experiments are then detailed, which confirm the results of his previous observations, that fecundation may be effected by the application of exceedingly minute quantities of spermatozoa to any part of its surface, some parts, however, being more susceptible of its application than other parts; thus, it was found, that when the egg was placed vertically, with the centre of the white surface uppermost, and the spermatozoa applied solely to this part, fecundation is rarely effected; when, on the contrary, the dark surface is uppermost, and the spermatozoa are applied directly to it, fecundation is then the almost invariable result.

The Motion of the Spermatozoon in relation to the Function of Impregnation is then examined. This motion he regards as only the visible exponent of a peculiar power in the impregnating agent, being essential to its function, and associated with its structure and composition, the degree of procreative efficiency being determined by the intensity of this motor power. He also believes that some portion of the substance of the body of the spermatozoon is communicated to the egg in fecundation. The author has also distinctly observed, by placing the egg beneath the microscope at the time of the spermatozoa being applied to it, that the spermatozoon always penetrates into the envelops at the part only to which it is applied, and that soon after, it strikes into the vitelline membrane by its thicker or body portion, in a line with the point at which it has entered and the centre of the yelk; and he has found that eggs so penetrated have become fertilized, and produce embryos. On the contrary, eggs in which the spermatozoa have been observed on their surface, *but have not penetrated, so as to come in contact with the yelk membrane,* have been unfruitful. The spermatozoon invariably enters the egg with its thicker or body portion foremost, passing onwards

* See British and Foreign Medico-Chirurgical Review, vol. viii. p. 253; vol. ix. p. 255.

† On the Impregnation of the Ovum in the Amphibia, and on the Direct Agency of the Spermatozoon. By George Newport, F.R.S., June 17, 1852.

in a direct centripetal direction, and with a slight serpentine motion, to the vitelline membrane.

All the above-mentioned results obtained by artificial impregnation were confirmed by the author, who examined some of the eggs impregnated by the natural concurrence of the sexes. Spermatozoa were observed striking into the vitelline membrane many hours after the time at which the egg must have been fecundated.

The Nature of the Influence of the Spermatozoon, which is determined from three different sets of experiments, is then considered.—1st. By immersion of the eggs before, and at the period of, fecundation,and during the segmentation of the yelk in solution of potash, he observed that the endosmic action of the envelops of the egg is exceedingly rapid, as decomposition of the yelk commenced in some within *three minutes* of the application of the solution. In a weak solution, the result was favourable to the action of the spermatozoon.

2ndly. By reducing the bodies of recently obtained spermatozoa to a fluid state by trituration, and then applying the fluid to the egg immediately it is expelled from the female, the result was, that no fecundation was effected; the yelks, however, being in some cases shrivelled and contracted, showing that the broken-down spermatozoa had passed to the yelk by endosmosis.

3rdly. Portions of seminal fluid, when not triturated, on being applied to the eggs, resulted in each case in fecundation.

These experiments show that fecundation is not the result of the application of a portion of the body of the spermatozoon to the egg, but seems due to some dynamic power in the spermatozoon, which becomes lost when it has ceased to give evidence of the retention of it in its power of motion.

[These later observations of Mr. Newport's appear to be of the very highest importance, distinctly proving the fact of the *entrance of the spermatozoa into the interior of the ova*, and confirm, in a most marked degree, the observations lately made by Dr. Nelson on the Reproduction of the Ascaris Mystax, where is also noticed the fact of the spermatic particles becoming imbedded in the substance of the yelk itself, previous to fecundation being effected.]

PATHOLOGY AND PRACTICE OF MEDICINE.

On Progressive Atrophic Muscular Paralysis. By M. Cruveilhier.

M. Aran has described, in the 'Archives Générales,' a form of muscular paralysis, under the term "progressive muscular atrophy;" and M. Thouvenet has described the same lesion under the title "atrophic muscular paralysis." Since 1848, this form has been familiar to M. Cruveilhier; and in the present memoir various cases of it are related. The first case was that of a lady, aged 40, with general paralysis, more marked in the upper than in the lower extremities, and unaccompanied by lesion of sensation, or alteration of intellect. Death ensued by extension of the paralysis to the diaphragm and laryngeal muscles. A profound lesion of the spinal cord was diagnosed, but after death the nervous centres were found to be perfectly healthy. The true nature of the case was not recognised, and M. Cruveilhier, not content with the term "névrose" given to the case by other physicians who witnessed it, accused pathological anatomy of want of power to recognise some lesions of the brain and cord. The second case was that of a man, aged 18, with general paralysis, sensation and the intellectual faculties being unaffected. An affection of the anterior column of the cord was diagnosed, but after death the cord was found perfectly healthy. The muscles were carefully dissected, and were found to be atrophied in two ways—viz., by simple atrophy, and atrophy with fatty degeneration. The state of the nerves was not examined. In the third case there was gradual muscular atrophy and paralysis, with retention of intellect and sensation. In addition to the paralysis there were tremors, or little convulsive shocks, of the muscles of the extremities, as long as the atrophy was not complete. There was also, occasionally, a kind of general trembling or shivering. Death finally ensued from general bronchitis, and "œdematous pneumonia." Many of the muscles were atrophied and in a state of fatty degeneration, exactly resembling, as M. Cruveilhier remarks, the state of the muscles described by Dr. Meryon in the last volume of the 'Medico-Chirurgical Transactions.' M. Mandl, in drawing the microscopic appearances, produced plates precisely similar to those of Dr. Meryon. The brain was perfectly healthy; so also was the spinal cord and the posterior roots of the nerves. *But the anterior roots, especially in the cervical region, were found to be greatly diminished in size: in fact, atrophied.* This condition was traced till the union of the roots; in the conjoint nerve on the distal side of the ganglion no change could be detected; the trunks forming the brachial plexus, and this plexus itself, were healthy. The nerves running in the thickness of the muscles were, however, atrophied; and this was traced most exquisitely in the tongue, of which there had been perfect paralysis. The lingual (gustatory) nerve was well fed and of proper size, but the hypoglossal (motor) nerve was extremely atrophied; many of its branches seemed to consist of nothing but neurilemma.

M. Duchenne had electrolized this patient, and found that as the paralysis advanced, the muscles became inexcitable.

M. Cruveilhier remarks on these three cases, that the first case showed only paralysis without disease of the nervous centres; the second, more completely examined, exhibited great muscular atrophy and degeneration; while the third, still more carefully dissected, showed, in addition, atrophy of the anterior roots and of the muscular branches of the nerves. He remarks, also, that the clinical history and the morbid anatomy exactly accord. There is conservation of intelligence, and want of disease in the brain; conservation of sensation, and the cord and posterior roots are unaffected; paralysis of motion, and the motor nerves and muscles are atrophied.

But what is the connexion between the atrophy of the muscles and of the nerves? Which is primary and essential?

The coincidence of nervous and muscular atrophy cannot properly be regarded as an exceptional case; nor, in all probability, is it a simple coincidence. Cruveilhier, after referring to the rapidity with which the atrophy occurs; to the great influence of the nerves; and to a case (of Dupuytren's) in which atrophy of one half of the tongue succeeded compression of the hypoglossal nerve by a cyst; regards as demonstrated, that the atrophy of the nerves is the primitive lesion and the atrophy of the muscles is consecutive, and a consequence merely of diminution of function.

But what is the cause of the nervous atrophy?

Here observation at present fails, and future clinical experience must solve the problem. M. Cruveilhier believes that he has accomplished one step of progress in showing the implication of the nerves. How the nerves become implicated must now be learned.—*Arch. Gén.* Mai. pp. 561—603.

On Uræmia. By Dr. Ed. Schottin.

After alluding to the symptoms of uræmia and to the difficulties of its explanation, the author refers to the hypothesis advanced by Frerichs (that the conversion of urea into carbonate of ammonia is the cause of the symptoms formerly ascribed to the retention of urea *per se*), and dissents from this opinion. He does not deny that carbonate of ammonia injected into the veins will give rise to the symptoms described by Frerichs; but he states that other substances have the same effects, and especially the sulphates of potash and soda. With respect to the emission of ammonia in the breath of uræmic patients, he holds the test with a rod dipped in hydrochloric acid, and held before the mouth, to be altogether insufficient; as in ill-ventilated rooms, and in most individuals, the breath will cause a cloud to form around the moistened rod. The author, therefore, proceeded to experiment with reddened litmus-paper, moistened with pure water, and held before the mouth. The results were curious. A man with typhus, delirious and soporous, whose urine contained no albumen or cylinders, had ammonia in the breath in quantity: he recovered. A woman with puerperal pyæmia, thirty hours before death was in a condition resembling pyæmia: there was a little albumen and some cylinders in the urine; the breath was strongly ammoniacal. After death the kidneys were healthy. Ammonia was detected in the breath of a patient dying of phthisis. A woman with angina had the breath very ammoniacal, but this was found entirely to depend on carious teeth and a filthy state of the mouth. A young woman, dying with mania and with excoriated mouth, had also ammonia in the breath.

In all these cases the author believes that the cause of the ammonia was simply the decomposition of the secretions of the mouth. As a corollary to these cases, he has examined 16 decided cases of uræmic poisoning, and only in *one* case was there a little ammonia in the expired air, and this was probably derived from the mouth.

After some further criticism on the alleged decomposition of urea in the blood, and after suggesting that urea may be sometimes poured out vicariously from the mouth as it is known to be from the stomach and intestines, and may then decompose, the author remarks, that the mere retention of urea in the blood will not account for the symptoms of uræmia. But referring to the composition of the blood in Bright's disease, and to the great increase in the quantity of extractives (in healthy blood, albumen 100 = extractives 5; in blood of Bright's disease, albumen 100 = extractives 40), he believes that the causes of uræmic poisoning must be looked for in an impediment of the metamorphosis of the tissues, in a destruction of the process of endosmose and exosmose between blood and tissues, and perhaps in a generally diminished oxidation power in the blood.—*Vierordt's Archiv.*, 1853, Heft i. p. 170.

On Leukæmia and Pyæmia. By Dr. Griesinger.

The author has determined the relative quantity of pale corpuscles in various parts of the vascular system, in certain cases of leukæmia and pyæmia, and affirms that the number of corpuscles differs greatly in different systems of vessels. In the first case (pyæmia) there was a large abscess in the liver. There was pulmonary apoplexy of the left lung, and compression, œdema, and some pneumonia of the right. In the blood of the *left* heart, the colourless corpuscles were less numerous than in a state of health; in the blood of the *right* heart they were in enormous numbers, and were, in many cases, aggregated together into masses. The fluid squeezed from hepatized portion of lung showed numerous cells like the colourless cells of the blood (except that the envelop was more slowly acted on by acetic acid), and many large granular cells. The author believes, that the only way to account for the difference in the number of white corpuscles on the two sides of the heart, is to admit that they were arrested in the lungs, and argues that this arrest would be assisted by the great disposition of the white corpuscles in this case to adhere into masses—a disposition not always observed—and by the weakness of the circulation in this individual. In a second case of enlarged liver and spleen with lung-infiltration, which was less marked than in the last case, there were numerous white corpuscles on both sides of the heart, but many more on the right than the left side. They did not form into masses, as in the first case. In the blood of the splenic artery there were more white corpuscles than in the blood of the vein, and hence the author believes that there was arrest of them in the spleen as well as in the lungs. In a third case (of splenic leukæmia), *without* alteration of the lungs, there was no difference in the number of white corpuscles on the two sides of the heart. In a fourth case (doubtful whether pyæmia or leukæmia), the quantity of colourless cells was also equal in the arterial and venous blood, except in the splenic vessels, where they were most numerous in the artery. The author, after a full consideration of all these facts, concludes that "many (not all) secondary pneumonias arise from heaping up and arrest of these bodies (colourless cells) in the lungs." Virchow remarks, in a note appended to the communication, that in a case of leukæmia, formerly communicated by him, the superficial pulmonary vessels were "filled with a whitish puriform mass (colourless cells)," and promises a drawing of the appearances.—*Virchow's Archiv.*, Band v. p. 391.

[We shall take another opportunity of referring to Virchow's late elaborate paper on leukæmia.]

On the Significance of Coagula in the Urine in Kidney-diseases. By Dr. Mayer.

In order to examine into the validity of Virchow's opinion, that under the term "Morbus Brightii" several processes are confounded (viz., "simple catarrhal," "croupous, and degenerative processes"), the author has examined the urine in a number of persons, and describes the following coagula:

1. The casts of renal tubes usually described.

2. Pale, flattened, homogeneous, or longitudinally plaited coagula, dissolving in acetic and hydrochloric acids, and therefore neither fibrinous nor mucous. They decidedly come from the kidney, as they are seen in its structure.

3. Long, very pale, membranous, and plaited, often coiled threads, such as are delineated by Rayer,* and considered to be mucus. These have never been found in the kidney.

4. Coagula resembling those described by Donne,† as derived from the prostate. They are two or three times as broad as the renal casts.

Referring now solely to the true kidney-tube casts, these were found in 22 cases out of 44 cases indiscriminately examined. 11 of these cases were fatal, and on section, in 9, there was croupous-catarrhal (croupös-katarrhalische), and in 2, degeneration processes. 6 of these cases are detailed at length, with, as far as the kidneys are concerned, the following titles:—1. Lightest grade of croup, with scarcely perceptible catarrh of the kidneys; 2. Nephritis crouposa; 3. Croupous-catarrhal kidney-inflammation; 4. Catarrhal-croupous kidney-inflammation; 5. Intense catarrhal-croupous nephritis; 6. Chronic degeneration of kidneys. [We have given these titles to show our readers the phraseology now very current in Germany.] The author then proceeds to his conclusions, and states that, in the lightest form of croupous inflammation, albumen and cylindrical kidney coagula appear in the urine, or, in other words, no croup of the kidney can exist without renal casts. On the contrary, catarrh of the kidney exists with simple albuminuria without renal casts; and the relative quantity of albumen and casts in mixed cases of catarrh and kidney-croup, observes no constant relation. With respect to the possibility of diagnosing "croupous nephritis" and deeper degenerative processes by the condition of the urine, the author believes, that although in some cases the colossal quantity of albumen, of blood-corpuscles, and of fibrinous coagula, may indicate an *acute* degenerative kidney-disease, yet that from the characters of the renal-casts, "the distinction between croupous affections and *chronic*, and even often *subacute*, degenerations is not possible." Whether in these different forms of disease a different chemical composition of the urine exists remains to be seen.

The cases narrated prove, also, that croupous inflammation can last for a long time without leading to degeneration; it can also be of very short duration, and be entirely cured. Of rapidly cured kidney-croup, three cases are related. The first

* Mém. de la Soc. de Biol., Paris, 1851, Plate III. fig. 3, 4.
† Cours de Microscopie, fig. 57.

was a case of pleuritic effusion: on the third day, there were a few pale kidney-casts and albumen in the urine; the casts were seen for two days; the albumen existed for three, and during this time the pleural effusion occurred; in the succeeding fifteen days, absorption went on, but neither casts nor albumen again appeared. The second case was one of pneumonia with casts in the urine from the second to the fifteenth day, and albumen to the tenth. The third case was also pneumonia with casts and albuminuria appearing on the fourth day; the albumen disappeared on the next day; the casts lasted till the eighth, when the pneumonia was being resolved; on the tenth day the patient became worse, and on the following day pleuritic effusion was present; tube-casts (with albuminuria) reappeared, and lasted till the sixteenth day.—*Virchow's Archiv.*, Band v. p. 198.

Three Cases of Hemiplegia caused by blocking-up of a Cerebral Artery. By Dr. Ruhle.

The author remarks, that sudden hemiplegia, occurring in heart-disease, with or without loss of consciousness, is not uncommon; dissection shows, in such cases, sometimes hæmorrhage, sometimes softening, and with the latter lesion blocking-up of an artery is often combined. Virchow, in his work on 'Acute Inflammation of Arteries,'* first showed that such blocking-up of arteries could arise from the arrest in them of foreign bodies brought from distant parts. The three cases of hemiplegia now related prove the same fact. In the first case, sudden hemiplegia on the right side came on in a patient with hypertrophied heart and diseased aortic valves; after death, the left arteria fossæ sylvii was found blocked-up with a calcified mass, evidently detached from an aortic flap, and the left corpus striatum was softened. The author declines to say whether the softening followed the obliteration, but has little doubt that the sudden hemiplegia was attributable to the latter cause. The second case seems to render this opinion highly probable. In this instance, sudden right hemiplegia came on, followed by death in seven hours. On section there was blockage of the "carotis cerebralis sinistra," caused by lodgment of a clot which had been detached from an old chalky concretion hanging down from a contracted mitral orifice into the ventricle; there was, however, no softening of the cerebral substance, so that here *the mere arrest of circulation, in a particular part of the brain, caused hemiplegia, without visible alteration of the brain-substance.* In the third case (phthisis pulmonalis), without any cardiac murmurs, sudden right hemiplegia came on; on section, the "carotis cerebralis sinistra" was blocked-up with a white, tough, elastic plug; there was yellow softening of the under and middle part of the left hemisphere. On the mitral valve were two fibrinous coagula, white, elastic, and seated on and firmly adhering to excrescences on the valves. In all other vessels between the mitral valve and cerebral artery were no other coagula. In all these three cases the coats of the cerebral arteries were normal, and there was no local cause for their blocking-up.—*Virchow's Archiv.*, vol. v. p. 189.

[These observations were evidently made without cognizance of those previously recorded by Dr. Kirkes;† and they are therefore extremely valuable as confirmatory evidence, as well as in the novel fact they prove, that mere deprivation of blood will cause hemiplegia.]

Croup and Tracheotomy. By Dr. Karl Weber.

Two cases of croup are related. In the first case, the symptoms on the sixth day were desperate; tracheotomy was performed, and the treatment (calomel every three hours) was persevered in. The symptoms were immediately alleviated, and the wound had closed on the twentieth day. In the second case, croup came on after measles; tracheotomy was performed on the ninth day; the patient died on the

* Archiv. für pathol. Anat., vol. i. p. 272. † See No. 22, p. 384.

forty-sixth day, and after death it was found that the canal of the trachea above the canula was completely closed. This was discovered first of all when, on the tenth day after the operation, the symptoms being exceedingly favourable, the canula was removed: immediate suffocation ensued, and the canula was reinserted. Further operative proceedings were contemplated, when the child suddenly died, not from asphyxia, but from an undetermined cause. The larynx and trachea were removed, but no further examination of the body was permitted. The larynx, examined and described by Henle, was healthy till below the lower vocal chords, where there was complete closure by means of a vascular, firm, white substance, torn with difficulty, and constituted partly by uniting tissue, and partly by a similar forming structure. The mucous membrane could not be found. The fistulous opening formed by the canula was clothed with smooth plicated membrane, resembling a mucous membrane, and covered with flat nucleated epithelium cells (0·008‴ in diameter). The thyroid and cricoid cartilages were peculiarly changed, but their condition can hardly be understood without the figure, which is given in the original.

"The thyroid normally formed on the upper border, and on the under border on the left side, shows on the left, near the middle line, an irregular and rough cartilaginous prolongation, which arises out of and is articulated with the rudimentary cartilaginous ring. On the right side the under border of the thyroid is obliquely cut off, and bears, instead of the lower horn, a broad articulating surface, with which an oblique four-sided cartilaginous piece is moveably joined, but not by an articulation."

The cricoid was formed by four pieces, two of which were perhaps divided by the operation, and the forms of which are given in the plate, but can hardly be described. Henle considers this condition as an original malformation, but thinks that it gave rise to the obliteration *after* the operation.

To this account Weber adds some general considerations on croup and tracheotomy. He believes that in true croup there is always exudation in the larynx.

The Diagnosis.—The only disease with which croup can be confounded is the laryngismus stridulus, or false croup. This is, however, distinguished by the want of precursory symptoms, by the suddenness of the attacks, which frequently occur in the night, and by the remissions. True croup commences gradually, and the severe attacks never come on till the illness has lasted some little time; there are no true remissions or intermissions, as in laryngismus; in which disease the child, after a severe attack at night, may appear the next day perfectly well. The voice is another distinction, as the natural tone is regained after the attack of laryngismus, but never in croup. True croup never relapses; laryngeal cramp often. Weber believes croup to be probably contagious, and to be epidemic.

The Prognosis.—Jurine states that 25 of 28 were cured; Boudet, on the contrary, from the records of the "Hôpital des Enfans Malades," states that 57 children died out of 63. Guersant saw 80 die out of 100. Weber believes these latter statistics to be the correct ones, and that the more favourable result obtained by Jurine and others, arose from the inclusion of cases of laryngismus, which is a rarely fatal disease, but of the mortality of which there are no certain statistics.

The Treatment.—Emetics are used as *abortive* treatment, but Weber thinks with little success; the local applications of irritating substances, such as nitrate of silver, is much more efficacious, and Weber speaks highly of its use. Blisters are discountenanced; leeches are considered to be of only very moderate utility, or to be even hurtful, by reducing the strength without having the least effect on the exudation of the pseudo-membrane. General antiphlogistic treatment is said to be useless; but calomel is spoken of as being extremely useful, both given internally and used as inunction.

Tracheotomy is strongly recommended, and the statistics of Trousseau are relied upon, which give the extraordinary number of 222 operations and 127 cures.

Trousseau operates early, and introduces a solution of nitrate of silver (10 grains to ℥j of water) into the trachea. This appears to be an important modification, and to have an immense effect in loosening the membranous exudation.—*Henle's Zeitschrift*, Band iii. Heft 2, p. 8.

SURGERY.

On Hæmorrhagic Cysts of the Neck. By MM. Seutin & Michaux.

M. Seutin treats not only of the *hæmorrhagic*, but also of the *serous* and *hæmatic* cysts which are met with at the anterior part of the neck. The serous cysts contain serosity of different colours and degrees of transparency, and the hæmatic a dark grumous fluid, which is evidently blood that has undergone transformation during long sojourn in the cysts. The hæmorrhagic, if opened, furnish large and dangerous quantities of fluid-blood, which will continue to exude from the walls of the cyst after it has been emptied of its contents.

M. Seutin has paid much attention to the pathological anatomy of these cysts. The serous form is composed of a whitish or transparent membrane, analogous to serous or synovial membrane, which, usually smooth on its internal surface, may be rugous, when the fluid it contains becomes grumous. The cellular tissue on which it lies sometimes becomes, and especially in old cases, thickened and condensed, giving it a cartilaginous feel, and rendering its distinction from a solid tumour difficult. The lining membrane of the hæmatic cysts is of a similar character, but it is usually much more rugous and dark, as if ecchymosed. That of the hæmorrhagic cysts is entirely different, being neither uniform, like the serous, nor rugous, like the hæmatic cyst, but of a soft, spongy, velvety character, composed of numerous minute interlaced vessels, resembling, in fact, erectile tissue. The vessels do not open by apertures into the sac, for injections will not penetrate; but the blood is discharged by exudation.

The hæmatic and hæmorrhagic cysts bear a certain resemblance to each other, inasmuch as both contain blood; but while, in the first case, the blood, if removed, is succeeded by turbid serum, in the other, renewed hæmorrhage takes place. This hæmatic form may have been produced in different modes. 1. Blood effused into the cellular tissue may, by the irritation it causes, lead to a condensation of this around it in the form of a sac. 2. An originally serous sac may become converted into a hæmatic one by contusions, which rupture its parietes and lacerate its vessels. 3. At one period, the parietes of a hæmatic cyst may have been of the same structure as those of the hæmorrhagic; but by reason of the compression exercised by the tumour or a diminished afflux of blood, the vessels composing it underwent diminution or obliteration.

When the cysts occupy the *thyroid gland* itself, they may be due to the development of one of its areoles, as suggested by Lebert; but their production is probably quite as often due to another mechanism. The patient usually attributes their origin to a blow. This probably gives rise to effusion of blood, and the conversion of the surrounding cellular tissue into a cyst. If the blood remains on this, the cyst continues hæmatic. If the colouring matter is resorbed, it becomes serous; while, if continued determination of blood to the point leads to dilatation of vessels, it takes on the hæmorrhagic form. As was to be expected in so vascular an organ, the hæmorrhagic cysts of the thyroid gland are by no means rare.

The cysts may also form in other parts of the neck, being formed by the condensation of cellular tissue around the effusion of blood due to traumatic causes, or an effusion of serum due to irritation. They may also become developed within the lymphatic gland—the proper glandular substance disappearing by transformation into a cyst.

M. Michaux describes only the hæmorrhagic form of cysts under the title of *Hæmatocele, or Sanguineous Cysts of the Neck,* believing them to constitute a form of tumour of the neck hitherto unnoticed. He considers the tumour to be quite distinct in its nature from vascular goitre, aneurism, erectile tissue, or fungus hæmatodes. As no opportunity has, however, offered itself to him to examine the parts after death, his views of its nature is somewhat conjectural; and we have no doubt that the tumour he alludes to, as giving rise occasionally to troublesome hæmorrhage, is the hæmorrhagic cyst of M. Seutin, lined with erectile tissue. He recommends an exploratory puncture to be made, and if the blood is found so fluid as to admit of the cyst being emptied, an iodine injection may be thrown in; while if it is too thick to obtain issue, the sac may be incised through a part or all of its extent—a practice, however, which he admits is sometimes dangerous.—*L'Union Médicale*, No. 33; *Gaz. des Hôp.*, No. 36.

Observations on the Operation for Cataract. By M. Sichel.

M. Sichel, in some observations on cataract, recently drew attention to the case of a young girl, in whom he had lacerated a secondary capsular cataract, as exemplifying the ***hereditary*** character of ***congenital cataract***. Her father and uncle were both operated upon, about two years since, for congenital cataract of both eyes, which, however, had only become complete in the 33rd year—an interval of a year elapsing in each case before such completion and the operation in the second eye. The cousin of the girl, æt. 15½, son of one of the brothers, was operated upon at 18 for a congenital cataract, which had become complete at 14. There is one in the other eye not yet complete. In none of the ascending branches of the family had cataract manifested itself.

Influence of Position after the Operation.—In this young girl, one of the capsular shreds remained floating at the upper and inner part of the pupil; and M. Sichel resorted to the simple expedient he is accustomed to employ on such occasions—viz., causing decubitus on the side of the body towards which the mobile capsular shred inclines, and towards which it is in this position drawn by its own weight. It then disappears definitively behind the pupil, contracting adhesions with the posterior surface of the iris.

Mode of Operating.—M. Sichel does not give exclusive preference to any mode of operating. He depresses hard cataracts at all ages, although he prefers their extraction in the aged, in order not to leave the lens as an irritant body, capable of giving rise to internal inflammations, difficult to combat in persons with whom antiphlogistic means must be used so cautiously. He breaks up all soft or half-soft cataracts in individuals below the age of 40, and especially below the age of 30, and exceptionally above the age of 40. He extracts in senile, soft, and half-soft, in individuals above 40, and especially above 50; as in these latter the breaking-up such cataracts are followed by considerable swelling of the lens, which compresses the internal membranes, and causes violent inflammation. If this is attacked with vigour, debility and marasmus follow; while, if attacked insufficiently, disorganization of the internal membranes and incurable amaurosis result. In persons below 40, and especially in children from 6 months to 15 years, tolerance after the use of the needle by a skilful hand is very great; while reaction is inconsiderable, especially with the last-named, and between 20 and 40 the resistance of the constitution to the consequences of antiphlogistic treatment is at its maximum. Thus the normal, eclectic operation is extraction for soft and half-soft senile cataracts, breaking-up for soft and half-soft in the young, and especially for congenital cataracts, which are seldom very consistent. Hard cataracts may be depressed at any age; but should preferentially be extracted in old age.

M. Sichel always extracts by the upper section, the advantages of which, owing to the complete and prompt cicatrization of the wound, are very great. The upper eyelid forms a kind of supporting bandage, the effect of which is still further increased by the mode of dressing adopted. This consists in the application of five strips of adhesive plaster over the eyelids of each eye, and then of graduated compresses, in such a manner, that the chief compressing power is exerted on the edges of the wound. The whole is supported by a bandage. In this way a gentle compression prevents the edges of the wound becoming separated during the movements of the patient.—*Gaz. des Hôp.*, No. 54.

On Alteration of the Form of the Pupil. By M. Foucher.

Before enumerating the various circumstances under which the pupil undergoes an alteration of form, M. Foucher details the results of the examination of the eyes of 164 persons, taken at hazard in the wards of La Charité. Among the 164, the *direction* of the pupil was central in 98, carried upwards and inwards in 31, upwards in 15, inwards in 12, and upwards and outwards in 2. He has often observed the pupil carried upwards and inwards in hysterical and chlorotic persons; but since this disposition may exist under various other circumstances, he does not attach much importance to its existence in these, or as a sign of masturbation in children, which it is said to be by Barbier. The *form* of the pupil was circular in 120 of the 164. Its large oblique diameter was downwards and inwards in 10, upwards and inwards in 8, transverse in 6, vertical in 6, upwards and outwards in 4. Thus, in a considerable number of cases, the pupil may be other than circular; but what is especially remarkable, is the change in form that may take place during contraction or dilatation of the iris. Thus a pupil, circular during contraction, may become elongated in one of its diameters during dilatation, and *vice versâ*—the iris acting apparently with unequal energy in its different parts. Again, the pupil may be quite circular in one eye, and one of its diameters may predominate in the other—such varieties of form being fugacious and transformable. M. Foucher enters into a detailed examination of the various affections which may alter the form of the pupil, illustrating the paper by various cases.—*Rev. Med. Chir.*, xii. p. 207.

On the Aqueous Discharge from the Ear in Fracture of the Basis Cranii. By M. Bussy.

As our readers are aware (B. & F. M. C. R., v. 342), much discussion has taken place in France as to the source of the aqueous fluid, which, flowing from the ear, may be considered as pathognomonic of fracture of the base. A. Berard and Robert regard it, from its large quantity, rapid reproduction, and other circumstances, as the cerebro-spinal fluid itself; and in the present report made to the Académie, M. Bussy gives an account of an analysis of the fluid made by M. Deschamps—comparing it with two others already made by Chatin and Rabourdin—which tends to corroborate this view.

These analyses show, that while the serum of the blood (which the fluid has by some been conjectured to be) contains about 8 per cent. of albumen, this fluid, notwithstanding strong alkaline impregnation, contains none or the merest trace. So, too, while the serum contains, according to different observers, 0·6 to 0·58 of chloride of sodium, this fluid contains 1 per cent. Its density is 1007, while that of serum varies from 1027 to 1029. The composition of the cerebro-spinal fluid, as given by Lassaigne, agrees almost exactly with these analyses.—*Bull. de l'Acad.*, xviii. pp. 240—252.

MIDWIFERY, &c.

On the Sclerema of New-born Infants. By Dr. Elsaesser.

Dr. Elsaesser's observations are founded upon 53 cases (29 males and 24 females) which he has met with in the hospital during the years 1828—51. Of this number, 10 only were full-timed, and of the 43 others 10 were twins. During the same period there were born in the hospital 4468 children at full time, and 267 premature.

The disease was especially prevalent during 1849—50, when puerperal fever also prevailed; and it is oftenest met with in November and December, no case having been seen in May. In 10 cases it began to manifest itself within the first 12 hours after birth; but usually it was not met with until the 3rd, and from that to the 5th day. The diminution of weight that occurred was remarkable, as was also the diminution of temperature, both generally and in the indurated parts. Whenever any improvement occurred, this rose again. Of the 53 cases, all but 4 proved fatal, either from the sclerema itself or some incidental disease. Icterus was the most frequent complication, then pemphigus and erysipelas.

On incising the parts after death, a turbid fluid, resembling that of anasarca, often flows out. The adipose tissue consists of solid fatty granules, which are easily detached, being already separated by the effused fluid. This tissue, of a deep yellow, is usually from ½ to 3 lines thick, and sometimes below it a gelatinous one is found. The indurated tissue is traversed by numerous veins gorged with black blood; but no lesion of the large vessels, or obliteration of capillaries, noted by Bouchut, have been met with. The induration rarely extended below the chorion. The muscles were pale, as if infiltrated, and in those of the buttock small apoplectic deposits have been found. The veins and sinuses of the brain were mostly filled with dark, diffluent blood, and effusion was found in the ventricles or at the base. In a tenth of the cases lobular pneumonia was present, and in a third, portions of the lungs were impermeable to air. Intestinal lesions and hyperæmia of the abdominal viscera were common, and in 8 cases peritonitis was present.

Whatever hygienic care may be lavished on the children of hospitals, these diseases are far more frequent and fatal among them than in private practice. Among the most prominent causes, vitiation of the air must be noticed, another being the influence of chill. Cold impedes the functions of the skin, leading probably to a determination to the kidneys, which are found hyperæmic, albuminuria being also often present. A principal predisposing circumstance is the innate debility or incomplete development of the organism, premature children constituting four-fifths of the subjects. On the other hand, vigorous children are sometimes attacked; and in premature twins one shall have the disease and the other be exempt from it.

The treatment has consisted chiefly in the employment of hot, aromatic baths, and the administration of stimuli, such as musk. Dr. Elsässer has not met with the *chronic* form of the affection.—*Archiv. Gén.*, N. S., i. 539.

On the Etiology of Mammary Abscess. By M. Nelaton.

M. Nelaton states that, as the result of his examination of the history of a great number of these cases, he is very sceptical as to the truth of the supposition which attributes their occurrence to exposure of the breasts. He believes that abscesses are very often due to the existence of chaps or sores of the nipple, whence the irritation is propagated along the lymphatics of the organ—just as a wound of the foot or hand will give rise to inflammation in the vicinity of the glands of the groin or axilla. Once excited in the breast, the inflammation may become speedily propagated to the deeper parts of the organ. M. Sappey's preparations of the lymphatics exhibit the great abundance of these vessels, which almost all arise at the nipple or areola, spreading and ramifying from this common centre in all directions along the fibrous partitions of the gland. Another important point is the pretty direct relation which exists between the situation of the chap and that of the mammary inflammation. If situated at the upper part of the nipple, the inflammation will usually be found at the upper part of the breast, and so on for the other localities.

The case which immediately gave rise to these observations also led to another. of interest in relation to *diagnosis*. The puncture, after having at first furnished pure, healthy pus, yielded a sanguinolent pus to compression two days afterwards. It may be laid down as an *absolute rule*, that whenever an abscess at the surface of the body thus comports itself—the fluid taking on a bloody appearance without obvious cause—a second abscess has become developed in the vicinity of the first. Our attention being thus directed to this point, we shall usually be able to discover this second tumour. The wall interposed between the two collections becomes the seat of great turgescence, and of a considerable afflux of blood; and a sanguineous transudation takes place into the interior of the first cavity.—*Rev. Med. Chir.*, xiii. 169.

On Painful Distension of the Vagina after the Birth of the Child. By Dr. Leopold.

Dr. Leopold states that he has several times met with examples (never in primiparæ) of excessive suffering, coming on from half an hour to an hour after the passage of the child, and referred to the vagina. It is of the most agonizing character, described by the women as worse than when the child is passing, and causing them to twist and toss about in agony. It arises from distension of the vagina, either by accumulated coagula, or a very large placenta. In the first case, owing to the quantity of blood lost, there are always the early symptoms of uterine hæmorrhage; and the women, usually having suffered already from hæmorrhage in former labours, are dreadfully frightened at their danger. The softish but not distended uterus is felt pushed up into the umbilical region. The hand should be at once introduced into the vagina, and after the coagula are completely removed, it should be retained there for at least half an hour, keeping two fingers on the watch within the relaxed os uteri, and irritating this if required.

In the other case there is always a large placenta, which will not yield to moderate traction, partly because its size prevents it from easily traversing the vaginal passage, partly from a spasmodic action of the *constrictor cunni*, and partly because the membranes still remain in connexion with the uterus to some extent. Notwithstanding the pain it will cause, the entire hand must be passed into the vagina, so as to embrace the whole placenta and bring it down.—*Neue Zeitschrift für Geburtskinde*, xxxiii. 352.

MATERIA MEDICA.

On Ferruginous Collodion. By M. Aran.

Having observed the utility of the salts of iron in erysipelas, M. Aran, to facilitate their application, combined them with collodion, forming a preparation which united the compressive and astringent effects. It consists of equal parts of collodion and Bestuchef's tincture (ethereal tincture of perchloride of iron). Spread on the skin, it forms a somewhat thinner pellicle than ordinary collodion, but it is much more supple and resisting, so that the limb can be moved in any direction without the cracking which takes place when collodion alone is used. Its adhesion is also more prolonged.—*Bull. de Thérap.*, xliv. 370.

On the Employment of General Frictions in obstinate Chlorosis. By M. Aran.

This disease is almost exclusively treated by iron, only some practitioners even adding to this good diet, exercise, and insolation. By others, iron is regarded as necessary for its cure as is quinine in ague, or mercury in syphilis; while not only is it not indispensable, but in certain cases it entirely fails, exerting no influence upon the affection, and if persevered in, inducing congestion of the viscera, especially of the lungs, which may give rise to hæmoptysis. In a much larger number of cases, ferruginous preparations are well supported at first, and produce marked amelioration; but after a while such amelioration becomes stationary, and however much the dose may be increased, the patient is not prevented from falling back into her former state. Such patients are sometimes seen, although gorged with iron for months or years, still exhibiting all the characters of chlorosis. Dr. Aran has found that by the employment of *dry and stimulant frictions*, aided by good regimen, and in some cases by wine *lavements*, these obstinate cases may be very satisfactorily treated, when iron has failed. Either flannel or a brush may be used, and occasionally a stimulating fluid, such as spirit of camphor, or some ammoniacal preparation, may be added, so as to induce rubefaction. The frictions should be continued for five or ten minutes, every night and morning, being chiefly directed along the back and limbs. In a few days a marked modification of all the functions is produced. The patient becomes more lively and alert, her countenance acquires colour, and appetite, flesh, and strength begin to return, and that although no internal medicines whatever have been employed. In some cases in which progress is not so rapid, vinous enemata are of great service.—*Bull. de Thérapeutique*, tom. xliii. p. 415.

THERAPEUTICAL RECORD.

Aneurism.—M. Raoult-Deslongchamps (L'Union Méd., Avril) relates a case of aneurism (?) of the sub-orbital artery, about the size of a pigeon's egg, which was cured by the injection into the sac of 10 or 12 drops of a concentrated solution of perchloride of iron, as recommended by Pravaz. The sac was opened by a sharp very narrow bistoury, and the point of a fine glass syringe was passed along the instrument into the sac. Considerable inflammation about the swelling followed, and a little purulent discharge, but at the end of a month the aneurism was quite cured. In this case compression was previously unavailingly tried for 25 days.

[We believe that in this case the swelling afterwards returned, and proved to be an erectile tumour. In addition to this case, three others have now been treated in this way—viz., 1, popliteal aneurism, *cured;* 2, varicose aneurism of the brachial artery, from bleeding, *cured, after suppuration and limited gangrene;* 3, aneurism of the brachial after bleeding, *sloughing and amputation.—Révue Med. Chir*, xiii. 275.]

Ascites.—Dr. Falcot (Rev. Thérap. du Midi, and Lancet, May) recommends, in cases of ascites, when the stomach is irritable, fomentations with decoction of digitalis. Two ounces of digitalis are boiled in a quart of water down to a pint, and compresses dipped in the decoction are laid on the abdomen, and covered with oiled silk. The kidneys are soon powerfully affected.

Asthma, Spasmodic.—Dr. Eben Watson (Glasg. Med. Journ., April) remarks, that sufficient attention has not been paid to the glottidean spasm in asthmatic attacks, and relates cases to show the efficacy of the local application of a strong solution of nitrate of silver (℈j to ℥j). At the same time other measures are not discountenanced. Dr. Watson recommends the same treatment in hooping cough.

Bloodletting, Local.—Mr. Struthers (Monthly Journ., April) discusses the effect of local bloodletting on organs not immediately connected with the surface, such as the viscera of the chest and abdomen. After showing that the vessels of the lungs, heart, and abdominal viscera, can only be affected by the influence produced [on] the general circulation, and that local bleeding on the abdominal and thoracic [wall]s can be only regarded *quoad* the contained organs as a general bleeding, he [r]ecommends local bleeding, as the abdominal or thoracic wall *may* also be affected (as in peritonitis, pleurisy, &c.), and then local bloodletting is immediately useful.

Bubo.—Dr. Claiborne (Amer. Journ. of Med. Sc., April) applies *collodion* over a bubo when there is not much local inflammation; the collodion is applied layer after layer, until considerable compression is produced. If there be any amount of inflammation, leeches are previously applied.

Chordee.—M. Doringer (Rev. Méd. Chir., xiii. 240) relates a case of chordee, in which, after trying all the usual internal remedies in vain, when the penis had become relaxed by means of cold, he covered the organ with a thick layer of *collodion*, with the effect of completely preventing the erection. Next day, however, when the collodion was removed, the erection returned.

Croup.—M. Trousseau (Gaz. des Hôp., No. 39) speaks most highly of the employment of sulphate of copper as an emetic in croup, as recommended by Beringnier. The efforts which it induces often detach the false membranes, this emetic seeming to act less on the stomach than the pharynx, while it does not derange the digestive organs, as antimony sometimes does. Vomiting occurs very soon, and is repeated, at very short intervals, three or four times; and in three or four hours the medicine may be again given. M. Beringnier gives from 2 to 3 grains; but M. Trousseau gives as much as 10 grains, divided into two doses. (For tracheotomy in croup, vide p. 271.)

Dropsy.—M. Abeille (Gaz. des Hôp., No. 59), in the present essay, confirms statements he made some years since, of the great utility of large doses of gamboge in dropsy, these acting as diuretics, and often succeeding when purgative doses of the same remedy, and all other means, have failed. There may indeed be relapses, as the drug is only prescribed for the epiphenomenon, which it rapidly disperses: a very great point, as all practitioners well know. It is in dropsy alone that tolerance of these large doses takes place; and when the fluid disappears, the tolerance ceases. The gamboge should be digested in alcohol ($\frac{2}{5}$ths of a grain to ℈j of alcohol) and given in orange-flower water. The patients should be well fed, and the medicine not given within two hours prior to and subsequent to a meal. The quantity at commencing is 6 grains per diem, given in divided doses; and this is to be increased by 2 grains daily.

Dysentery.—M. Delioux (L'Union Méd., Mars) employs in chronic dysentery an enema composed of tincture of iodine ʒij to ʒiij, iodide of potassium 15 to 30 grains, and water ℥vj to ℥viij. An emollient lavement is first administered to clear the intestine, and the iodine is then at once thrown up. Occasionally it causes slight colic, which can be prevented by opiate injection. Of 12 cases mentioned, 10 were cured; 2 were unaffected. A great part of the iodine is absorbed and appears in the urine.

[Eimer has already recommended the use of iodine injections in *acute* dysentery.]

Empyema.—M. Boinet, after taking an historical view of the practice of injecting the pleura, in which he shows, that among the ancient practitioners it was much oftener resorted to than usually supposed (Arch. Gén., N. S. i. 521), refers to the cases in which iodine has been the substance employed. He relates three cases in which the iodine injection was successful, and quotes from M. Aran two others. He takes little note of the precautions for preventing the admission of air, having found it, in his numerous researches on iodine injections, never to act injuriously.

In regard to the operation, although in recent and acute effusion all the fluid should be at once evacuated, to allow the prompt dilatation of the lung, in chronic disease, advantage results from the more gradual evacuation of the fluid, and the gradual dilatation of the long-compressed lung. To this end a gum-elastic tube, much smaller than the canula, should be kept in the opening, so that the pus may drain gradually away by its side. Two or three days after the paracentesis, an emollient injection is thrown in to clear out the cavity, and then the iodine. This should at first be used weak, the injection being composed of 10 parts of tinct. iodine, 1 of iodide of potassium, and 100 of water.

[The French are at present much engaged with this subject of injecting iodine into all the morbid or natural cavities; and although the general report is highly favourable to the practice, we may notice a recent case in which this has been fatal. It occurred in a patient of M. Robert's (Gaz. des Hôp., No. 49), who presented a large abscess, supposed to be dependent upon disease of the spine. After evacuating the pus, an iodine injection was thrown in; but soon after, constitutional irritation was set up, and the man sank prostrate in 36 hours. The only cause of death that could be discovered was the infiltration of the cellular tissue of the thigh with a mixture of the pus and the iodine. M. Robert thinks this might have been prevented, by making a direct, instead of an oblique, aperture into the abscess.]

Erysipelas.—Dr. Balfour (Mon. Jour., May) has treated twenty cases with tincture of sesquichloride of iron, and believes that we have now "a certain and unfailing remedy, whether the erysipelas be infantile or adult, idiopathic or traumatic." The dose is 10 to 20 minims every two hours. A purgative is prefaced and occasionally repeated.

Fever, Typhoid.—An epidemic of typhoid fever has, during several months of the year, prevailed in different parts of France, and especially in Paris. In general, it has not been of a very severe character, although assuming in some patients an adynamic form, with pulmonary complications sufficiently formidable. The mortality has not, upon the whole, exceeded 10 per cent. in Paris. The most varied treatment has been tried with very similar success. The application of cold in every variety of mode has been followed by remarkably good effects. Emetics were very useful when the chest was overloaded with mucosities. Tonics, too, have been well borne, and have always expedited convalescence. One peculiarity observed at the commencement of the cases, was the absence of purging, and even the existence of constipation of several days' duration. As the disease, however, progressed, the purging came on, and became very persistent, even after convalescence was established. It was in these cases, as well as at a less advanced period of the disease, but always when purging was present, that M. Aran (Bull. de Thérap., xliv. 272) found tr. of iodine so highly useful. Under its use the foul tongue rapidly cleaned, the meteorism and pain subsided, and the number of stools at once diminished. The quantity given was 5 drops, from 15 to 30 being taken in the 24 hours.

Fever, Continued.—Dr. Fletcher (Med. Times & Gaz., April) deduces, from an experience of 80 cases, that, in "all cases of fever, with a tendency to bowel complication," the treatment by quinine (as advocated by Dr. Dundas) is extremely useful. "In the majority of the cases, *cinchonism* established a permanent convalescence within 48 hours."

Fracture.—M. Nélaton (Gaz. des Hôp., No. 30) recently called the attention of his class to a deformity of not infrequent occurrence after fracture of the leg. The astragalus and the whole foot, as well as the malleoli, are carried backwards, and the lower extremity of the bones of the leg forwards. If this is unattended to, a vicious setting takes place, and the patient suffers great loss of power in the foot, especially on ascending stairs. As extensive movements are prevented by the contact of the tibia, only short steps can be taken. To prevent this deformity, the foot should be carried forwards and the leg backwards, and this is to be effected by a strong splint placed behind the leg, and supported by the heel, which is securely bound to it. To protect the heel from painful pressure, circles of agaric, having apertures central, may be placed between the splint and the heel, so as to render the pressure diffused and supportable.

M. Maisonneuve (Gaz. des Hôp., No. 23) likewise suggests a contrivance for preventing the upper ends of the bones projecting forwards in compound fracture of the leg. He prevents this by a plantar-splint, which, 28 or 30 centimetres long, is applied along the sole of the foot, so as to project 5 or 6 centimetres beyond the heel. It is kept on by two bandages. The first is carried circularly round the splint to keep it *in situ;* and the other, which is very short, is applied by its middle under the heel, while the two ends cross each other at the upper end of the splint, where they are secured. The foot is thus suspended to the plantar-splint, the lower extremity of which rests on the cushion that supports the rest of the limb. The foot, thus gently suspended in the air, is, with the fragment attached to it, carried forwards, while the weight of the leg carries the upper fragment backwards.

Gonorrhœa.—M. Alquié (Bull. de Thérap., xliv. 277) speaks in the highest terms of the great utility of the tannate of zinc (1 part to 100 of water) injection employed, night and morning, in gonorrhœa, after the acute symptoms have subsided.

Hæmorrhoids.—Dr. Cooke (Med. Times & Gaz., April) relates cases to show the efficacy of strong nitric acid applied to external piles. A dossil of lint is dipped in the acid and pressed upon the pile; for 15 or 20 minutes the pain is usually severe. Six cases are related, in most of which the condition of the parts after the application could not be well ascertained, as the patients rapidly got well. In one case, abscess close to the sphincter followed, but ultimately got well.

Insanity.—M. Michea (Gaz. Méd., 1853, Nos. 10 & 18) has published the results of some trials he has been making of the curative powers of opium and hyoscyamus in insanity. Of 17 persons to whom *opium* was given, 11 have been cured and 3 ameliorated—a mean period of 1½ to 2 months elapsing after treatment was commenced. There were 9 men and 8 women, and of these, 8 men and 3 women were cured. The 3 ameliorations all occurred in women. Of the 3 failures, 2 occurred in women, and 1 in a man. The cases were examples of more or less limited insanity in 12, and of general delirium in 5; while of the 11 cures, 4 occurred in maniacs, and 7 in monomaniacs. Of the 3 who were improved, 2 were monomaniacs, as were all in whom the opium failed. The gummy extract of opium was commenced in doses of 1 grain, or morphia in ⅕th of a grain daily, increasing them by the same quantity daily—occasionally suspending the administration for a few days. The mean quantity requisite for the cure was 113 grains of the extract, or 15½ grains of the morphia.

Among the 10 cases in which *hyoscyamus* was employed, there were 6 cures and 1 amelioration. There were 8 women and 2 men, and both the latter were cured. The dose has varied from 10 to 14 grains daily, and has never exceeded 15½ grains—the entire quantity required having varied from 190 to 250 grains.

Iodoform.—This substance (the analogue of chloroform, but a solid and slightly volatile substance) has been recommended by Righini (Journal de Chem. Méd.) as a disinfectant. It is mixed with starch, and spread upon paper; it slowly escapes, and is said to lessen smells, and to be also useful in phthisis.

Perspirations, Nocturnal.—M. Delioux (L'Union Médicale, Avril) has employed the *tannate of quinine,* in doses of 6 to 8 grains daily, in the sweats of phthisis, and in other diseases attended with diaphoresis. Pure tannin appeared in some cases to be even more powerful than the tannate of quinine.

Pertussis.—Dr. Eben Watson (Glasgow Medical Journal, April) recommends the topical application of solution of *nitrate of silver* in hooping-cough. Combining together his own cases and those mentioned by Joubert (Bulletin de Thérap., Jan. 1852), he finds that by this means, in 125 cases, 62·4 per cent. were cured within a fortnight; 31·2 per cent. were cured in 3 or 4 weeks; and 6·4 per cent. resisted the treatment.

Phthisis.—Bonorden (Schmidt's Jahrbuch, May) has employed, for the last five years, *sulphate of iron* in phthisis, in the following way. He dissolves ʒj in ℥j of water, and gives ♏xx to xxx every 2 hours; the pulse becomes slower, the temperature falls, and the hectic fever lessens; the physical signs improve. If in 10 days no improvement occurs, the strength of the solution is increased to ʒij in ℥j. If any uneasy sensation is felt in the stomach, the medicine is discontinued for a few days.

Prostate, Enlargement of.—M. Vanoye (Gazette Médicale, Feb.) recommends *muriate of ammonia* (in doses of 1, 2, 3, or even 4 drachms in 24 hours, dissolved in a mucilaginous fluid). A miliary eruption sometimes occurs when the remedy is given in such large doses, and indicates its discontinuance.

Rheumatism.—M. Trousseau (L'Union Médicale, Avril) has employed *veratrine* in *acute* articular rheumatism. He gives, on the first day, 1 pill containing $\frac{7}{100}=\frac{1}{14}$ of a grain of veratrine; on the second day he gives 2 pills; on the third, 3; on the fourth, 4; and so on, till he gives 6 or 7 daily. The symptoms are notably improved on the fourth, fifth, or sixth day; 3 or 4 pills are then given daily till complete convalescence; at the least sign of gastric irritation the medicine is discontinued. Five cures are related as evidence.

Rubeola.—Dr. Walz (Schmidt's Jahrbuch, April, p. 166) has employed, after the manner of Schneemann, frictions with fat, in 343 cases of measles, 57 of which were severe; all were cured very speedily. In 30 of these cases the patients were tuberculous, and the progress of the phthisis was arrested.

Sarcina Ventriculi.—Dr. Neale (Med. Times & Gaz., June) records a case of obstinate vomiting, with sarcinæ in the fluid, (urate of ammonia or soda was detected in the vomited matters,) which was much benefited by the *hyposulphite of soda*, in doses of 15 grains three times daily, in infusion of quassia.

Scarlatina.—Dr. Walz (Schmidt's Jahrbuch, April) has employed, after the manner of Schneemann, frictions with fat in 74 patients with scarlatina: all were cured. In 69 cases there was no desquamation; in 4 cases there was secondary dropsy, which was easily cured in one case by diaphoretics, in three by sulphur. The same treatment has been employed in measles.

Spermatorrhœa.—M. Lucien Corvisart (L'Union Méd., Avril) records 3 cases of extreme spermatorrhœa greatly benefited by *digitalis*. The effect of the remedy was first accidentally observed in a patient with spermatorrhœa, to whom digitalis was given for palpitation of the heart. The dose is not mentioned. In no case does there seem to have been complete cure.

Stomatitis Materna.—Dr. Byford (Amer. Journ. of Med. Sc., April) describes stomatitis as occurring in mothers who are suckling, and recommends immediate cessation of the lactation, and the administration of nourishing diet and strengthening medicines, especially cod-liver oil and the carbonate of potash. Local remedies are considered to be merely palliative: solutions of alum, borax, and sulphate of zinc or copper, are mentioned.

Tonsillitis.—In ulcerated sore throat, with much exudation (diphtheritis), Mr. Blyth (Med. Times and Gaz., April) recommends the internal administration of *chlorine* conjoined with quinine. He puts into a pint bottle eight grains of chlorate of potash, and one drachm of hydrochloric acid; a violent action ensues, during which time the bottle is corked. An ounce of water is then poured in, and the bottle is well shaken until the chlorine is partly absorbed; another ounce of water is then poured in, and so on till the bottle is filled. A weak solution of chlorine is thus prepared, of which, to a child, two teaspoonfuls, with a quarter of a grain of quinine, may be given every two or three hours. In scarlatina anginosa and maligna the same treatment is useful.

Dr. Hamilton Roe (Lancet, March) had previously recommended a similar preparation of chlorine in scarlatina.

Ulcers.—Mr. Gay (Med. Times and Gaz., April), in a case of old callous ulcer on the leg, which had not been treated for twenty years, noticing that the extreme tension of the sound skin round the ulcer appeared to prevent its cicatrization, made a longitudinal incision, three inches in length, through the tense sound skin, about two inches from the ulcer. The ulcer and new wound both healed-up readily.

Uterus.—M. Faure (Arch. Gén., i. 551) believes that vaginal injections of water have never been properly estimated, the practitioner regarding the ingredients they hold in solution as the only active agents. After long trying the usual heroic remedies in affections of the cervix uteri, he has now discovered that cold water is a far more powerful therapeutical agent, both as regards the rapidity of the cure and the less tendency to relapse. If the patients prefer it, the injection may at first be tepid. Two or three irrigations of twenty minutes daily may in some cases suffice, while in others a more prolonged or oftener repeated application is required. A small pump, on the principle of the garden-syringe, is employed, the orifice by which the water escapes being two centimetres in diameter. The woman can manage it herself, while in the sitting posture, passing the pipe well into the vagina, and propelling the fluid, with considerable force, against the engorged parts.

Uterus: Ulcers of Os.—M. Aran observes (Bull. de Thérap., xliv. 34) that ulcerations of the cervix uteri being almost always considered as the result of chronic inflammation, they are combated by antiphlogistics and caustics. Although in a great number of cases these means will succeed, in others the ulcerations continue open for months. They may, in fact, arise from different occasional causes, one of the most frequent of which is friction of the cervix against the vagina when inclined forwards, and especially backwards. It is in such cases that Recamier recommends the daily interposition, between the cervix and vagina, of small pledgets of charpie powdered with starch. It is a good plan, but it is inferior to that now adopted by M. Aran, which consists in carefully smearing the cervix every third or fourth day with collodion, under the protection of which the ulcers soon heal.

Warts.—Dr. Peez, of Wiesbaden (Rev. Méd., 1853, p. 502) confirms the statement, made by several German practitioners, of the rapid curative agency which attends the internal use of carbonate of magnesia in cases of warts.

BOOKS RECEIVED FOR REVIEW.

On Lithotrity and Lithotomy. By William Coulson, Surgeon to St. Mary's Hospital. London, 1853. 8vo, pp. 388.

A Text-book of Physiology. By Dr. G. Valentin. Translated from the Third German Edition by William Brinton, M.D., Medical Tutor in King's College. Part I. 1853. 8vo, pp. 320.

Inflammation of the Breast, and Milk Abscess. By Thomas W. Nunn, Surgeon to the Western Dispensary. London, 1853. fcp. 8vo, pp. 52.

The Right of Marischal College and University, Aberdeen, to confer Degrees in Divinity, Law, and Medicine, &c. By one of the Professors. 1853. pp. 55.

Homœopathy, its Tenets and Tendencies, &c. By James Y. Simpson, M.D., F.R.S.E., Professor of Midwifery, University of Edinburgh. Third Edition. Edinburgh, 1853. 8vo, pp. 292.

The Treatment of Obstinate Ulcers and Cutaneous Eruptions on the Leg. By Henry T. Chapman, F.R.C.S. Second Edition. London, 1853. 8vo, pp. 156.

Coup d'Œil sur la Chirurgie Anglaise—Des Hernies Crurales. Par M. Le Dr. Deville. Paris, 1853. 8vo, pp. 98.

On Prophylactic and Curative Syphilization. By Victor De Méric, M.R.C.S. London. Fcp. 8vo, pp. 24.

Six Lectures on the Non-Metallic Elements. By Professor Faraday. Arranged, by permission, from the Lecturer's Notes, by J. Scoffern, M.B. London, 1853. 8vo, pp. 293.

The Theory of Menstruation in Early Pregnancy, Superfœtation, and the Site of Insertion of the Ovum. By J. Matthews Duncan, A.M., M.D. (From the Monthly Journal of Medical Science for April, 1853.)

The Medication of the Larynx and Trachea. By J. Scott Alison, M.D. London, 1853. 8vo, pp. 49.

The Modern Advancement of Practical Medicine and Surgery; an Inaugural Address to the Medico-Chirurgical Society of Edinburgh. By James Y. Simpson, M.D. (From the Monthly Journal of Medical Science.) Edinburgh. pp. 18.

On Continuous Molecular Changes, more particularly in their relation to Epidemic Diseases. By John Snow, M.D. London. 8vo, pp. 38.

A Pathological and Practical Treatise on Epidemic Cholera. By Dr. B. Malony, L.F.P. & S. London, 1853. 8vo, pp. 190.

On a New Method of Managing Fractures. By James Tony Hester, F.R.C.S. London, 8vo, pp. 11.

A Clinical Phrase-book in English and German, containing the usual Questions and Answers employed in Examining and Prescribing for Patients, &c. By Montgomery Johns, M.D. Philadelphia, 1853. Small 8vo, pp. 308.

The Sophistry of Empiricism. London, 1853. 8vo, pp. 84.

The Opium Trade as carried on in India and China. By Nathan Allen, M.D. Lowell (U.S.), 1853. 8vo, pp. 80.

The Transactions of the American Medical Association. Vol. V. Philadelphia, 1852. pp. 939.

Anatomical and Physiological Bibliography, from 1849 to 1852 inclusive. By G. E. Day, M.D. 8vo, pp. 156.

On the Application and Effect of Electricity and Galvanism in the Treatment of Cancerous, Nervous, Rheumatic, and other Affections. By Richard Moore Lawrence, M.A., M.D. London, 1853. pp. 101.

Change of Climate, considered as a Remedy in Dyspeptic, Pulmonary, and other Chronic Affections, &c. By D. J. T. Francis, M.D. Lond. London, 1853. pp. 339.

The Harmonies of Physical Science in relation to the Higher Sentiments, &c. By William Hinds, M D. London, 1853. 8vo, pp. 195.

On the Pathology and Treatment of Acute Rheumatism; being the Lumleian Lectures delivered before the Royal College of Physicians in 1853. By James Alderson, M.D., F.R.S., Senior Physician to St. Mary's Hospital. London, 1853. 8vo, pp. 107.

The Progress of Improvement in the Treatment of Consumption, and other Pulmonary and Laryngeal Diseases, and on some New Remedial Means. By James Turnbull, M.D., Physician to the Liverpool Royal Infirmary, &c. London, 1853. 8vo, pp. 74.

On the Use of an Artificial Membrana Tympani in cases of Deafness, dependent upon Perforation or Destruction of the Natural Organ; to which is added a Paper, entitled, Ought the Tonsils or Uvuli to be Excised in the Treatment of Deafness? By Joseph Toynbee, F.R.S., Aural Surgeon to St. Mary's Hospital, &c. London, 1853. 8vo, pp. 46.

Monographie des Eaux Minerales de Wiesbaden. Par M. Charles Braun, M.D. Wiesbaden, 1 et 2 Cahier, 1853. 8vo, pp. 105 & 117.

Ueber Geburtshülfe und Gynœkologie in Frankreich, Gross Britannica und Irland. Von Dr. F. H. Arneth. Wien, 1853. 8vo, pp. 356.

Esperienze Chimiche su Fluidi di Persone che usarono internamente Preparazioni d' Iodio; Memoria del Dott Giacinto Namias. Venezia, 1852. pp. 14.

An Apology for British and Colonial Medical Degrees, &c. By A. Hall, M.D. Edin. Montreal, 1853. pp. 18.

Observations on the Nature and the Treatment of the Asiatic Cholera. By William Stevens, M.D., D.C.L. Oxon. London, 1853. 8vo, pp. 475.

On the Nature and Treatment of the Deformities of the Human Frame, &c. By W. J. Little, M.D. London, 1853. 8vo, pp. 412.

Beiträge zur vergleichenden pathologischen Anatomie der Gelenkhrankheiten. Von Dr. E. Gurlt. Berlin, 1853. pp. 656.

Handbuch der rationellen Pathologie. Von Dr. J. Henle, Professor der Anat. in Göttingen. Zeiter Band. Fünfte Lieferung. (Schluss des Werkes.) Braunschweig, 1853. pp. 493.

A Treatise on General Pathology. By Dr. Henle, translated from the German, by H. C. Preston, A.M., M D. Philadelphia, 1853. pp. 391.

A Treatise on Diseases of the Heart. By O'B. Bellingham, M.D., F.R.C.S. Dublin, 1853. 8vo, pp. 252.

The Present State and Prospects of Psychological Medicine, with Suggestions for Improving the Laws relating to the Cure and Treatment of Lunatics. By Joseph Seaton, M.D. London, 1853. pp. 23.

On the Nature and Proximate Cause of Insanity. By James George Davey, M.D. London, 1853. 8vo, pp. 76.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By James Paget, F.R.S., lately Professor of Anatomy and Surgery to the College, &c. 1853. 8vo. Vol. ii. pp. 499—637.

The Thistle and the Cedar of Lebanon. By Habub Risk Allah Effendi, M.R.C.S., and Associate of King's College. London. 8vo, pp. 380.

Chemistry of the Four Seasons, &c. By Thomas Griffiths, late Professor of Chemistry in the Medical College of St. Bartholomew's, &c. London, 1853. 8vo, pp. 436.

Small-pox and Vaccination: copy of a Letter from Dr. Edward Seaton to Lord Palmerston, with enclosed copy of a Report on the State of Small-pox and Vaccination in England and Wales, and on Compulsory Vaccination, &c., presented to the Epidemiological Society by the Small-pox and Vaccination Committee. Ordered by the House of Commons to be printed. (Parliamentary Paper.)

Records of Maculated Typhus, or Ship Fever. (With Plates.) By J. B. Upham, M.D. New York, 1852. 8vo, pp. 60.

Plates of the Brain, in Explanation of the Physical Faculties of the Nervous System. By Joseph Swan. London, 1853. 4to, pp. 67.

Erratum.—In the January number (xxi.), in noticing the paper of Dr. Siegmund, on the 'Dumb-bell Crystals of Carbonate of Lime,' the word "guinea-pig" is by mistake put for "rabbit."

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1853.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *De Partibus Similaribus.* By G. FALLOPIUS. (Date uncertain, before 1562.)
2. *Theoria Generationis.* By CASPAR FRIEDRICH WOLFF. 1759.
3. *Theorie von der Generation.* By C. F. WOLFF. 1764.
4. *Theoria Generationis.* By C. F. WOLFF. Ed. Nova. 1774.
5. *Von der eigenthümlichen und wesentlichen Kraft der Vegetabilischen sowohl als auch der animalischen Substanz.* By C. F. WOLFF. 1789.
6. *Entwickelungs Geschichte d. Thiere.* By K. E. VON BAER. 1828.
7. *Untersuchungen über Phytogenesis.* By SCHLEIDEN. 1837.
8. *Mikroskopische Untersuchungen.* By SCHWANN. 1838–9.
9. *Ueber das Bindegewebe.* By REICHERT. 1845.
10. *Die Vegetabilische Zelle.* By H. VON MOHL. 1850.

The Vegetable Cell. By H. VON MOHL. Translated by A. HENFREY, F.R.S. 1852.

11. *On the Mutual Relation of the Vital and Physical Forces.* By W. B. CARPENTER, M.D., F.R.S., &c. ('Phil. Trans.') 1850.
12. *Handbuch der Gewebelehre.* By A. KÖLLIKER. 1852.

IF we separate the elementary and essential facts of Life from all the various and complicated phenomena with which they are associated among the higher forms of living beings—if we examine those lowest and most rudimentary states of animal and vegetable existence, which are presented to us by the so-called 'unicellular' organisms, we find that the sole definable difference between a living thing and a mere formed morsel of some protein compound, fresh from the laboratory of the chemist, is, that while the protein compound undergoes no change which may not be traced to the immediate and direct operation of some new or varying external condition, the Navicula or the Gregarina passes through the most remarkable successions of form, of size, and of chemical composition, which are equally definite in their nature, and equally certain in the order of their occurrence, whatever, within certain limits, be the change in external conditions, or whatever pains may be taken to prevent any variation in them.

Broadly, we may thus state the difference between the subjects of Physical and those of Biological Science: the former, the stone, the gas, and the crystal, have an *inertia;* they tend to remain as they are, unless some external influence affect them. The latter, animals and plants, on the other hand, are essentially characterized by the very opposite tendencies. As Reichert well expresses it:

"All organic bodies, therefore, represent, in relation to one another, different and manifold states succeeding one another definitely upon a similar and homogeneous foundation; they form a common differential series, in which, *independently of external conditions,* a continual increase in the mutual differences and a diminution of the resemblances, occur." (p. 12.)

Linnæus seems to have wished to express his insight into this difference between living and dead matter, in his celebrated aphorism: "Stones grow; plants grow and live;" but so long as this "and live" was not analyzed into its true meaning, the phrase marked the difference, but failed to define it.

Bichât recognised the independence of action of living beings in another way. All things which surround living beings tend, he says, to destroy them; but they nevertheless follow out their own appointed course. "La vie est l'ensemble des fonctions qui resistent à la mort."*

Now, this faculty of pursuing their own course, this inherent law of change, introduces, it will be observed, an element into the study of living beings, which has no analogue in the world of ordinary matter. The latter frequently possesses structure, and may therefore be the legitimate subject of anatomy; but it undergoes no definite cyclical alterations;† and, therefore, it offers nothing which corresponds with what in living beings is

* It is amusing to find M. Comte, a mere bookman in these subjects, devoting a long argument (Philosophie Positive, tom. iii. p. 288) to a refutation (?) of what he calls the "profonde irrationalité" of Bichât's definition. As a specimen of the said refutation, we may select the following passage: "Si comme le supposait Bichât, tout ce qui entoure les corps vivans tendait réellement à les detruire, leur existence serait par cela même radicalement inintelligible; car, où pourraient-ils puiser la force nécessaire pour surmonter même temporairement un tel obstacle?" What a question for a positive philosopher! Does M. Comte doubt his own power to get up from his easy chair, because it is unquestionably true that the action of the whole globe "tends" to retain him in his sitting posture, and because he cannot tell whence he gets the force which enables him to rise?

† It is not easy to frame a definition of the differences between living and not living bodies which shall perfectly defy cavil. That in the text—based on the inertia of not living bodies, the internal activity of living bodies—marks the difference strongly but not unobjectionably, for it might be said that a nebula undergoing change would, by this definition, be a living body, and in the next place, it might be urged, how do we know that the activity of living bodies is not really the result of some external cause with which we are unacquainted? It might be said, that the apparent absence of change in external conditions, is no more evidence that the vital phenomena are independent of some such causes, than the continuous running of a stream when the dam is opened, independent of any further alteration of external conditions, is evidence of spontaneity. The action of the spermatozoon, e.g., might be compared to the raising of the dam. We have preferred above, however, a vivid to an exact definition of our conception of life, as likely to be more useful. If we were to attempt an exact definition, it would be, that a living being is a natural body which presents phenomena of growth, of change of form, and of chemical composition, of a definite nature, and occurring in definite cycles of succession. This definition will separate living beings from all other terrestrial bodies. It separates them from cosmical bodies (nebulæ, &c.) only by the nature of the phenomena which succeed one another; so true is it that the microcosm and the macrocosm are reflections of one another.

called the History of Development, that branch of the investigation of structure which does not concern itself with the mere study of one state of a being—like anatomy—but examines into the manner in which the successive anatomical states are related to, and proceed out of, one another.

A profound physiologist and thinker, a contemporary and worthy rival of Haller, has beautifully expressed the relation of anatomy, of physiology, and of development (which he calls Generation), in the following words:

"The relations between anatomy, the doctrine of generation, and physiology, are about these. By anatomy we learn from observation the composition and structure of an organized body. We, however, are unable to explain this composition and structure; we only know that they are thus, and further than this we know nothing. But now, on the one hand, comes the doctrine of generation, in which that which we know from anatomy historically, is traced to its causes; on the other hand, we have physiology, in which the actions which an organized body is capable of producing are explained. Physiology is related to anatomy exactly as the corollary to the theorem from which it is deduced; my theory (of generation) is related to anatomy as its demonstration to the theorem."*

And again we find the relation of development to anatomy admirably and epigrammatically expressed in the 'Theoria Generationis' of the same writer. Development is, he says, "anatomia rationalis."

It has been said, and without doubt with profound truth, that the study of the structure of living beings originated in the wonder excited by their actions. But though this may, nay must, have been the case at first, and though the curiosity of man has for three centuries past directed itself, with almost equal impartiality, to physiological, anatomical, and developmental inquiries, still it is clear, that if the above account of the correlation of these branches of science be correct, their logical connexion, and the order, therefore, in which they must eventually arrive at perfection, is precisely the reverse. The striking and mysterious character of many of the functions may have led to the study of structure; but assuredly the understanding of the former presupposes a thorough knowledge of the latter.

It is conceivable that structure might be thoroughly made out without the least acquaintance with function, just as the ancient anatomists were well acquainted with the construction of the muscular system, and yet had no suspicion of its being the motor apparatus, and as at the present day we know full well the structure of the "vascular glands," though we can but guess their purpose; but it is quite impossible to attain to a complete knowledge of function without a thorough anatomical analysis. The action of the whole of any organ depends upon and is, that of the sum of its parts; it is, mechanically speaking, their resultant; so that until the nature and the precise modes of operation of all these parts have been made out, we can have no security that any law propounded concerning the functions of the whole, is other than a mere empirical generalization, liable to be interfered with at any moment, by the properties of some of the elementary parts with which we are unacquainted. Thus, up to within a few years ago, contractility was affirmed to be a general property of the cellular tissue of the skin; and this would have remained as an ascertained law, had not Kölliker shown, by the discovery of the extensive distribution of

* Wolff: Theorie von der Generation, p. 12.

muscular fibre in it (that is, by pushing anatomical analysis a step further than it had previously been carried), that the supposed law was but an empirical generalization, and that the property of contractility, supposed to be inherent in the ordinary connective tissue of the skin, was, in fact, deducible from the presence of a totally different structure.

So again, Haller and his followers quoted the contraction of the heart, when removed from the body, as evidence of the innate contractility of muscle, apart from all nervous influence. This *vis insita* may exist or it may not, but further anatomical investigation has at any rate destroyed the force of the argument, by demonstrating the existence of nervous ganglia within the substance of the organ.

But enough of illustration of what must be sufficiently plain to any one who will reflect upon the subject; namely, that however much might be done towards the establishment of broad physiological truths, while the knowledge of structure was in a rough and imperfect state, still an exhaustive study of structure is absolutely necessary, before any successful attempt can be made to establish the true laws of function, or to build the science of physiology upon an exact foundation.

Herein lies, consciously or unconsciously to their authors—for the man of genius is such, in virtue of having true and just tendencies and impulses, of which he often can give himself no logical account—the secret of the repeated attempts which have been made from the time of the very fathers of biology, to found what we now call the doctrine of general anatomy or histology, which is, in other words, the exhaustive anatomical analysis of organized bodies. That animals and plants, complex as they may appear, are yet composed of comparatively few elementary parts, frequently repeated, had been noticed by the profound intellect of Aristotle; and Fallopius tells us, that Galen had attained to still more clear and definite conceptions, with regard to these "partes similares" or "simplices:"*

"Galenus per simplices partes eas intelligit quæ non constant ex dissimilibus substantiis, in quas resolvitur corpus humanum, nec ultra datur progressio et istæ partes dicuntur simplices quia cum ad hoc ventum fuerit in resolutione corporis humani, non amplius progredi possumus." (p. 103.)

Such, indeed, must be the definition of elementary parts at all periods of science—they are ultimate, because we can go no further; though it is of course a very different matter whether we are stopped by the imperfection of our instruments of analysis, as these older observers were, or by having really arrived at parts no longer analyzable.

The celebrated professor of Modena, whose words we have just cited, was one of the first of those who carried the light shed by the revival of letters into the region of medicine and its allied sciences; and his work 'De Partibus Similaribus,' from which they are taken, must excite the admiration of every modern reader, not merely by the critical acumen and original genius which it displays, but by the scientific and absolutely accurate manner, in which the whole subject of general anatomy is handled.

The classes of "partes similares," or tissues, of which he treats, are bone, cartilage, fat, flesh, nerve, ligament, tendon, membrane, vein, artery, nails, hairs, and skin; and he examines and details under each head the minute structure, so far as it was accessible to his means of investigation; the

* Terms by no means always convertible, but which may for the present be taken to be so.

chemical and physical properties (expressed, of course, in the language of the day), and even the peculiarities manifested by the diseased state. Nor is he at all wanting in what has been considered, and justly, to be Bichât's great merit—an essentially *positive* method of studying the tissues, inasmuch as he particularly insists on the necessity of investigating the properties of each tissue for itself, and of avoiding all hypothetical speculation; in fact, with the quaint plainness of the age, he does not hesitate to insinuate, that Averrhöes must have been "ebrius" when he discoursed touching "spiritus qui insensibiles sunt."

The vitality of each tissue, independently of every influence save the general conditions of nutrition, is maintained by Fallopius, not as a mere speculation, but on sound embryological grounds. How can the liver, he asks, be the sole source and prime mover of all vital organization, as some have maintained, when, in the development of the chick, we see other organs appear before it? All that the liver and the vessels can do, is, to modify the supplies, by affecting the "restitutiones spirituum ac nutrimentis" (p. 98), the "partes similares" themselves having a "regimen insitum," or, as in our day it would be called, "vital force" of their own; and he quotes, as expressing his own views, the following remarkable passage from Actuarius:

> "Quod partes naturales agunt propriâ formâ ac cum instrumento quod dicitur spiritus animalis: nam hoc instrumento per propriam formam attrahunt, concoquunt et expellunt, et hic spiritus est immediatum instrumentum vis naturalis, et hic spiritus dicit Actuarius, *originem ducit unâ cum formâ ipsius particulæ, et ex eâdem materiâ eodemque tempore fit.*"

Substitute here for the indefinite "particulæ" definite vesicular particles or cells, and for "spiritus animalis" the modern terms of equivalent meaning or no meaning—vital-force or cell-force, and this passage would serve very well as a concise expression of the "cell-theory," such as may be found in many a hand-book of the day. So far, and no further, have three centuries brought us!

In fact, it must be confessed, that these old writers were fully possessed (more so, in truth, than many of their successors) with the two fundamental notions of structural and physiological biology; the first, that living beings may be resolved anatomically, into a comparatively small number of simple structural elements; the second, that these elementary parts possess vital properties, which depend for their manifestation only upon the existence of certain general conditions (supply of proper nutriment, &c.), and are independent of all direct influence from other parts.

But it would seem, that Truth must pass through more than one Avatar, before she can attain a firm hold upon the mind even of men of science—and at the end of the eighteenth century it required all the genius of Bichât to sift the wheat from the chaff, amongst the great mass of facts which the observation of the past ages had accumulated—and strengthening whatever place was weakest by new investigations—to establish these very two propositions, upon a broad and henceforward firm foundation. Great as was the service which Bichât rendered in this way to biology—and wide as the difference between the treatise 'De Partibus Similaribus' and the 'Anatomie Generale' may be—still the one is the intellectual progeny of the other, and exhibits neither alteration nor improvement in the method pursued.

In the meanwhile, however, an aid to investigation had arisen, by the means of which this method could be pushed to its uttermost limits—we refer to the invention of the microscope. The influence of this mighty instrument of research upon biology, can only be compared to that of the galvanic battery, in the hands of Davy, upon chemistry. It has enabled *proximate* analysis to become *ultimate*. Without the microscope the ultimate histological elements were, as we have seen, defined *negatively*, as parts in which any further structural difference was too small to be detected. The microscope, on the other hand, enables us to define the tissues *positively*—to say, a given tissue has such a structure, and magnify it as you will, it will present no further differences.

The amount of such positive information as to the ultimate structure of the tissues, collected by Leuwenhoek, Malpighi, and their successors, between the middle of the seventeenth and the fourth decade of the present century, was very great, and in fact the most important and characteristic features presented by the histological element of plants and animals may be said to have been well made out, at the time of the appearance of the celebrated treatises by Schleiden and Schwann, cited at the head of this article; and these writers, therefore, added but little to the body of knowledge in this direction. It is most unquestionable, however, that the biological sciences, and more especially histology, received a wonderful stimulus at their hands. Whatever cavillers may say, it is certain that histology before 1838 and histology since then, are two different sciences—in scope, in purpose, and in dignity—and the eminent men to whom we allude, may safely answer all detraction by a proud "*circumspice.*"

But wherein does the real value of their work lie? We think this question may be readily enough answered by those who admit the force of what has been said in our opening paragraphs—who acknowledge that mere anatomy does not exhaust the structure of living beings—and that before histology can be said to be complete, we must have a histological *development*, as well as a histological *anatomy*. Leuwenhoek and the majority of his successors had enough to do in making out the "*historicam cognitionem*," the simple anatomy of the tissues; it tasked all their powers to arrive at a clear statement of the "*theorem*," while it is the great merit of Schleiden and Schwann, that they sought to arrive at an "*anatomia rationalis*," and to furnish the "demonstration of the theorem." The old method of investigation had been carried as far as it would go, and they applied the only other which remained, and made it familiar to the general mind. Turn to any of Schleiden's works, and we find the logical acuteness, and the vituperative sarcasm, which he wields with equal force, employed in urging the study of development as the one thing needful for scientific botany. And Schwann's entire essay testifies to what he expressly tells the reader, that his investigations are distinguished from all others by being based upon the study of development. Let one citation suffice:—"The theory of the present investigation was, therefore, to show . . . . that there exists a common principle of development for all the elementary parts of the organism." (Schwann, pp. 193—196.)

Intending as we do to venture upon a critical examination of the absolute value of Schleiden's and Schwann's contributions to biological science, which may lead us to conclusions not ordinarily admitted, we have been particularly desirous to estimate fairly the position which they occupy in its history, and the influence which their labours have had upon its progress—which is a widely different matter—for, in attempting to weigh the labours of others, we should be in danger of committing great injustice, if we did not carefully bear in mind, that, paradoxical as it may seem, the value of a theory and its truth, are by no means commensurate. In so complex a science as that which relates to living beings, accurate and diligent empirical observation, though the best of things as far as it goes, will not take us very far; and the mere accumulation of facts without generalization and classification is as great an error intellectually, as, hygienically, would be the attempt to strengthen by accumulating nourishment without due attention to the primæ viæ, the result in each case being chiefly giddiness and confusion in the head.

In biology, as in all the more complicated branches of inquiry, progress can only be made by a careful combination of the deductive method with the inductive, and by bringing the powerful aid of the imagination, kept of course in due and rigid subordination, to assist the faculties of observation and reasoning; and there are periods in the history of every science when a false hypothesis is not only better than none at all, but is a necessary forerunner of, and preparation for, the true one. As Schwann himself well expresses it:

> "An hypothesis is never hurtful, so long as one bears in mind the amount of its probability, and the grounds upon which it is formed. It is not only advantageous, but necessary to science, that when a certain cycle of phenomena have been ascertained by observation, some provisional explanation should be devised as closely as possible in accordance with them; even though there be a risk of upsetting this explanation by further investigation; for it is only in this way that one can rationally be led to new discoveries, which may either confirm or refute it." (p. 221.)

The value of an hypothesis may in fact be said to be twofold—to the original investigator, its worth consists more in what it suggests than in what it teaches; let it be enunciated with perspicuity, so that its logical consequences may be clearly deduced, and made the base of definite questions to nature—questions to which she must answer yes or no—and of its absolute truth or falsehood, he recks little: for the mass of men, again, who can afford no time for original research, and for the worker himself, so far as respects subjects with which he is not immediately occupied, some system of artificial memory is absolutely necessary. This want is supplied by some "appropriate conception" which, as Dr. Whewell would say, "colligates" the facts—ties them up in bundles ready to hand—by some hypothesis, in short. Doubtless the truer a theory is,—the more "appropriate" the colligating conception,—the better will it serve its mnemonic purpose, but its absolute truth is neither necessary to its usefulness, nor indeed in any way cognizable by the human faculties. Now it appears to us that Schwann and Schleiden have performed precisely this service to the biological sciences. At a time when the researches of innumerable guideless investigators, called into existence by the tempting facilities offered by the improvement of microscopes, threatened to swamp science in minutiæ, and to render the noble calling of the physiologist identical with that of the 'putter-up' of preparations, they stepped forward with the cell-theory as a colligation of the facts. To the investigator, they afforded a clear basis and starting-point for his inquiries; for the student, they grouped together immense masses of details in a clear and perspicuous manner. Let us not be ungrateful for what they brought. If not absolutely true, it was the truest thing that had been done in biology for half a century.

But who seeks for absolute truth? Flattering as they were to our vanity, we fear it must be confessed that the days of the high *à priori* road are over. Men of science have given up the attempt to soar eagle-like to some point amidst the clouds, whence the absolute relations of things could be securely viewed; and at present, their more useful, if more ignoble course, may rather be compared to that of the flocks of sparrows in autumn, which one sees continually halting, yet always advancing—flying from tree to tree, noisily jubilating in each, as if that were assuredly the final resting-place and secure haven of sparrows, and yet as certainly taking their departure after awhile, in search of new acquisitions. We must build our theories, in these days, as we do our houses: giving up all attempt at Cyclopean architecture, let us bethink ourselves rather of the convenience of our successors, who will assuredly alter, and perhaps pull them down, to suit the needs of their own age; and if we seek their gratitude, let us strive not so much to knit our materials firmly together (which will only give them more trouble and yield us less thanks), as to see that they are separately sound and convertible. This much digression has seemed necessary, by way of securing ourselves from any suspicion of a desire to under-estimate the historical value of Schleiden and Schwann's researches, in the course of an attempt to show that they are based upon errors in anatomy, and lead to errors in physiology.

Again, with regard to that value, we have a few words to say in a merely historical point of view. The sketch we have given of the progress of general anatomy, we believe, omits mention of no ordinarily recognised epoch, nor fails to indicate the acknowledged order of the successive introduction of those great leading ideas with which we are at present concerned.* In their own belief, and in that of their contemporaries, Schleiden and Schwann have not only worked out developmental histology, but originated it; and the latter, in his reclamation against Valentin (loc. cit., pp. 260, 261), defends his claim to be considered the originator of the idea that "a common principle governs the development of the elementary parts of all organisms." Now, we fully recognise the originality of these writers; we believe that they deserve all the credit which can attach to a noble plan carried out with no small success; and we further remember that the majority always sympathizes with the cry, "Pereant qui ante nos, nostra," &c.; but, as we have said, truth often has more than one Avatar, and whatever the forgetfulness of men, history should be just, and not allow those who had the misfortune to be before their time, to pass for that reason into oblivion.

Such was the position into which his great genius forced Caspar Friedrich Wolff—such the fate with which he has met. The manuals of physiology tell us that he was the founder of the doctrine of epigenesis—

* Compare Kölliker's Handbuch, Introduction; or Sprengel's Geschichte der Arzneykunde.

a doctrine which, in the present day, seems so plain and obvious, that we do not give him much credit for it, forgetting that he had to struggle against the authority of Malpighi and of Haller, and the attacks of Bonnet; influence and authority so great, that though every reader of the 'Theoria Generationis' must see that Wolff triumphantly establishes his position, yet, seventy years afterwards, we find even Cuvier* still accrediting the doctrine of his opponents.

It is less generally (we might say hardly at all) known that Wolff demonstrated, by numerous observations on development, the doctrine of the metamorphosis of plants, when Göthe, to whom it is commonly ascribed, was not quite ten years old;† but it seems to have been wholly forgotten that he endeavoured to work out, upon the basis of the strict study of histological and morphological development, that "identity of structure of plants and animals" which is the thesis defended by Schwann. Had Wolff's teaching been founded upon one of those clever guesses upon which an able man will often build up a plausible hypothesis, we should have thought it quite unnecessary to make even historical reference to him; but the most cursory examination of the 'Theoria Generationis,' or of the more popular and discursive exposition of his views in the 'Theorie von d. Generation,' is enough to dispel any such notion. The passage we have already quoted is sufficient to show how just and accurate Wolff's ideas upon the importance of the study of development, as a method, were; and the whole of his work is the laborious application of that method. The parts of the calyx, of the corolla, and of the pericarp, are for him "modified leaves;" not because certain observed modifications had suggested that they might be so considered—which is the whole gist of Göthe's subsequent argument—but because he had carefully traced back their development, and had found that they all proceeded from the same original form. The homology of the wing of the chick with its leg is placed by Wolff on precisely the same basis—the only one, be it observed, on which any homology can ultimately rest; and following out the argument to its legitimate conclusion, he shows that the appendicular organs of plants and animals are developed after the same fashion. The limbs of animals, he says, are developed in the same manner from the body of the embryo, as the leaf from the stem, or the lamina of the leaf from its mid-rib. Ordinary four-footed animals are like pinnatifid leaves, while "the bat is a perfect leaf—a startling statement, but, as I have shown, the analogy is not chimerical, for the *mode of origin of the two is the same.*"‡

Wolff's doctrine concerning histological development is shortly this.§ Every organ, he says, is composed at first of a little mass of clear, viscous, nutritive fluid, which possesses no organization of any kind, but is at most composed of globules. In this semi-fluid mass, cavities (*Bläschen, Zellen*) are now developed; these, if they remain rounded or polygonal, become the subsequent cells—if they elongate, the vessels; and the process

* Histoire des Sciences Naturelles.

† The world, always too happy to join in toadying the rich, and taking away the "one ewe lamb" from the poor, persists in ascribing the theory of the metamorphosis of plants to Göthe, in spite of the great poet himself (see Göthe's Werke, Cotta, 1840, B. 36, p. 105: "Entdeckung eines trefflichen Vorarbeiters"), who not only acknowledges his own obligations to Wolff, but speaks with just wonder and admiration of the 'Theoria Generationis,' the work of a young man of six-and-twenty.

‡ Theorie von der Generation, § 64.

§ Theoria Generationis, and Von der eigenthümlichen Kraft, p. 48.

is identically the same, whether it is examined in the vegetating point of a plant, or in the young budding organs of an animal. Both cells and vessels may subsequently be thickened, by deposits from the "solidescible" nutritive fluid. In the plant, the cells at first communicate, but subsequently become separated from one another; in the animal, they always remain in communication. In each case, they are mere cavities, and not independent entities; organization is not effected by them, but they are the visible results of the action of the organizing power inherent in the living mass, or what Wolff calls the *vis essentialis*. For him, however, this "vis essentialis" is no mythical *archæus*, but simply a convenient name for two facts which he takes a great deal of trouble to demonstrate; the first, the existence in living tissues (before any passages are developed in them) of currents of the nutritious fluid determined to particular parts by some power which is independent of all external influence; and the second, the peculiar changes of form and composition, which take place in the same manner.

Now there is really no very great difference between these views of the mode of development of the tissues, and those of Schleiden and Schwann. The "solidescible nutritive fluid" of Wolff is the "cytoblastema" of Schleiden and Schwann; with the exception of the supposed relation of the nucleus to the development of the cell (which, as we shall see, is incorrect) Wolff's description of the latter process is nearly that of Schleiden; Wolff maintains that the "vessels" of plants are the result of the greater activity of the nutritive currents in particular directions; and so does Schleiden.*

Examining his statements closely, we notice, indeed, that his imperfect means of investigation led Wolff into two important errors—that of supposing the cells of plants to communicate in their youngest state, and thence deducing a false analogy with the areolar tissue of animals; and that of supposing that animal and vegetable tissues are always, in their very youngest state, absolutely structureless. However, as we shall see subsequently, Wolff is by no means singular in having started with grave anatomical mistakes, and we cannot perceive that in his case these errors, one of which, at any rate, Schleiden shares with him, vitiate those other and more important parts of his views, to which we are about to refer.

We have said, in fact, that not merely speculatively, but by observation, Wolff established a theory of the development of the vegetable tissues very similar to that of Schleiden, and that "identity of structure as shown by their development," between plants and animals, to prove which, was the purpose of the microscopical investigations of Schwann.

* It is very curious to find even Schwann's definition of cell-development as the "crystallization of a permeable body" anticipated by Wolff, Von d. eigenthümlichen Kraft, &c., p. 63: "..... so, from the case of many other attractions, especially of crystallization, which, among all known phenomena, comes nearest to vegetation, that without the second property—viz., that by means of which the mutually attractive substances interpenetrate and mingle with one another—the attractive force, although it should possess the first property, yet could as little effect nutrition. The particles of a salt dissolved in water attract only particles of salt—i. e, homogeneous substances, and repel all heterogeneous matters; for we get pure (crystals of the) salts. But out of this attraction comes nothing that can be compared with nutrition; for although the whole mass of substance is increased by degrees, yet crystals once formed remain as they are, and are not increased in their substance, and nothing less than the formation of new organization and continual change of figure accompanies this increase. . . . . Crystals once formed attract the saline particles only to their outer surface, upon which the attracted parts are deposited. They do not attract these particles into their substance. . . . . If the saline particles, on the other hand, penetrated the crystals and increased their substance homogeneously in all parts, this process would be indistinguishable from nutrition, and would consequently be a true nutrition." Compare Schwann, pp. 239—257.

But he did much more than this. In the 'Theoria Generationis,' and in the essay on the vital forces published thirty years afterwards, Wolff developed some very remarkable views on the relation of life to organization —of the vital processes to the organic elements—in which he diverges very widely from all who preceded, and from most who have followed him, most of all from Schleiden and Schwann. We may best exhibit the bearing of these views by contrasting them with those of the latter writers.

Schleiden and Schwann teach implicitly that the primary histological elements (cells) are independent, anatomically and physiologically; that they stand in the relation of *causes* or *centres*, to organization and the "organizing force;" and that the whole organism is the result of the union and combined action of these primarily separate elements. Wolff, on the other hand, asserts that the primary histological elements (cells too, but not always defined in the same way) are not either anatomically or physiologically independent; that they stand in the relation of *effects* to the organizing or vital force (vis essentialis); and that the organism results from the "differentiation" of a primarily homogeneous whole into these parts. Such a doctrine is, in fact, a most obvious and almost a necessary development of the doctrine of epigenesis in general. To one who had worked out the conclusion, that the most complex, grosser, animal or vegetable organizations, arise from a semi-fluid and homogeneous mass, by the continual and successive establishment of differences in it, it would be only natural to suppose that the method of nature, in that finer organization which we call histological, was the same; and that as the organ is developed by the differentiation of cells, so the cells are the result of the differentiation of inorganic matter. If the organism be not constituted by the coalescence of its organs and tissues in consequence of their peculiar forces, but if, on the other hand, the organism exists before its organs and tissues, and evolves them from itself,—is it not probable that the organs and tissues also, are not produced by the coalescence of the cells of which they are composed, in consequence of *their* peculiar forces, but, contrariwise, that the cells are a product of the differentiation of something which existed before them?

For Schwann the organism is a beehive, its actions and forces resulting from the separate but harmonious action of all its parts (compare Schwann, l. c., p. 229). For Wolff it is a mosaic, every portion of which expresses only the conditions under which the formative power acted and the tendencies by which it was guided.

We have said above, not without a full consciousness of the responsibility of the assertion, that we believe the cell-theory of Schleiden and Schwann to be based upon erroneous conceptions of structure, and to lead to errors in physiology, and we beg now to offer some evidence in favour of these views. We need not stop to prove, what must be familiar to every one who is acquainted with Schwann's work, that in making his comparison of animal with vegetable structures, he rests wholly upon Schleiden's statements concerning the development, and upon the commonly prevalent views with respect to the anatomy, of the latter.

It is clear, then, that however logically consequent Schwann's work may be in itself, its truth and the justice of its nomenclature will depend upon that of these latter views and statements. Schwann took these for granted, and if they were untrue he has been trusting to a rotten reed. Such, we fear, has indeed been the case. Schwann's botanical data were:

1. The prevalent notion of the anatomical independence of the vegetable cell, considered as a separate entity.
2. The prevalent conception of the structure of the vegetable cell.
3. The doctrine of the mode of its development.

Each of these, as assumed by Schwann, and as taught by Schleiden, has since, we shall endeavour to show, been proved to be erroneous. We will take them *seriatim*.

1. The first observer who, aided by the microscope, turned his attention to the structure of plants, was the versatile Hooke, and, as might be expected, the most noticeable thing to his mind was the existence of the innumerable cavities or "cells" scattered through their substance. Malpighi, the first proper botanical histologist, found that the walls of these vesicles were separable, that they could be isolated from one another, and therefore, doubtless urged more by the obvious convenience of the phraseology, than by any philosophical consideration upon the subject, he gave each the definite name of "utriculus," and regarded it as an independent entity. Of course it was a natural consequence that the plant should be regarded as constituted by the *union and coalescence* of a great number of these entities.

Grew, who, if all scandal be true, is so much indebted to Malpighi, did not appropriate this view among other things; on the other hand, he compared the utricles to the cavities in the foam of beer; and subsequently Wolff propounded the idea, that the cells were cavities in a homogeneous substance, as we have mentioned above. In modern times, the most important defender of this mode of regarding the matter has been Mirbel, who (escaping the error of Wolff, that the cavities of the cells communicate) endeavoured to demonstrate its truth, by tracing the formation of the cambium; but, at the time when Schwann wrote, it must be considered to have been wholly discredited, the opposite view having one of its strongest supporters in the caustic Schleiden himself—as, indeed, would necessarily be the case, from the tendency of his researches upon phytogenesis. As we shall see below, however, Schleiden was quite wrong in his ideas of cell-development—and we have therefore merely to consider the purely anatomical arguments for the independence of the cell. Now these amount, however various their disguise, to nothing more than this—that, by certain chemical or mechanical means, a plant may be broken up into vesicles corresponding with the cavities which previously existed in it: of course no one denies this fact; but of what value is it? Is the fact, that a rhombohedron of calcareous spar breaks up, if pounded, into minute rhombohedrons, any evidence that those minuter ones were once independent, and formed the larger by their coalescence? Is the circumstance that wood itself tears up into fibres, any evidence that it was formed by the coalescence of fibres? Assuredly not; for every hand-book will tell us that these fibres are the result of a metamorphosis of quite different parts. Is it not perfectly clear, that the behaviour of a body under mechanical or chemical influences, is simply an evidence of the disposition of the lines of greatest cohesion or affinity among its particles *at* *the time being*, and bears not in the slightest degree upon the question as to what these lines indicate; whether they are the remains of an ancient separation among heterogeneous parts, or the expression of a recent separation which has arisen in a homogeneous whole. So that, if the walls of the cells were really, as distinct from one another as is commonly supposed, it would be no argument for their vital independence: but they are not so. Von Mohl has shown that, in the great majority of cases, the assumption of the existence of a so-called intercellular substance, depends simply on imperfect chemical investigation, that there exists no real line of demarcation between one cell and another, and that wherever cells have been separated, whether mechanically or chemically, there is evidence that the continuous cellulose substance has been torn or in some way destroyed. In young tissues—such, for instance, as the cambium, or the base of a leaf, we have been quite unable to detect the least evidence of the existence of any line of demarcation between the cells; the cellulose substance forms a partition between cavity and cavity, which becomes evenly blue throughout by the action of sulphuric acid and iodine, and which certainly, even under the highest powers, exhibits no symptom of any optical difference; so that, in this state, vegetable tissue answers pretty closely to Wolff's idea. It is a homogeneous cellulose-yielding, transparent substance, containing cavities, in which lie peculiar vesicular bodies, into whose composition much nitrogen enters. It will be found a great aid if in the present confused state of terminology the reader will accept two new denominations for these elementary parts, which express nothing but their mutual relation. To the former, and to everything which answers to it, we shall throughout the present article give the name of *Periplast*, or periplastic substance; to the latter, that of *Endoplast*. So far, then, from the utricles or cells in the plant being anatomically distinct, we regard it as quite certain, that that portion which corresponds with the periplast, forms a continuous whole through the entire plant.

2. In 1837-8, each utricle of the plant was considered to have the following composition. In the first place, there was the cellulose cell-wall, or the portion of periplast answering to any particular endoplast; secondly, there were the cell-contents, a substance of not very defined nature, which occupied the cavity of the cell; and thirdly, there was the *nucleus*, a body to whose occurrence attention was first drawn, as is well known, by our own illustrious botanist, Robert Brown. He, however, cautiously remarked only its very general occurrence, without pretending to draw any inference from the fact; while Schleiden made the belief in its existence in all young tissues, the first article of the faith botanical. This is, however, most certainly incorrect; there is no trace of a nucleus in many Algæ, such as Hydrodictyon, Vaucheria,* Caulerpa; in the leaf of Sphagnum, nor in young germinating Ferns.†

Whatever opinion may be entertained upon this head, there is one point quite certain—the enumeration of the elements of the vegetable-cell given above is incomplete; there being one, and that the most important, which is omitted. We refer to the *primordial utricle*, which was only discovered by Von Mohl in 1844. This is a nitrogenous membrane, which always lies in close contact with the periplast, and forms, in fact, an included vesicle, within which the "contents" and the nucleus lie. Instead, therefore, of

* On Alex. Braun's authority, Ueber Verjüngung, &c., p. 186. † Henfrey, Linn. Trans. 1853.

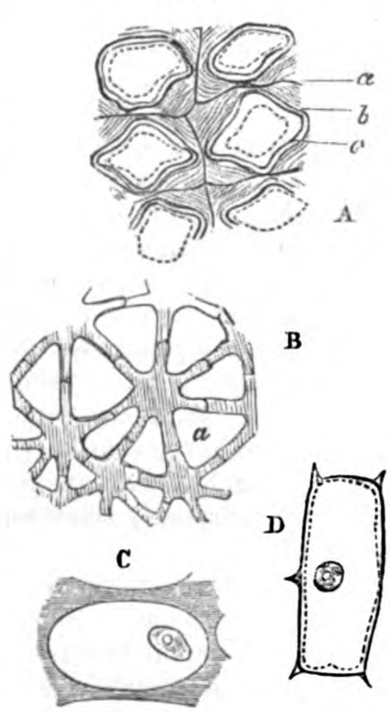
the endoplast consisting merely of contents and a nucleus, it is a vesicle containing the two latter, when they exist at all; and they are of subordinate importance, for while, as we have seen, a nucleus and formed contents may be absent in young or even fully formed tissues, the primordial utricle is invariably present in the young structures, and often persists until they have attained their full size. Since, then, the functions of the vegetable "cell" can be effectually carried on by the primordial utricle alone; since the "nucleus" has precisely the same chemical composition as the primordial utricle; and since, in some cases of cell-division, new nuclei are seen to arise in the substance of the endoplast, by a mere process of chemical and morphological differentiation (Von Mohl, l. c., p. 52), it follows, we think, that the primordial utricle must be regarded as the essential part of the endoplast—the protoplasm and nucleus being simply its subordinate, and, we had almost said, *accidental* anatomical modifications.

3. Finally, with respect to Schleiden's observations upon the mode of cell-development, according to which in all cases the new production of vegetable-cells takes place by the development of nuclei, round which the cell-membrane is deposited, subsequently expanding and becoming separated from the nucleus, so as to form a complete cell; we need only say, that they have been long since set aside by the common consent of all observers; in Von Mohl's words (p. 59): "The whole of this account of the relation of the nucleus to the cell membrane is incorrect." The fact is, that in by far the greater proportion of cases, new cell-development occurs by the division of the previous endoplasts, and the growth or deposition round them and between them, of fresh periplastic substance. The extent of this process of division will be understood, if we remember that all observers now agree in its being the method by which "cell-development" always occurs, except in the embryo-sac of the Phanerogamia, the sporangia of Lichens and of some Algæ and Fungi. The so-called free cell-development of the latter, however, by no means takes place in accordance with Schleiden's views, but by the development of a cellulose membrane (periplast) around a mass of nitrogenous substance (endoplast), which may or may not contain a nucleus; subsequently increasing, *pari passu*, with the periplast. And it is well worthy of consideration, how far the process deserves any distinction, except in degree, from ordinary cell-division, since the new endoplast is only one portion of that of the parent cell, set aside for the purpose of fresh development, while the rest undergoes no corresponding change. However this may be, it may be regarded as quite certain, that, leaving out of view the immediate results of sexual reproduction, the whole of the "cells,"—the entirety of the periplasts and endoplasts—of which a plant, whether it be a moss or an oak, are composed, never are independent of one another, and never have been so, at any period of their existence; but that, while the original endoplast of the embryo-cell, from which the plant sprung, has grown and divided into all the endoplasts of the adult, the original periplast has grown at a corresponding rate, and has formed one continuous and connected envelop from the very first. The ground of his comparison, therefore, is cut away from under Schwann's feet; every statement of Schleiden's on which he relied turning out to be erroneous—as we shall see if we turn to his original comparison of cartilage with a vegetable tissue (pp. 9—17).

Schwann, finding in cartilage, cavities with more or less distinct walls, in each of which lay a corpuscle, singularly resembling the nucleus of the vegetable-cell; finding also that the cell-wall was close to this corpuscle in the younger parts, more distant from it in the older (p. 24), naturally concluded that he had here, in the animal world, an exact confirmation of Schleiden's supposed discoveries, and of course gave to the corpuscle of cartilage the name of "cytoblast," or "nucleus," as indicating its homology with the structure of that name, in the plant.

The primordial utricle was, as we have said, not then discovered in the latter, and of course Schwann was not led to look for anything corresponding to it. Indeed, had he done so, his search would have been unsuccessful, for the young and unaltered cartilage cavity contains the corpuscle, and nothing else. The circumstance, therefore, which Schwann considered to demonstrate the identity of structure of plants and animals—i. e., the correspondence of the cartilage-corpuscle with the nucleus of the vegetable-cell, and of the chondrin-wall with the cellulose-wall, would, if it were really the case, be the widest possible ground of distinction between the two, for it would leave the most important element of the latter, the "primordial utricle," without any homologue in the animal, and totally unaccounted for.

Fig. 1.



A, Collenchyma cells of Beta vulgaris; B, Stellate tissue of the pith of the rush; C, a cartilage-cell with its corpuscle, to compare with D, a vegetable-cell with its nucleus, the primordial utricle in the latter being indicated only by a dotted line.

It is precisely the neglect of this important change in the whole subject, effected by the discovery of Von Mohl, which has, we think, led to the confusion which prevails at present, not only in the comparative, but in the absolute nomenclature of animal histology. Animal physiologists go on using Schwann's nomenclature, forgetting that the whole doctrine of the vegetable-cell, from which he drew that nomenclature, has been completely upset; and at present, beyond the mere fact of a common vesicularity at one period of their existence, one would be led, on opening successively two works on animal and vegetable structure, rather to predicate their total discrepancy, than any uniformity between them.

Now does this discrepancy lie in the facts, or in our names of them? To decide this question, it seemed to us that the only plan was to follow Schwann's steps, and to compare cartilage with a vegetable tissue—for he has shown logically and conclusively enough, that whatever is true of the corpuscle of cartilage, to which he gives the name of "nucleus," is true also of all those corpuscles in the other tissues, to which he gives the same name.

Let us compare, then, some young vegetable tissue, say that of the base of a Sphagnum leaf (fig. 2. A), which is in many respects very convenient for examination, with that of young cartilage (fig. 3. A); the identity of structure is such, that it would be difficult, without the aid of chemical re-agents, to distinguish one from the other: in each, we see highly nitrogenous, more or less vesicular endoplasts imbedded in a homogeneous transparent substance, whose cavities they wholly fill. If we trace the further development, we find that in the Sphagnum leaf the endoplasts and their cavities rapidly increase in size (fig. 2. B), the former becoming, in certain localities of the leaf, regular primordial utricles without any nucleus, and growing in exact proportion to the cavities in the periplast (*b*), while in other directions, having attained a certain size they cease to grow, and rapidly disappear, leaving the periplastic cavity empty. (*a*) In cartilage precisely the same thing occurs. The endoplasts increase in size for awhile, and then stop, while the periplastic cavities continue to increase, and thus we have eventually a cartilage-cavity with its corpuscle. In old cartilage the latter frequently disappears, or is converted into fat. We have here purposely selected, in both the animal and the plant, simple cases, in which the endoplast becomes a primordial utricle, without any nucleus. Had we selected the cambium of a phœnogamous plant, it would have been merely necessary to add, that as the endoplast grew, a nucleus appeared in its interior; and in ossifying cartilage, near the ossifying surface, we have repeatedly seen endoplasts such as those described above, some of which contained definite "nuclei," while those in their immediate neighbourhood possessed none.

In the case of cartilage, then (and it is a conclusion at which Leidy and Remak have already arrived), we hold it to be proved, that the corpuscle does not correspond with the nucleus of the plant, as Schwann supposed, but with the primordial utricle, contents and nucleus; or, in other words, that the "nucleus" of cartilage, is the equivalent of the "primordial utricle" of the plant—that they are both endoplasts. It follows, hence, that the chondrin-wall of the cartilage, is the homologue of the cellulose wall of the plant, and that they both represent the periplastic element. The phenomena of growth and multiplication exhibited by these corresponding elements are perfectly similar. The process of cell-division, as it is called, is identical in each case. In the plant, the primordial utricles divide, separate, and the cellulose substance grows in between the two. In young cartilage the same thing occurs, the corpuscles divide, separate, and the chondrin substance eventually forms a wall of separation between the two. There is neither endogenous development nor new formation in either case. The endoplasts grow and divide, the periplast grows so as to surround the endoplasts completely, and, except so far as its tendency is, to fill up the space left by their separation, there is no evidence that its growth is in any way affected by them, still less, that it is, as is often assumed, deposited by them. We are led, then, to the conclusion, that though Schwann's great principle of the identity of structure of plants and animals is perfectly correct, his exposition of it is incorrect, inasmuch as the corpuscle of cartilage (his "nucleus," whence he reasoned to the other "nuclei,") answers not to the "nucleus," but to the "primordial utricle" of the plant; since the mode of development of new "cells," though identical in each case, is different from what Schleiden stated, and

Schwann believed; and finally since, for the notion of the anatomical independence of the cells, we must substitute that of the unity and continuity of the periplastic substance in each case.

Intimately connected with these structural errors, as we cannot but think them, are Schwann's views of the nature and powers of the "cell," and those subsequently developed (principally by Kölliker) with respect to the action of the nuclei as "centres of force." Led apparently by his views of its anatomical independence, Schwann maintains, as a general proposition, that the cell as such possesses powers which are not inherent in its separate molecules.

'We must, in fact, ascribe an independent life to cells—i.e., the combination of molecules which take place in a single cell are sufficient to set free the force, in consequence of which the cell has the power of attracting new molecules. The cause of nutrition and growth lies not in the organism as a whole, but in the separate elementary parts—the cells. That in point of fact every cell, when separated from the organism, is not capable of further growth, as little militates against this theory, as its incapability of existing separate from the swarm would be an argument against the independent life of a bee. . . . . . The inquiry into the fundamental forces of organisms, therefore, is reduced into one concerning the fundamental forces of the single cells." (p. 229.)

And yet, strongly as Schwann maintains, not only here but in many other places, the view that the vital forces are manifested by the cells as machines, and are not inherent in the matter of which these cells are composed, apart from their form; he gives it up in effect when he comes to treat of these forces in detail. The fundamental cell-forces are, he says, of two kinds, the attractive and the metabolic, the former regulating growth, the latter determining the chemical changes; and he shows very justly that these forces are not located in any special centres in the cell, but are exhibited by all its solid constituents (pp. 233, 236), and that they may be exhibited by different portions of these solid constituents, and to a different extent by these different portions (p. 233); proving hereby, very clearly, as it seems to us, that the forces in question are not centralized in the cells, but are resident in their component molecules. All Schwann's able comparison of cell-development with crystallization, tends in fact to the same conclusion. When matter crystallizes from a solution, the presence of a foreign body may determine the place and form of the deposit; but the crystals themselves are the result, not of the attractive forces of the foreign body, but of the forces resident in their component molecules. So in cell-development, if it is to be rigorously compared to crystallization, even if the nuclei represent the foreign bodies, which determine the place of the chemical and morphological alterations in the surrounding substance, it by no means follows that they are their cause.

Kölliker (§§ 11, 13), resting especially upon the phenomena of yelk-division and of endogenous cell-development, advocates the existence of a peculiar molecular attraction proceeding from the nucleolus first, and subsequently from the nucleus. Now as regards endogenous cell-development, we must confess that we can find no more ground for its occurrence among animals than among plants. Nägeli's cell-development around portions of contents, upon which Kölliker lays so much stress, is nothing more than a case of division of the endoplast (primordial utricle) and subsequent development of periplastic substance round the portions. In cartilage, which is so often quoted as offering marked endogenous cell-development, we must agree with Leidy and Remak, that nothing but division of the endoplasts (nuclei, primordial utricles) and ingrowth of the periplast (intercellular substance, cell-wall) occurs. In these endoplasts again, the very existence of a nucleus is in the highest degree variable and inconstant, and division occurs as well without it as with it.

The process of yelk-division—that remarkable manifestation of a tendency to break up, in the yelk of most animals, into successively smaller spheroids, in each of which a nucleus of some kind appears—seems, at first, to offer very strong evidence in favour of the exertion of some attraction by these nuclei upon the vitelline mass. But we think that a closer examination completely deprives this evidence of all weight. In the first place, the appearance of the nuclei is in many cases subsequent to segmentation. It is thus in Strongylus auricularis (Reichert), in Phallusia (Krohn), in the hen's egg (Remak). In the second place, it seems difficult to conceive any mode of operation of a central attractive force which shall give rise to the phenomena of segmentation, for the resulting spheroids always pass into one another by extensive plane surfaces, whereas the even action of two attractive centres, in a mass free to move, would give rise to two spheroids in contact only by a point. Again, Remak has observed, that in the frog's egg the time occupied by the formation of the groove, indicating the first line of cleavage upon the upper half of the yelk, is very much shorter than that required to give rise to the corresponding line upon the lower half—a fact which is quite unintelligible upon the theory of a central attraction.

Thirdly, in Cucullanus, Ascaris dentata, &c., Kölliker has shown, that though nuclei are developed, no yelk-division occurs; and in the later stages of division of the frog's egg, yelk masses are found undivided, and containing many nuclei.

Finally, in Ascaris mystax, according to Dr. Nelson,* the embryonic vesicles absolutely revolve in circles during the progress of yelk-division—a phenomenon which seems incompatible with the existence of any mutual attractive reaction between themselves and the vitelline mass.

We see, in short, that the effects of the force supposed to be exerted by the nuclei may take place without them, and, on the other hand, that the nuclei may be present without exerting the peculiar forces which they are supposed to possess; and finally, that even if such forces exist, they must be something very different from all the attractive forces of which we have any conception; and therefore that the hypothesis of nuclear force is no explanation, but merely a fresh name for the difficulty.

We are as little able to discover any evidence of the existence of metabolic forces in the nuclei. The metabolic changes of the tissues—such as we see, for instance, in the conversion of cartilage into bone, of cartilage into connective tissue—do not take place, either primarily or with greater intensity, in the neighbourhood of the nuclei; a fact of which striking evidence is afforded by ossifying cartilage, in which the first deposit of calcareous matter occurs, not in areæ surrounding each nucleus, as we should expect if they exerted a metabolic influence, but in straight lines,

* Philosophical Transactions, 1852.

which stretch from the ossified surface into the substance of the matrix of the cartilage, and the amount of calcareous matter in which gradually diminishes as we recede from the ossified part, without the least reference to the nuclei. It is the same with the metamorphosis of the periplast of cartilage when it passes into tendon.

From all this we consider it to be satisfactorily shown, that there is no evidence that the "cells" of living bodies are, in any respect, centres of those properties which are called vital forces. What, then, are these cells? it may be asked;—What is the meaning of the unquestionable fact, that the first indication of vitality, in the higher organisms at any rate, is the assumption of the cellular structure?

In answering these questions, we would first draw attention to the definition of the nature of development in general, first clearly enunciated by Von Baer. "The history of development," he says, "is the history of a gradually increasing differentiation of that which was at first homogeneous." The yelk is homogeneous; the blastoderma is a portion of it which becomes different from the rest, as the result of the operation of the laws of growth; the blastoderma, again, comparatively homogeneous, becomes differentiated into two or more layers; the layers, originally identical throughout, set up different actions in their various parts, and are differentiated into dorsal and visceral plates, chorda dorsalis and bodies of vertebræ, &c. &c. No one, however, imagines that there is any causal connexion between these successive morphological states. No one has dreamt of explaining the development of the dorsal and visceral plates by blastodermic force, nor that of the vertebræ by chorda-dorsalic force. On the other hand, all these states are considered, and justly, to result from the operation of some common determining power, apart from them all—to be, in fact, the modes of manifestation of that power.

Now, why should we not extend this view to histology, which, as we have explained, is only ultimate morphology? As the whole animal is the result of the differentiation of a structureless yelk, so is every tissue the result of the differentiation of a structureless blastema—the first step in that differentiation being the separation of the blastema into endoplast and periplast, or the formation of what is called a "nucleated cell."* Then, just as in the development of the embryo, when the blastodermic membrane is once formed, new organs are not developed in other parts of the yelk, but proceed wholly from the differentiation of the blastoderm,—so histologically, the "nucleated cell," the periplast with its endoplast, once formed, further development takes place by their growth and differentiation into new endoplasts and periplasts. The further change into a special tissue, of course, succeeds and results from this primary differentiation, as we have seen the bodies of the vertebræ succeed the chorda dorsalis; but is there any more reason for supposing a causal connexion between the one pair of phenomena, than between the other? The cellular structure precedes the special structure; but is the latter, therefore, the result of a "cell-force," of whose existence there is on other grounds no evidence whatever? We must answer in the negative. For us the primarily cellular structure of plants and animals is simply a fact in the history of their histological development—a histologically necessary stage, if one may so call it, which

* Compare Reichert, p. 35.

has no more causal connexion with that which follows it, than the equally puzzling morphological necessity for the existence of a chorda dorsalis or of Wolffian bodies has, with the development of the true vertebræ or of the true kidneys.

If this be true, we might expect, as we find, that the differentiation of the germinal disc, for instance, into a primitive groove and lateral portions —the first stage of development in the embryo of all vertebrate animals— does not occur in mollusks; as we find, again, that the differentiation of the embryo into plumula and cotyledons which occurs in a great number of plants is absent in others; so, if, like these, the histological differentiation into cells have no necessary causal connexion with the action of the vital forces, but be merely a genetic state, we may expect to meet with cases in which it does not occur. Such, in fact, are the so-called unicellular plants and animals—organisms which often exhibit no small complexity of external form, but present no internal histological differentiation. In the genus Caulerpa we have an Alga, presenting apparent leaves, stems, and roots, and yet which, according to Nägeli, consists of a single cell— that is, is not composed of cells at all. The Vorticellæ furnish us with examples of animals provided with a distinct œsophagus, a muscular pedicle, &c., and yet in which no further histological differentiation can be made out. As Wolff* says—

"The latter (Roesel's Proteus) has no structure, no determinate figure, and even the indeterminate figure that it has at any given time does not remain the same, but alters continually. We can, in fact, regard all these plants and animals as little else than living or vegetating matters—hardly as organized bodies.

"§ 74. However, all these plants and animals nourish themselves, vegetate, and propagate their species, just as well and as easily as the most artificial pieces of mechanism to be met with in the vegetable or animal kingdom."

It is true, indeed, that the difficulty with regard to these organisms has been evaded by calling them "unicellular"—by supposing them to be merely enlarged and modified simple cells; but does not the phrase an "unicellular organism" involve a contradiction for the cell-theory? In the terms of the cell-theory, is not the cell supposed to be an anatomical and physiological unity, capable of performing one function only—the life of the organism being the life of the separate cells of which it is composed? and is not a cell with different organs and functions something totally different from what we mean by a cell among the higher animals? We must say that the admission of the existence of unicellular organisms appears to us to be virtually giving up the cell-theory for these organisms. If it be once admitted that a particle of vitalizable matter may assume a definite and complex form, may take on different functions in its different parts, and may exhibit all the phenomena of life, without assuming the cellular structure, we think that it necessarily follows that the cells are not the centres of the manifestation of the vital forces; or that, if they be so, the nature of these forces is different in the lower organisms from what it is in the higher—a proposition which probably few would feel disposed to maintain.

So much for the critical, and therefore more or less ungrateful, portion of our task. We have seen how the great idea, fully possessed by Fallopius,

* 'Von der wesentlichen Kraft,' p. 40.

that life is not the effect of organization, nor necessarily dependent upon it, but, on the other hand, that organization is only one of the phenomena presented by living matter—carried to absurdity by Stahl and Van Helmont—has, on the other hand, been too much neglected by the later writers who have attempted to reduce life to the mere attractions and repulsions of organic centres, or to consider physiology simply as a complex branch of mere physics. We have seen how this latter notion has been fostered by the misconceptions of a great botanist, only too faithfully followed in the animal world by the illustrious author of the cell-theory; and we have endeavoured to show how the solitary genius of Wolff had kept in the old track, and that the choice of modern histologists lies between him and Schleiden and Schwann. It will be sufficiently obvious that our own election has long been made in this matter, and we beg to submit the following sketch of a general theory of the structure of plants and animals—conceived in the spirit, and not unfrequently borrowing the phraseology, of Wolff and Von Baer.

Vitality, the faculty, that is, of exhibiting definite cycles of change in form and composition, is a property inherent in certain kinds of matter.

There is a condition of all kinds of living matter in which it is an amorphous germ—that is, in which its external form depends merely on ordinary physical laws, and in which it possesses no internal structure.

Now, according to the nature of certain previous conditions—the character of the changes undergone—of the different states necessarily exhibited —or, in other words, the successive differentiations of the amorphous mass will be different.

Conceived as a whole, from their commencement to their termination, they constitute the individuality of the living being, and the passage of the living being through these states, is called its development. Development, therefore, and life are, strictly speaking, one thing, though we are accustomed to limit the former to the progressive half of life merely, and to speak of the retrogressive half as decay, considering an imaginary resting point between the two as the adult or perfect state.*

The individuality of a living thing, then, or a single life, is a continuous development, and development is the continual differentiation, the constant cyclical change of that which was, at first, morphologically and chemically indifferent and homogeneous.

The morphological differentiation may be of two kinds. In the lowest animals and plants—the so-called unicellular organisms—it may be said to be *external*, the changes of form being essentially confined to the outward shape of the germ, and being unaccompanied by the development of any internal structure.

But in all other animals and plants, an internal morphological differentiation precedes or accompanies the external, and the homogeneous germ becomes separated into a certain central portion, which we have called the *endoplast*, and a peripheral portion, the *periplast*. Inasmuch as the separate existence of the former necessarily implies a cavity, in which it lies, the germ in this state constitutes a vesicle with a central particle, or a "nucleated cell."

* Dr. Lyons, in his interesting 'Researches on the primary stages of Histogenesis and Histolysis,' has invented a most convenient and appropriate term for this latter half of development ,so far as the tissues are concerned—viz., Histolysis.

There is no evidence whatever that the molecular forces of the living matter (the " vis essentialis" of Wolff, or the vital forces of the moderns) are by this act of differentiation localized in the endoplast, to the exclusion of the periplast, or *vice versâ*. Neither is there any evidence that any attraction or other influence is exercised by the one over the other; the changes which each subsequently undergoes, though they are in harmony, having no causal connexion with one another, but each proceeding, as it would seem, in accordance with the general determining laws of the organism. On the other hand, the " vis essentialis" appears to have essentially different and independent ends in view—if we may for the nonce speak metaphorically—in thus separating the endoplast from the periplast.

The endoplast grows and divides; but, except in a few more or less doubtful cases, it would seem to undergo no other morphological change. It frequently disappears altogether; but as a rule, it undergoes neither chemical nor morphological metamorphosis. So far from being the centre of activity of the vital actions, it would appear much rather to be the less important histological element.

The periplast, on the other hand, which has hitherto passed under the names of cell-wall, contents, and intercellular substance, is the subject of all the most important metamorphic processes, whether morphological or chemical, in the animal and in the plant. By its differentiation, every variety of tissue is produced; and this differentiation is the result not of any metabolic action of the endoplast, which has frequently disappeared before the metamorphosis begins, but of intimate molecular changes in its substance, which take place under the guidance of the " vis essentialis," or, to use a strictly positive phrase, occur in a definite order, we know not why.

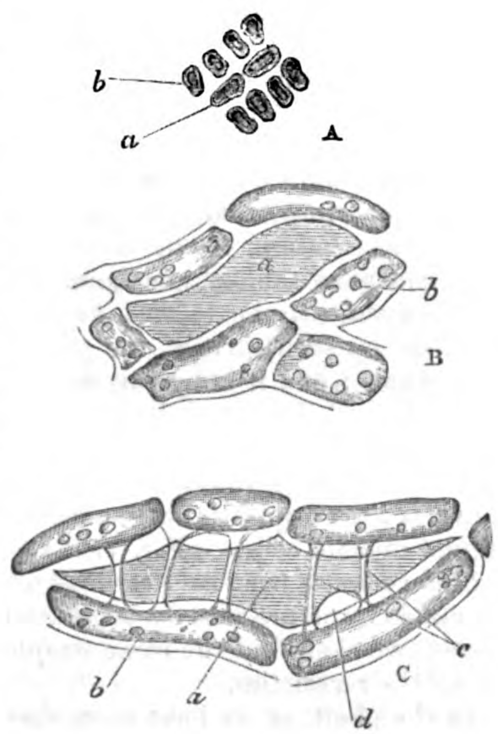
The metamorphoses of the periplastic substance are twofold—chemical and structural. The former may be of the nature either of *conversion:* change of cellulose into xylogen, intercellular substance, &c., of the indifferent tissue of embryos into collagen, chondrin, &c.; or of *deposit:* as of silica in plants, of calcareous salts in animals.

The structural metamorphoses, again, are of two kinds—*vacuolation*, or the formation of cavities; as in the intercellular passages of plants, the first vascular canals of animals; and *fibrillation*, or the development of a tendency to break up in certain definite lines rather than in others, a peculiar modification of the cohesive forces of the tissue, such as we have in connective tissue, in muscle, and in the " secondary deposits" of the vegetable cell.

Now to illustrate and explain these views, let us return to the vegetable and animal tissues, as we left them in describing the base of the Sphagnum leaf and fœtal cartilage, and trace out the modification of these, which are identical with all young tissues, into some of the typical adult forms.

The point of the Sphagnum leaf is older than the base, and it is easy to trace every stage from the youngest to the complete forms in this direction. At the base of the leaf, we find, as has been said, nothing but minute endoplasts, each resembling the other, embedded in a homogeneous periplastic substance (A); as we trace these upwards, we find that some of the endoplasts increase in size more rapidly than the others (B), and eventually *totally disappear*, leaving only the endoplastic cavity, or "cell," which contained them. In the surrounding cells, the endoplasts are very obvious as granular primordial utricles (C). After the disappearance of the endoplast, changes commence in the periplastic substance or wall of the cell (*a*), more or less circular or spiral thickenings (*c*) taking place in it, so as to form the well-known fibre-cell of the sphagnum leaf; and at the same time, a process of resorption occurs in particular parts of the wall, so that round apertures are formed (*d*). Nothing can be more instructive than this case, the leaf being composed of a single layer of delicate and transparent cells, so that there are no interfering difficulties of observation; and we see demonstrated, in the most striking manner, that the endoplast or primordial utricle has nothing to do with the metamorphoses which occur in the periplastic substance. The disappearance of the primordial utricle in cells which are undergoing thickening, was, in truth, long ago pointed out by Von Mohl; but neither he nor any of his successors seem to have noticed how completely this fact does away with that activity of the primordial utricle, and passivity of the cell-wall, which they all assume. We have here, in fact, the cell-wall commencing and carrying through its morphological changes after the primordial utricle has completely disappeared, and we see that the so-called secondary deposit in this case, is a morphological differentiation of the periplast, which at the same time exhibits its peculiar powers by setting up a resorption of its substance at another point. Here, however, we have no marked chemical differentiation; for an instance of which we may turn to the collenchyma of the beet-root (fig. 1, A). There is no question that, at one period of its development, the whole periplastic substance here, as in the Sphagnum, was homogeneous, and of the same chemical constitution. In the fully formed beet-root, however, we have no less than three compounds disposed around each cell cavity. The periplastic substance has, in fact, undergone both a chemical and a morphological differentiation—the innermost layers (*c*) consisting of ordinary cellulose; the next of a substance which swells up in water (*b*); and the outermost of a different, but not exactly defined, substance (*a*). We may call one of these portions "cell-membrane," and another intercellular substance, but they are, assuredly, all nothing but differentiated portions of one and the same periplast.

Fig. 2.



Portions of the leaf of sphagnum. A, from the base; B, more towards the point; C, fully formed. *a*, endoplasts which disappear; *b*, those which remain; *c*, spiral thickenings of periplast in the cavities of the former; *d*, apertures formed by resorption.

Woody tissue presents precisely the same phenomena, the inner layers of the periplastic substance having, very generally, a different composition from the outer.

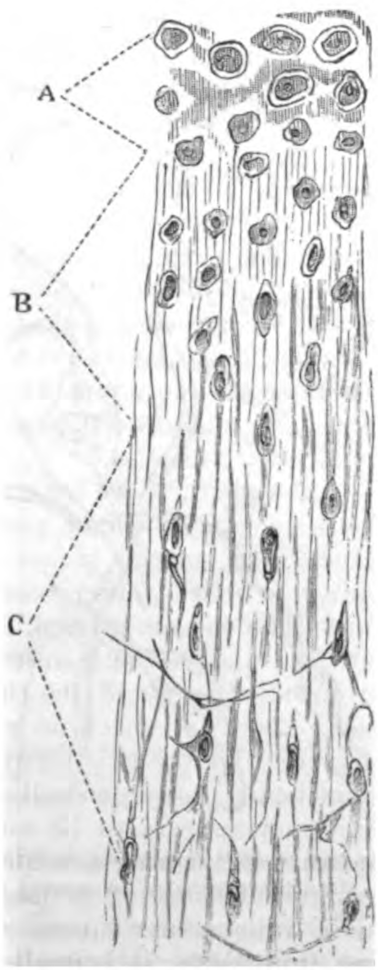
Morphologically, we have already noticed the lamination of the periplastic substance, and we may mention its fibrillation, a process which takes place almost invariably in the inner layers of the periplast, and to which the well-known spirality of the so-called secondary deposits must be referred; but a more important process for our present purpose is what we have called Vacuolation; the development of cavities in the periplastic substance independent of the endoplast, and which, to distinguish them from the cells, may conveniently be termed *Vacuolæ*. In the youngest vegetable tissues there are no such cavities, the periplastic substance forming a continuous solid whole; and it is by this vacuolation, which occurs as the part grows older, that all the intercellular passages are formed, and that many cells obtain that spurious anatomical independence, to which we have adverted above. The exaggerated development of the vacuolæ in the pith of the rush converts the periplastic substance, with its proper endoplastic cavities, into regular stellate cells. (Fig. 1, B.)

Sufficient has been said to illustrate the differentiation of the primitive vegetable structure into its most complex forms. If we turn to the animal tissues, we shall find the same simple principles amply sufficient to account for all their varieties.

In the plant, as we have seen, there are but two histological elements—the periplastic substance, and the endoplasts, cell-wall and intercellular substance, being merely names for differentiated portions of the former; cell-contents, on the other hand, representing a part of the latter. In the animal, on the other hand, if we are to put faith in the present nomenclature, we find cell-wall, intercellular substance, and cell-contents, forming primitive elements of the tissues, and entering into their composition as such: there have been no small disputes whether the collagenous portion of connective tissue is intercellular substance or cell-wall, the elastic element being pretty generally admitted to be developed from distinct cells. Again, it appears to be usual to consider the fibrillæ of striped muscle as modified cell-contents, while the sarcolemma represents the cell-walls. The hyaline substance of cartilage is asserted by some to be cell-wall, by some to be intercellular substance; while the walls of the epithelium cavities are admitted, on all hands, to be cell-walls. We confess ourselves quite unable to find any guiding principle for this nomenclature, unless it be that the toughest structure surrounding a "nucleus" is to be taken as cell-wall, anything soft inside it being contents, and anything external to it intercellular substance; which is hardly a caricature of the vagueness which pervades histological works upon this subject. This results, we think, from the attempt to determine the homology of the parts of the tissues having been made from the examination of their embryonic conditions, where it is often very obscure, and hardly to be made out. It is another matter if we adopt the "principle of continuity" of Reichert—a method of investigation which has been much neglected. This principle is simply, that whatever histological elements pass into one another by insensible gradations are homologous and of the same nature; and it is so clear and easy of application, that we can but wonder at its hitherto limited use. We will now proceed to analyze the nature of the constituents of some of the most characteristic tissues in this way, starting from that of embryonic cartilage, as we have described it above.

Connective tissue occurs in two forms,—which, however, pass into one another by infinite gradations,—the solid and the areolated: of the former we may take a tendon as an example; of the latter, the loose areolar tissue, which is found forming the inner layer of the skin and mucous membranes. Fig. 3 represents the junction between the tendo-Achillis and the cartilage of the os calcis, in a young kitten. At A, we have pure cartilage, the endoplasts lying within cavities whose walls present more or less defined contours. At B, the cavities and their contained endoplasts are somewhat elongated, and a faint striation is obvious in the upper portion of the periplastic substance, which becomes stronger and stronger, as we proceed lower down, until it ends in an apparent fibrillation. A chemical change has at the same time taken place, so that in this portion the striated part of the periplast is swollen up more or less by acetic acid, the walls of the cavities remaining unaffected, and thence becoming more distinct; while in the portion A, the whole periplast was nearly equally insensible to this re-agent. The portion C, nearest the tendon, and passing into it, is completely tendinous in its structure. The periplast exhibits strong fibrillation, and is very sensitive to acetic acid, while not only the walls of the cavities, but the intermediate periplast, in certain directions, which radiate irregularly from them, have changed into a substance which resists acetic acid even more than before, and is in fact elastic tissue. Compare this process with that which we have seen to be undergone by the collenchyma of the beetroot, and we have the fibrillation of the outer portion of the periplast around each endoplast, and its conversion into collagen, answering to the lamination of the "intercellular substance," and its conversion into a vegetable gelatinous matter, while the elastic, corresponds with the cellulose inner wall.

Fig. 3.



Junction of tendo-Achillis and cartilage of the calcaneum in a kitten. A, Pure cartilage; B, intermediate portion; C, tendon. It must be understood that the transition is in reality much more gradual, the different stages having here been approximated for the sake of economizing space.

The testimony of numerous observers agrees that cartilage is converted into connective tissue in the way described. Professor Kölliker, who unwillingly admits the fact, suggests, nevertheless, that such connective tissue as this, is not true connective tissue, inasmuch as it presents differences in its mode of development, the collagenous element in the latter being always developed from cells.*

Now, we might be inclined to ask, if the substance of the tendo-Achillis is *not* connective tissue, but only "*täuschend ähnlich*," what is? But it is better to attack Prof. Kölliker's stronghold, the areolated gelatinous connective tissue, which is, as he justly observes, the early form of fœtal connective tissue generally. (l. c., p. 58.) If the outer layer of the corium of the skin, or the submucous gelatinous tissue in the enamel organ, be teased out with needles, we shall obtain various stellate or ramified bodies, containing endoplasts (fig. 4), which Kölliker calls cells, and which, as he states, do assuredly pass into and become, bundles of fibrillated connective tissue. But is this really a different mode of development from that already described? We think not. Indeed, if that portion of this young gelatinous connective tissue, which lies immediately adjacent to the epidermis or epithelium be examined, it will be found to present a structure in all respects similar to fœtal cartilage, that is, there is a homogenous matrix in which the endoplasts are dispersed (fig. 5 B). If this be traced inwards, it will be found, that the endoplasts become more widely separated from one another, and that the matrix in places between them is softened and altered, while in their immediate neighbourhood, and in the direction of irregular lines stretching from them, it is unaltered. This is, in fact, the first stage of that process which we have called vacuolation. In this condition the intermediate softened spots still retain sufficient consistence not to flow out of a section; but yielding, as it does, in these localities, much more readily than in others, it is easy enough to tear out the firmer portion in the shape of "cells," which are fusiform, irregular, or stellate; and the whole tissue has therefore been described (Reichert, Virchow, Schwann) as consisting of cells, connected by an "intercellular substance." Both "cell-walls" and "intercellular substance," however, are portions of the same periplast, and together correspond with the matrix of the cartilage. When, therefore, in the course of further development, the "intercellular substance" becomes quite fluid and so disappears, the outer portion of these cells being converted into fibrillated collagenous tissue,

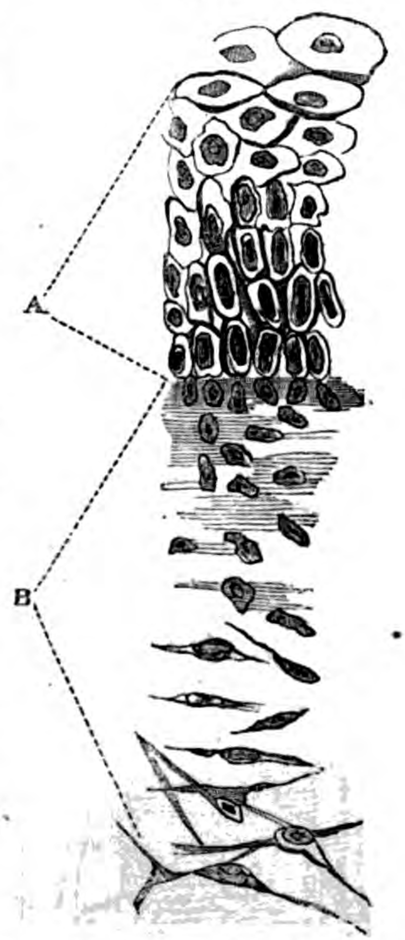
Fig. 4.

Stellate "cells" of young connective tissue from the actinenchyma of the enamel organ of the calf.

* Handbuch, pp. 58, 59, 218.

and the inner into elastic substance, we have, notwithstanding the apparently great difference, in reality exactly the same mode of metamorphosis of the same elements, as in the preceding instance. Connective tissue, therefore, we may say, consists in its earliest state of a homogeneous periplast inclosing endoplasts. The endoplasts may elongate to some extent, but eventually become lost, and cease, more or less completely, to be distinguishable elements of the tissue. The periplast may undergo three distinct varieties of chemical differentiation, e.g., into the gelatinous "intercellular substance," the collagenous "cell-wall," and the elastic "cell-wall;" and two varieties of morphological differentiation, vacuolation, and fibrillation—and the mode in which these changes take place gives rise to the notion that the perfect tissue is composed of elements chemically and mechanically distinct.

Fig. 5.



Submucous tissue and epithelium of the tongue of the kitten. A, Epithelium; B, young connective tissue.

The proper understanding of the nature and mode of development of the component parts of connective tissue is, we believe, of the first importance in comprehending the other tissues. If we clearly bear in mind, in the first place, that the periplast is capable of undergoing modifications quite independently of the endoplasts; and secondly, that in consequence of their modification, elements may become optically, mechanically, or chemically separable from a perfect tissue, which were not discoverable in its young form, and never had any separate existence; many of the great difficulties and perplexities of the cell-theory will disappear. Thus, for instance, with regard to the structure of bone, there can be no doubt that the "nuclei" of the corpuscles are endoplasts, and that the calcified matrix is the periplast. This calcified matrix has, however, in adult bone, very often a very regular structure, being composed of definite particles. To account for these, Messrs. Tomes and De Morgan, in their valuable essay on ossification, which has just appeared,* suppose that certain "osteal cells" exist and become ossified. We have no intention here of entering upon the question of the existence of these "osteal cells" as a matter of fact, but we may remark, that they are by no means necessary, as the appearance might arise from a differentiation of the periplast into definite particles, corresponding with that which gives to connective tissue its definite and fibrillated aspect. So with regard to the vexed question whether the lacunæ have separate parietes or not, how readily comprehensible the opposite results at which different observers

* Phil. Trans., 1853.

have arrived become, if we consider that their demonstrability or otherwise results simply from the nature and amount of the chemical difference which has been established in the periplast in the immediate neighbourhood of the endoplast, with regard to that in the rest of the periplast. In fig. 3 substitute calcific for collagenous metamorphosis, and we should have a piece of bone exhibiting every variety of lacunæ, from those without distinct walls, to those which constitute regular stellate "bone corpuscles." Finally, in bone, the formation of the "Haversian spaces" of Tomes and De Morgan is a process of vacuolation, strictly comparable to that which we have described as giving rise to the areolated connective tissue. Cancellated bone is, in fact, areolated osseous tissue. Once having comprehended the fact that the periplast is the metamorphic element of the tissues, and that the endoplast has no influence nor importance in histological metamorphosis, there ceases to be any difficulty in understanding and admitting the development of the tubules of the dentine and the prisms of the enamel, without the intervention of endoplasts. These are but extreme and obvious cases in which nature has separated for us two histological elements and two processes, which are elsewhere confounded together.

One of the most complicated of tissues is striped muscle, yet the true homology of its elements seems to us to become intelligible enough upon these principles. Dr. Hyde Salter has pointed out,* that in the tongue the muscles pass directly into the bundles of the submucous connective tissue which serve as their tendons. We have figured such a transition in fig. 6. The tendon A may be seen passing insensibly into the muscle B, the granular sarcous elements of the latter appearing as it were to be deposited in the substance of the tendon (just as the calcareous particles are deposited in bone), at first leaving the tissue about the walls of the cavities of the endoplasts, and that in some other directions, unaltered. These portions, which would have represented the elastic element in ordinary connective tissue, disappear in the centre of the muscular bundle, and the endoplasts are immediately surrounded by muscle, just as, in many specimens of bone, the lacunæ have no distinguishable walls. On the other hand, at the surface of the bundle the representative of the elastic element remains, and often becomes much developed as the sarcolemma. There is no question here of muscle resulting from the contents of fused cells, &c. It is obviously and readily seen to be nothing but a metamorphosis of the periplastic substance, in all respects comparable to that which occurs in ossification, or in the development of tendon. In this case we

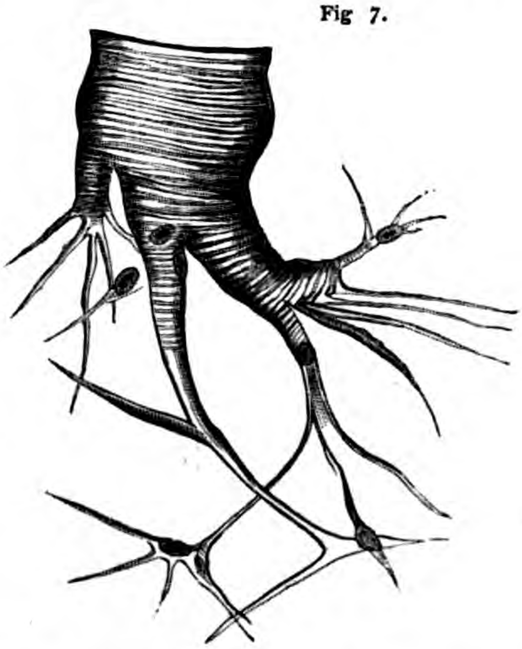
Fig. 6.

Continuity of muscle with connective tissue from the tongue of the lamb. A, Connective tissue; B, muscle.

* Art. 'Tongue,' Todd's Cyclopædia.

might expect, that as there is an areolar form of connective tissue, so we should find some similar arrangement of muscle; and such may indeed be seen very beautifully in the terminations of the branched muscles, as they are called. In fig. 7 the termination of such a muscle, from the lip of the rat, is shown, and the stellate "cells" of areolated connective tissue are seen passing into the divided extremities of the muscular bundle, becoming gradually striated as they do so.

Fig 7.



Branched muscle, ending in stellate connective "cells," from the upper lip of the rat.

We have already exceeded our due limits, and we must therefore reserve for another place the application of these views to other tissues. There is, however, one application of the mode of termination of the branched muscles to which we have just referred, which is of too great physiological importance to be passed over in silence. In the muscle it is obvious enough, that whatever *homology* there may be between the stellate "cells" and the muscular bundles with which they are continuous, there is no *functional analogy*, the stellate bodies having no contractile faculty. But a nervous tubule is developed in essentially the same manner as a muscular fasciculus, the only difference being, that fatty matters take the place of syntonin. Now, it commonly happens that the nerve-tubules terminate in stellate bodies of a precisely similar nature; and these, in this case, are supposed to possess important nervous functions, and go by the name of "ganglionic cells." From what has been said, however, it is clear that these may be genetically and not functionally, connected with the nervous tubules, and that, so far from being *the* essential element of the nervous centres and expansions, it is possible that the "ganglionic cells" have as little nervous function, as the stellate cells in the lip of the rat have contractile function.

We cannot conclude better than by concisely repeating the points to which we have attempted to draw attention in the course of the present article.

We have endeavoured to show that life, so far as it is manifested by structure, is for us nothing but a succession of certain morphological and chemical phenomena in a definite cycle, of whose cause or causes we know nothing; and that, in virtue of their invariable passage through these successive states, living beings have a development, a knowledge of which is necessary to any complete understanding of them. It has been seen that

Von Baer enunciated the law of this development, so far as the organs are concerned; that it is a continually increasing differentiation of that which was at first homogeneous; and that Caspar Friedrich Wolff demonstrated the nature of histological development to be essentially the same, though he erred in some points of detail. We have found Schwann demonstrating for the animal, what was already known for the plant—that the first histological differentiation, in the embryo, is into endoplast and periplast, or, in his own phrase, into a "nucleated cell;" and we have endeavoured to show in what way he was misled into a fundamentally erroneous conception of the homologies of these two primitive constituents in plants and animals —that what he calls the "nucleus" in the animal is not the homologue of the "nucleus" in the plant, but of the primordial utricle.

We have brought forward evidence to the effect, that this primary differentiation is not a necessary preliminary to further organization—that the cells are not machines by which alone further development can take place, nor, even with Dr. Carpenter's restriction (p. 737), are to be considered as "instrumental" to that development. We have tried to show that they are not instruments, but indications—that they are no more the producers of the vital phenomena, than the shells scattered in orderly lines along the sea-beach are the instruments by which the gravitative force of the moon acts upon the ocean. Like these, the cells mark only where the vital tides have been, and how they have acted.

Again, we have failed to discover any satisfactory evidence that the endoplast, once formed, exercises any attractive, metamorphic, or metabolic force upon the periplast; and we have therefore maintained the broad doctrine established by Wolff, that the vital phenomena are not necessarily preceded by organization, nor are in any way the result or effect of formed parts, but that the faculty of manifesting them resides in the matter of which living bodies are composed, as such—or, to use the language of the day, that the "vital forces" are molecular forces.

It will doubtless be said by many, But what guides these molecular forces? Some Cause, some Force, must rule the atoms and determine their arrangement into cells and organs; there must be something, call it what you will—Archæus, "Bildungs-trieb," "Vis Essentialis," Vital Force, Cell-force—by whose energy the vital phenomena in each case are what they are.

We have but one answer to such inquiries: Physiology and Ontology are two sciences which cannot be too carefully kept apart; there may be such entities as causes, powers, and forces, but they are the subjects of the latter, and not of the former science, in which their assumption has hitherto been a mere gaudy cloak for ignorance. For us, physiology is but a branch of the humble philosophy of facts; and when it has ascertained the phenomena presented by living beings and their order, its powers are exhausted. If cause, power, and force, mean anything but convenient names for the mode of association of facts, physiology is powerless to reach them. It is satisfactory to reflect, however, that in this comparatively limited sphere the inquiring mind may yet find much occupation.

T. H. Huxley.

Review II.

1. *Observations on Intestinal Obstructions depending on Internal Causes, and on the Means to be employed for their Relief.* By Benjamin Phillips, F.R.S. ('Med.-Chir. Trans.' vol. 31.)
2. *Case of Stricture of the Colon, successfully treated by Operation; with an Analysis of Forty-four Cases of Artificial Anus.* By Cæsar Hawkins, Senior Surgeon to St. George's Hospital, and President of the Royal College of Surgeons of England. ('Med.-Chir. Trans.' vol. 35.)
3. *Remarkable Case of Foreign Bodies in the Stomach and Duodenum, Complete Obstruction of the Bowel, and Mechanical Displacement of Organs.* By John Marshall, Esq. ('Med.-Chir. Trans.' vol. 35.)
4. *Two large masses of Hair and String matted together, taken from the Stomach and Duodenum, and which produced Obstruction and Death.* By George Pollock. ('Trans. Patho. Soc. of London.' 1852-53. p. 327.)
5. *A Case of Constipation of Nine Days' duration from Scirrhous Rectum, relieved by Opening the Sigmoid Flexure of the Colon in the Left Groin.* By John Adams, Surgeon to the London Hospital. ('Med.-Chir. Trans.' vol. 35.)
6. *An Account of Two Cases of Intestinal Obstructions, in which the Operation for the Formation of an Artificial Anus was performed—one in the Ascending, the other in the Descending, Colon.* By William J. Clement, of Shrewsbury, F.R.S. ('Med.-Chir. Trans.' vol. 35.)
7. *A Case of Intestinal Obstruction from Disease of the Rectum, treated successfully by Opening the Descending Colon in the Left Loin.* By Alfred Baker, Surgeon to the General Hospital, Birmingham. ('Med.-Chir. Trans.' vol. 35.)
8. *Case of Intestinal Obstruction.* By J. Luke, Vice-President of the Royal College of Surgeons of England, and Senior Surgeon to the London Hospital.
9. *Intestinal Obstruction of Twelve Days' duration from Stricture of the Ascending Colon, with Operation for Inguinal Hernia, supposed to be Strangulated.* By Alexander Shaw, Esq., Surgeon to the Middlesex Hospital. ('Trans. Patho. Soc. of London.' 1852-53.)
10. *Internal Strangulation of a portion of the Small Intestine by a strong Fibrous Band connected with the Mesentery.* By N. Ward, Esq., Assistant-Surgeon to the London Hospital. ('Trans. Patho. Soc. of London.' 1852-53.)

Mr. Phillips, has observed in the commencement of his paper, which stands first on our list, that—

"At this advanced period in the history of medicine, it might reasonably be supposed that a disease must be either very insignificant in its consequences, or of a very uncommon occurrence, if it have escaped very ample, and it may be sufficient, consideration; and no doubt the expectation would be usually realized. But there are diseases which are neither uncommon nor insignificant, and yet their history remains to be told; and of the number, are obstructions of the bowels, dependent on internal mechanical causes."

Since the publication of the paper from which the foregoing extract has been borrowed, the medical periodicals and the medical societies have repeatedly furnished us with the particulars of cases which bear upon this subject, accompanied with many most valuable observations, especially illustrating certain points of treatment. Such cases, and such observations, clinically considered, are of the utmost importance, when any additional facts or suggestions, in relation to a subject surrounded with so much that is obscure and difficult, must be received with satisfaction, and with a hope that something more valuable may follow.

Under present circumstances, and with our past experience, should we be right in considering that the "history" of this class of cases has been rendered complete?—Are we wrong if we still consider that "their history remains to be told"? To Mr. Phillips and to Mr. Cæsar Hawkins the thanks of the profession are eminently due for having especially drawn attention to this subject, by their excellent communications to the Medico-Chirurgical Society. With every wish to render all credit to those others, who have devoted any attention to the subject of obstructions of the intestines, dependent on internal causes, we venture to hope that we shall not have wasted the time of our readers, nor employed our own to disadvantage, should we endeavour to add something to a chapter in their history, by entering into an examination of the subject, and sketching out some general arrangements of the causes of these obstructions, accompanied with the observations lately brought to our notice with respect to the remedies by which these obstructions are to be met.

The consideration of all the various interesting and important practical points, in connexion with the cases and communications which introduce this article to our readers, accompanied with an examination as complete and satisfactory as our inclination would dictate, or as the subject demands, should embrace the consideration of all that is known of the pathology and treatment of obstructions of the intestinal canal—all that has come within our experience and observation—and much that may be found in the medical literature of the past and present period. But on this occasion, we cannot hope, as neither our space nor our time will allow us, to chronicle all that is concerned in the origin, progress, treatment, and termination of the varieties of these affections.

Nor do we consider that any apology is necessary for introducing in a prominent position, and for special notice, such a subject in such a manner, particularly as the attention and eminent experience of many surgeons have been so happily directed to the consideration of some of the difficulties inherent to these cases, and as there has been added by them something that is valuable to this division of practical medicine. Only within a comparatively recent period has any great attention been directed to this class of cases. As regards the treatment of many of them, this remark most especially holds good: and though, formerly, pathological knowledge and practical experience, no doubt in certain instances correctly, diagnosed the causes of obstruction, surgeons had not yet been emboldened to carry out measures, which now stand approved of, by the results of experiment, and are not only considered serviceable, but essential in treatment, as tested by the beneficial effects of their application.

In the consideration of the causes of obstruction of the intestinal canal dependent on certain internal mechanical or diseased conditions, it is necessary to dismiss, as foreign to our division of the subject, those causes of obstruction dependent on the varieties of umbilical, femoral, or inguinal herniæ.

We reserve for our investigation—Firstly, Those causes of obstruction which originate in and implicate the muscular and mucous walls of the intestine. Secondly, Those causes which act from without, or affect the serous covering of the bowel. Thirdly, Those collections of matter, or lodgments of foreign substances, which occur within and obstruct the passage of the bowel.

Each of these divisions possesses its own variety of diseases; and it may be more satisfactory to have them placed before us, previous to the consideration of that most important question—viz., the question involving the amount of relief that can be offered, or the remedy that can be applied, in the individual cases.

In the first division, in which the causes of obstruction originate in or implicate the muscular or mucous walls of the intestine, we find—

a. Cancer—cancerous stricture.
b. Contraction of cicatrices following ulcerations. } Non-cancerous stricture.
c. Contractions of walls of intestine, the result of inflammation, non-cancerous deposit, or injury. } Non-cancerous stricture.
d. Non-cancerous growths, in the form of polypi, projecting into and obstructing the cavity of the intestine.
e. Intus-susception and its results.

In the second division, in which the causes act from without, or affect the serous covering of the bowel, we observe—

a. Effusions of lymph, in the form of bands or layers, by contraction pressing on the intestine, and producing obstruction.
b. Loops or openings, formed by adhesions of omentum or intestine, "holes or fissures (congenital or acquired) in mesentery,"* &c., in which portions of bowel become entangled and strangulated.
c. Effusions of lymph, by which portions of intestine become adherent to each other at acute angles.
d. Diverticula of the small intestines, either by their own adhesions or the adhesions of cords frequently attached to them, producing pressure, or loops in which bowel becomes strangulated.
e. Peritonitis—local, general, scrofulous, &c.
f. Strangulations through natural openings—foramen of Winslow, &c.
g. Tumours or abscesses pressing upon, or implicating secondarily, the structure of the intestine.

In the third division, in which collections of matter or lodgments of foreign bodies occur within and obstruct the bowel, we may place—

a. Fæculent matter.
b. Calculi.
c. Foreign bodies swallowed by accident, or under other circumstances.

* Rokitansky, vol. ii. p. 53.

There are also some additional causes which cannot be included under any one of these divisions, to which an independent and separate consideration should be given, and which should rather be appended to the foregoing than be placed with them. The following appear to us the exceptional causes:

1. Displacements or twistings of the bowels.
2. Congenital malformations, causing obstructions.
3. Ileus.

That these divisions are open to certain objections there is no doubt—both anatomically and pathologically considered, they are neither so accurate nor so defined as they otherwise might be, did our space at present allow of more than a general review of these affections. The highest aim of medical philosophy is the endeavour to examine into and unravel the causes of disease, before it can satisfactorily contemplate its effects, or hope to apply to those effects measures of a remediable or palliative nature. At present, however, we wish rather to consider the ascertained effects than the causes, in conjunction with those measures by which these effects may be removed, or rendered less. In greater detail, and for more minute accuracy, we should necessarily examine into the particular characteristics of each form of disease—and on a future occasion we hope to do so to some extent—but, as it is, the above divisions appear to us ample for our purpose, and satisfactory for the elucidation of our subject-matter; in fact, equivalent for the illustration of the most important points occurring in the cases selected for our notice.

One important point to which an investigation must be directed, and which in importance is co-equal to an accurate knowledge of the nature and situation of the disease itself, is the *relative frequency* of the various diseases or disorders met with in the human subject giving rise to these obstructions—a point most important in assisting us to decide upon the probability of relief or remedy—and in allowing us to assume a favourable or adverse prognosis. In commencing this investigation, let us first consider the causes of obstruction mentioned by Mr. Phillips. He states, that in 169 cases of obstruction, the causes may be divided in the following manner:

	No. of cases.
1. From invagination of the intestine	63
2. From tumours pressing upon ditto	16
3. From stricture, the result of disease of the parietes	19
4. From intra-intestinal tumours, collections of fæces, or concretions	11
5. From constrictions, bands, adhesions, strangulations through abnormal openings, or by twists of intestine	60
	169

Of these 169 cases Mr. Phillips states that he found 136 on record. We have classed them according to his own arrangement and division; but we must confess that we have some hesitation in offering a collection of cases thus arranged as one from which we should be inclined to draw accurate or satisfactory deductions—either, in the first place, as to the causes of obstructions; or, in the second place, as to the relative frequency of occurrence of such cause; or, lastly, as to the considerations of treatment. For, as to causes, we find too many varieties placed together; as to the relative frequency of occurrence of each cause, the numbers do not therefore assist us in deciding how often most of the causes were met with; and as to considerations of treatment, the want of accurate minutiæ in the former would necessarily confine or render somewhat indefinite the principles of the latter.

We trust we shall hereafter be able to point out how very desirable an object it is to obtain something like accuracy of detail in the consideration of each cause of obstruction. It must not be supposed that by these observations, indicating a certain deficiency, we wish to detract from the merit due to Mr. Phillips for the valuable paper with which he has furnished us. He has collected materials far and near, and has constructed from them, and from his own experience and opportunities of observation, that which well deserves, not only the consideration, but the commendation of the profession.

Our chief object in this, as in every other inquiry in medicine, must be an approach towards some general principle with regard to the manner in which we should view this class of cases, so that we may apply our remedies with something like precision, and a prospect of success. But a principle without facts, in our department of medicine, would be something analogous to the superstructure built upon a sandy foundation; whereas the facts to establish the principle must be authenticated and accurate, otherwise they would, as a foundation upon which to build our principles, be as useless as the sand itself.

Mr. Cæsar Hawkins has drawn up a table of cases, in which altogether 47 instances of obstructed bowel are recorded, and in which record, careful examination and accuracy of detail provide us with much more satisfactory evidence respecting the actual disease that produced, and the relative frequency of the cause of, the obstruction. We find the causes to be as thus divided:

			No. of cases.
1.	From cancerous stricture	18	20
2.	From cancer of sigmoid flexure or omentum	1	
3.	Adhesion of ileum and rectum to cancerous uterus	1	
4.	From stricture supposed to be non-cancerous		20
5.	From twist of the intestine		1
6.	From band producing strangulation		1
7.	From adhesion of rectum to uterus after abscess		1
8.	From fibrous tumour pressing on rectum		1
9.	From ulceration of rectum and contraction		1
10.	Unknown and doubtful		2
			47

The published 'Transactions' of the Pathological Society of London have furnished us with the particulars of 32 cases, in which various causes of obstruction existed, and in most of them the obstruction was the cause of death. None of these 32 cases have been included in Mr. Hawkins' list; and that we may follow out an accurate investigation of them, we have tabulated them together in a condensed form, in order that reference may be readily made to them by our readers, while they peruse our examination of the causes of obstruction therein found and described.

TABLE OF THIRTY-TWO CASES OF INTESTINAL OBSTRUCTION.

No.	Sex.	Age.	Reference to account of case.	Disease or Cause of Obstruction.	Symptoms.	Remarks.
1	F.	46	. . Pathol. Trans., vol. i. p. 66, Dr. Barlow.	. . Stricture of rectum ten inches from anus; fish-bone lodged in it; admitted passage of little finger.	. . Complete constipation nineteen days before death; passed little water, and with pain; constant vomiting; peritonitis.	. . General health good until two months before death, when suffered from constipation and difficulty in passing motions.
2	M.	16	. . Ibid. vol. i. p. 67, Mr. Busk.	. . Stricture of rectum from medullary sarcoma deposited external to muscular fibres, pressing on intestine, three to four inches from anus; ulceration of mucous membrane.	. . Death from peritonitis of acute character; other symptoms not mentioned.	
3	F.	34	. . Ibid. vol. i. p. 68, Dr. O. Ward & Mr. Hewett.	. . Fibrous polypus of size and shape of pear, connected by long pedicle to inner surface of bowel; and invagination of small intestine about three feet from cæcum.	. . Vomiting and constipation seven days previous to death; no peritonitis.	. . Invaginated portion easily drawn out; no marks of inflammation on surface; mucous membrane dull and grey, and of gangrenous odour; immediately below invagination the polypus was attached, and the tumour was not, therefore, apparently connected with the invagination.
4	M.	46	. . Ibid. vol. i. p. 68, Mr. Hewett.	. . Fibrous polypus of intestine, and invagination of ileum two feet from the cæcum.	. . Obstinate constipation of eight days' duration was followed by diarrhœa, which continued till death; peritonitis; escape of fæculent matter.	. . Above the invagination there was enormous dilatation of intestine, and below slight contraction; the polypus depended from the invaginated portion of gut into the cavity of the portion below not invaginated. The polypus was two and a half inches long, and one and a half in diameter; and attached by pedicle to the inner surface of the bowel. The peritoneal surfaces of the invaginated portion had firmly united, and the portion itself was dark and livid. In fact, it had nearly separated with the polypus attached from the gut immediately above the invagination, as a large ulcer passing two-thirds round the inner surface of the bowel had almost allowed this to occur.

5	M.	65	. . Ibid. vol. i. p. 70, Dr. Peacock.	. . Portion of ileum strangulated by passing through opening formed by fibrous band attached to omentum and caput cæci and Poupart's ligament; oval polypus the size of a marble, attached by broad base to inner surface about two inches above cæcum, and so encroached on canal that the forefinger could barely pass.	. . Constipation six days before death; peritonitis.	. . The adhesions in this case were numerous; above the contraction there was much distension of the bowel. *A long band of omentum had furrowed another portion of bowel.*
6	F.	67	. . Ibid. vol. i. p. 73, Mr. Hewett.	. . Obturator hernia, complicated with reducible femoral hernia.	. . Stercoraceous vomiting; symptoms of strangulation six days before death.	. . Old hernial sac, small size, laid open, but found empty during life; narrow opening leading from it into cavity of peritoneum. Two-thirds diameter of small intestine eight feet above cæcum, strangulated in left obturator foramen.
7	M.	28	. . Ibid. vol. i. p. 76, Mr. Busk.	. . Twisting of sigmoid flexure of colon.	. . Constipation five days before death; had suffered from previous attacks of constipation.	. . Sigmoid flexure occupied the greater portion of the abdomen, and was gangrenous; *rupture of the serous coat* before death.
8	M.	65	. . Ibid. vol. i. p. 76, Mr. Pollock.	. . Twisting of the sigmoid flexure of colon.	. . Tendency to diarrhœa at first; constipation not complete; great distension, great pain, and constant vomiting; peritonitis.	. . Sigmoid flexure occupied the greater portion of the abdomen; mucous membrane of sigmoid portion dark livid colour; the part that was twisted still allowed slight communications with upper and lower portions of bowel.
9	M. (a sailor.)	. .	. . Ibid. vol. i. p. 77, Dr. R. Quain.	. . Intus-suception of ileum, six inches in length, near cæcum.	. . No account to be obtained beyond his lying down in railway carriage while on a journey, and found dead at the end of it.	. . Portion of bowel intus-suscepted in a gangrenous condition.
10	F.	27	. . Ibid. vol. i. p. 255, Dr. Peacock.	. . Biliary calculus lodged in ileum, having escaped through ulcerated opening between gall-bladder and duodenum.	. . Constipation six days; vomiting; peritonitis.	. . The adhesions between gall-bladder and duodenum gave way while vomiting, and contents of bowel escaped into peritoneum.
11	M.	36	. . Ibid. vol. i. p. 262, Dr. Fuller.	. . Displacement of sigmoid flexure, and enormous distension of bowel; obstruction as if from want of muscular action.	. . Constipation seven days before death, subsequent to previous dysentery; vomiting; peritonitis.	. . The sigmoid flexure filled the greater part of the abdomen. The mucous membrane of this portion most extensively destroyed by old ulceration.

TABLE OF CASES—*continued.*

No.	Sex.	Age.	Reference to account of case.	Disease or Cause of Obstruction.	Symptoms.	Remarks.
12	F.	68	.. Pathol. Trans., vol. i. p. 265, Dr. Thurnam.	.. Cancerous stricture of transverse colon, involving muscular and mucous coats.	.. Constipation with complete obstruction; fæcal matter and gas passed through fistulous opening; blood in motions.	.. Fistulous openings formed during life externally through abdominal wall, with portion of bowel above stricture; patient lived five months afterwards; stricture barely allowed little finger to pass after death.
13	M.	35	.. Ibid. vol. i. p. 267, Mr. W. Adams.	Colloid cancer of rectum	.. Constipation; sometimes diarrhœa; fæcal vomiting; great pain.	.. Transverse colon dragged down, and adherent to diseased mass; sigmoid flexure remarkably twisted and involved in the deposit; transverse colon, sigmoid flexure, and rectum, each opened into cavity formed by mass of colloid deposit.
14	M.	28	Ibid	.. Ileum strangulated through loop formed in omentum.	Constipation five days; peritonitis . . .	Bowel perforated by ulceration.
15	M.	$4\frac{1}{4}$	.. Ibid. vol. ii. p. 63, Dr. Hare.	.. Intus-susception of ileum through ileo-cæcal valve into colon, for three inches and a half, firmly held by valve.	.. Constipation four days before death; sudden pain: blood passed by motions; constant desire to go to stool; intense abdominal pain; vomiting; *ascarides.*	.. The appendix was carried through the ileo-cæcal valve with the intus-suscepted portion of ileum. *The strangulated portion was so firmly held by the valve, and so swollen, that reduction appeared impossible without tearing* the bowel; a second intus-susception existed higher up, of slight extent.
16	Child.	6 m.	.. Ibid. vol. ii. p. 56, Mr. E. Smith.	.. Intus-susception of cæcum, ascending and transverse colon, into sigmoid flexure.	.. Obstinate constipation; vomiting; bloody mucous discharge from rectum, sixty hours before death.	.. The mucous membrane was purple from strangulation.
17	F.	22	.. Ibid. vol. ii. p. 56, Dr. Peacock.	.. Stricture of sigmoid flexure of colon.	.. Habitual constipation and dyspepsia for several months; sudden pain and vomiting; constipation rather more than three weeks before death; tenderness and fulness in course of descending colon; some scybala passed with enema, and several dark fœtid motions, with shreds like membrane; peritonitis.	.. Colon was of deep brown colour, and distended from cæcum to sigmoid flexure; coats of intestine at strictured portion much thickened and indurated, and upper part of stricture surrounded by large deep ulcer. From this point to cæcum the mucous membrane was gangrenous.

18	F.	18	.. Ibid. vol. ii. p. 58, Mr. Hewett.	.. A pin covered with calcareous matter lodged in the appendix cæci; portion of cæcum and appendix attached to pelvis.	.. Not particularly mentioned; death appeared rather sudden; bowels enormously distended; no peritonitis.	.. Occupying the cæcum was a portion of the pin, surrounded by calcareous matter, like a large-headed nail, the remainder of it being in the appendix. The cæcum and appendix were both perforated by ulceration.
19	M.	30	.. Ibid. vol. ii. p. 60, Dr. Peacock.	Meso-colic hernia	None; death from typhus	.. On the abdomen being opened, none of the small intestines were to be seen; they were concealed in a sac formed in the folds of the meso-colon, behind the transverse colon, which dipped down towards the brim of the pelvis.
20	M.	27	.. Ibid. vol. ii. p. 60, Dr. Peacock.	Meso-colic hernia	.. Pain in abdomen; vomiting of fæcal character; constipation; forty-one hours after symptoms commenced, death.	.. Descending colon pushed to right side, lying close to cæcum. The small intestine contained in sac formed by folds of left meso-colon. The ileum passed out of sac about two inches above cæcum, and at that point was contracted, thickened, and gangrenous.
21	M.	20	.. Ibid. vol. ii. p. 62, Mr. Avery.	.. Constriction of ileum by very slight process of membrane attached between the mesentery of ileum and vermiform process.	.. Abdomen greatly distended; stercoraceous vomiting; constipation fifteen days before death; had suffered several times previously from painful attacks of the bowels; *no peritonitis.*	.. Small intestines greatly distended, and the large contracted; near termination of ileum was a portion of it two feet long, much distended and darker than any other portion, and firmly constricted at its upper and lower portions, especially at the latter, where it was crossed by the band.
22	M.	50	.. Ibid. vol. ii. p. 218, Mr. Ward.	.. Stricture of rectum about twelve inches from anus, complicated with old reducible hernia.	.. Constipation nine days before death; uneasiness about abdomen; vomiting; on fifth day, while vomiting, felt his rupture descend suddenly, but after three or four hours he managed to return it.	.. As it was considered that the hernia might have been reduced *en masse*, Mr. Luke made an exploratory incision into the inguinal canal, and opened the abdomen at the upper and outer part of the internal ring; but as nothing was observed to indicate obstruction, the operation was terminated. An ordinary-sized probe could only be passed through the stricture.
23	F.	47	.. Ibid. vol. iii. p. 94, Mr. Stanley.	.. Obturator hernia complicated with femoral hernia (omental).	.. Constipation three weeks before death; stercoraceous vomiting; as femoral hernia existed, an operation was performed for its relief, but a portion of omentum was only found in it, not strangulated.	.. A portion of ileum surrounded by omentum was found strangulated in the obturator foramen; bowel not sufficiently constricted to cause its strangulation; and some doubt was expressed by author of case, whether this was the sole cause of the constipation.

TABLE OF CASES—*continued.*

No.	Sex.	Age.	Reference to account of case.	Disease or Cause of Obstruction.	Symptoms.	Remarks.
24	F.	34	.. Pathol. Trans., vol. iii. p. 95, Mr. Obré.	.. Stricture of ileum a little above cæcum, following shortly after strangulated femoral hernia, which was relieved at the time by operation.	.. Six months after operation, flatulence and pain after food; occasional vomiting; bowels acted, but abdomen increased in size; enormous distension of bowels; at last constant vomiting.	.. The small intestines were filled to the utmost; they were lying vertically in front of the other viscera; a few dark points of gangrene on them; one had given way. The contraction about twelve inches from cæcum: at this point the intestine was so thickened, that water would only drip through it. This portion had apparently adhered to the inguinal ring; but from its altered position and distension, the adhesion was stretched into a band seventeen and a half inches long.
25	M.	42	.. Ibid. vol. iii. p. 101, Mr. Gay.	.. Internal strangulation of small intestine, between appendix adherent to ileum and band of false membrane. Gastrotomy.	.. Pain in abdomen; vomiting and constipation five days before operation; fæcal vomiting on fifth day; abdomen hard and tumid; had suffered from several previous attacks of constipation and sickness.	.. Died morning after operation of opening abdomen, when the strangulation was relieved; sickness ceased, but no evacuation after operation; recent peritonitis seen after death. The strangulated portion had been quite relieved by the operation.
26	M.	34	.. Ibid. vol. iii. p 106, Mr. Callaway.	.. Enormous dilatation of sigmoid flexure of colon.	.. Sickness, pain and tension of abdomen; bowels acted freely after some medicine, but on the second day distension increased; no amount of action of bowels six days before death; long tube relieved bowel of flatus, but no mention of fæces passing through it.	.. In this case there is no account of twist, displacement or constriction; but obstruction from some cause produced similar effects. The bowel was punctured by a fine trochar needle, and gave great relief; nor did its use indicate any bad effects, the punctures being hardly visible after death. The coats of the whole large intestine were thickened; *very small quantity of lymph was smeared over intestinal wall.*
27	F.	46	.. Ibid. vol. iii. p. 111, Dr. Hare.	.. Strangulation of five feet of ileum, in opening formed by adhesion of omentum to parietes of abdomen; adhesion of small intestine to cancerous uterus, producing secondary slight constriction of bowel.	.. Vomiting; griping pains; constipation, though not quite complete, very considerable for some five weeks before death.	.. The sub-peritoneal tissue was infiltrated with granules of cancerous matter; most abundant in pelvis, and probably influencing the adhesion of the omentum; the latter was folded up into a flattened cord-like body.

28	F.	18	. . Ibid. vol. iii., Mr. Pollock.	. . Large collections of hair and string matted together, and impacted in the stomach and small intestine, the duodenum, and upper portion of jejunum.	. . Constant vomiting, and difficulty in partaking of solid food; pain in abdomen; tumour, *which was moveable*, to be felt in epigastrium.	. . The stomach was much enlarged, and the upper portion of small intestine much distended; she appeared healthy, and had not been observed to eat hair since a child, when she was in the habit of putting it into her mouth. Peritonitis was general.
29	M.	75	. . Ibid. vol. iii.	. . Cancerous stricture of colon, complicated with omental inguinal hernia; for the latter, operation performed.	. . Twelve days before seen the hernia descended, the vomiting commenced, and the constipation became evident; during operation, passed a small quantity of fæces, but subsequently bowels did not again act; urine of usual quantity; death four days after operation.	. . The stricture was situated at the commencement of the transverse arch; it was an inch and a half in length, and so narrow, that a goose-quill could not pass through it. Cancerous matter was deposited in and around the walls of intestine. *The colon beyond the stricture was loaded with large lumps of fæces.*
30	F.	6	. . Ibid. vol. iii. p. 362, Mr. Ward.	. . Strangulation of small intestine by a ring formed by adhesion of false membrane.	. . Constant vomiting and constipation four days before death; not much pain.	. . Intestine was dark, livid and lustreless; slight recent peritonitis.
31	F.	37	. . Ibid. vol. iii. p. 365, Mr. Ward.	. . Mass of gall-stones lodged in small intestine, producing chronic inflammation and thickening of lower portion of ileum and cæcum, with ulceration of mucous membrane.	. . Prior to recent illness had severe bilious attack and constipation; subsequently, chronic diarrhœa, with constant vomiting before death.	. . Ileum three inches from valve, much contracted, and mucous membrane entirely destroyed; mass of gall-stones, two inches long and one and a quarter inch in diameter, impacted three feet from cæcum; gut ulcerated and thickened, and strictured below it; large gall-stones in ileum and cæcum in addition to the first mass.
32	M.	14	. . Ibid. vol. iii. p. 366, Mr. Beale.	. . Diverticulum of ileum containing orange-pips, cherry-stone, &c.; perforation from ulceration.	. . Vomiting; constipation eight days before death; acute peritonitis; subject to previous attacks of constipation.	. . Diverticulum about fifteen inches from cæcum, of the same diameter as the intestine at its origin, but increased considerably as it extended into pelvis; at extremity at least twice as broad as at attachment; about three and a half inches long. At upper part its coats were thick, and resembled that of intestine; but its lower half was thin and rotten, and was readily torn while being examined.

We now wish to draw attention to the relative number of the causes of obstruction included in this list of cases, and for this purpose we divide them as we have already done those which Mr. Cæsar Hawkins collected in his table.*

	No. of cases.
1. From cancerous stricture	4
2. From non-cancerous stricture 3. From stricture after hernia, 1	4
4. From internal strangulations by bands, adhesions, &c.	6
5. Displacements, twisting, and of sigmoid flexure	4
6. Foreign bodies or calculi lodged in intestine	4
7. Diverticulum containing foreign bodies	1
8. Intus-susception	3
9. Fibrous polypi, associated with intus-susception	2
10. Meso-colic hernia	2
11. Obturator hernia	2
	32

In the investigation of such an important subject as that upon which we have now entered, one great difficulty is to offer sufficient evidence by illustration of cases, without tiring the patience of the reader, or rendering this article a tedious essay. This difficulty we have experienced; for, in the analysis of such papers as we have undertaken, it is our bounden duty, if we accord merit to the authors of those writings, or venture to criticize their recorded opinions—it is our bounden duty, in justice to them and to ourselves, to prove by facts, rather than by theories, or by both, if in our power, that where we bestow praise, it is not from flattery—where we differ, it is not from prejudice.

To test the value of these tables (that given by Mr. Hawkins, and the one drawn up from the cases presented to the Pathological Society) by merely recording many other cases, would be a matter of no very great difficulty. But the objection that weighs against such a proceeding, is, that there is wanting, in the record of many of these cases which have been published from time to time, that accuracy of detail respecting the post-mortem conditions, which renders so many of them void, as evidence or a test, where accurate pathological observations are so essential an element in the sifting of evidence, to establish the relative, or even positive, occurrence of obstruction. In this difficulty we have, though with some hesitation, determined to introduce, as a comparison to the summary of cases reported by Mr. Hawkins and by the Pathological Society, a summary of the pathological specimens illustrating this subject in the collection of the Museum of the College of Surgeons. Objection will, no doubt, be made to a collection in a museum being brought forward as evidence to test the accuracy of a statement respecting the *relative frequency* of a disease, or to prove the truth of any *statistics* of medicine or surgery; and in most diseases—we might say almost all—the objection would be held on right principles; but in a disease affecting a set of organs as the intestines, and which disease has, in most cases, so rapidly and invariably proved fatal in its result, a museum like that from which we have selected the following list, may

* Some cases published in the Pathological Society's Transactions have been omitted, as they have already appeared in Mr. Cæsar Hawkins' table of cases. See Med.-Chir. Trans., vol. xxxv.

surely be taken as evidence which furnishes something of a comparison, regarding the relative frequency of the causes of obstruction affecting the intestinal canal.

From the collection in the Museum of the College we find the following causes of obstruction:

	No. of cases.
1. From cancerous obstructions	14
2. From non-cancerous strictures	16
3. From adhesions of intestines	3
4. From intus-susception	8
5. From polypus, warty growths, or epithelial cancer, associated with intus-susception	3
6. From foreign bodies	3
7. From obturator hernia	1
	48

If we now take into consideration these three masses of evidence, they will at any rate furnish us with one hundred and twenty-seven cases for analysis; and though, as far as symptoms are concerned, the specimens from the College are, in many instances, devoid of history, still with respect to the actual seat of disease, they offer much that is valuable for our notice.

	No. of cases.
From Mr. Hawkins' table we have taken	47
From the Pathological Society's 'Transactions'	32
From the Museum of the College	48
	127

From these we have arranged the following table:

	Mr. Hawkins.	Path. Soc.	Coll. Mus.	Total.
1. Cancerous obstructions	20	4	14	38
2. Non-cancerous stricture	21	4	16	41
3. Twists or displacements	1	4	0	5
4. Bands producing internal strangulation	1	6	3	10
5. Obstruction from external tumours or abscesses	2	0	0	2
6. Foreign bodies, calculi, &c.	0	4	3	7
7. Diverticulum	0	1	0	1
8. Intus-susception	0	3	8	11
9. Ditto, associated with polypi or other growths	0	2	3	5
10. Meso-colic hernia	0	2	0	2
11. Obturator hernia	0	2	1	3
12. Unknown	2	0	0	2
				127

If we are permitted to draw any conclusions from the evidence now before us, the conclusions must necessarily be, that in a large majority of cases, especially in advanced life, the causes of obstruction will be dependent on stricture of the intestine—the result of cancer affecting its walls, or some other deposit or disease existing therein—producing thickening, gradual contraction or diminution of the diameter of the bowel, and ultimate and effectual impediment of the passage of its contents, if the disease is beyond the reach of local treatment.

It is interesting, before we consider the effects of the various causes of obstructions, to note more particularly the relative situation of these strictures in the large and small intestine, as far as our cases enable us to do so; for in alluding to the question of treatment, we shall have again to refer to these points. Separating the various specimens of stricture depending on an altered condition of the bowel itself from all other causes of obstruction, we observe them to have affected the intestine in the proportions noted in the following table:

Portion of Intestine.	Cases of Cancer.					Cases Non-cancerous.				
	Mr. H.	P. S.	C. M.			Mr. H.	P. S.	C. M.		
Small intestine	0	0	3	3		0	1	1	2	
Large intestine, colon	0	2	2	4		4	0	2	6	
Ileo-cæcal valve & cæcum	1	0	0	1		0	0	1	1	
Sigmoid flexure	3	0	2	5	29*	11	1	0	12	31*
Rectum	15	2	7	24		5	2	12	19	
Total of cancerous cases				37		Total non-cancerous			40	

From this table we find that 77 cases of stricture of the intestine were observed in 127 cases of obstruction, and that in 60 of these the situation of the stricture was *below* the descending colon, either affecting the sigmoid flexure or the rectum.

We have already observed that the cases we have selected should not be considered as pre-eminent examples for the purposes of analysis or comparison; and we cannot but feel the force of an objection which might be urged against this latter table, as fallacious in affording any correct data with respect to the relative frequency of the occurrence and seat of stricture of the intestinal canal. We allude to the fact of taking Mr. Hawkins' table of cases, as furnishing any indications of the general causes of obstruction, when probably many or most of those cases were selected by the operators as favourable instances for treatment, in consequence of the cause of stricture being suspected or known to exist low down in the gut. But upon a careful perusal of this table (i.e. Mr. Hawkins'†) it is not presumptuous to conclude, that before very many of the operations were performed, great doubts existed as to the exact seat of the stricture; that the diagnosis was but in comparatively few cases accurate or certain; and that the cases were decided to be operated on, as often on account of the urgency and danger of the symptoms, as from the circumstance of the causes of that urgency and danger being positively ascertained. At any rate, it is only by examining such accurately recorded and well authenticated cases that we shall be enabled to arrive at any established conclusions as to their condition and frequency; and if the tables now put forth should be objected to on account of the reasons we have mentioned, it will, we hope, be conceded, that though we have not quite arrived at the truth, and that therefore our conclusions are in this respect premature, yet that we have advanced towards an approximation of the truth, and that this investigation has, so far, not been unprofitable.

Respecting the effects of obstructions, both locally and generally, there

* Total cases *below* descending colon.

† Med.-Chir. Transactions, vol. xxxv.

is a great deal that is interesting to reward us for our enquiry, and much that is valuable to repay us for an investigation. Careful enquiry may elicit something that should lead to beneficial practical results. Minute investigation must yield more towards the elucidation of its numerous pathological obscurities.

Mr. Cæsar Hawkins has so clearly noted for us the causes of death in 30 out of 43 cases mentioned in his first table, that we cannot do better than transfer the statement in his own language to these sheets. He says—

"Of the 21 cases which did not recover from the operation [for artificial anus] the assigned causes of death were—

"In one case, two pounds of mercury given previously, dragging the ileum into the pelvis.

"In one case, unrelieved, the obstruction being above the opening.

"In one case, fæcal evacuations incomplete.

"In five cases, exhausted by the disease.

a. Cæcum burst and fæces escaped into pelvis.
b. Ulceration of bowel.
c. Rupture of six inches of peritoneal coat from distension.

"In seven cases, peritonitis.

a. Old, as well as recent.
b. From the operation.
c. Begun before operation.
d. From cancerous ulcer.

"In one case, unknown, but cancerous.

"In one case, chiefly sloughing of sacrum.

"The deaths of nine patients who recovered from the operation have been recorded, of which—

"Four cases were cancerous: one died of phthisis; one of the disease and dropsy; and two of the disease.

"Five cases were stricture of the colon and rectum not cancerous."

In the appendix added to the table there are four more cases mentioned, all of which are dead; one died unrelieved by the operation, one had rupture of the peritoneal coat in three or four places, one died of sloughing of the wound, and one apparently worn out by the disease nearly five months after the operation. It is probable that at this present time only two of the cases tabulated by Mr. Hawkins are alive.

In taking a hasty view of the effects related to have occurred in the cases tabulated from the 'Pathological Transactions,' we observe that

In four cases of simple stricture (Nos. 1, 17, 22, 24,) two cases presented evidence of peritonitis: the first after nineteen days' constipation; the second after three weeks' constipation; and in the latter gangrene of the mucous membrane had commenced; the third case sank after nine days' constipation; the fourth died from the effects of long-continued obstruction, though to the last the obstruction was not complete.

In four cases of cancerous stricture, all of the larger intestines, one died of peritonitis, history not complete; in the second a spontaneous communication externally occurred some time before death, above the stricture; the last two sank exhausted, one after sixteen days' constipation.

In ten cases of obstruction resulting from internal strangulation, obturator and meso-colic hernia, four died, either from peritonitis or gangrenous intestine; one from perforation of the intestine; though in one case of constipation of fifteen days, and in another of three weeks, there were no marked symptoms of peritonitis.

In four cases of twists &c. of intestine, there were indications of peritonitis, or aggravated mischief in the bowel itself; six or seven days appear the longest period of constipation in these cases.

In three cases of intus-susception, the bowel was seriously affected by inflammation in two of them; they all terminated fatally within a few days, four days being the longest period of duration mentioned.

In two cases of fibrous polypi complicated with intus-susception, one had gangrene of the intestine; the other extensive ulceration of the bowel and peritonitis.

In five cases of foreign bodies lodged in the bowel, one had perforation of the intestine; one sank after long-continued irritation and ulceration of the bowel; two had peritonitis; and one sank under sudden collapse.

The longest period of constipation noted amongst these cases was that of three weeks, occurring in a case of obturator hernia (No. 23), in which no active symptoms of peritonitis existed. The shortest period in which death occurred after symptoms of obstruction were noted, was forty-one hours, in a case of internal strangulation. The length of time that constipation may persist, unattended with any serious local mischief, is a matter as surprising as it is interesting, with respect to the consideration of our remedies in such cases. It is almost incredible to what an extent the bowels can adapt themselves to their daily increasing contents, or accommodate themselves to difficulties which are imposed upon them by degrees, not hastily, or continued for too long a period.

In two cases in Mr. Hawkins' table, it is stated that constipation had endured for forty days, operation for artifical anus was performed, and as regards the immediate condition of the patients, they recovered. In another case in which constipation had existed for some forty-five or fifty days, the patient survived the operation ten days. In several other cases constipation had continued thirty or more days. It has occurred to our own knowledge that constipation continued for two months in a case of partial stricture of the rectum, and from this the patient recovered, the stricture now freely allowing the passage of fluid fæces. Dr. Burne quotes an instance of constipation of still longer duration, though not dependent on stricture, and from which the person recovered.

Must constipation always be present in order to establish the existence of obstruction? Should we invariably consider its absence conclusive evidence as contra-indicating the *possibility* of obstruction? Let us look for an answer among the cases furnished from the Pathological Society.

In Case 8, a twist of the intestine, the constipation was not complete; the symptoms in the first instance were those of diarrhœa. In Case 12, cancerous stricture of the transverse colon, constipation was not complete; and in Case 13, colloid cancer of the rectum, there was sometimes diarrhœa.

In Case 24, stricture of the ileum following an operation for strangulated hernia, the constipation never appeared to be complete, *but the intestine became gradually distended.* In Case 27, strangulation of the ileum, the constipation was not quite complete, though great obstruction existed for five weeks. In Case 31, gall-stones impacted in the intestine, the latter being ulcerated and contracted, there was diarrhœa up to the period of death, though in the primary symptoms constipation was well marked for some days. We shall have again to refer more minutely to the different causes producing these variations in the symptoms—that is, with respect to the action of the bowels. But it will be familiar to our readers to have observed the alternations that may occur, especially in cancerous obstructions, between constipation and purging, from the ulceration of the parts, or the breaking-down of the soft masses which sometimes project into the cavity of the intestine; and in the management of those cases of cancerous stricture within reach of the finger, Mr. Hawkins illustrates the advantage of bearing this circumstance in mind. He was consulted in the case of a lady labouring under six days' obstruction of the bowels, whom he had seen frequently on previous occasions, believing her disease to be cancerous. For some time before the six days' obstruction, numerous abscesses and sinuses had formed, and the whole of the fæces had been for some weeks discharged by a large ulceration into the vagina. Mr. Hawkins, with some difficulty, broke through the obliterated anus, and nearly five inches of the rectum filled with the morbid growth; and some portion beyond the reach of the finger, through which he could not force a bougie, gave way four hours afterwards, so as to empty the distended bowel. Notwithstanding this extensive disease, the health of the lady was, after three years' duration, perfectly unaffected; and the bowels continued more amenable to medicine than before Mr. Hawkins employed these measures.

Constipation cannot, therefore, be always considered as essential to the existence of obstruction. Must it not, however, be always essential to the proposal for any surgical interference? We think, even on this point we may hesitate to give unconditional consent; and it will be our endeavour to argue the question from the facts already before us, when we have to speak upon remedial measures. As constipation varies in its degrees, it cannot be regarded as an invariably attendant symptom in all cases of obstruction; but it must still be considered as one of the most important and one of the least capricious symptoms for the guidance of our diagnosis.

Accuracy of diagnosis with respect to the seat of obstruction and its cause can perhaps never be arrived at with any degree of certainty in the large majority of cases; in some, however, the evidence is sufficient to enable us to determine not only the cause and its situation, but the remedy best adapted to the urgency of the symptoms.

Mr. Phillips observes, in reference to the difficulty of diagnosis from general symptoms, and after relating the particulars of several cases,—

"The impression which must be produced by reading the preceding cases is this, that, no matter what may be the cause of the obstruction, no certain remarkable difference is observed in the more prominent symptoms by which it is accompanied. There are, in all, abdominal pain, abdominal tension, obstinate constipation, and sickness; but in the mode of their occurrence it would be difficult to point out any distinct difference. Thus constipation, abdominal pain or uneasiness, with tension and sickness of the stomach, are present, in greater or less variety, in most cases; but there is no certain or definite line to be drawn, either as to the time they occur, or the severity with which they are ushered in, whether the obstacle be a biliary calculus or a band of false membrane."

He then goes on to say:

"Taking the symptoms altogether, it is true, when the obstruction is complete and suddenly developed, they are usually more urgent than when it is slowly produced; but there are cases in which the obstruction has terminated life within forty-eight hours, without much sickness or pain. In cases of invagination, or where a tumour gradually encroaches upon the calibre of the tube, where the canal becomes contracted by change of structure, the pain does not commonly, even up to a late period, exhibit so much intensity as in the former case. But I say again, that we cannot with confidence rely upon these distinctions, the exceptions are so many."

There is much that is too true in this; the history and post-mortem conditions of the cases in our table confirm in a great measure the correctness of the conclusions at which Mr. Phillips has arrived. For it will be observed that constipation was not a symptom attendant on all the cases of obstruction; diarrhœa even existed in one of them till the time of death; and in one case related by Mr. Shaw, there was such a collection of fæculent matter contained in the intestine beyond the seat of the stricture, that a copious evacuation *might* have occurred, and yet any favourable expectations that would have been formed from such an event, in treatment, would ultimately have ended in disappointment.

The cases in which we may, to a great extent, venture a tolerably positive diagnosis, are the cases in which obstructions are situated in the large intestine, and more particularly its lower portions; and fortunate indeed does it appear, as the table drawn up by Mr. Cæsar Hawkins fully establishes, that these are the cases in which our remedies are most readily applicable and most favourably applied—not from the greater facility of the diagnosis, but rather from the situation of the strictured portion of intestine; and these remedies appear likely to be attended with more satisfactory results and greater success than any attempts we may make to relieve the effects of obstruction from other causes, or elsewhere situated.

Our space will not now permit such a full consideration of the various points relating to intestinal obstructions as we could desire; we have already been compelled to omit something—we are obliged to defer much to another opportunity; when that opportunity offers, we shall resume the subject of their diagnosis, their treatment, and their pathology.

George Pollock.

REVIEW III.

A Treatise on Percussion and Auscultation. By Dr. JOSEPH SKODA. Translated from the fourth edition by W. O. MARKHAM, M.D., Assistant-Physician to St. Mary's Hospital.—*London.* 8vo, pp. 346.

DR. MARKHAM has rendered essential service to the medical profession in England by his translation of Skoda's work. The public mind in this country has already been well prepared by Dr. Herbert Davies' lectures, and by various papers and extracts that have appeared from time to time,* but it is always satisfactory to read an author's own explanation of his peculiar views, and the reasons that have induced him to differ from his predecessors. Dr. Markham's translation is in general literal, and yet elegant and readable: we have noticed but few inaccuracies in it. In his preface he gives a short summary of Skoda's leading doctrines. Before proceeding to examine the work before us in detail, it may interest the reader if we premise a very few observations on Skoda's method of clinical observation, and on his merits as a practical observer.

Skoda has for many years been clinical professor at the general hospital at Vienna, an establishment containing about 2500 beds. His clinical wards, though only containing about forty patients, are always full of instructive cases, as the clinical professor has the right of first selection from the new patients, who are admitted into the hospital. The cases in his wards are mostly acute, or are grave organic lesions; the mortality is consequently high, and the opportunities of verifying the diagnosis during life by post-mortem examination, frequent.

Skoda comes every day at about 8 A.M., and examines the patients in the presence of his class, making any observations which he may think instructive. New cases are taken by the students, and the report is read over at the bedside. Skoda makes observations on the report, and then examines the patient himself, lecturing upon the case as he proceeds, and pointing out his reasons for coming to his conclusions. The diagnosis is sometimes not completely settled the first day, nor until he has seen the patient two or three times—but when made, is recorded with great minuteness, and is scarcely ever changed. If the patient die, the post-mortem examination is made by Rokitansky, or his assistant, and the result recorded without reference to the report of the symptoms or diagnosis during life. The two statements of the physical lesions, the one made before, the other after death, are then read out to the students attending the clinical wards, and if any difference exists, the causes of the false diagnosis, and the mode of avoiding such an error, if possible, in future, are pointed out.

Sometimes the diagnosis is doubtful—i. e., two diseases are named, either of which might have produced the signs present; but the probability may be in favour of one rather than of the other. But in the majority of cases the diagnosis is precise; and when once given in a thoracic disease, we have not seen more than one or two mistakes during four months' attendance

* In the Edinburgh Medical and Surgical Journal for 1841, there is an analytical account of Skoda's work by Dr. Drysdale and Dr. Russell, which, however, is too short and defective to give an adequate idea of the changes introduced by Skoda.

at the dead-house and in the clinical wards. The accuracy and certainty of Skoda's diagnosis appeared to us to be very great, and we believe that most of those who have had the advantage of watching Skoda's practice for any length of time, will agree with us in this opinion.

A method by which such accurate results can be arrived at, deserves our most attentive consideration, and cannot be passed over by treating it as German mysticism. Added to this, Skoda's work has now become the text-book in Percussion and Auscultation at most of the German universities, and its great value is admitted in Germany by all those best qualified to judge of its merits.

The tendency of Skoda is to simplify instead of to complicate the subject of which he treats, though possibly he has himself fallen into one or two errors, from not accurately perceiving the essential conditions upon which some of the sounds he describes depend. We here allude more particularly to the distinction he makes between the "full" and "empty" percussion-sounds. We also agree with Dr. Markham, that his nomenclature would have been better had the theory of consonance not been introduced into it.

"The introduction into our language of a plain, practical nomenclature of auscultation is much required, and would be a great boon conferred upon the science. Skoda's nomenclature is eminently plain and to the purpose, but unfortunately is in part based upon his theory of consonance: he speaks, for instance, of consonating râles; so that its assumption can, I fear, hardly be hoped for, until such time, at least, as his theory has met with more general approbation." (Markham; preface, p. 11.)

Skoda's theory of consonance is well summed-up by Dr. Markham in his preface; but as we have on a former occasion referred to this subject in a review of Dr. Herbert Davies's work, it is unnecessary to return to it on the present occasion.

The first part of the work before us treats of the phenomena observable by the aid of percussion and auscultation. Skoda considers that one percussion-sound may differ from another in one or more particulars.

1st. It may be full or empty.
2nd. It may be clear or dull.
3rd. It may be tympanitic or non-tympanitic.
4th. It may be high or low.

1st Class.—"A full percussion-sound may be clear or dull, tympanitic or non-tympanitic, high or low; and the same is true as regards an empty sound."

The difference between full and empty is thus described:

"We do not judge of the size of a resonant body by the strength of the sound which strikes upon the ear: the slightest vibration of a large bell tells us of its magnitude; the loudest ringing of a little bell misleads no one as to its smallness; neither do we judge of the dimensions of bodies from the pitch of their sounds. There is no good general term to designate that quality of sounds which characterizes the size of bodies. I believe that in singing, and instrumental music, the word *full*, or full-toned, or sonorous, is used; I shall therefore borrow the expression in speaking of the percussion-sound. When any one percusses with equal force different parts of the thorax and abdomen, he will find that in some places the sound appears more persistent, and, as it were, spread over a larger surface than it does in others: the first kind of sound I call full; the second, the less full, or empty percussion-sound." (Markham, p. 8.)

This distinction between a full and an empty sound is, we believe, more difficult to catch than anything else in the practice of percussion; and this is partly due to a complication unnecessarily introduced by Skoda. One sound can only differ from another in one of four particulars—

1st. A difference in loudness.
2nd. A difference in duration.
3rd. A difference in pitch.
4th. A difference in timbre, by which we mean, that character of a sound which enables us to determine the instrument whence it proceeds: thus, if the same note be sounded on the horn and on the violin, we at once know from which it has proceeded; and the character which enables us to form this judgment, is the *timbre* of the sound.

Beyond these four differences, it is not conceivable that any distinction can exist. What, then, becomes of Skoda's *full* and *empty* sounds?

To take Skoda's own example. What are the peculiarities in the sound of a large bell, that tell us of its size? They are, 1st, the longer *duration* of the sound than that of a small bell, and this difference Skoda seems to have perceived: 2nd, the deeper note of a large than of a small bell, although Skoda himself does not allow that the *pitch* affects its fulness or emptiness.

In our opinion, then, Skoda's full sound is one which is long in duration, and at the same time *deep* in pitch; while his empty sound is one which is shorter in duration and *higher* in pitch. Dr. Traube of Berlin, whose skill in percussion and auscultation we have had frequent opportunities of observing, discards altogether Skoda's *full* and *empty* sounds, substituting in their stead the words *deep* and *high*, and this nomenclature has the advantage of being precise and intelligible.

A deep percussion-sound is almost always longer in duration than a high sound, so that for practical purposes the duration may be disregarded, and the pitch alone attended to.

If the meaning above attached to the words *full* and *empty* is borne in mind, the conclusions arrived at by Skoda are not interfered with, and greater precision is obtained.

2nd Class.—Clear and *dull.* Skoda does not attempt to define these terms, but uses them in their commonly received meaning; yet we think that much is to be gained by accurately finding the meaning of the terms we employ; and we would suggest, notwithstanding the mystery which has often been attached to these differences, that the only real distinction between a clear and a dull sound is, that the former is louder, and of longer duration than the latter.

Differences in pitch have, in our opinion, nothing to do with the clearness or dulness of a sound, but have reference merely to its being full or empty. A completely dull sound is, at the same time, a completely empty sound, as in both the duration is reduced to a minimum.

3rd Class.—Tympanitic and *non-tympanitic.* The word *tympanitic*

(*Tympanitische Percussions-schall*) is open to some objection, as it is often employed in common language to denote any loud, deep sound produced by percussing over a large air-containing cavity. What Skoda means by it, may, however, be readily understood by percussing the healthy abdomen. The abdomen (unless enormously distended with air) always yields a tympanitic sound.

"The non-tympanitic sound is represented by the sound which percussion produces at those parts of the thorax beneath which lies healthy lung, normally distended with air. An abnormally distended lung, as in vesicular emphysema, gives at one time a tympanitic, at another a non-tympanitic sound. A partial emphysema in the midst of lung deprived of air (as happens in pneumonia, where not unfrequently the tissue around the hepatized portion, and especially at the borders of the lung, is emphysematous), generally produces a tympanitic sound; but if the whole of the lung is emphysematous, the sound is seldom distinctly tympanitic.

"When the lung is much reduced in volume by compression, but still contains air, its sound is invariably tympanitic." (p. 13.)

"In pneumothorax, the walls of the thorax, if they are not much distended, yield a tympanitic sound; but if much distended, their sound is almost constantly non-tympanitic." (Markham, p. 15.)

Skoda disputes the accuracy of Dr. Williams' explanation of the cause of the tympanitic percussion-sound, occurring at the apex of the lungs in some cases of pneumonia and pleurisy, and our observations lead us to concur in Skoda's explanation; for we have more than once heard the sound in question, where the subjacent lung was proved by post-mortem examination to be perfectly pervious, and consequently not in a condition to conduct the tracheal percussion-sound with increased intensity.

Skoda believes that the tension of the walls of the air-containing cavity diminishes the tympanitic percussion-sound, while their flaccidity favours its production. One of his experiments is very simple and conclusive. Take a stomach, one of whose apertures has been tied; gradually inflate it, and percuss it in different stages of inflation. When its distension is moderate, the percussion-sound is tympanitic; but as soon as the walls become very tense, the tympanitic character of the sound ceases, and at the same time, the sound becomes more dull.

The 4*th class* of percussion-sounds (high and low) needs no further discussion.

Having disposed of the subject of *Percussion*, Skoda passes on to *Auscultation*. We shall select some points here and there for consideration.

Pectoriloquy is a sign which he discards as being undistinguishable from Bronchophony; and he affirms, what is indeed now admitted by many, that Laennec's pectoriloquy may exist over hepatized lung without a cavity, and, on the other hand, that a cavity may exist without pectoriloquy. To this point we have also formerly alluded, in reviewing the works of Drs. Walshe and Davies.

Laennec's *ægophony*, as pathognomonic of pleuritic effusions, Skoda would also discard, as he affirms that he has often heard it where no effusion existed; for example, in cases of pneumonia and in tubercular infiltration, with or without cavities. This statement is also now received by many able auscultators. Skoda's own division of the modifications of the thoracic voice is not, however, characterized by his usual simplicity. He says—

"I distinguish the following modifications of the thoracic voice:

"1. The voice, accompanied by a concussion in the ear, completely traverses the stethoscope—*loud* bronchophony, which may be either clear or dull.

"2. The voice, unaccompanied by concussion in the ear, passing incompletely through the stethoscope—*weak* bronchophony.

"3. An indistinct humming (*Summen*), with or without a barely appreciable concussion in the ear.

"4. Amphoric resonance, and the metallic echo of the voice." (Markham, p. 75.)

As the first two classes of bronchophony are merely differences in degree, it is not safe to form a diagnosis from the presence of one or other of them, for what one person would call strong, another might call weak bronchophony; so that a want of precision is likely to arise from this division, similar to that arising from Laennec's separation of pectoriloquy and bronchophony.

The *murmurs heard during respiration* are defined by the character of the sound produced in imitating them by the mouth. Thus the murmur of the air in the larynx, trachea, and large bronchi, may be imitated by pronouncing *ch* guttural (as in the Scotch *loch*), or it falls between *h* and *ch*. If, while in this position of the tongue and soft palate, the air is drawn in and out, a murmur much resembling that of the larynx or trachea will be produced. The *vesicular* murmur

—"may be imitated by narrowing the opening of the mouth, and then drawing-in the air. The consonant of this murmur falls between *v* and *b*. This character, however, refers only to the murmur of inspiration; that of expiration, in the natural state of the respiratory organs, causes little or no sound in the air-cells and finer bronchial tubes; whatever sound is heard, differs from the murmur of inspiration, and resembles rather a gentle aspiration (*Hauchen*), or blowing (*Blasen*). It can only be imitated by the mouth during expiration; the consonant which represents it falls between *f* and *h*." (p. 86.)

The pitch of the murmur is expressed by the vowel, which must be combined with the consonant, to imitate it by the mouth—*e* corresponding to the highest note, and *u* (or rather *oo*, as in good) to the lowest.

This accurate determination of the consonant and vowel required for the imitation of any particular sound has advantages besides the increased precision of description which it allows. The student not very familiar with the practice of auscultation, will find, that it will greatly assist his recognition of a doubtful sound, if, while listening, he imitates the sound with his mouth, and thus determines the consonant and vowel belonging to it.

Having defined the terms vesicular and bronchial breathing, Skoda considers all intermediate sounds as *indeterminate* (*unbestimmt*), i.e., if they are not distinctly vesicular, or distinctly bronchial in character, he draws no conclusion from them, for such indeterminate breathing may exist over perfectly healthy lung, or over lung in various degrees of consolidation.

Laennec's "*cavernous breathing*" is discarded for the same reasons as pectoriloquy and gurgling—viz., because it occurs, and that very frequently, over simply consolidated lung, and is undistinguishable from bronchial breathing. This must of course be understood only of Laennec's cavernous breathing, not accompanied by amphoric echo, to which Skoda attaches the same importance as Laennec, and considers it to be pathognomonic of a large cavity.

The râles are enumerated as follow:

1st. The *Vesicular Râle* is characterized by the small and equal size of the bubbles, and is produced by the entrance of air into the smallest bronchial tubes and air-cells, in which blood, mucus, serum, &c., exist.

2nd. The *Consonating Râle*, corresponding in pathological meaning to bronchial breathing, and marked by its clearness and its high pitch. The bubbles are of unequal size.

3rd. The "*dry Crepitating Râle* with great bubbles," as described by Laennec.

4th. The *Indeterminate Râles.*

"Under this head I comprise all râles which are neither vesicular nor consonating, and are not accompanied by amphoric resonance or metallic tinkling; they offer no special indications respecting the condition of the lung-tissue." (p. 131.)

Sonorous whistling and hissing sounds are distinguished from the râles; they may or may not consonate.

Amphoric echo and metallic tinkling are next discussed. Of the relation of these sounds to each other, Skoda says:

"Any one may convince himself, by the production of metallic echo in a room, and by directing his voice into a vessel, that amphoric echo and metallic tinkling are phenomena which occur under similar conditions, and that metallic tinkling bears the same relation to the amphoric resonance, as does a high to a deep flageolet tone of a guitar-string." (Markham, p. 133.)

He then proceeds to show, that for the production of metallic tinkling, fluid is not necessary, and for that of amphoric resonance of the voice and breathing, it is not necessary that a communication should exist between the cavity and the bronchi, which indeed is very rarely persistent in cases of pneumothorax.

"If a person speak into a stethoscope, placed on a stomach filled with air, both metallic tinkling and amphoric echo will be heard sounding within the stomach, and this, whether the stomach be partially filled with, or contain not a drop of fluid." (p. 134).

This experiment illustrates both the foregoing propositions. Skoda does not by this mean to deny that the falling of drops of liquid in a large cavity will produce metallic tinkling, but only asserts that other sounds, such as crepitations &c. in the neighbourhood, may also cause it.

It will be seen that *gurgling* is not a sound recognised by Skoda. Laennec's gurgling is what Skoda calls a consonating râle with large bubbles, and this is not a mere change of name. Laennec affirmed that *gurgling* is pathognomonic of a cavity; Skoda, that it may occur over consolidated lung, where there is no cavity, and we think few physicians of experience will deny the frequency of this occurrence. But gurgling is, in the minds of many, so associated with the idea of a cavity, that if by other signs they determine that no cavity exists, then the sound, which a minute before was *gurgling*, now is called "large crepitation approaching to gurgling," yet the sound remains the same; and had there not been signs present to disprove the existence of a cavity, it would have passed for *true gurgling*, and a cavity would have been diagnosed. We have seen very experienced auscultators fall into this mistake, which might always have been avoided, by recollecting that the sound in question may proceed from fluid contained in the tubes of consolidated lung, as well as from a cavity. Pectoriloquy is of no assistance to us here, for this sound, as Skoda has shown, has the same signification as, and passes by imperceptible shades into, bronchophony. Of course, if these sounds occur at the apices of the lungs in a chronic disease, with profuse expectoration, where there has been hæmoptysis, &c., the probability is, that they arise in cavities; while if they occur in the course of pneumonia, the probability is, that they merely denote consolidation of the lung.

The chapter on the *pleural friction-sounds* need not delay us, except to notice that Skoda does not sufficiently discuss the great difficulty there often is in distinguishing the finer crackling friction-sound from the crepitating or crackling râles.

Chapter IV. treats of the auscultatory phenomena presented by the organs of circulation, that have been brought forward in explanation of the heart's impulse, which he himself attributes to the following causes:

1st. The blood propelled from the ventricles into the large arteries, and especially from the left ventricle into the aorta, presses against the walls of the ventricle with a force that may be estimated by its velocity in the artery, and this force is relatively exerted in a greater degree at the apex—viz., the part opposite to the aorta and pulmonary opening. This is the recoil theory of Dr. Gutbrod.

2nd. Lengthening of the ascending aorta, allowing the descent of the heart during systole.

3rd. "When the heart is enormously enlarged," says Skoda, "I believe that it may raise the walls of the thorax in consequence of its antero-posterior diameter becoming greater during the systole, than the space between the fore part of the thorax and the vertebral column; *from which it must be admitted,* that the heaving of the thoracic walls, when no sign is present to demonstrate a simultaneous and equal pressure on the vertebral column, is only explicable by Dr. Gutbrod's theory." (Markham, p. 160.)

This does not make sense. There is here an error in the translation; the words printed in italics are in the German, "wobei übrigens zugestanden werden muss," which ought to have been translated, "*although at the same time it must be allowed,*" &c.; that is to say, as under such circumstances the vertebral column must be pressed upon equally with the anterior wall of the thorax. If it be not so pressed upon in any case, Dr. Gutbrod's theory becomes the only conceivable one.

4th. "Lastly, it must not be forgotten that the heart, by becoming rigid from contraction during its systole, takes a different *position* and direction from those it had in its relaxed state; and that such a change of position may aid in producing its impulse, even though the idea of a lever-like elevation of its apex against the thoracic walls be hardly explicable by the arrangement of its muscular fibres. (Skoda, p. 154.)

Dr. Markham has here also made an oversight in his translation, the word *lage* (*position*) he has rendered by *form,* which makes the meaning obscure.

After a critical examination of some of the leading theories of the *sounds* of the heart, Skoda prefaces his own view (which we shall not discuss) by a very elaborate description of the mechanism of the valves. He adopts Weber's explanation of the cause of the insertion of the tendinous cords, of the auriculo-ventricular valves, into papillary muscles, and not directly into the walls of the ventricles.

"The area of the cavity of the ventricle at the commencement is different from its area at the end of the systole; and the points of origin of the papillary muscles approach constantly nearer to the attached border of the mitral and tricuspid valves, as the systole progresses. Now the length of the tendinous cords always remaining the same, it becomes evident, that for the perfect closure of the valves, the tendinous cords, which retain the valves in their proper position, must of necessity take their origin from the papillary muscles. If, for example, they arose immediately from the walls of the heart, and were of due length for the performance of their functions at the commencement of the systole, they would necessarily be of too great a length during its progress; and, on the other hand, would impede the diastole, if their length was sufficient merely to retain the valves in their proper position towards the end of the systole. But as the length of these cords is invariable, they must necessarily have some muscular attachment; and the object of the papillary muscles is evidently, by alternate extension and contraction, to retain the valves in their proper position. During the progress of the systole these muscles contract; the distance between their points of origin and the attachment of the mitral and tricuspid valves is constantly diminishing; in consequence of this contraction, the tendinous cords arising from the muscles retain the same degree of tension during the progress of systole, which they had at its commencement: and they also retain it during the diastole, the papillary muscles being lengthened, while the heart expands. The correctness of the views here offered of the functions of the papillary muscles seems to be confirmed by the fact, that that division of the tricuspid valves which lies against the heart's septum, receives its tendinous cords either from very short papillary muscles, or immediately from the walls of the heart. The points of origin of these tendinous cords approach little, or not at all, during the systole, to the points of attachment of the valve, and recede as little during the diastole. Now, here a simple tendinous cord suffices for the proper restraint of the valve, because change in the length of the cord is not required." (Markham, pp. 190, 191.)

Skoda lays great stress on the importance, as a diagnostic sign, of a strengthening of the second sound of the pulmonary artery, as compared with that of the aortic valves. The former is to be listened for between the second and third ribs to the left of the sternum, the latter in the corresponding position to the right of that bone. In this position we auscultate, not over the respective valves, but in the course of the large vessels, since the aorta inclines to the right, and the pulmonary artery to the left. Experience shows, that in these two positions the second sound of the heart is of different intensity in some diseases. An increase in the intensity of the sound of the pulmonary artery proves increase of pressure in the pulmonary artery, which, if not due to obstruction in the lungs, and if the patient has no fever, almost always proves, according to Skoda, obstructive or regurgitant disease of the mitral valve. It is easy to see how such a disease will increase the pressure in the system of pulmonary veins and arteries, and to this cause is soon added the compensating hypertrophy of the right ventricle. Skoda does not think that we are warranted in diagnosing regurgitant disease of the mitral valve from a systolic murmur loudest at the apex, unless at the same time the strengthening of the second sound of the pulmonary artery shows an increase of pressure in this vessel. We refrain from discussing the correctness of this opinion.

Part II. treats of the physical signs as diagnostic of a healthy condition or of various morbid states of the thoracic organs. Chapter I. treats of these organs in their normal condition.

In Chapter II., he proceeds to notice those deviations from the normal physical signs which take place in different diseases. We can here do no more than notice, very briefly, a few of the points which are not so generally recognised in this country as they deserve to be.

When pneumonia is invading a portion of lung, and before infiltration is complete, a tympanitic percussion-sound often appears over the affected portion, which is often the earliest physical sign of the impending mischief. This modification of the percussion-sound can, of course, only lead to a conclusion, when from the other signs we have reason to suspect incipient pneumonia. We have already noticed the reasons which make Skoda dissent from the opinion of Dr. Williams as to the cause of this modification.

With reference to the possibility of diagnosing by physical signs between red and grey hepatization, Skoda says—

"I have frequently examined patients suffering from pneumonia, in whose lungs newly-formed abscesses were found after death; but I have never in any single instance recognised the presence of abscess by the aid of auscultation and percussion. In every case, the abscess, though communicating with the bronchial tubes, was filled with pus or sanies. If the abscess be accidentally emptied of its contents, it will be rapidly filled again by the secretion, unless it be of long standing, and its walls have become solid, and their secreting power diminished." (Markham, p. 277.)

Laennec considered the "râle crepitant" to be pathognomonic of the first stage of pneumonia. Skoda allows that it is sometimes heard in this stage, but asserts that he has often met with cases, in which, though he has watched them from the commencement, no crepitating râle has occurred, and he goes on to say—"It would be hard to say whether the râle, which is heard previous to the hepatization, or the consonating râle attendant upon complete hepatization, has been most frequently taken for Laennec's crepitating râle." The consonating râle which Skoda here mentions, differs from Laennec's *râle crepitant* by having the bubbles of which it is composed of unequal size, while in the *râle crepitant* they are equal, and exceedingly small.

When speaking of the signs of vesicular emphysema, Skoda says—

"Vesicular emphysema does not render the percussion-sound tympanitic, unless the more fully inflated portion of lung is bounded by another portion of lung that is entirely deprived of air, which not unfrequently happens in hepatization and tubercular infiltration; or unless the emphysematous lung has completely lost its contractile power." (Markham, p. 295.)

With this observation we fully concur, it being remembered that Skoda's tympanitic percussion-sound, is that character of sound which is obtained by percussing the healthy abdomen.

Solitary tubercles, even when developed in great numbers, do not, according to Skoda, produce any change in the percussion-sound, as long as the intervening lung tissue is healthy; and it is useful to bear this in mind, as tending to increase our caution in giving an opinion of the absence of tubercles, from the absence of changes in the percussion-sound. Where, however, the deposit of solitary tubercles is confined to one portion of the lung, the percussion-sound will, according to our experience, be raised in pitch over this part; but when solitary tubercles are equally distributed throughout the whole of the lungs, even if they did raise the pitch of the percussion-sound, this change could not be appreciated, from the absence of any standard of comparison.

It is often stated, that a partial pleuritic effusion may be distinguished from partial pneumonia, by causing the patient to change his posture, when the line of dulness is said to change, if the dulness be due to pleurisy. This diagnostic sign Skoda considers of very slight value, as this change of position of the fluid in pleurisy very rarely occurs; and a little consideration of the pathological changes in the parts will show the reason of this permanence in the position of the fluid; for the fluid-effusion is almost always bounded by an exudation of albumino-fibrin, glueing the pleural surfaces together, so that almost every case of acute pleurisy is sacculated. In partial *hydrothorax*, on the contrary, where the fluid is of the nature of a dropsical effusion, no such limitation takes place, and the fluid does change its position with alterations in the posture of the patient, though this change of position of the fluid requires some time for its occurrence.

As a sample of the manner in which our author sums up the principal diagnostic signs in cardiac diseases, we shall give those of contracted mitral orifice.

" *Contraction of the Mitral Orifice.*—As in insufficiency of the mitral valve, the dulness of the percussion-sound is increased in extent, ***in a direction corresponding to the breadth of the heart***, for in contraction of the left ostium venosum, hypertrophy of the ***right ventricle*** ensues still more rapidly.

" The second sound in the left ventricle is replaced by a murmur, which is often so prolonged as to be only momentarily interrupted during the heart's systole. The second sound of the pulmonary artery is strengthened. During the systole, either no sound at all is heard in the left ventricle, or only an ***indistinct*** sound; or again a murmur may be audible, because deficiency of the mitral-valve is generally associated with contraction of the mitral orifice. As a rule, the sounds of the aorta are weak, and the heart's impulse increased, and perceptible over a greater extent than natural." (Skoda, p. 312.)

Dr. Markham has, in his translation of this passage, not faithfully rendered the meaning of the original. On comparing our translation with his, it will be seen that he says, " the dulness is increased," without translating " der Breite des Herzens entsprechend" *in a direction corresponding to the breadth of the heart;* and instead of specifying that the increased dulness is owing to enlargement of the *right ventricle*, Dr. Markham says, " hypertrophy of the *heart* occurs more rapidly."

Dr. Markham has perhaps made these omissions with the intention of being concise, but by this abridgment, part of Skoda's meaning has been lost, and, in addition, an inaccuracy is introduced; for contraction of the mitral-orifice, though it is necessarily followed by hypertrophy of the right ventricle, does not give rise to hypertrophy of the whole heart. The left ventricle, indeed, may be atrophied. Another mistake in this passage is, that the word *indistinct* (*undeutlich*) has been by Dr. Markham translated by *indeterminate*, which has quite another meaning in the writings of Skoda, and for which the German word " *unbestimmt*" is always used. But some slight defects do not detract from the general merit of

Dr. Markham's translation, which, taken as a whole, is faithful, and an acquisition to our medical literature. In conclusion, we may remark, that though there are some parts of Skoda's work which, we think, might be improved; and though the controversial parts are sometimes rather longer than will suit the generality of readers, yet we can recommend the work before us, as well worthy of an attentive perusal; and we think its general circulation in this country will materially advance the science of percussion and auscultation.

T. R. Armitage.

REVIEW IV.

Der Nervus Phrenicus des Menschen. Eine Monographie. Von Dr. HUBERT LUSCHKA, ausserordentlichen Professor der Medizin an der Universität Tübingen.

A Monograph on the Phrenic Nerve. By Dr. HUBERT LUSCHKA.—*Tübingen,* 1853.

OUR present knowledge of the anatomy and the functions of the phrenic nerve might be nearly summed-up in the following words—viz., that it is distributed to the diaphragm, and that the movements of this muscle are interfered with when the phrenic is injured, or are arrested when the spinal cord is destroyed above the origin of the nerve. The deficiency of knowledge concerning this nerve may undoubtedly be traced to the want of precision amongst anatomists, as shown, for instance, in the work of one of the most recent authorities on the nervous system (Valentin), in which the most rash, and, as Dr. Luschka shows, inaccurate statements respecting its distribution are contained. As long as this uncertainty in anatomical detail continued, it was not surprising that the true interpretation of actions should remain ungiven. It is therefore with pleasure we have perused a production in which fact is clearly distinguished from imaginings.

The task that the author proposes to himself is, to prove the twofold function of the nerve, as in an ordinary spinal branch; to indicate the place of destination of its offsets; and lastly, to attempt to show its influence on parts in health, as well as to connect certain symptoms in disease with its deranged function. These several points will be referred to in the following pages, though our notice must of necessity resemble more an abridgment than a critical examination of the views propounded; for without following the author with the scalpel and the microscope, and undergoing nearly the same amount of labour, we cannot be in a position to impugn the accuracy of his statements. But Dr. Luschka's inquiries are too well known to need any recommendation at our hands; though, if the opinion of the reviewer may have any influence in securing an attentive perusal, it may be stated as his conviction, that our acquaintance with the anatomy, if not with the function of the phrenic nerve, has been much enlarged, and what was doubtful rendered more certain, by the investigations of the author.

As many of the explanations of the functions of the nerve might be unintelligible without an accompanying survey of the anatomical facts, we purpose to connect the two shortly, and to consider the nerve at its origin in the neck; in its course in the chest; and at its ending in the diaphragm, in the serous membranes of the thoracic and abdominal cavities, and in some of the viscera.

Cervical part of the Nerve.—A reference to the origin of the nerve may be considered unnecessary at this time, and it would not be introduced here if the writer had not been informed, though the statement is scarcely credible, that there are some members of the profession who still entertain the purely theoretical views of Bell concerning the origin and the functions of the large diaphragmatic nerve. At its origin the nerve is but an offset of the cervical plexus, like others to the depressor muscles of the hyoid bone; it is connected with both anterior and posterior (motor and sensory) roots of the spinal nerves, and possesses centrifugal fibres of the sympathetic, as do the intercostal trunks for instance. Respecting the number of spinal nerves from which it springs, there has always been much difference of opinion, and the decision of this point by the author, through the numerical method, has some interest. For the most part the phrenic comes exclusively from the fourth cervical nerve (twelve times out of thirty-two), and in the remaining instances it was also joined to one or more of the other nerves—being sometimes to only one, at others to both of the two contiguous nerves. A difference, as might be expected, has been recognised on opposite sides; thus, on the one side the nerve arose from the third, and on the other side from the fourth spinal trunk; so that injuries of the spinal cord between the attachments of the third and fourth nerves may in one instance only impair the respiration, but in another destroy the action of the diaphragm.

The connexions between this diaphragmatic and the other surrounding nerves in the neck have been subjects of keen dispute; but our author's inquiries, with the aid of the microscope, have given much more satisfactory results than the unaided eye and the scalpel could furnish. The commonly-received communication with the lower or middle cervical ganglion of the sympathetic is allowed; but the assertions of Haller and Wrisberg respecting the union of the nerve with the vagus are denied; and the occasional apparent junction of the phrenic with the hypoglossal nerve is explained in the following way. The intercommunicating offset between the descendens noni and the phrenic was seen only three times out of thirty-two observations; and on unravelling the connecting thread and examining its fibres with a lens, it consisted entirely of nerve-fibres derived from a higher spinal nerve, and not from the hypoglossal or ninth cranial nerve. As referring to the loop between the descendens noni and the spinal nerves, it may be stated that sometimes the loop or "ansa" between them is formed solely by the spinal nerves, so that what seems to be a branch descending from the arch of the hypoglossal is only an ascending offset from the connecting branches of the cervical plexus: this same arrangement exists in some animals.

By means of the close connexion between the diaphragmatic nerve and the other offsets of the cervical plexus, the pain in the shoulder, which is occasionally indicative of inflammation of the liver or its serous covering, receives its explanation; for as the phrenic, and a cutaneous branch of the shoulder, are both derived from the fourth cervical spinal nerve, it is to be supposed, according to our present mode of explaining this and other signs of a like kind in the body, that the stimulus conveyed along the phrenic is transferred in the spinal cord to other fibres of the fourth cervical nerve, and is perceived as if it was felt at the ending of this nerve. An example in which the effects are similar is furnished by the trunks of the fifth nerve, where pain in one tooth may occasion pain in all points of the peripheral ending of the trunks of the trifacial nerve.

Occasionally Dr. Luschka found arched fibres connecting the diaphragmatic with the cutaneous branch of the fourth cervical nerve to the shoulder, similar to those before pointed out by Arnold between the optics, and the posterior and anterior primary branches of the spinal nerves. These fibres were directed centrifugally in the two nerves alluded to; and once he succeeded in tracing them one inch and a half towards the periphery of the body, and convinced himself most positively that they were not connected with the central end of the trunk of the spinal nerve. The use of these enigmatical loops he cannot explain, for it is said: "Respecting the mode of action of these fibres thus unconnected with the central organ of the nervous system, an idea can scarcely be formed: it is just probable that they come into contact with ganglionic cells in their course, and take their origin in these."

The Nerve in the Thorax.—There is nothing novel communicated, of its course through the thorax to the diaphragm, though the offsets of the nerve to the serous membranes of the lung and heart, and to the heart itself, will supply us with material for subsequent remarks. Attention has, however, been directed by the author to a diseased condition of the trunk of the nerve as this lies against the pericardium, connected with the sequelæ of tuberculous deposit, as it is often found injured, after the disappearance of the disease, by the metamorphosis of a new product deposited around it.

"Since," says the author, "I have given special attention to this matter, I find now and then the most marked disturbance of the phrenic during life, accompanied by peculiar symptoms [what?]. I have repeatedly made the observation, that by the softening of tubercle and the destruction of the lung-substance united with the pericardial pleura, the phrenic was almost destroyed and interrupted in its continuity at a certain point. One instance of this I have preserved, in which the right phrenic was so surrounded in one part by calcareous tuberculous material of the hardness of stone, that it was impossible to detach it. With these observations before us, we should not omit to give close attention to the disturbances in the functions of the phrenic, as we may explain by them many obscure symptoms in the train of diseases of the chest."

The visceral and parietal parts of the pleura receive nerves from different sources, and possess them in a very markedly different number. In the pulmonic pleura they are very few, and are supplied by the vagus at the root of the lung; these are very difficult to detect at a short distance from their origin, and in no case could division of nerve-tubes be recognised. Dr. Luschka does not appear to confirm the observation of Kölliker respecting the existence of nerve-cells amongst the nerve-fibres; but he does not fail to dissent from the opinions of Bourgery, whose inaccuracy he designates as "den colossalen Irrthum Bourgery's." In the parietal part of the pleura the nerves are more numerous, and are derived from the spinal and the sympathetic: thus, in front, there are offsets from the phrenic opposite the third rib; and behind, from the knotted cord of the sympathetic; above, offsets are supplied by the last cervical ganglion of the sympathetic, and below, from the ramifications of the phrenic nerve at the diaphragm. The free supply of nerves to the pleura from the phrenic —a sensory nerve in direct communication with the spinal cord and the brain—will, it is thought, explain anatomically why pain in the parietal pleura is so acute, and so aggravated by the movements of the walls of the thorax; and further, the author has no doubt that many of the shoulder pains accompanying pleuritis, and commonly called rheumatic, depend upon disease of the pleura involving the terminal parts of the phrenic. A practical application, of more moment as a remedial means, is attempted to be made, from the consideration of the anatomical facts, in the following passage:

> "The place of origin of the nerve points out the spot at which derivative or remedial means may be applied for the relief of acute pain. There is no doubt that blisters, and narcotic remedies applied to vesicated surfaces, will act most efficaciously above the clavicle, at the spot where direct branches of that nerve are found, whose ramifications are affected at the peripheral termination."

Like the pleura, the pericardium is supplied with nerves from the same three trunks. Spinal nerves come to it directly from the phrenic on each side opposite the third rib, and indirectly through the ramifications of the same nerve at the diaphragm. Sympathetic offsets are continued to it from the lower cervical ganglion and the loop around the subclavian artery, as well as from the solar plexus through the midriff. There is a branch from the vagus only on the right side: this arises near the top of the chest, and supplies the pericardium and the upper vena cava. In pain in the arm and shoulder in pericarditis, the same reasoning is to be employed in its explanation as is used to account for that in the same parts in pleuritis—viz., the stimulus is conveyed through the medium of the phrenic, upwards to the spinal cord, where it is supposed to be transferred to the sensitive roots of one or more other spinal nerves that are distributed to the spot where pain is said to be felt.

The right auricle of the heart and the two systemic veins entering it are supplied by the same three nerves in the thorax. The upper vena cava is provided with the branch from the vagus to the pericardium; whilst the lower cava and the contiguous part of the right auricle have separate fine nerves from the right phrenic and mixed nerves, phrenic and sympathetic, from the diaphragmatic plexus. And in support of the anatomical facts here announced, the author has made some interesting experiments:

> "In dogs and rabbits," he says, "mechanical and chemical stimuli applied to the trunk of the phrenic, isolated on a glass plate, produced very evident contractions in the right auricle of the heart, previously at rest, which were isochronic with the movements of the diaphragm, and not conjoined with contraction of the other compartments of the heart. I saw this most satisfactorily in a rabbit that died suddenly through blowing air into the internal jugular vein, and whose heart, distended like a bladder by the air, did not show any trace of movement. In this case, chemical stimuli, and pinching the right phrenic with the forceps, caused at once very evident contraction."

The Nerve at its termination.—Near the diaphragm, the special nerve of this muscle divides into branches, and many of its ramifications, piercing the muscular fibres, end by joining the sympathetic on the under surface. On the right side, a ganglion marks the plexiform arrangement, whilst on the left side it is absent, as we already know from the labours of our countryman, Mr. Swan.

"A very free intercommunication exists between the phrenic and the sympathetic; and offsets are given to the muscular fibres, to the peritoneal and pleural membranes, to the inferior cava and the right auricle, and to the liver and the supra-renal bodies. The points that our space will permit us to notice are only the supply of nerves to the diaphragm, the distribution of the phrenic to the peritoneum, and the action of the phrenic and spinal nerves on the alimentary canal."

Besides the branches from a special nerve, the midriff receives twigs from the six lower intercostal trunks, and from the sympathetic. In the fleshy part of the muscle, the nerves have an arched arrangement, and some observations respecting their termination favour the opinion that some end without loops. Since Dr. Luschka saw many times nerve-tubes lose themselves between bundles of muscular fibres, and though he could not ascertain their exact disposition, he is convinced they did not form loops. In the tendinous structure, the ordinary spinal nerve-fibres were perceived, but their mode of ending, in consequence of the fewness in their number and the difficulty of the observation, was not satisfactorily ascertained. In the rabbit, where the part is more favourable for microscopic examination, both nerve-fibres—like spinal, and fine granular with the vessels—were easily detected, and were seen to bifurcate and lose themselves in the web; but the manner how, as in man, it was not possible to detect. Each phrenic nerve is not confined exclusively to its own side, but sends offsets across the middle line to the opposite side. In consequence of this interchange of nerve-fibres, it is conceived that inflammation of the liver, or of the serous membrane of only one-half of the diaphragm, may give rise to pain on both sides, near the origin of the phrenic nerve.

The chief interest attaching to the supply of nerves to the peritoneum, is the tracing branches directly from a spinal nerve into it: these come from the phrenic directly through the fore part of the diaphragm, or from the conjoined phrenic and sympathetic on the under surface of the muscle. Some of the immediate offsets of the phrenic are directed along the abdominal wall in a direction towards the navel; and through them, it is suggested, the pain that begins about the umbilicus in inflammation of the peritoneal covering of the abdominal wall will be felt, whilst a greater degree of pain in this spot than in any other will be perceived. In a similar way, these offsets are said to originate as a reflex act, the violent paroxysmic vomiting accompanying inflammation of the peritoneum, since the irritation applied to their ending acts through the spinal cord on certain nerves of muscles that are used in the expulsion of the ingesta from the alimentary tube.

The author has made some experiments on animals, to ascertain the action of the phrenic on the intestinal canal, and has put forward some views respecting the action of the spinal nerves on that part. These views may be interesting in the province of practical medicine, and are contained in the following extract:

"It is scarcely to be doubted that the branches connecting the sympathetic with the cerebro-spinal nerves conduct the influence, emanating in the central parts (brain and cord) to the different organs through the medium of the branches of the sympathetic. And it is a dietetic measure, well founded in experience, to bring the abdomen into activity by violent running or other bodily exercise in a congested state of the vessels of the abdomen, in sluggishness of the alimentary canal, or in interruption of the secretory action of the liver. Thus, where no structural change is present, but only an insufficient stimulus gives origin to those affections, it appears that an increased activity of the spinal cord is not confined to the voluntary muscles alone, but is continued to all nerves that are in union with it. And as all the spinal nerves communicate with the abdominal ganglia in different ways, and especially through the phrenic (?), so the stimulating influence of the voluntary power, originated in the cerebro-spinal system, cannot fail to reach the abdominal organs. That such an excitation is produced on the intestinal canal through the intervention of the sympathetic, is rendered more than probable by my experiments. In rabbits, the movements of the small intestine were rendered very evident, at different spots, by mechanical and chemical stimulation of the phrenic nerve, even after all motion had previously ceased. From the difficulty of judging of the cause that originates the intestinal movement in recently-killed animals, I will at present only note these observations, and recommend them to the regard of others."

With this extract the notice of the work may be closed. In this monograph, professing to treat only of the phrenic nerve, there is much that is interesting to the anatomist: such as the structure of the vascular secretory fringes in the serous membranes in the chest and belly, the structure of the areolar tissue of the omentum, and the tendinous part of the midriff, with the different kinds of nerves that are found in the peritoneum, &c. &c. A careful perusal of the original treatise will afford much information in return for the time employed on it.

G. Viner Ellis.

Review V.

1. *Die Pathologischen Pigmente.* Von Rudolph Virchow. (Virchow und Reinhardt's 'Archiv für pathologische Anatomie und Physiologie.' Vol. I. p. 379, and 407. 1847.)

The Pathological Pigments. By Rudolph Virchow.

2. *Hæmatoidin und Bilifulvin.* Von R. Virchow. ('Verhandlungen der Physikalisch-Medicinischen Gesellschaft in Würzburg,' p. 305. 1850.)

Hæmatoidin and Bilifulvin. By R. Virchow.

3. *Beobachtungen über eine eiweissartige Substanz in Crystallform.* Von K. E. Reichert, in Dorpat. ('Mueller's Archiv,' p. 197. 1849.)

Observations on an Albuminous Crystalline Substance. By K. E. Reichert.

4. *Ueber das Milzvenenblut.* Von Otto Funke. ('Zeitschrift für rationelle Medicin, von Dr. Henle und Dr. Pfeufer. pp. 172—218. 1851.)

On the Blood of the Splenic Vein. By Dr. O. Funke.

5. *Neue Beobachtungen über die Krystalle des Milzvenen und Fischblutes.* Von Dr. O. Funke. ('Zeitschrift für rationelle Medicin,' von Dr. Henle und Dr. Pfeufer. Vol. II. p. 198. 1852.)

Recent Observations on the Crystals of the Blood of the Splenic Vein and of Fishes. By Dr. O. Funke.

6. *Ueber Blut Krystallisation.* Von Dr. O. Funke. (Zeitschrift für rationelle Medicin, von Dr. Henle und Pfeufer. Vol. II. p. 288. 1852.)

On Blood Crystallization. By Dr. Otto Funke.

7. *Ueber Krystallbildung im Blute.* Von. Dr. F. Kunde. ('Henle und Pfeufer's Zeitschrift.' Vol. II. p. 271. 1852.)

On the Formation of Crystals in the Blood. By Dr. F. Kunde.

8. *The Formation of Crystals in Human Blood.* By E. A. PARKES, M.D. ('Medical Times and Gazette,' 1852.)

9. *Atlas der physiologischen Chemie.* Von Dr. O. FUNKE.—*Leipzig*, 1853. *Atlas of Physiological Chemistry.* By Dr. O. FUNKE.

A NEW element has been recently added to our not inconsiderable stock of bio-chemical puzzles, which promises in many respects to modify the doctrines of the physiologist and the pathologist, and which appears even to affect the entire question of the distinction between the organic and inorganic kingdom. The theory of binary compounds characterizing chemical metamorphoses of the latter, while the former was supposed to evince an exclusive predilection for ternary or analogous combinations, has long since been shown to be untenable. It has still, however, been hitherto held to be the rule, that crystallization was peculiarly a property of inorganic chemistry, and that crystalline forms were only met with in the animal economy, when the material giving rise to them had lost all claim to be considered an integral part of the living tissue. The discovery of crystals which presented all the characters of an albuminous compound, and the subsequent discovery of the crystallizing power of all kinds of red blood, from whatever organ, or from whatever species of animal it may be taken; and the observation of analogous formations under circumstances which leave little doubt of their having been formed during life, renders it probable that a re-construction of some of our present doctrines will be necessary, in order to reconcile the facts which we shall lay before our readers, with our previous views of morphology. As yet we are but on the threshold of the enquiry. Only enough has been done to excite a lively interest. The metamorphoses to which we are about to advert, possess considerable attraction on account of the delicate manipulation required to detect or produce them, the elegance of the forms, and the frequently brilliant colouring characterizing them. The very circumstance, however, of the products being almost exclusively microscopic, has hitherto prevented the enquirers who have engaged in the subject from acquiring very positive data relative to their chemical constitution; hence the evidence in this respect is, as we shall see, mainly of a negative kind.* As we have no desire to prejudge the case, and as the circumstances have as yet met with very little attention in this country, we shall be justified in giving a detailed account of the observations hitherto made, and we shall do so, as far as can be done conveniently, in the chronological order of the publications from which our information is derived.

As it will be convenient to use a designation for the forms which are to engage our attention, and Virchow's term, *hæmatoidin*, has become in a measure established, we shall employ it generically for crystals derived from the blood. In doing so, we pledge ourselves to no theory, and any future determination of their exact chemical constitution will find us at liberty to modify our present views, or to classify under different species, varieties which at present it may be most convenient to regard as identical. The prevailing forms are the rhomboidal and the oblong rectangular, while

* Since this review was written, it has been stated by a writer in the Monthly Journal, that in a private communication, Professor Lehmann has announced that he has succeeded in isolating the crystals, and in proving their albuminoid composition.

the pink or ruby colour appears to be a uniform characteristic, the intensity of which may amount on the one hand to black, or on the other fade away entirely.

1. It appears that Sir Everard Home was the first to make an observation bearing upon this point. In a short tract on the formation of tumours, published in 1830, he gives a drawing of a section of an aneurismal tumour, in which some small crystals are observed; a microscopic view represents the latter as well-defined rhomboidal or oblong lamellæ, and their darker shading implies a deeper colour than the surrounding tissues. Sir Everard only speaks of them as the crystallized salts of the coagulum, and does not enter into any further particulars concerning them. They are, however, manifestly distinct from the well-known lamellæ of cholesterine.

2. The next reference we meet with on this point is in the 'Chemische und Mikroskopische Untersuchungen,' by Dr. Joh. Jos. Scherer (Heidelberg, 1843). In his fifty-ninth case, in which, in consequence of a contusion of the thigh, an extravasation of blood and pus had formed, he examined the blood discharged by an incision a few days after the contusion: this exhibited nothing remarkable; three days later, a further quantity of extravasated fluid was evacuated; this was red, and after standing a short time, separated into a reddish-white sediment, and a dark red serum. Upon the latter small white shining points were observed, proving under the microscope to be oil-globules; the majority of these exhibited an arborescent formation in their interior, and occasionally small reddish yellow rhomboidal crystals. Several of the arborescent forms resembling confervæ were also contained in the fluid, external to the fat-globules. Dr. Scherer expresses it as his opinion that the ramified forms were crystals of margarin, and the rhomboidal crystals cholesterine. A further evacuation, made three days later, exhibited only pure pus.

3. Zwicky,* in the following year, was the first to examine these crystalline forms more minutely. He found them in the corpora lutea of cows, rabbits, and pigs, and classed them under two heads, 1, yellow or chesnut-coloured prisms and needles; 2, square or rhomboidal plates of a more or less intense red. He observed that chlorinated water destroyed their colour, that the chesnut crystals were soluble in ether, and he concluded that they must be therefore assumed to be a fatty compound. The red crystals were produced by the action of various reagents upon the fat; they were not altered by alcohol, ether, liquor potassæ, acetic, muriatic, or nitric acids, while they were rendered blue and then converted by concentrated sulphuric acid into irregular black balls, which subsequently dissolved with disengagement of gas. These results appear so striking, and at the same time so positive, that it is much to be regretted that the experiments have not been carefully repeated and thoroughly sifted, with a view to determine their real value. We shall see that Virchow entirely denies the conclusions arrived at, that the crystals are of a saline and fatty constitution.

* In his inaugural dissertation, De corporum luteorum origine et transformatione; cum tabula. Turici, 1844. As we have been unable to obtain this work, we give this author's observations on the authority of Virchow, in Virchow und Reinhardt's Archiv für pathologische Anatomie und Physiologie, vol. i. pp. 379, 407. 1847.

4. Dr. Günsburg* is quoted by Virchow as the next person who observed crystals belonging to the same category; they are described as small cubical octahedra found in a thymus gland affected with fatty degeneration.

5. In the first volume of Rokitansky's Pathological Anatomy (published in 1846, p. 170), we find the statement that in the changes that ensue after hæmorrhage has occurred into the tissues, the hæmatin is subjected to a special metamorphosis; it is converted either in the corpuscles, or external to them, into a brown, rusty, yellow, or black pigment; this is seen in the shape of round corpuscles or granular molecules, either free or enclosed in cells, or attached to small prismatic crystals of triple phosphates," and at a subsequent part of the same volume (p. 302), the author attributes the varieties of colour, theoretically, in the different pigments, to combinations formed between hæmatin and acids like the carbonic or hydrochloric, giving rise to carbonates, muriates, &c.

6. We now arrive at the first comprehensive treatise† on the subject, based upon numerous researches and experiments, and attempting to harmonize the phenomena of crystallization with the known changes in the pigments of the animal economy. In the paper alluded to, Rudolph Virchow assumes, with Breschet, Rokitansky, and others, that the pathological pigments are derived from the colouring-matter of the blood. He expresses his views thus: the diffused colouring-matter passes into a granular form; the hæmatin-containing cells are metamorphosed into pigment-cells. Their colour is at first always reddish, subsequently assuming tints which appear to be uniformly the same, according to the organ affected; thus in the lungs and intestines the colour is always black, while in the skin it is orange, or reddish-brown; in the brain, purple; in the vessels, a dirty greenish yellow. The effect in the intestines is attributed to the presence of hydrosulphuretted gas. Virchow proceeds to describe the granular matter as being rarely spherical, but presenting a well-defined outline; the terminal step of the process of pigmentary formation he regards to be the conversion into crystals, which he finds to occur invariably in extravasated blood. He states that he first observed these crystals in 1844, but that his attention was not especially directed to them until the publication of Zwicky's memoir; he has since found them in coagula occurring in the Graaffian vesicles, the brain, the skin, and obliterated vessels, in hæmorragic infarctions of the spleen, and in abscesses of the extremities. He describes the crystals as oblique rhombic columns of a lateritious, yellowish-red or deep-ruby tint, according to their thickness, varying much in size, transparent, and occurring free or in cells. The crystals occasionally presented the size of ordinary triple phosphates in the urine; some, taken from an old apoplectic clot in the brain, measured 0 0042 lines in length, 0·0021 in breadth, and 0·0055 diagonally from one acute angle to another. By apposition of their narrow surfaces, they are apt to form

Fig. 1.

Virchow's hæmatoidin crystals.‡

* See Häser's Archiv, 1845, p. 104. This is another book not to be found in the medical libraries of the metropolis.

† Die pathologischen Pigmente, von Rudolph Virchow, in Virchow und Reinhardt's Archiv für pathologische Anatomie und Physiologie, vol. i. pp. 379, 407. 1847.

‡ Copy of plate in Virchow's Archiv, 1847.

strange figures resembling ribands. These crystals are met with at different periods after the occurrence of the extravasation. Virchow has found them on the 17th, 25th, 27th, and 29th days, showing that the process is rather a slow one, and one that appears to indicate a withdrawal of the effused blood from the vital power. This coincides with what we observe in the production of crystalline forms in blood removed from the body, to which we shall advert subsequently. A most important question, which Virchow determines in the affirmative, is the isolation of the pigmentary matter within the blood-corpuscles. Every microscopist knows that pigmentary matter occurs in two main forms, either as free amorphous granules, or enclosed in cells that vary in size and shape, but are considerably larger than the blood-corpuscle. The most manifest instance is the black matter of the pulmonary tissue, which may be seen as opaque, black, isolated granules, or invested by an oval or circular transparent envelope. Again, the coloured compound corpuscles that we find in inflamed tissues or in inflammatory exudations, may equally present the appearance of a mere accidental agglomeration, or the glomeruli may exhibit a sharp outline, indicating the presence of an albuminous sheath. The colouring matter of malignant growths exhibits a similar tendency to either mode of deposit. The theoretical view of these pigments being derived from the blood, has now been confirmed by the direct observation of Virchow, who concludes his first paper with the following remarks:—"The pathological pigment originating in hæmatin may be diffuse, granular, or crystalline; it may assume these forms within and external to the bloodvessels, within and external to cells; it may be yellow, red, or black, or exhibit any of the intervening shades; the hæmatin may have previously quitted the blood-corpuscles, and diffused in other parts, in order subsequently to become aggregated into granules or crystals; the blood-corpuscles may, however, unite directly, combine their hæmatin, and at once form granules or crystals."

In the second paper, Virchow enters more fully into the consideration of the effect produced by reagents upon the pigment-crystals. Their analysis is a matter of great difficulty, as no one has hitherto succeeded in entirely isolating them, and in obtaining a quantity sufficient for the determination of their chemical constitution. When we attempt to form crystals from the blood, the material causes the two glass plates to adhere so firmly, that even with the device of previously introducing a hair, it is almost impossible to cause any liquid to pass in; the operculum, with the greatest care, will often break rather than yield to the finest wedge. The most satisfactory mode that we have ourselves adopted, is to employ an animalcula-case, which consists in an object-glass, fitted in a brass ring, and covered with an operculum, similarly adjusted, by which means it is possible, not only in the first instance to adjust the exact mode of compression, but also to remove one from the other after the crystals have formed. Virchow describes liquor potassæ as the most effective reagent: he states the colour of the crystals under its influence to become more intense, after which the colouring matter is diffused, and lastly, dissolved. The concentrated mineral acids he found gradually to round-off the crystals and destroy them, the colour at first being rendered rather darker; the colouring matter could not subsequently be recovered, and nothing but a light, granular, membranous stroma remained. Sulphuric acid gave rise to a most beautiful play of colours, producing successively a brownish or purplish-red, green, blue, violet, red, and yellow. Acetic acid does not appear to have exerted any material influence upon the crystals, though it is stated to have occasionally had some effect upon the diffused yellow pigments, and in rare cases also upon the granular yellow pigment. Alcohol and ether were found entirely ineffectual. From these results he argues that the crystals cannot be of a fatty nature, as maintained by Scherer, and Zwicky, and Henle,* and we gather it to be his opinion, though not formally expressed, that the hæmatoid crystals are a combination of the colouring matter of the blood or hæmatin with protein-matter.

The Transactions of the Medico-Physical Society of Würzburg contain an article by the same physiologist on hæmatoidin and bilifulvin, which, though not strictly in the chronological order which we are pursuing, may most suitably be adverted to as a sequel to the memoir just considered. The author here again enters into the question of the ultimate constituents of the crystals, and their relation to the colouring matters of the blood. He commences by observing that he has frequently found the hæmatoidin crystals in connexion with the fat-cells of subcutaneous fat, or attached to free oil-globules, as, for instance, the exudation of a case of hæmorrhagic pericarditis, and in an echinococcus cyst in the liver; in each of these cases bright red spots were seen studding drops of yellowish fat, which, on microscopic examination, proved to be hæmatoidin crystals. From other observations, he is led to conclude, further, that fat not only exerts an attraction upon hæmatoidin, but also appears to promote the metamorphosis into hæmatoidin; thus in a case of amputation of the arm, four days after the operation, hæmatoid crystals were observed in the adipose cellular-tissue of the flap. The crystallizing process may take place under three different circumstances: first, in cells; secondly, in amorphous protein substances (in both cases from diffused hæmatin); thirdly, from heaps of blood-corpuscles, which coalesce with diffusion of hæmatin, and undergo a common metamorphosis. Having found the blood-corpuscles to contain, in addition to hæmatin, a protein-substance resembling fibrine (which he terms globulin), and a matter analogous to alkaline solutions of albumen, he infers, that in the blood-crystals, protein invariably co-exists with the hæmatin. At the same time, he observes, that as all protein substances occurring in the human body contain a small amount of fat, this adhering fat enters into the metamorphosis, and may be present in the colourless nubecula, which remains after the extraction of the hæmatoidin. He concludes the subject by maintaining that the crystallizing power is inherent in the protein, and that the colouring matter in the crystals bears no other relation to them than the metallic colours impregnating the crystals of quartz.

7. In this view Virchow is supported by the investigations of K. E. Reichert, in Dorpat, who, apparently without any knowledge of Virchow's first paper, or the other observations already alluded to, in 1849 published the paper which stands third in our list at the head of this article. In examining the membranes of the almost mature fœtuses of a guinea-pig, six hours after death, he found a substance upon the placentæ of each of

* Handbuch der rationellen Pathologie, vol. ii. p. 738.

Fig. 2.*

Reichert's albuminous crystals

the four animals contained in the uterus, which at first sight resembled dried blood. He was much surprised to discover that this substance was composed of tetrahedral crystals, surrounded by more or less mucus or epithelium. Though varying much in size, their goniometric relations were identically the same. They appeared to be perfectly homogeneous, and when cut through with a cataract-needle they suffered no alteration. A further minute examination led him to conclude that these crystals were of an albuminous or protein compound; when exposed to heat on an object-glass, they at first contracted, then carbonized, and were then volatilized; no residue remained after this process, which effervesced with acids. They were heated to redness in a vacuum with potassium, and a considerable amount of nitrogen was evolved. No change was produced by boiling them with alcohol, ether, sulphuret of carbon, fatty or ethereal oils. No acids or bases destroyed them after a brief application, but they were dissolved by the continued influence of concentrated nitric acid, and by boiling in a concentrated solution of potassa. When introduced into a strong glass tube, hermetically closed at both ends, and exposed to a temperature of 140° to 160° Cent. (252° to 288° Fahr.), they were entirely fused. The inclination of the planes to one another was 70° 3′ 43″, the height of the central axis from $\frac{1}{15}$ to $\frac{1}{600}$ of a Paris line; though of an intense blood-red colour, they were perfectly transparent. The smaller ones exhibited a slightly yellowish tinge. The colouring matter he believes not to constitute an essential feature, but to be derived from a foreign pigment. A parallel striation was often perceptible on the surface; they had a semi-solid consistency, and were elastic, for when compression was employed they could be reduced to a lamina, and on removing the compression they resumed their previous shape. They sank in water, as well as in the acids or alkalies with which they were brought into contact. The following is a brief summary of Reichert's experiments with various reagents:

Concentrated acetic acid caused the crystals to expand without in any way altering their character; they merely became lighter in colour.

Hydrochloric acid: the crystals expanded, and assumed a slightly yellow or brownish hue; otherwise unaltered.

Sulphuric and phosphoric acids acted like the last.

Nitric acid caused an enlargement of the crystals, and the evolution of some air-bubbles.

Solution of iodine produced no expansion, but an opacity and a dark-brown colour.

Liquor potassæ and liquor ammoniæ expanded and rendered them yellowish-brown.

* Copy of plate in Müller's Archiv, 1849.

From these results, Reichert argues that the crystals enter into combination with acids and alkalies, which uniformly enlarge them and change their colour, and that nitric acid converts them into xanthoproteic acid. The application of nitrate of silver, muriate of iron, and prussiate of potash, produced no marked alteration; but when the crystals had expanded under the influence of the acids, they instantly returned to their previous size when water was added. Reichert has instituted a series of experiments upon xanthoproteic crystals, which lead him to conclude that they suffer exactly the same changes under the influence of acids and alkalies as the crystals under consideration; and as it appears that every albuminous substance is converted into xanthoproteic acid by the action of concentrated nitric acid, the inference as to the albuminous character of the former certainly appears justifiable. Gluge* dissents from this doctrine, and assumes that the hæmatoidin exhibits a combination of colouring matter with carbonate of lime; this, however, is based upon only one observation of the evolution of gas-bubbles from the action of concentrated nitric acid; which in itself rather tends to fortify Reichert's view, the more so as we shall find that there is reason to suppose that the mere form of the crystals is, so to speak, accidental, and that while the blood of the same individual may be induced to assume various shapes, certain animals exhibit a tendency to produce peculiar and characteristic forms. It is not impossible that eventually the crystallization of the blood may become a means of predicating its origin, and that a practical application of the fact may be made in medico-legal inquiries, where a doubt exists as to the nature of a sanguineous fluid.

8. We have as yet only traced the formation of crystals from the animal fluids or the colouring matter of the blood *after* its elimination from the blood-corpuscles. Kölliker† is the first who observed them *in* the corpuscles. In the blood of a dog's liver he found a great number of blood-globules, containing from one to five rodlets of a dark yellow colour, which remained unchanged in water, but seemed to disappear under the influence of acetic acid. He next observed the same appearances in the splenic vein of a dog, and of the fresh-water perch, and in the splenic pulp of the latter; the crystalline rods varying from $\frac{4}{1000}$th to $\frac{6}{1000}$th of a line, occasionally of the entire length of the diameter of the blood-corpuscle, at other times less. On the application of water, a membrane was upraised from these small rods, and a nucleus came into view. An accurate examination clearly proved that they lay in decoloured blood-globules; and in unchanged corpuscles the gradual formation of one or two of the crystals could be followed. An enormous quantity of really free crystals were found in the spleen of the barbus fluviatilis, sparingly in the kidneys, liver, and the blood of the heart. They were of a nail or spindle shaped form, and of a violet or reddish colour, and dissolved freely on the addition of acetic acid, leaving some colour behind. Kölliker views these crystals as allied to the hæmatin of the blood, and suggests that they may be identical with Virchow's hæmatoidin: he considers their occurrence in the spleen to be of physiological interest. Kölliker, who regards the function of this organ as consisting mainly in a destructive metamorphosis of the blood-corpuscles, would

* Atlas der pathologischen Anatomie, 1850, Art. Histologie, Plate I. No. 47, and p. 24.

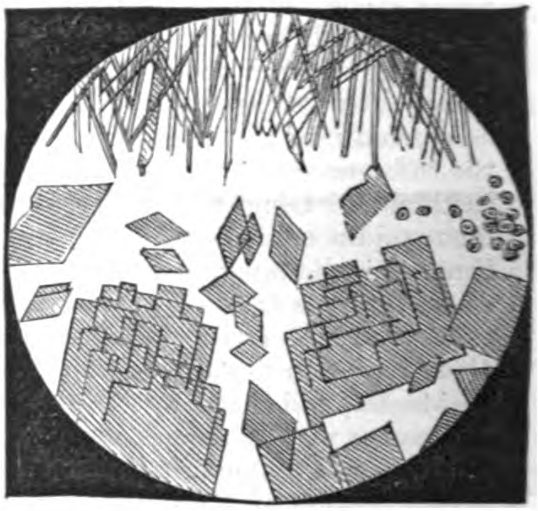
† The Cyclopædia of Anatomy and Physiology, by R. B. Todd, M.D., Art. Spleen, by Professor Kölliker, 1849.

probably give an explanation to account for the presence of the crystals in unison with this retrograde process.

9. The converse theory, that the spleen is rather destined to form new blood-globules, is maintained by other physiologists, among whom we espe-

Fig. 3.

Fig. 4.*



cially mention Dr. Otto Funke, as his researches into the functions of the spleen have led to his discoveries regarding the crystallizing power of the blood in various animals. He justly argued, that to determine the functions of the organ in its influence upon the vascular current, a result would be most satisfactorily attained by comparing the ingoing with the outgoing blood, the arterial with the venous. His first series of experiments were performed on the blood from the splenic vein of the horse, in pursuing which he met with a phenomenon which he describes in the following words:

"If we add water to a drop of blood spread out upon the object-glass, when it has just begun to dry up, in consequence of spontaneous evaporation, and observe the edges of the heaps of blood-corpuscles, they are seen to undergo a sudden change. A few blood-corpuscles disappear, others receive dark thick edges, become angular and elongated, and are extended into small well-defined rodlets. In this manner an enormous quantity of crystal-embryos are formed, which are too small to enable us to determine their shape; they rapidly extend more and more lengthways, while the transverse diameter remains the same, or increases but slightly, the entire field of vision being gradually covered with a dense network of acicular crystals crossing one another in every direction. This process takes place with such extreme rapidity, that it is difficult to observe the first origin and the gradual development of the crystals; for this reason, it was only after numerous observations that I convinced myself that the crystals are really formed from the blood-corpuscles."

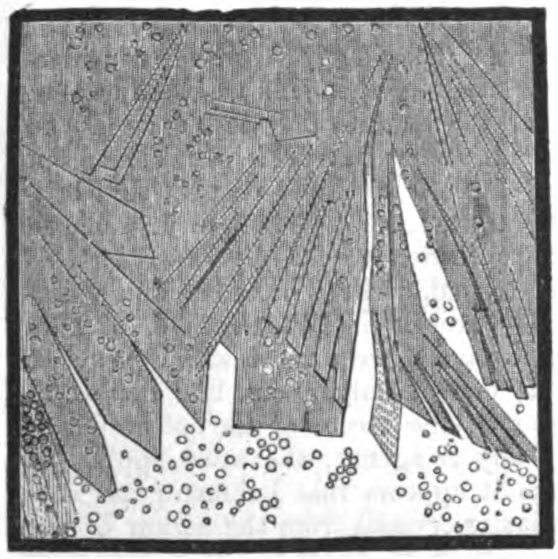
The crystals themselves presented two forms, the one exhibiting an acicular prismatic shape, the other occurring as rhombic plates. The majority of the former disappeared after a few hours, the fluid drying-up contracted into a few red plates and bands; in this concentrated residue, pale cholesterin-like rhombic laminæ were developed, which varied much in their

* Figs. 3 & 4 are copies of plates in Funke's Atlas.

dimensions. The acute and obtuse angles measured (by Schmidt's microscopic goniometer) respectively about 60° and 120°.

The blood-corpuscles never entirely disappear from the field after the formation of the crystals—an observation which we have also made in our experiments upon the crystallizing power of human blood; they remain upon and about the crystals, and seem to indicate a varying power of resistance to solvents, while their occasionally darker but more frequently lighter hue suggests a different chemical constitution from those which have undergone the crystalline metamorphosis. In the adjoining drawing (fig. 5) of crystals, which we succeeded in forming from blood squeezed

Fig. 5.



Hæmatoid crystals formed in blood of a deeply-congested lung, on addition of a minute quantity of water, and allowing it to evaporate for ten hours under an operculum.

Fig. 6.*

Crystals formed from coagulum in a man's heart, one hour after putting-up the preparation. No blood-corpuscles remained, but some granular colouring matter was seen in the fluid surrounding the crystals. These were transparent, and of a crimson hue. Magnified 400 diameters.

out of an apoplectic lung of a female, this is attempted to be shown. The whole field presented a pinky hue, in which the blood-corpuscles were seen; the crystals were several shades darker, but of the same tint, and upon them were also corpuscles slightly varying in size and depth of hue. To exhibit the fact that the crystalline forms vary much according to accidental circumstances, we also annex two other drawings of crystals obtained from human blood. Those in fig. 6 were formed by treatment with water, within an hour after putting-up the preparation. The blood was a part of the coagulum taken from the heart of a man. At the time this second specimen was prepared, the blood-corpuscles had ceased to be visible. The third illustration (fig. 7) we subjoin in order to show that the ordinary laminated appearance of the crystals formed in the human blood is a fortuitous circumstance, probably depending upon the approximation of the glass plates between which they have formed. The three crystals to the

* Figs. 5 & 6 are copies of drawings by Dr. Sieveking.

left in the drawing were formed in a drop of blood squeezed from the apoplectic lung, and though isolated, they presented a deep ruby colour. The three crystals to the right of the drawing appear to be hexagonal columns,

Fig. 7.*

Large ruby crystals formed in human blood.

or they may be four-sided prisms, but they manifestly possessed considerable thickness. The latter were formed from the same heart-blood used in producing the laminar crystals (fig. 6); the animalcula-case was employed in this case; on removing the operculum the crystals were persistent, but instantly disappeared on the addition of acetic acid, and without that brilliant play of colours which liquor potassæ exhibited in one instance in which we were satisfied of its actual application to the crystals. In this case the crystals were immediately destroyed, and the most brilliant variation of yellow, green, pink, crimson, and scarlet (no blue), ensued. It may be well to add, that the drawings, figs. 5 and 6, were taken with a power of about 400 diameters; while for fig. 7, one of only about 250 diameters was employed. In Funke's experiments, the serum of the splenic blood could not be made to crystallize by any re-agents; the same applies to blood taken from other parts; though it appears that Lehmann has succeeded in obtaining a network of acicular crystals from the serum of the portal blood of the horse on the addition of water.† In a report of M. Kunde's observations, to which we shall shortly advert, in the 'Gazette Medicale' for May, 1852, it is also stated that Messrs. Robin and Verdeil produced tetrahedral colourless crystals, like those described by him, from the serum of the blood alone, but no particulars are given.

Funke has extended his experiments to the blood of other animals; he has found the same tendency to exist in the splenic-vein blood of the dog, when perfectly recent, showing, as he conceives, that it is not the result of post-mortem changes. The blood of fishes is eminently crystallizable, but he failed to obtain crystals from the blood of the splenic vein of the bullock. According to his observations, the crystals dissolve easily in all reagents; he met with none in which they did not dissolve, and has failed in all his attempts to concentrate the crystallizable substance. We are obliged to omit all reference to other matters treated of in Dr. Funke's elaborate papers, though we do not undervalue the importance of his investigations, in their bearing upon the physiology of the spleen. We therefore turn to his second paper, in which he more particularly examines the crystallizing power of the blood of some fresh-water fish. By treating

* Copy of drawing by Dr. Sieveking.

† A paper by Professor Lehmann, to which we have not had access, is quoted by Dr. Funke as containing some experiments on the crystallization of the blood, in Berichte über die Verhandlungen der Königlich Sächsischen gesellschaft der Wissenschaffen der Leipzig, 1852.

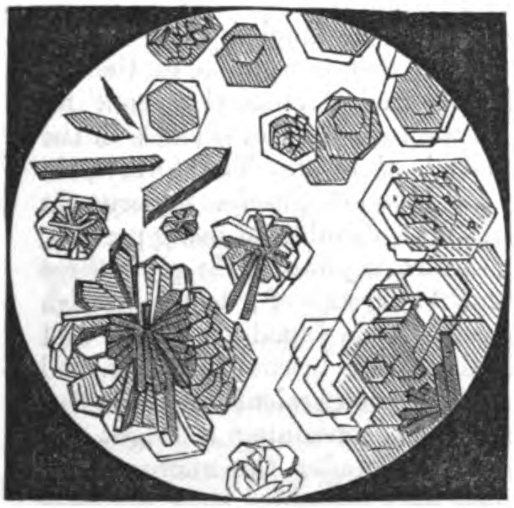
the recent blood of Leuciscus dobula, Cyprinus erythropthalmus, and Abramis blicca, by partial evaporation, and the subsequent addition, under the operculum, of a small portion of water, he has found that all the blood-corpuscles were converted into crystals, these being generally half or twice as long as the former, their ends rectangular, and their sides rectilinear or slightly curved. This phenomenon he explains by assuming that the contents of the blood-corpuscles crystallize without a rupture of the envelope, and that the resolution of the crystal enables the corpuscle to return to its normal shape. On the further addition of the smallest quantity of water, the crystals are re-converted into blood-disks. Some variations in the mode of crystallization are produced by varying the amount of water added; if it be equal to about half the quantity of the blood, the corpuscles resist its action for some time; after a few hours they appear distended and pale, and finally they vanish. At a certain point of dilution, the preparation after a few hours exhibits a close resemblance to the frozen vapour on a window; the entire field is covered with a layer of acicular crystals arranged in the form of plumes. Very regular crystallization ensues if equal parts of water be added after partial evaporation; large isolated, well-defined, quadrilateral prisms, varying much in size, result. They may attain a length of two or three lines, and then become visible to the naked eye. Some are as thick as they are long, others so thin and pale, that the vertical lateral planes are not to be recognised. If the blood-corpuscles were closely aggregated, the resulting network of crystals was so dense that it resembled a confused mass of needles. The colour was vermilion. Funke failed to produce the large laminated crystals in fishes which were met with in the blood of horses. It does not appear that he has made many experiments upon the crystallizing power of human blood; he states that he obtained crystals in two instances, in which he tested some blood from the splenic vein; in one there was leucocythemia, in the other the spleen was healthy. The crystals that resulted presented a double character; there were large numbers of very long, irregular, narrow rods, arranged in pencils, or like antlers, in a drop allowed to crystallize under an operculum, without the addition of water; when treated with water, the blood crystallized slowly, and in addition to the rod-shaped forms, two varieties of rhombic plates appeared: the one small, very dark coloured, and isolated, the other in composite layers resembling cholesterine, red, and almost rectangular. Both varieties are considered as belonging to the same crystallometric system, of which the rhombic form would represent the type, and to which all transition forms may be referred.

Funke describes these crystals as extremely destructible, and so undoubtedly they are; but scarcely to the extent he states, for he has found that the mere raising of the operculum instantly converts them into a formless mass; by employing an animalcula-cage, as above mentioned, we have succeeded in maintaining the crystals after removing the operculum, and we are inclined to think that their destruction in ordinary cases is the result rather of the adhesion between the two glasses than from any destructive influence of atmospheric contact. Funke supposes the crystallization to depend upon the crystallizable material being dissolved by the water within the envelopes, and being set free by the dehiscence of the latter, to assume its characteristic forms as soon as the fluid attains that degree of concentration in which the crystallizing material begins to be insoluble. If the quantity of water only suffice to distend, but not to burst the envelope, the crystal forms within the latter, and is coated by it, and on the further addition of water the crystals are re-dissolved, and the corpuscles restored to their original form. His view as to their constitution does not essentially differ from that of Reichert, in regard to the crystals discovered by him; he considers them to consist of the albuminous contents of the blood-corpuscles combined with hæmatin, and he regards them as entirely distinct both from the crystalline forms observed in the blood by Kölliker, and from the hæmatoid crystals of Virchow. The only reagents of whose action he obtained satisfactory proof were alcohol, acetic and nitric acids, by each of which they were dissolved.

In the very careful and well-executed drawings contained in the chemico-physiological Atlas, by the same author, the title of which terminates the list of works given at the head of this article, the various crystalline forms derived from the blood of man and animals are represented with the hues they exhibit. We can strongly recommend this work to those of our readers interested in the subject; for the drawings are admirably executed, and most faithful representations of nature; while the explanatory letter-press is not overloaded with dry technicalities; the very moderate price further enhances the pleasure of contemplating the evidence of Dr. Funke's graphic powers. The definite crystallometric character which many would appear to possess, certainly indicates a typical distinction bearing a ratio not yet sufficiently determined to the genus from which it is derived. Many varieties occur in the same blood which may be fairly referrible to the same fundamental type, but we should scarcely be justified in insisting upon an identity of constitution in crystals that respectively exhibit the rhombic, the tetrahedral, or the hexagonal type. The blood of the squirrel (fig. 8), for instance, forms regular hexagons, which but for their colour closely resemble cystic oxide; the blood of the guinea-pig (fig. 9) invariably appears to form tetrahedral pyramids, we have ourselves seen them within two hours after death in a minute renal vessel of this animal, filling up the entire calibre of the channel. The case is of some interest, as showing that the contact of the air is not an essential ingredient in the production of these forms. In the horse, as in man, the rhombic form may be considered typical; the blood-crystals of the cat are regular quadrilateral or hexagonal columns, while those obtained from fishes (Leuciscus dobula) are more of an acicular character, though also presenting columnar forms.

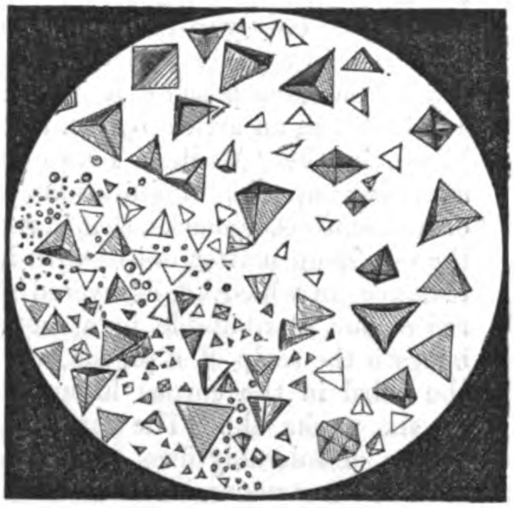
10. Kunde confirms most of the observations made by Funke, though he is inclined to doubt the fact of the crystals being formed directly from the blood-corpuscles; the positive testimony of Funke ought, however, to outweigh a negative opinion, the more so as his confrère has not examined the blood of the same fishes, and as Kölliker has also noted the actual presence of crystals within the blood-disk. The animals whose blood Kunde has experimented upon are the ox, the horse, the dog, the guinea-pig, the squirrel, the rat, the mouse, the bat, the rabbit, the pigeon, the turtle, the leech, and the frog. In all, excepting in the frog, he succeeded, though with more or less difficulty, in producing crystals, and if we admit the exception as

Fig. 8.



Crystals formed from the blood of the jugular vein of the squirrel.

Fig. 9.



Crystals obtained from the blood of the guinea-pig.*

proving the rule, we may assume with him that all red-blooded animals exhibit this property. Funke states that he has occasionally obtained crystals by treating a drop of the recent blood from his own finger with water. In this Kunde has failed. In several experiments of the same kind, we must confess that we have also been foiled; while we have met with little difficulty in securing the most beautiful crystalline forms from human blood taken from the lungs or the heart of the dead subject. The effect of reagents upon the crystals, and especially upon those produced from the blood of the guinea-pig, though the latter were identical in form and colour with those spoken of by Reichert, induced Kunde to assume an essentially different constitution between the two. Every chemical applied, dissolved the blood-crystals, and they were soluble in water at a comparatively low temperature (40°—45°C.=62°—71°F.), while it will be remembered that Reichert's crystals exhibited a very indestructible character. Nor has Kunde ever found the crystals pre-formed on the mucous surfaces, but he has observed that if the liquor amnii of a guinea-pig be allowed to evaporate a little, and then covered with an operculum, crystals result that closely resemble hippuric acid; the day after the preparation was put up, regular, colourless, transparent cubes and octahedra were found, adjoining crystalline masses of an irregular outline. The question whether the blood-crystals are identical in different animals, is answered by Kunde, as we should anticipate, even from the manifest difference in the facility with which their blood crystallizes, and the distinct types exhibited, in the negative. Their solubility in water is greater in man than in the guinea-pig and squirrel, and those of the latter

* These appear identical with the albuminous crystals above described as having been discovered and analyzed by Reichert. The crystals form easily, and are a pretty microscopic object. The tetrahedral is the prevailing form, and most of the irregular aggregations that we meet with in our experiments are referrible to this type, though we have not seen any transition forms which we could look upon as connecting links between the tetrahedral and the polygonal, of which a specimen is seen at the lower margin of the field in fig. 9. Figs. 8 & 9 are copied from Funke's Atlas.

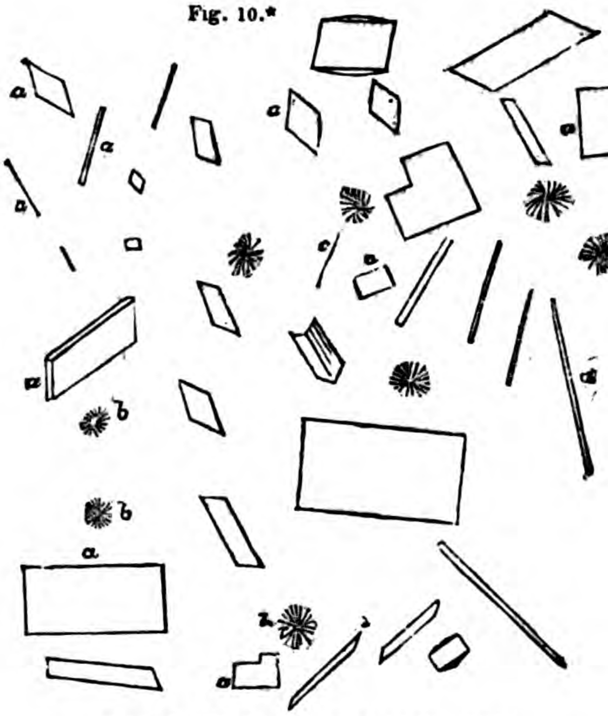
are found to be insoluble in perfectly cold water, as well as in alcohol and ether.

11. The only records of blood-crystals given by English observers are contained in a paper by Dr. Sanderson, on the metamorphosis of the coloured blood-corpuscles, in the 'Monthly Journal of Medical Science' for 1851, and in an article by Dr. Parkes,* on the formation of crystals in the blood, in the 'Medical Times and Gazette' of 1852. The former, in summing up the modifications which the blood-corpuscles undergo in extravasations, remarks, that in rare cases, a crystallizable fat being present, the colouring matter unites with it so as to form crystals. He gives three instances in which he discovered rhombohedral crystals of a deep yellowish red colour, attributable to a metamorphosis of the blood; one in a cyst beneath the scalp of a pigeon, another in the corpus luteum of a cow, and the third in the corpus luteum of a human female, calculated to be five or six weeks old. The latter is described as containing a bright red centre of tolerably firm consistence, presenting under the microscope a fibrillating structureless material, in which were imbedded large numbers of decoloured blood-corpuscles, others more or less converted into grains of pigment, spherical or ovoidal bodies of about $\frac{1}{4000}$ inch in diameter, and of a brick-red colour, and crystals of a rhombohedral form, and very regular, but of small size. Intermediate between these, there seemed to be semi-crystalline bodies, as well as irregular masses, all of which consisted of the same brick-red pigmentary material, which was unaffected by acetic acid, diluted mineral acids, or caustic potash.

12. Dr. Parkes' paper details the appearances observed in blood that has been allowed to undergo partial decomposition. In one case some half-putrid, half-liquid blood, obtained by venæsection, exhibited, after the addition of water, prismatic crystals of various lengths, cut off sharp at the ends, or tapered; they occasionally appeared to emerge from the sides of a large red mass, which was also penetrated in all directions by them, and which seemed to have been originally formed of an accumulation of red corpuscles, but no perfect red corpuscles could be seen. In another case, blood drawn from the arm, and allowed to remain in a stoppered bottle for three months, until it was covered with a thick crust, exhibited in the subjacent oily purple liquid myriads of more or less deep-red crystals of various forms; most of them tables of some thickness, others thinner or heaped together so as to look like cholesterine; others long and rod-like, with straight or acuminated terminations, readily dissolving in water, and destroyed by exposure to air. (Fig. 10.) Mixed up with these were circular agglomerations of acicular crystals, apparently consisting of margaric acid, and insoluble in water, though soluble in ether. Again, some defibrinated blood was exposed to the air until it had assumed a syrupy consistence; this showed no crystals until water was added, when, after two or three hours, numerous needle-shaped and rod-like crystals were produced, which were entirely soluble in acetic acid. The same blood being allowed to dry, and water being then added, no crystals were visible after

* In Schmidt's Jahrbuch, 1852, No. 12, p. 383, it is stated that Dr. Parkes' paper is an account of Funke's and Kunde's observations. This is erroneous, as the paper was written before Funke's second paper reached England, and appeared before his third paper and the paper of Dr. Kunde (in which the crystallizability of all sorts of blood was recognised) were published in Germany.

Fig. 10.*



a, a. Soluble in water. *b, b.* Insoluble.

two days; but after a lapse of five days a few acicular crystals and numerous margaric-acid crystals became apparent. In a specimen of blood taken from the jugular vein of a man who died of Bright's disease, and allowed to stand for a fortnight, until it had become ammoniacal, the microscope detected triple phosphates, small rod-like crystals that were insoluble in acetic acid, and margaric-acid crystals. In other experiments Dr. Parkes has observed large pale crystals of an irregularly prismatic form, resembling some kinds of phosphates. He points out that if we exclude the known forms of margaric acid and the earthy phosphates, three varieties of crystals remain, which are distinguishable

1. By perfect solubility in water.
2. By insolubility in water, by the addition of which they are produced, and by solubility in acetic acid.
3. By insolubility in water and acetic acid.

The foregoing analyses rather exhibit the complicated nature of the question we have to deal with, than enable us to arrive at a definite conclusion already; but we would suggest, that until good grounds be shown for an adverse opinion, we should not treat the forms discovered in recent blood as necessarily identical with those found in blood that has manifested symptoms of evident decomposition. We are inclined, from our own observations, to infer that while under certain conditions recent blood exhibits a tendency to form crystals, a period arrives at which this tendency disappears, and is replaced by a second stage of crystallization, in which

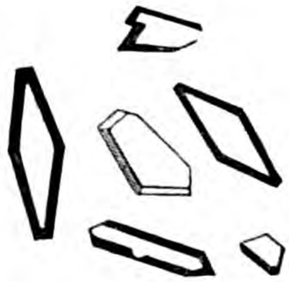
* Copy of plate in Medical Times and Gazette.

the ordinary affinities of inorganic nature come into play. In the sequence of time we must establish an earlier stage of crystallization than either of these two—viz., that in which it is manifested in the living body itself. Here the tendency of the blood to give rise to crystalline forms, exclusive of those of the well-known salts, is evidently greater than has been hitherto supposed. In the human body we have repeatedly met with crystals that we were justified in regarding as pre-formed during life. In two cases of enlarged spleen, and one of a melanotic character, that fell under our notice in 1852, we were surprised to find a large number of rhomboidal and prismatic crystals (figs. 11 and 12), chiefly in the vicinity of the capsule,

Fig. 11.

Crystals found under the sheath of a spleen of normal size in a man æt. 70. Magnified 400 diameters.

Fig. 12.*



Crystals found in a spleen containing melanotic deposit, somewhat enlarged and firmer than in the normal condition. Magnified 400 diameters.

which were imbedded in the soft tissue of the organ, but only exhibited a slightly yellowish appearance, which seemed to be merely a reflex of the adjacent tissues. They did not present the definite cherry or crimson hue displayed by Virchow's crystals, or the blood-crystals that we have succeeded in forming from the blood. They disappeared on the addition of acetic acid, with an evolution of air-bubbles, but were not altered by liquor potassæ. We have since examined numerous spleens, with the view to examining the nature of these crystals more minutely, but have only recently succeeded in again meeting with them, though without being able to isolate them, or define their constitution. Here they were accompanied by numerous opaque, black, rectilinear bodies, differing in size and outline, but generally approaching a quadrilateral form; concentrated sulphuric acid did not change them. The well-defined linear margin clearly distinguished them from the amorphous forms in which black pigment ordinarily appears, either in the lungs, the intestines, the spleen, or other organs. While, however, some of these formations leave a doubt whether they are directly derived from the blood, we meet with other crystals in pathological products resembling blood-crystals, where they are manifestly not immediately deducible from this source. Thus, in a case of recent pleuritic effusion of straw-coloured fibrine, investing the lower left lung of a man aged forty, among numerous prismatic crystals, was a crystalline mass of reddish-yellow colour, apparently composed of several layers of rectangular laminæ. Acetic acid readily dissolved the various forms. In the same case a drop

* Figs. 11 & 12 are from drawings by Dr. Sieveking.

of blood from the lung, covered by an operculum, was found after twelve hours to exhibit numerous rectangular and prismatic colourless crystals, which apparently were not touched either by acetic or muriatic acid.

It does not appear that the coloured rhomboidal crystals of Virchow are capable of being artificially formed. In our experiments of producing blood-crystals from human blood, we have experienced no difficulty in forming the laminated rectangular and prismatic crystals described and delineated by Funke; and on one occasion, while fan-shaped aggregations of these were still visible in the same field, there were several delicate, well-defined, perfectly colourless rhomboids, which contrasted strongly with the brick-red material surrounding them. The quantity of blood employed has a marked influence in determining the arrangement of the crystals, as well as their dimensions. In following the directions of Funke we commonly obtain asteroid groups, or long rows resembling a paling, of well-defined crystals of a laminated character, of considerable length, and cut off square or in lance-points. It is, however, not an essential character that they are merely laminated, for the same blood may yield larger crystals, in which the edges exhibit considerable thickness, or in which the prismatic character is undeniable. In a successful case the crystalline forms occupy so large a space, that this circumstance alone would justify the conclusion that they are composed of the bulk of the material spread out on the object-glass. The further evidence that has been detailed seems to warrant the inference, that while there are essential differences between many of the forms hitherto described as being derived from the blood, either within or external to the body, they exhibit a tendency of a protein compound to crystallize, not hitherto admitted.

E. H. Sieveking.

Review VI.

Ueber Geburtshülfe und Gynækologie in Frankreich, Grossbritannien, und Irland; grossentheils nach Reiseergebnissen. Von Dr. F. H. Arneth, d. Z. suppl. Primarius am k. k. Gebärhause, und an der Abtheilung für Frauenkrankheiten, &c. &c.—*Wien*, 1853. 8vo, pp. 360.

On Midwifery and Gynecology in France, Great Britain, and Ireland; chiefly drawn from Observations made during Travels in those countries. By Dr. F. H. Arneth, at the time principal Assistant-Physician to the Imperial and Royal Lying-in Institution, and to the department of the Hospital appropriated to the Diseases of Women, Vienna.

Perhaps the most striking characteristic of the present age, as distinguished from all previous times, is the extent to which international communication is now happily being carried; men are, in a sense and to a degree which never before were possible, running to and fro, and knowledge is, as a necessary consequence, being largely increased. The earth itself promises ere long to be, in yet another mode, bound into one vast organism, and human thought to be transmitted between the most distant climes, on the chords of a newly developed, as it were, nervous system. And there are probably none who must benefit more from the daily increasing facilities for the interchange of ideas and of the knowledge of newly-ascertained facts, than the students of an experimental and inductive science like medicine. But not the least interesting or useful circumstance connected with the state of things we have alluded to, is the power we derive from it of "seeing ourselves as others see us"—in other words, of learning the impressions made by our views and institutions upon foreigners, and of reading, in the mirror which they hold up to us, the history of our defects and our advantages.

For such reasons we hail with pleasure the appearance of a work by an intelligent and impartial observer like Dr. Arneth, chiefly devoted to an examination of the medical institutions of these islands. For although the title-page professes merely to embrace the obstetric branch of medicine, the author does not confine himself to this department, but enters into a review of the systems of education, both professional and general, and of the colleges and hospitals of the several countries he has visited, and concludes his work with a comparison of English and German midwifery practice.

In the preface we are informed, that after the author had spent three years as assistant-physician to the great Lying-in Hospital of Vienna, he undertook a journey through Germany, France, Great Britain, Ireland, and Belgium, in order, by personal observation, to learn the state of midwifery in those countries. As time unceasingly advances, and as much of what he has seen and described may in a short space have undergone great alteration, he thinks it right to mention, that his visit to Paris occurred in the months of November and December, 1850, and January, 1851; to London in January and February, to Dublin in March, to Edinburgh in April and May of the latter year.

The author first enters upon an examination of the systems of medical instruction prescribed at the three universities of Paris, Montpelier, and Strasburg, and at the secondary schools of France, which he afterwards compares with the plan pursued in Austria. He is most struck with the shortness of the period of clinical medical instruction required in the former empire (one year against two in Austria); and with the entire omission of ophthalmic surgery, which in his country is so much attended to, and has so properly its separate chair and clinique. On the other hand, midwifery, which in Austria is unaccountably struck out of the official plan of study, is obligatory in France, and is taught theoretically during six months, and practically during an entire year. He cannot understand why materia medica and pathological anatomy, which in Austria are placed at the commencement of the practical studies, of which the latter branch of science at least is one of the chief pillars, should in France be deferred to the last half-year.

Hospital visiting is to commence in the second session, in order that the student may become acquainted with the details of so-called "minor surgery." This and other regulations show a desire on the part of the French medical legislators to favour practical studies, and may partially compensate for the short period of proper clinical instruction above alluded to.

The uncommonly great number of examinations and "concours," which continue in an uninterrupted chain during the whole period of study, and are afterwards resumed for those who wish to obtain appointments, is characteristic of France, and quite corresponds to the genius of the people, and may give a certain readiness in disputation, which, while it is in its higher degree neither always necessary, nor a proof of a greater amount of knowledge, may have its advantages, and probably has advantages for so mobile a people as the Gauls, though it seems to be less suited to the German character; at the same time, the author observes that he cannot deny that he has seen similar regulations in operation among the less excitable Britons, and apparently with favourable results.

In the examination for the degree of doctor, the French regulations appear to the author to be in many respects preferable to those of his own country, and chiefly in this: that while in both countries the necessity of making the examination practical, and not merely theoretical, is recognised —in Austria the examiner represents a patient, and submits to be questioned by the candidate, who is required, from the description he obtains in answer to his queries, to state his opinion as to the diagnosis, prognosis, and treatment of the disease under which the examiner supposes himself to labour, and within a quarter of an hour at least to name the disease; while in France, on the contrary, he is brought to the bedside of one or more patients in the hospital, where not only his *knowledge*, but his *power* of using his knowledge, are practically tested; the superiority of this latter mode is obvious.

In France, the "faculty" consists only of the college of professors, with the Dean elected from among themselves at their head; the other doctors form no part of the corporation, and consequently exercise no influence on the renovation of their order. In England, on the other hand, the right to practice is only obtained through admission to a society (College of Physicians, College of Surgeons, or Apothecaries' Company), the members of which are but exceptionally professors. Both the Austrian and French regulations have the advantage over the English of uniformity in requirements from candidates for practice, while each of the English corporations has its own regulations, whence arises the great variety to be found in this respect in the British isles.

In 1851, 1352 doctors of medicine were practising in Paris, of whom the amazing number of 366 were decorated with the Order of the Legion of Honour.

A great advantage enjoyed by France is the simplicity of the division of the profession, there being since 1803 only doctors of medicine and of surgery (the education of whom is quite similar, with the exception of a slight difference at the *last* examination, according as the candidate intends to devote himself to one or the other branch of science), and officers of health; which contrasts favourably with the separation in England into physicians, surgeons, general practitioners, and apothecaries; and in Austria into doctors of medicine, doctors of surgery, and masters and patrons of surgery. In France, the members of the more highly educated branches, the doctors, far outnumber the officers of health; while in England the apothecaries, and in Austria the patrons of surgery, whose requirements are least, are unfortunately the most numerous.

Having thus given a sketch of the state of medical education in France, the author next proceeds to describe the hospitals of that country, dwelling more particularly on those devoted to midwifery. The first he notices is the Strasburg hospital, which has the great and rare advantage of containing a clinique for children. The lying-in house of this town is divided into two parts, "la clinique" for physicians, and "le service," in which midwives are instructed; the deliveries amount only to about 120 in the year.

Dr. Arneth next conducts his readers to Paris, the general hospitals of which city collectively contain 3420 beds, while those for special objects possess 2784: of the latter, the Hôpital des Enfans Malades has 600 beds; La Maternité 514. In the Hôpital des Cliniques there are also wards appropriated to midwifery under C. Dubois. The direction of the studies both at the "Clinique," where medical students are instructed in practical midwifery, and at the Maternité, where midwives are educated, is placed under the eminent Paul Dubois.

In none even of the smaller Parisian institutions did Dr. Arneth find the retirement, stillness, and domestic appearance which characterize many of the English hospitals. The surprise which the splendour of the wards at first excites in strangers soon gives way to unfavourable impressions created by the over-crowding and imperfect ventilation of the apartments, and the want of cleanliness in the inferior attendants.

The lying-in women in the Maternité are lodged in two great wards, each divided by partitions into thirty-six small apartments, resembling cells, the arrangements for ventilating which are so very defective, that the "puerperal odour" prevails in most disagreeable intensity; and there can be no doubt that this want of sufficient ventilation is a main cause of the great mortality for which these apartments are so notorious. A table is given at p. 46, showing the comparative average mortality in the three largest lying-in hospitals of Europe—viz., the Maternité of Paris, and the hospitals of Vienna and Dublin, for the years 1828—1849, both inclusive: in the first-named institution it amounted to 4·18 per cent.; in the second, to 5·35; and in the third, to but 1·34.* In a note to p. 45 the author points out the curious fact which appears to be established by ample statistical observations, that proportionally more boys are the result of a given number of legitimate than of illegitimate births.

Dr. Arneth observes, that as the midwives at the Maternité do not apply themselves to anatomical investigations, the prevalence of puerperal fever at that institution proves that the disease is not exclusively attributable to the examination of patients without sufficient attention to washing after dissections; but that its source is principally to be found in the morbific matters impregnating the atmosphere which surrounds the patients.

It appears from the table, that the most unfavourable years at each of the three great hospitals by no means coincide—a fact which would show that the opinion of those who hold that a peculiar constitution of the air capable of producing puerperal fever prevails at certain times simultaneously throughout the whole of Europe, is incorrect. The same want of agreement has been observed in the two divisions of the Vienna hospital, and also between the "Clinique" and the Maternité, at Paris.

The great lying-in hospital of Vienna, which dates from 1784, is situated in the general hospital, and is divided into two parts, one of which is appropriated to the instruction of medical students, the other to the education of midwives. The former usually take out their practical midwifery at the end of the fifth year of their medico-chirurgical studies. Having previously attended a six-months' theoretical course on the subject, they spend two months in this practical school; but this is not obligatory on all medical students, nor does practical midwifery form a subject of examination for the degree of doctor of medicine and surgery, which consequently confers no right to practise this branch; in order to obtain the degree of "Magister artis Obstetriciæ," they must, previously to admission to the special examination, attend a second practical course of two months' duration.

The author next devotes fifty-two pages to an examination of the differences in practice between the French and German obstetricians, which he prefaces with a description of Paul Dubois, who has for years been at the head of the two greatest midwifery institutions of France, and who appears to reign as autocrat in that branch of science; for in France, observes Dr. Arneth, there is no obstetric republic; all standards are lowered before Dubois—a position the more singular, as a similar supremacy had been yielded to his father before him. Interesting and useful as an investigation of this important portion of Dr. Arneth's volume would be, it includes so great a variety of subjects that we must content ourselves, on the present occasion, with simply enumerating the principal points he passes in review. They are—incision of the rigid os uteri; operation for hare-lip; spontaneous and unaccountable, sometimes fatal, hæmorrhages from the nose, mouth,

* Through the kindness of the master, Dr. Shekleton, we have obtained from Dr. Sinclair, senior assistant-physician to the hospital, a statement of the mortality at the Dublin Lying-in Hospital for the three years 1850—1852 which have elapsed since the period at which Dr. Arneth's table terminates. During that time the deliveries have been 6012, the deaths 38—i. e., one in 158·21, or 0·632 per cent. Of the 38 deaths, however, 13, although occurring soon after delivery, were the result of non-puerperal causes, as phthisis, pneumonia, acute bronchitis, typhus, perforation of stomach, destitution, &c.

and anus of new-born infants; incisions of the perineum in cases of its extreme tension; observations of Dubois and others on congenital syphilis; an examination of Dubois' and Simpson's views on the subject of the attitudes and positions of the fœtus in utero; induction of premature labour; section of the symphysis pubis; the forceps and its application; perforation; the Cæsarean operation, including an examination of the facts brought forward by Dr. Hoebecke, of Brussels, who is said to have operated in thirteen cases with preservation of the mother's life; and Jobert's (de Lamballe) operation for urinary fistula.

The second division of Dr. Arneth's work commences with some general remarks on the hospital system of Great Britain and Ireland:

> "It is," he observes, "an ancient fundamental principle of the English government, to interfere as little as possible in the domestic circumstances and affairs of the country. This principle is likewise adhered to in the case of hospitals, the erection and maintenance of which are, with few exceptions, left to the people, and to the benevolence of private individuals; consequently, the government exercises no influence over their administration." (p. 121.)

The advantages of this plan are, according to the author, that the people, who are necessarily best acquainted with their own wants, are entrusted with the management of the institutions intended to relieve them, and that this self-government is much prompter, simpler, and cheaper, than any other. It is, moreover, well suited to a wealthy community like that of England, in which gifts have been made of amounts of property seldom attained to in other lands; opening as it does a channel for the exercise of true beneficence, at the same time that it gives the donor a voice in the disposal of his bounty; and through the system of admission into hospital by recommendatory notes, and the consequent direct support by the rich of the poor in the hour of their greatest need, it forms, adds Dr. Arneth, "one of the many bonds which, in this remarkable country, unite the needy to the wealthy and the noble."

On the other hand, he admits that the hospitals are thus rendered liable to be affected by changes in the circumstances, the residences, or the goodwill, or by the death, of their benefactors; and to be prevented, for want of a fixed income, from undertaking many expenses. The patients are from this cause often limited to a much smaller number than could otherwise be accommodated. Other objections mentioned by the author are—possible delay in procuring a recommendatory note, and the embarrassment which a non-professional committee may experience in selecting the persons best suited to fill the medical offices of the institutions. "To this," he says, "the English answer—what certainly would speak for the tact of the public and of such committees—that *all* the men who were afterwards distinguished for their science, have been elected as hospital physicians;" but in estimating the value of this statement, it must be recollected, that hospitals, if properly used, are the ladders to fame.

Coming from the French hospitals, with their showy floors and gaudy display (*Flitter*), the author was much struck with the absence of decoration, and with the uncommon simplicity which prevails in England. Low, unornamented beds, without curtains, stand far apart in small number, and offer a striking contrast to those in the enormous and brilliant wards which in Paris and Strasburg are so incredibly filled with patients.

The open fireplaces, excellent ventilation, and the chapels attached to the hospitals, next attract the author's attention; the latter are the most highly-ornamented part of the houses, but the tablets which decorate their walls, setting forth the names of the benefactors of the institutions, and the sums they have contributed, have in his eyes a singular and almost profane appearance.

The author observes, that the dispensaries usually attached to the hospitals compensate in some measure for the deficiency which exists, at least in London, of places of shelter open for the needy. He was particularly struck with the vast numbers which sometimes resort to these dispensaries.

Dr. Arneth concludes his chapter on the English hospitals by expressing his admiration of the museums of anatomical preparations and plaster casts, often of singular beauty, generally connected with them, and partly formed by the contributions—legacies, as it were—of their former medical officers. Thus, that at Guy's Hospital contains preparations by Sir Astley Cooper, and St. George's has been enriched by the labours of Sir Benjamin Brodie. He points out the great advantages presented by these museums in contrast to many similar collections on the continent; in the liberality with which they are thrown open to students; and the free use accorded to the pupils of numerous copies of perfect catalogues of their contents.

The next chapter, on the state of the medical profession in the United Kingdom, opens with an expression of surprise at its division into the three grades of physicians, surgeons, and apothecaries, and at the diversity of the standard of qualification required by the several licensing bodies. The author finds fault with the short period of actual study required by the Apothecaries' Company, thinks it strange that they should consider their pupils fit to attend lectures on midwifery and diseases of women and children, at the end of merely a six months' course of chemistry and anatomy and physiology; and that, notwithstanding the very short time allotted for the acquirement of professional knowledge, they should prescribe both a winter and a summer course of morbid anatomy. He further reprobates the union in the same individual of the prescribing and sale of medicines, remarks on the professional etiquette which obtains in London of committing internal diseases to the physician, and entrusting the surgeon with only such as are external: and in proof of the stringency with which this is observed, quotes the anecdote of a physician who, travelling in a railway carriage, declined to bleed his intimate friend, who was suddenly stricken with apoplexy, but in spite of the danger of delay, sent to a distance for a surgeon, and when asked his reason for so doing, appealed to professional usage. Dr. Arneth prophesies the gradual destruction of this partition wall, and observes that though but a short time has elapsed since accoucheurs have been admitted to the College of Physicians, he, at the period of his visit, found at the head of both the Irish and Scotch Colleges, men exclusively occupied with obstetric practice.

"It is scarcely comprehensible to strangers," observes the author, "how the British, who so warmly insist upon their independence and personal liberty, should so frequently allow themselves to be sold to their physicians." In proof of the existence of such a state of things, he quotes some advertisements from the last page of the 'Lancet,' and points to the establishment of a Medical Transfer Office in London, as evidence of the frequency of the sales of "practices." Equally little can he understand the system of partnerships, and that a patient, in answer to his summons, should be liable to see one or other of the partners, as either might chance to be at home. It struck him as singular that both customs should prevail more in England than in Scotland, and that in Ireland they should be almost wholly unknown.

The author observes that a considerable majority of the English physicians, and also many of the surgeons, have a great advantage over those of his own country, in belonging to wealthy families, and so obtaining a more careful education, and acquiring from their college connexions a better standing in society.

The author's remarks on the elementary and university education of future physicians are of general rather than of professional interest; we shall therefore not dwell upon them, but pass to the chapter on the hospitals of London. On this subject he has little to add to what he has already said when speaking of the English hospitals in general, but he bears testimony to their great cleanliness and neatness. The obstetric institutions of the capital of the world he divides into three classes, first, *Lying-in Houses*, of which there are five. The British Lying-in Hospital is the oldest, having been founded in 1749; the number of patients is small, and it is little used as a school of midwifery. The City of London Lying-in Hospital is the largest and handsomest midwifery hospital in the metropolis; it is roomy and well-ventilated; the author gives a table of the number of deliveries and of deaths which occurred in the house during the years 1827—1850, both inclusive, and from it draws the following conclusions:—

1. That the mortality, even in small, admirably situated, and well-kept lying-in hospitals, is always greater than among women delivered at their own houses. Thus, while 1 in 70 died in this truly "model hospital," it appears from the official report that but 1 among 171 puerperal women died in the entire of England during the years 1838—1841; and the Royal Maternity Charity states its mortality at but 1 in 222 deliveries. It would therefore appear that, on the whole, the poor woman in her hovel, badly or not at all attended to, often in the greatest filth, passes through her confinement more safely than in a healthfully situated house, which to her must seem a palace, surrounded with every care and attention, bedded with the most scrupulous cleanliness: the cause of this must lie in the hospitals themselves—must, if the expression may be allowed, be something endemic.

2. That the mortality varies extremely in the several years. Without wishing to deny the effects of atmospheric influences, the author cannot consider these as sufficient to account for the diversity; for the worst years at the City Hospital do not afford correspondingly unfavourable results from other similar institutions. Dr. Arneth here again refers to the occasion on which puerperal fever raged so fearfully in the Clinique at Paris, while there was not one sick in the neighbouring Maternité.

3. A comparison of the tables furnished by this and the other small lying-in hospitals of London, with that of the great institution in Dublin, shows that large lying-in hospitals do not necessarily, as such, demand more victims than small ones. While one mother in 51 died at the

British Lying-in Hospital, 1 in 70 at the City Hospital, and 1 in 59 at Queen Charlotte's; in the large Lying-in Hospital of Dublin there was, under Dr. Collins's mastership, but one fatal case among 100 deliveries, and while Drs. McClintock and Hardy were assistants under Dr. Johnson, the mortality was but one in 102.*

After a short description of the remaining lying-in hospitals of London, viz., Queen Charlotte's, the General Lying-in Hospital, and Queen Adelaide's, the author passes to the second class of the obstetric institutions of the metropolis; those, namely, which provide for the delivery of women at their own homes, the plan which throughout all England appears to be most popular. The society which has the most extensive sphere of action in this way, is the Royal Maternity Charity. We have already stated the mortality shown by its tables.

Dr. Arneth's third category contains the workhouses, which afford a refuge for those who are rejected at almost all the special lying-in institutions—namely, the unmarried.

The author deplores the great deficiency of opportunity of practical instruction in midwifery consequent on the want of large lying-in hospitals in London—a circumstance which sends most medical students to Dublin or Paris, in which latter city, especially, many English are always to be found in search of that obstetric education which is denied them at home.

Dr. Arneth next remarks on the very defective instruction of the midwives, whose practical education usually embraces no more than three or four months spent at one of the small hospitals of London, and he contrasts this with the case of the midwives of Paris, most of whom, having been taught with the greatest care during an entire year, in an institution in which three thousand births annually take place, voluntarily remain during a second twelvemonth. The author concludes this portion of his volume with an enumeration of the names and writings of some of the most eminent of the accoucheurs of London.

The general hospitals of Dublin, to which city the author now transfers his readers, contained at the time of his visit, on the whole, about 1060 patients. Among them, Sir Patrick Dun's was distinguished by its neat and pleasing appearance.

"The practical turn of mind," observes Dr. Arneth, "for which the English are so generally and so deservedly celebrated, appears to guide them in the arrangement of their museums. As in London, so there is scarcely an hospital of any considerable size in Dublin without one. Perhaps the most beautiful is the extensive museum belonging to the Richmond Hospital, chiefly formed by Dr. Robert Smith, the author of the valuable treatises on neuroma and on fractures in the vicinity of joints. This important collection, consisting of preparations, casts, and drawings, almost exclusively taken from cases which occurred in the hospital, is exceedingly instructive." (p. 196.)

The obstetric museums, which are also very instructive, are that of Dr. Montgomery at Sir P. Dun's Hospital, and the collection at the Lying-in Hospital.

"For a city which, like Dublin, possesses a considerable number of moderate-sized hospitals, the Pathological Society is a very useful association, and first

* For the mortality at the Dublin Hospital in later years, see note, p. 369.

established here, has since been imitated in many towns in Great Britain. Meetings are held on stated days, and each member brings with him the preparations of such cases as he may have had under his care, and which appear to him sufficiently interesting. This is attended with the advantage of explaining the symptoms which had been observed at the bed-side, by the appearances presented after death, or by parts removed by operation, and of making that the property of many, which would otherwise have gone to increase the experience of but one, who, perhaps, would have paid but little attention to what he saw, were it not for the opportunity of thus publicly bringing forward his observations." (p. 197.)

"One of the chief attractions," continues Dr. Arneth, "of the Dublin School, is the great lying-in hospital, which in respect to the number of deliveries is certainly inferior to that of Vienna, but about equals the Parisian Maternité, and the Prague Institution. The Dublin School of Midwifery is, properly speaking, the only one of importance in Great Britain, since all the others are arranged on the principle of sending the pupils to attend women at their own houses: but it need scarcely be mentioned how insufficient such a plan is, and how small a number of pupils can be instructed, compared to what can be taught in a well-arranged lying-in hospital. The advantage which Dublin thus possesses constantly attracts English and American students, who resort to it for the prosecution of their obstetric studies."

This institution is indebted for its existence to the philanthropic munificence of a private individual, Dr. Mosse, who in 1745 opened the Lying-in Hospital at his sole cost.

"The benefits arising from it having attracted other contributors, the first stone of the present building was laid in 1750, and in 1757 the new hospital was opened for the admission of patients. Behind it is a handsome square, which in summer is open, for the benefit of the hospital, as a place of public amusement; and, singular as it may sound to a stranger's ear, the garden of the Lying-in Hospital is the representative of Hyde Park, the Champs-Elysées, or the Prater, in bringing together the fashionable world of Dublin." (p. 199.)

Dr. Arneth enters into a minute description of the arrangement of the hospital, and of the mode of ventilation introduced by Dr. Clarke in 1783, consisting of one or two tubes passing from the top of each ward, and opening on the roof of the house, and of a number of small openings in the doors which admit the air. This plan was suggested by Dr. Clarke in consequence of his supposing that the immense mortality which prevailed among the children—2944, or 1 in 6, having died of convulsions or nine-day fits in the first twenty-five years after the opening of the hospital—was caused by imperfect ventilation; on the adoption of his suggestion the mortality immediately fell to 1 in $19\frac{1}{3}$, and a case of trismus is now of extremely rare occurrence.

Dr. Arneth remarks on the total absence from the Dublin Hospital of the "puerperal odour" so oppressive in the Maternité, and which it has been found impossible entirely to avoid in the overcrowded wards in Vienna. He also alludes to the favourable state of the health in the first-named institution, compared with the almost exactly opposite condition in the Clinique at Paris, and this he attributes partly to the abundance of space, the extremely great cleanliness, the favourable situation of the house, and the excellent ventilation; and partly to the previous habits of the women, most of those in Dublin being married, while many of the patients in Paris are persons addicted to dissipation.

Dr. Arneth gives a very full description of the hospital, of which the following is a summary:—All the wards open on a corridor. To each large ward, containing on an average seven patients, two smaller ones, each having but two beds, and likewise opening on the corridor, are attached. On the occurrence of any illness in the ward the patient is removed into one of these small adjoining apartments, and the communication with the large ward is, if necessary, cut off. Each of the large wards becomes in turn the "labour-ward," the women being delivered on a couch provided for the purpose, and in two hours after delivery the patient is carried from the couch to her bed. The women are kept in hospital for eight days after parturition, when, *if they are perfectly recovered,* they are discharged. The ward then remains empty, on an average, for two days, during which time the freest ventilation is maintained, and the floors, bedsteads, &c., are scoured and washed with solution of chloride of lime. The beds are composed of straw enclosed in ticken; the former is thrown out, the latter thoroughly aired, and, if necessary, washed after each patient. Each ward has an ample supply of linen, which is changed very frequently, not the least uncleanliness being permitted. On the roof of the house is a reservoir, which is kept, by means of a forcing-pump, constantly filled with water; from this an abundant supply reaches each corridor, and is in connexion with an apparatus for the speedy and complete removal of offal.

The rotation system above alluded to, in contrast with the plan of having one special labour-ward, is objected to by Dr. Arneth, as the noise of those still in labour tends to interfere with the rest of the women who have been already delivered; and he says it would be quite inadmissible in Vienna, where sometimes twenty-four women lie in one ward. But the advantage of having the labour-ward thoroughly and frequently cleansed and ventilated is so great, that our answer would be, that it is better to have the moderate-sized wards of the Dublin hospital, and the rotation system, than one constantly-occupied labour-ward, as in Vienna and Paris; involving, as the latter plan does, a long and often cold transit back to bed, which in Vienna is usually performed by the patient a few hours after delivery, *on foot!* (See p. 32, *note*, and pp. 48, 52.)

In Dublin the patients are not admitted until labour has commenced. This appears to us to be preferable to taking them in at the eighth month if they desire it, as is done in Vienna whenever there is room in the house; as surely a residence in hospital must be about the worst possible preparation for meeting the perils of child-birth. It should be mentioned that no student who has been engaged in a post-mortem examination is allowed to enter the labour-ward on the same day that he has been so occupied, and that the assistants who, under the master, are in charge of the house by alternate months, take no part in an autopsy during their period of duty.

The hospital is partly supported by a parliamentary grant, which in 1828 was 2769*l*, in 1835 was reduced to 1000*l*., and is now but 600*l*., at which last-named amount it is, we believe, to be continued to the institution as a chartered school of midwifery.

"We cannot," says Dr. Arneth, "pass over so large an institution without adverting to some points of comparison between it and the Vienna Lying-in Hospital" (p. 204). The use of the "binder" of linen or flannel, which in Dublin is applied soon after the birth of the child, and extends from the hips to the epigastrium, is scarcely known in Vienna. Its advantages are warmth, the greater ease with which it can be changed than the night-dress, by which cleanliness is more easily ensured; it is also said, by gentle pressure, to stimulate the uterus to contract, and so to facilitate the coming away of the placenta, and to render the occurrence of hæmorrhage less likely; which latter statement appears to be corroborated by a comparison of the relative frequency of hæmorrhage in Vienna and Dublin.

In Dublin the child is not applied to the breast until there is a considerable secretion of milk, as it is said that before that time the nipples are easily made sore, and the infant gets no nourishment. In Vienna the child is generally applied three hours after birth, without the occurrence of any bad consequences, and the suckling appears to hasten the secretion of the milk. This observation, so far as Dublin is concerned, only applies to primiparous cases, or where there is a great tendency to sore nipples.

1. The greatest and most important difference between the treatment employed at the two hospitals consists in the greater frequency with which perforation is used in Dublin, and the forceps in Vienna. The results appear to favour the Vienna treatment, where the mortality of the mothers subjected to these operations is 1 in 6, while in Dublin it is 1 in 4. The difference in the mortality of the children is of course much more striking. In Dublin, of 261 born of mothers delivered by these operations, 220 were dead, and but 41 living; while, in Vienna, of 49 children, 31 were born alive, and only 18 dead. Dr. Sinclair has furnished us with the following: "The sum of the forceps and crotchet cases for 1850, 51, and 52, is 170; of these women, 13, or about 1 in 13, died."

2. In rigidity of the os uteri, the tepid sit-bath is employed in Vienna with safety and success. In Dublin, bleeding when admissible, and nauseating doses of tartar-emetic when not contra-indicated by the presence of gastro-enteric symptoms, are had recourse to; the warm sit-bath, which is dreaded as tending to hæmorrhage, is not used, except in cases where the other means fail.

3. In prolapse of the funis, reduction with the entire hand is practised in Vienna as often as possible, when the os uteri is sufficiently dilated, the head of the child moveable, and when the funis is not in a flaccid state. In Dublin, as few cases were suited for the application of the forceps or for turning, it was found most advantageous to hold up the funis diligently with the fingers, assisted with a piece of sponge. This often proved useless, nor did greater success attend the plan of enclosing the funis in a little leathern bag, and pushing it back over the head. The use of catheter-like instruments, ergot, stimulating enemata, &c., did not appear to be followed by better results. In Vienna, of 76 children, 60 were born alive; in Dublin, of 200, only 53 were saved. Every practical man will see that Dr. Arneth has not paid sufficient attention to this subject; cases to admit of his mode of treatment are of very rare occurrence.*

4. The treatment of puerperal fever is usually commenced in Dublin

* We are informed, on the authority we have already quoted, that the cases of prolapse of the funis, in the Dublin Hospital, during the last three years, were 31 in number, in eleven of which the cord was pulseless on admission; of the remaining 20, 14 were born alive; version, forceps, and manual delivery being resorted to, according to the indication. The catheters, stilettes, sponges, and bags alluded to by the author, are, now at least, never used; and with regard to looping the funis on an extremity, it is considered expedient to turn, if the hand be once introduced.

with the administration of small doses of calomel, hot turpentine fomentations, poultices, and warm baths. Castor-oil and turpentine are often given internally. In most cases, venesection and leeching are employed. Dr. Arneth thinks this treatment was not more successful than the expectant plan pursued in Vienna.

As in the case of London, this chapter concludes with a notice of the principal accoucheurs of Dublin, and their writings.

Dr. Arneth does not give a description of any medical or obstetric institution in Scotland; but thinking that he cannot better describe the state of midwifery in that kingdom than by examining the views of "the most diligent, perhaps," he says, "of all living accoucheurs, whose influence is so great throughout the whole of Great Britain," devotes the chapter on Edinburgh principally to an investigation of the opinions and writings of Dr. Simpson. He describes the introduction by the latter of the employment of anæsthetics, first in abnormal and subsequently in ordinary labours, and gives a *resumé* of the views of those practitioners who have written on the subject. Dr. Arneth alludes to many other topics contained in the writings of Dr. Simpson, but as these have already appeared or been treated of in the British journals, and are but little commented on by Dr. Arneth, they do not call for more special attention here. One peculiarity in Scottish midwifery practice justly meets the author's reprobation—the custom, namely, which, originating with Burns and Hamilton, prevails in Edinburgh, of dilating the os uteri with the finger in certain cases during the first stage of labour. In connexion with this subject he remarks, that it is singular how in some countries and schools particular doctrines, which are elsewhere almost wholly given up, are adhered to with obstinacy and prejudice, and are handed down from generation to generation. Dr. Arneth states, that on this subject most German accoucheurs agree with him; and he reminds his readers that many years ago a fierce contest was carried on in reference to it between the Edinburgh and Dublin schools.

In his last chapter, the author institutes a comparison between the English and German midwifery practice. The first subject he treats of is the use of the forceps. In Great Britain and Ireland he found that most accoucheurs used the short forceps without the pelvic curvature. Dr. Conquest seems to have been one of the first to introduce the use of the long forceps into England. Drs. Simpson, Ramsbotham, &c., also make use of the so-called long forceps, but, like the Germans, only employ it when the head is either in the pelvis, or at least remains without moving on the brim. The "long forceps," in the French sense, is not used by either the English or the Germans.

From an examination of the writings of various authors, Dr. Arneth concludes that

—"the English accoucheurs in general—owing to the shortness and want of the pelvic curvature of their instruments, the long delay they insist on as necessary to justify instrumental interference, and the inconvenient position in which they place their patients—do not avail themselves to the full extent of the advantages offered by the forceps." (p. 316.)

In support of this opinion he quotes the conditions laid down by Dr. Collins as necessary to warrant the application of the forceps, which, Dr. Collins says, were only fulfilled in fourteen among 16,414 deliveries.

Dr. Arneth regrets that the definition of "protracted labours" is not fixed, and is perhaps incapable of being fixed; but he thinks that a comparison of the cases given by authors under this denomination, will show that a practitioner must use the perforator more frequently in proportion as he rejects the forceps; and that the more frequent use of the perforator than of the forceps, while it is necessarily destructive of the child, does not increase the mother's chance of life.

Dr. Arneth observes, that in the operation of craniotomy the English employ exclusively the scissors shaped instrument, and neither the trepan nor the cephalotribe.

German and English physicians differ widely, according to the author, as to the period of pregnancy at which it is allowable to induce premature labour. Most of the former insist that the life of the child is not to be wholly overlooked in anxiety for that of the mother, and that consequently the operation should not be undertaken until such time as hope might be entertained of bringing a viable child into the world—that is, that it should not be attempted before the seventh month. The English, he states, on the contrary, do not attempt to save the life of the child, but induce abortion so early as the fourth or fifth month. We cannot conceive how the author could have made this mistake, as Dr. Merriman and all the best writers have distinctly laid down that premature labour ought not to be brought on until the seventh month, or even a fortnight later, if the case will admit of the delay.*

The author attributes this supposed difference to the great and wholesome dread of the Cæsarean operation entertained by the English practitioners, who hold that this fearful operation is to be had recourse to only in cases of extreme narrowness of the pelvis.

"The indication for inducing artificial abortion is, besides, not always clear; the most experienced obstetrician will often feel doubtful whether it should be undertaken or deferred." (p. 322.)

"It is well known that the artificial induction of premature labour was first proposed and performed in England, and that it is still adopted more frequently there than in other parts of Europe. Thus Dr. Ramsbotham in eleven years performed it forty times; Dr. Hamilton performed it forty-six times; Dr. Lee in one patient induced premature labour thirteen times." (p. 328.)

It is, however, remarkable how much Ireland differs from England and Scotland in the frequency with which this operation is had recourse to. In 16,654 deliveries which occurred at the lying-in hospital during the seven years' mastership of Dr. Collins, the operation was but once performed; and more recently, among 6702 births reported by M'Clintoch and Hardy, we have only the feeble contingent of a single operation. There is, however, a fallacy in this observation of Dr. Arneth's, as such cases scarcely ever apply at the hospital. In Ireland, too, cases of deformity of the pelvis so great as to require the induction of premature labour are of rare occurrence—which may be accounted for by the circumstance of young girls not being employed in factories.

Dr. Arneth makes the difference between the English and German views on the communicability of puerperal fever by contact to consist in this: that the British practitioners consider that the disease, *qua talis*, may be

* See a Synopsis of the various kinds of Difficult Parturition. By Samuel Merriman, M.D. Fifth Edition. London: John Churchill. 1838. p. 182.

carried by a third party from one woman to another; while the Germans are of opinion that the mode in which this communication takes place is by the conveyance of putrid matters to the genitals of the female.

The decision of this question is, he observes, of great practical importance; for while the English, with their view of the case, will not prohibit persons who have been engaged in the post-mortem examination of the bodies of those who have died of other than puerperal affections, from attending midwifery cases immediately after; the Germans will not hesitate to go from a patient labouring under puerperal disease to visit another in her confinement, without changing their clothes and observing the precautions prescribed by the English. The author states, too, that in Vienna cases of erysipelas have not been observed to be more frequent during epidemics of puerperal fever than at other times. He will not deny, however, that diseases may differ in their properties according to the countries in which they occur—as in Germany, typhus is usually attended with ulceration of the small intestine; while in England (and more particularly Ireland), where this ulceration is rarely found, the exanthematous form of the disease prevails.

In this country, practitioners of midwifery seldom themselves perform post-mortem examinations, so that here the disease is not likely to arise from the reception of an animal poison from the hand of the accoucheur.

The author quite agrees with English practitioners in their great aversion to the Cæsarean operation, and thinks, with them, that it should, generally speaking, be limited to cases of extreme narrowness of the pelvis, but at the same time quotes Dr. Shekleton's case of pelvic tumour, published in the 'Dublin Quarterly Journal' for November, 1850, and Dr. Oldham's successful performance of the operation in a case of cancer of the cervix uteri, which appeared in the number of this journal for April, 1852, as proofs that other circumstances than deformity of the bones of the pelvis may indicate the operation.

As we premised in the commencement of the foregoing examination of Dr. Arneth's work, our principal object has been to ascertain and bring before our readers the impressions made upon the author by our institutions, and by the views of the profession in this country. In this we felt we should best succeed by drawing up a faithful summary of his observations; but as his volume embraces a great variety of subjects, our sketch must necessarily be imperfect, and we have been obliged to pass superficially over many points of practical importance; while to some interesting subjects, such as the Foundling Institutions in France and Austria, we have not at all adverted. Enough has, however, been given, to show the value of the book, which is evidently the result of minute and careful observation: we consider it to be a favourable specimen of a most useful class of publications; and we congratulate the author on possessing that ardour in investigation, which is the best element of success, and which alone could enable him in so brief a period of time to acquire the amount of knowledge he has proved himself to possess of the institutions of the countries he has visited, as well as of the opinions of the profession in each. Had his stay among us been of longer duration, he would doubtless have avoided some inaccuracies into which he has fallen, and which it has been our duty to point out.

William D. Moore.

Review VII.

1. *Archiv des Vereins für gemeinschaftliche Arbeiten zur Förderung der wissenschaftlichen Heilkunde.* Herausgegeben von Dr. J. Vogel, Dr. H. Nasse, und Dr. F. W. Beneke. Bd. i., Heft 1. 1853.

Archives of the Society for United Labours towards the Advancement of Scientific Medicine. Edited by Drs. Vogel, Nasse, and Beneke.

2. *Correspondenz-Blatt des Vereins für gemeinschaftliche Arbeiten zur Förderung der wissenschaftlichen Heilkunde.* Nos. I. to III.

Corresponding-Sheet of the Society for United Labours towards the Advancement of Scientific Medicine.

This new German journal is the organ of a society which has been lately founded in Germany by some of the most active labourers in the field of pathology, and which has for its special purpose the investigation of disease by the combined efforts of many observers working towards a given end. It is singular enough, that lately, in this country, several societies have been originated to carry out the same object;* and it would therefore appear that the need has been felt, both in Germany and in England, of extending the field of observation beyond the narrow limits to which an individual is necessarily restricted, and of bringing to bear on the elucidation of the complex phenomena of disease, the mighty force derived from intellectual combination.

As the action of societies of this kind differs necessarily in some respects from that contemplated in the formation of the older medical societies, it will be useful to inquire exactly what their founders wish to do, and how they intend to do it. In doing this we shall take for our text the works quoted at the head of our article.

The objects of the German society are laid down by Professor Vogel, in a very interesting paper, entitled 'What we Wish,' which opens the journal before us. They are as follow:

1. *To give mutual impulse to scientific researches by personal and written communications.*—On this object it is not necessary to comment.

2. *To originate and advance works which otherwise would not appear at all, or not in the same shape.*—The society will, for example, prompt an observer to a special course of investigation, will regulate the direction he should take, and prevent him from wasting his time and strength in abortive efforts. In the case of young men especially, who often have much leisure time and facility of investigation, but who want a knowledge of the exact course it is most expedient for them to pursue, such advice and aid as is here contemplated will be very valuable. Young men often throw away a vast amount of precious time and zeal in searching after shadows; and finding no substance, they abandon the path of observation

* For instance, the London Medical Society of Observation, which has lately published its method of clinical and post-mortem examination, under the title 'What to Observe at the Bedside and After Death in Medical Cases.' In the laws of this society one object is stated to be, "to exhibit the special advantages which may accrue to the science of medicine by the co-operation of several persons working on a uniform plan towards the elucidation of given medical questions." The Epidemiological Society, and a society organized by Dr. Fleming, for the purpose of investigating therapeutical problems, are also examples.

in despair. To use an image in 'Faust,' they dig for treasures, but they merely scratch the surface, and find earthworms. But under the direction of an older head, young men, scarce entered upon life, have done great things for science. It is certainly extraordinary how much of the progress of medical science of late years can be traced to those inaugural dissertations which, under the auspices of Bidder, Lehmann, Vogel, or Liebig, have issued from the laboratories of Dorpat and Giessen.

The society also contemplates the arrangement by its members of the various private and isolated memoranda which most practical men have accumulated, and of the many elaborate details of pathology which now lie buried and unproductive in the case-books of hospitals.

3. *To consider and determine problems to be investigated, as well as the necessary means to be adopted for their solution.*—This is the most important and vital object of the society, and may be thus expressed. The society will indicate to its members certain problems which it desires to see worked out, and it will also intimate the mode in which this is to be done. The necessity of arranging the method of investigation is evident; for if the observations of several hundred physicians are to be analyzed, they must have been conducted according to the same plan, otherwise comparison becomes impossible. If, for example, the problem be submitted, "What is the specific gravity of the urine or the blood?" it is of course clear that the specific gravity must be taken by one method only, and with reference to certain specified conditions.

4. *To lighten and facilitate the labour of research by division of, and assistance in, the work.*—The advantage, and indeed necessity, of examining physiological and pathological problems by the aid of various methods, with all of which a single observer cannot be familiar, is obvious. Many of the most important works in physiology have been compounded of the conjoint labours of a pure physiologist and a professional chemist; we need hardly refer to the classic work of Tiedemann and Gmelin, or to the more recent but not less valuable addition to knowledge which Bidder and Schmidt have given to the world. In pathology, unfortunately, we have as yet no good instance of this division of labour; but if it were possible to bring together the efforts of the clinical physician, the statician, the microscopist, and the chemist, no disease would long remain a mystery. As the modes of investigation in pathology acquire order and form, this co-operation will become easier; and it is for this reason, as well as for others equally important, that we regard as of the greatest moment a movement which tends to render precise and uniform the method in which the deviations from health are observed and recorded.

5. In this way it is hoped that the society will form, as it were, a committee of reference, which may accomplish the solution of such problems as not only surpass the powers of a single observer, but cannot even be solved by the additional help which this observer might be able to obtain for himself. In such a case he can fall back on the society, and submit the difficult point for their consideration.

The problems which the society propose for investigation, may refer, of course, to any department of medicine, to diagnosis, or to therapeutics, to surgery or midwifery, to ophthalmology or otology, to pathological anatomy or pathological chemistry. But the chief directions in which, at the present moment, investigation is needed, appear to Professor Vogel to be the following:

(*a*) Investigations on the metamorphosis of matter in health and disease, the study of excretions (urine, fæces, sweat) of the blood, of the influence of food, &c., present problems which will occupy hundreds of the most diligent investigators for many years.

(*b*) Anatomico-physical investigations (conducted in living and dead bodies) on the normal size and weight of the entire body and its organs, and the changes in them which can be discovered by auscultation, percussion, mensuration, spirometry, &c.

(*c*) Investigations on the relative frequency of certain diseases in different places, on the causes of disease, the influence of climate, occupation, &c.

(*d*) Accurate and numerous experiments on the action of various articles of food and medicine on healthy and diseased persons.

(*e*) Clinical observations of cases of disease, and the subsequent working up of these into form.

Without limiting itself precisely to these directions, the society will yet bestow its chief strength upon them, as the problems stated under the above heads are evidently of the greatest immediate importance for practical medicine.

Such being the objects of the society, let us see how they intend to carry them out. The society now consists of about 200 German, some ten or twelve English, and a few French members. Among the former appear the names of many of the most celebrated physicians of Northern Germany, such as Wagner, Henle, Virchow, Moleschott, Schlossberger, &c., and a great number of the younger men, such as Panum, Höfle, &c., from whom science has most to expect in the future. The business is carried on by local secretaries, and by Professors Vogel and Nasse, the editors in common with Dr. Beneke, the general secretary, of the 'Archiv.' The annual subscription amounts to some four to eight English shillings, and for this sum each member receives the 'Corresponding Sheet,' which is issued every month or six weeks, and gives an account of the progress of the society, the problems which are to be worked out, and the methods of doing so. From time to time the 'Archiv' is published, and paid for separately. It contains the results of the inquiries, and such papers as individual members may wish to insert, and which are approved of by the editors.

The first thing which the society obviously had to do, was to arrange their general methods of examination. The first number, therefore, of the 'Archiv' contains an elaborate paper by Dr. Beneke, on the mode of examining patients, and a second paper, by Professor Vogel, on the means of clinically determining the amount and rapidity of the metamorphosis of tissue, especially by the examination of the urine.

We shall not enter into any critical examination of these two chapters, as we prefer to allow Professor Vogel to state his own views in a review of the work lately published by the London Society of Observation, entitled, 'What to observe,' which we shall insert in our next number. The 'Archiv' contains, also, a paper by Dr. Conradi, bearing on clinical examination—viz., the modes of discovering the size and position of the lungs, heart, liver, and spleen, in which some new and important information is given, for which we refer to the 'Chronicle.'

In thus fixing its mode of examination, the society is fully aware that the subject is not yet nearly exhausted; and it is prepared to adopt any suggestions, and to make any improvements, which its members may from time to time urge on its attention. In this it will wisely follow the example of the cognate sciences, such as chemistry and astronomy, which have equally a common general method, in which alterations and improvements are constantly being made.

It remains now only to state what the society has already commenced to do. The first 'Corresponding Sheet' contains a statement of the weights and measures which are to be used, and formulæ for reducing others into these. The society is then invited to prove the accuracy of the modes now frequently adopted for the quantitative examination of the urine by the addition of measured quantities of reagents of known strength. This method (*Titrirmethoden*), if accurate, will be most valuable, as in two or three hours, instead of twice as many days, almost all the constituents of the urine can be quantitatively determined. Thus the amount of the phosphoric acid is known by the addition of measured quantities of chloride of iron, by which the phosphoric acid is thrown down as phosphate (Breed); the chlorine and urea are determined by the pernitrate of mercury (Liebig), &c. This is the first problem, and the most important one yet proposed.

The second problem is to determine the quantity of the urine, and of the solids contained in it, of healthy and diseased individuals at different times of the day, in relation to meals, bodily exercise, intestinal excreta, atmospheric conditions.

The third problem is to determine the quantity of ozone in the air, for the purpose of finally seeing whether or not the opinions of Schönbein have any foundation.

The fourth, fifth, and sixth problems are the determination of the frequency of the pulse at various times in the twenty-four hours; and the determination of the variations of temperature and weight in the same time. A scheme given for the investigation of the second problem answers for these also.

In the second and third 'Corresponding Sheet' some other problems are proposed, of which we extract only two or three specimens.

"The observation that in pneumonia and pleuro-pneumonia albumen not seldom appears in the urine; and that pneumonias thus characterized are not seldom antecedents or the commencements of tuberculous exudations in the lungs, or of chronic affections of other organs; make the answer of the following questions useful. In what conditions of the individual, and in what relation to the whole number of cases of pneumonia, does albuminuria occur in the beginning of the disease? Has it a prognostic value? How did the case of pneumonia proceed? What treatment was employed, and with what success?"

Another problem is the exact diagnostic value of dilatation of the pupil in disease.

Another is thus given in the form of a question to the members.

"Does dysentery occur in your district? Sporadic? Epidemic? When? What is the number of cases in relation to the other sick, and to the population? Mortality?"

We have now said enough to give a general idea of the objects and action of this society. While in this country the Epidemiological Society, in its own department, proceeds somewhat upon the same plan; and while the London Society of Observation seeks by a different organization to attain somewhat similar ends, we must admit that by extending its sphere of observation, the German Clinical Society (to give them a short title) have advanced still more this great cause of co-operation in science. When once they get into working order, there is hardly any problem they cannot easily investigate. Let any one picture to himself the labour it would cost him to determine accurately, and with reference to time of year, temperature, exercise, weight of body, kind and quantity of food, &c., the quantity of phosphoric or uric acid in the urine of a healthy man for twenty-four hours. Propose this problem to the society, and, with comparatively no labour at all, each member could furnish one or two observations. The collective observations would amount to two hundred or four hundred experiments, a number which it would take a single observer more than a year to collect. Or supposing that, in order to answer the question on the dilatation of the pupil, each member observes 100 patients, which he might very well do in three or four weeks, the society will have no less than 20,000 observations to arrange, and to deduce conclusions from. These considerations, simple as they are, strike the mind almost with wonder that scientific co-operation has not long ago been carried out more systematically and fully.

We need not occupy further space by adducing additional examples, or to show how easy it will be for the chemist and the anatomist to combine their modes of exploration. We think it will be admitted that the advantages which will result if the society can successfully carry out its plans, are great and manifold. Of course there are difficulties in the way, and good generalship will be required in directing the course of this scientific army. Perhaps in no other country but Germany could such a plan be promulgated with any chance of success. In Germany, however, the standard of education is now so high, and the members of the profession appear as a general rule to be so animated with true zeal for the advancement of scientific medicine, that a system which demands considerable labour, skill, and enthusiasm, and which will also obviously tend more to advance science than to make individual reputations (since personal labour will, to a certain extent, be merged in the general result), will probably be carried out. It will also, we believe, meet with sympathy in this country, and in America. In both countries, the old maxim that union is strength, is a household word, and it is fully known to be as applicable to science as to meaner things.

We may remark, that this German society wishes to include in its ranks men of all nations, and that it has already some English members and a secretary in London.* The more labourers, the sooner will the problems now submitted be answered, and fresh ones be proposed. We cannot too strongly urge upon our fellow-countrymen who wish to see our art rendered more useful even than it is, to join in this excellent undertaking. The general practitioner most occupied with practice would find some time to answer at any rate two or three of the problems,

* The secretary, Dr. Hermann Weber, 49, Finsbury-square, will answer any inquiries respecting the Society.

in the course of the year. Without neglecting his English societies, and the subjects he may individually wish to investigate, he might contribute his small share of labour towards the grand total. It is by the union of such small and apparently insignificant efforts, that the great force is constituted, which cleaves the rock and sets free the living waters. As the society has appointed a local secretary, the means exist of organizing both English and American corresponding societies on such a scale as to permit the 'Corresponding Sheets' and schemes, to be translated into English. There is so much sympathy already between the German and the English characters, that we cannot doubt the ease with which harmonious co-operation could be carried on. If this can be done, the generation now engaged in the practice of medicine will leave to that which is to follow them a very different position than they inherited from their predecessors.

E. A. Parkes.

Review VIII.

1. *Traité pratique des Maladies Vénériennes, contenant un chapitre sur la Syphilisation, &c.* Par S. G. Maisonneuve, M.D., et H. Montanier, M.D.—*Paris*, 1853. pp. 564.

Practical Treatise on Venereal Diseases, with a chapter on Syphilization.

2. *Traité des Maladies Vénériennes.* Par A. Vidal (de Cassis).—*Paris*, 1853. pp. 549.

Treatise on Venereal Diseases.

3. *Report à M. le Préfet de Police, sur la question de savoir si M. le Dr. Auzias Turenne peut être autorisé à appliquer ou à expérimenter la Syphilisation, à l'Infirmerie de la Prison St. Lazare?* Par MM. les Docteurs Melier, Philipe Ricord, Denis, Conneau, et Marchal (de Calvi).

Report to the Prefect of Police, on the question whether Dr. Auzias Turenne be permitted to practise or experiment on Syphilization, in the Infirmary of the Prison St. Lazare?

4. *Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation, as a differential agent in testing the character of these affections.* By John Egan, M.D., M.R.I.A.—*London*, 1853. pp. 346.

5. *Observations on Syphilis, and on Inoculation as a means of Diagnosis.* By John Crowch Christophers, M.R.C.S.—*London*, 1853. pp. 74.

6. *Traité pratique des Maladies Vénériennes.* Par le Dr. Philipe Ricord.—*Paris*, 1851.

7. *La Sipilizzatione studiata qual mezzo curativo e preservativo delle Malattie Veneree.* De Cusimino Sperino.—*Turin*, 1853. pp. 903.

Syphilization treated as a Curative Means in the Venereal Disease.

Whenever many remedies are recommended for one disease, there is presumptive evidence that none of them are of much value. If any one could be relied upon, the others would soon cease to be used, and fall into disrepute. It is the fact of the medicines employed not answering all that is required of them which opens the way for the trial of new remedies; these, in their turn, sustain their reputation until a sufficiently extended experience indicates the limit of their operations.

So is it with the medical theories upon which our practice is founded. If the symptoms observed in any disease are explained on many different hypotheses, we have presumptive evidence that its true nature is not thoroughly understood. It is the want of accordance of some of the symptoms observed with the existing theories of the nature of the disease which continually calls forth fresh explanations. These, on their first appearance, are generally made to account for several dissimilar actions, and an undue importance is attached to them, until their real value is ascertained by observation and experience.

The nature and treatment of syphilis, when judged of in this way from the works which continue to appear, is far from presenting a subject upon which nothing more is to be desired. The variety of views which have been maintained, the lengthened discussions which have taken place, and even the varied results of the experiments which have been recorded of late, all tend to show that the simplicity and unity so characteristic of truth, when once clearly perceived, are still wanted in the labours of those who have undertaken the elucidation of this subject.

When conflicting opinions obtain with regard to a disease, the only way to arrive at a satisfactory conclusion concerning its nature is to trace, as far as may be, the morbid processes by which it is developed. In as far as we can succeed in doing this, we have positive knowledge to guide us: all beyond is speculation. A clear line may at least thus be drawn between that which we know and that which we do not.

In the following article we propose to review the different theories of the nature of syphilis which have been lately promulgated on the continent and in this country, and we shall endeavour to bring the various views entertained to the above-mentioned test. An opportunity will thus be afforded of ascertaining how far the discordant evidence which they appear to present may be reconciled, and of distinguishing that which is demonstrated from that which is ideal.

To M. Ricord is undoubtedly due the credit of having introduced a new method of investigating the nature of the venereal diseases—namely, that of inoculating the products of the different affections which arise, with the point of a lancet, on some apparently healthy part of the skin of the affected person.

By this method of investigation, M. Ricord arrives at the following conclusions, which are adopted by MM. Maisonneuve and Montanier:

1. That blennorrhagia and chancre are two affections entirely distinct.
2. That blennorrhagia is an inflammatory, contagious affection.
3. That a chancre always produces a specific virus.
4. That blennorrhagia never gives rise to constitutional syphilis.
5. That a chancre always produces a constitutional disease when it becomes indurated.
6. That a non-indurated chancre is always a local disease, and is never accompanied or followed by secondary affections.
7. That a chancre is the only origin of syphilis.

8. That pus derived from a primary affection is the only contagious element in the disease. It may be secreted by the skin, by a lymphatic vessel or gland; and the ulcer which supplies it may be simple, indurated, or phagedænic.
9. That no secondary or tertiary disease is capable of being communicated by contagion.
10. That blennorrhagia may arise spontaneously, and never produces any disease but blennorrhagia.
11. That a chancre is always and of necessity the result of a chancre.
12. That there is but one syphilitic virus.

These conclusions are the results of experiments said to have been repeated a thousand times, under a great variety of circumstances. In the experiments made by M. Ricord, he found that simple blennorrhagia never gave rise to a chancre, and he therefore concludes that this disease is not syphilitic. If the discharge, however, arises from a concealed chancre in the urethra, then inoculation may produce a chancre; this affection M. Ricord considers syphilitic. He believes the pus from a chancre to be always capable of being inoculated when taken during its period of progress, or during the time that it remains stationary, but not when taken during the period of its reparation; it has then become a harmless ulcer. The pus from a bubo he likewise regards as inoculable when taken from the gland itself, but not when derived from the surrounding cellular tissue.

By this method of investigation, M. Ricord has endeavoured to draw a clear and well-marked distinction between those diseases which are syphilitic in their origin and those which are not. As general conclusions from all his experiments, he deduces—

That a primary venereal sore which has healed, or still remains open, does not prevent other similar sores from forming; and that there appears to be no definite limit to the number of successive inoculations which may take place.

That a person actually infected with primary disease in one part is never subject to the occurrence of similar affections elsewhere, unless the disease be conveyed by the contact of the matter from the primary sore, or be communicated in like manner from some other person.

That secondary disease depending upon a general infection of the system will never prevent a person from contracting fresh primary sores.

That the frequency with which constitutional symptoms manifest themselves is by no means in proportion to the number of primary sores developed at the same time.

We have thus been particular in giving an accurate outline of M. Ricord's doctrines, as upon them have been in a great measure based the discussions which have lately so much agitated the scientific meetings of the profession on the Continent. Upon these propositions we shall now make some remarks, and endeavour to bring the principal of them to the test which has been proposed.

The concurrent testimony of all observers has now ceded to M. Ricord his first point—namely, that the pus derived from a chancre contains a very powerful agent which escapes detection by our senses, and which is not found in the pus of blennorrhagia; when this subtle and occult agent is inoculated, the morbid process which ensues may be traced in the most satisfactory manner. If the pus from a chancre during its period of progress, or during the time that it remains stationary, be inoculated, the following succession of effects may be observed:

"During the first twenty-four hours, some redness appears at the spot where the inoculation has been made. From the second to the third day there is some swelling, affording the appearance of a pimple, surrounded by a red areola. From the third to the fourth day, the cuticle is raised by a more or less turbid fluid, which often forms a vesicle with a little dark spot in its centre. This spot results from a small effusion of blood at the time of the inoculation. About the fourth or fifth day the secretion becomes increased in quantity, and more or less purulent in appearance. The centre of the pustule now becomes depressed, in this respect resembling the pustules of small-pox; at the same time the surrounding redness may become fainter. From the fifth day, the subjacent tissue, which had before undergone no change beyond being slightly œdematous, becomes infiltrated by a plastic lymph, which affords to the touch the peculiar hard and elastic feeling of certain forms of cartilage. After the sixth day the pus becomes thicker, the cuticle over it gives way, and scabs form on the surface of the part. When these scabs are detached, a chancre presents itself below, characterized by its indurated base, involving the whole thickness of the skin; by its greyish white surface, composed of a lardaceous matter, or of a false membrane; and by its sharp and undermined edges, having generally a circular form." (Maisonneuve and Montanier, pp. 14, 15.)

Nothing similar to this is ever observed when the pus inoculated is derived from a simple case of blennorrhagia. M. Ricord and his school believe that these results *always* follow inoculation from an indurated venereal ulcer; and Dr. Egan, although differing, as we shall see, on other points, affirms "that in the indurated and excavated form, inoculation supplies a valuable and unerring test, the characteristic pustule being *always* the result of the operation." (p. 39.)

M. Vidal, the latest writer upon the opposite side, is of opinion that certain persons are naturally incapable of being inoculated with the syphilitic virus, and that in others this immunity may be acquired under certain circumstances—as an instance of the first kind, he cites the case of a man well known to many of those who had taken part in the discussion on syphilization in the Academy of Medicine at Paris, (p. 47.) This person had never been affected with syphilis, although he had continually exposed himself to contagion. He was, moreover, twice inoculated with pus taken from a chancre during its active stage, with no result whatever. It may be said that this natural immunity supposed to be enjoyed by certain persons from the effects of inoculation, does not invalidate this means of ascertaining the real nature of venereal sores, inasmuch as in *their* case the very same conditions would render this test unnecessary.

"But," says M. Vidal, "this disposition may be acquired," and he instances the case of M. Laval, who, after having been repeatedly inoculated on the upper extremities, at length became proof against any further inoculation. "I am perfectly certain," he says, "that M. Laval was inoculated upon three occasions by M. Gosselin (with pus which produced chancres upon another patient) without effect." This same M. Laval, however, was at length successfully inoculated by M. Ricord. This is explained by M. Vidal, upon the supposition that after a certain number of inoculations, the system loses its susceptibility to the effects of the syphilitic virus, for a time, but that after an interval of rest, it may again, as before, become subject to the disease.

With this explanation, M. Ricord and his school appear little satisfied. "Give me," says he, "a person who has been syphilized, and who is proof against further infection, who will come before the class at the Hôpital du Midi, or before the Academie; who will enter the lists with me and defy me with the arms of my choice." This challenge was published on the 12th of August; on the 22nd M. Auzias accepts it; on the 23rd of September M. Ricord declares himself ready; on the 4th of November he announces that the experiments have commenced, and that the results will be published in the 'Union Medicale.'

"Search the records," cries M. Malgaigne before the Academie de Medicine; "the promised communication has never arrived. A week afterwards M. Ricord presents M. Laval to the Chirurgical Society, and on the 18th of November to the Academy of Medicine. Not a word is said about the above-mentioned experiments. On the 9th of December M. Marchal writes to the 'Gazette des Hôpitaux,' that M. Laval has presented himself to M. Ricord, who had twice made seven inoculations with three different kinds of pus, each of acknowledged efficacy, without producing any result. M. Ricord answers nothing, and the 'Union Médicale' is silent."

M. Malgaigne continues:

"This silence appears to me very like a defeat. I should, however, have preferred an open and public avowal of the result of the experiments, and therefore I urged some explanation upon the point. Several presented themselves. But what did M. Ricord himself say? That he had produced upon M. Laval an ecthymatous pustule, sufficiently characteristic not to require further proof; and that the other inoculations which had failed had been done with pus, which had also failed to produce its specific effects when inoculated on the patients from whom it was taken.

"No other details of these experiments are given. M. Ricord declares that his dignity would be hurt if he gave any further explanation. That does not satisfy me," says M. Malgaigne; "what connexion is there between M. Ricord's dignity and the details of an experiment? By his own avowal, upon his own ground, having entered the lists with the 'arms of his choice,' M. Ricord succeeded only once in seven times. But the inoculations failed upon other patients, which proved that the matter was not good! Who would have thought that M. Ricord, after so solemn a challenge, in his immense practice, and with his choice of arms, should not have been able to find some good pus?

"But at length one inoculation succeeded. Granted! provided that the counter experiment, which M. Ricord himself declares always indispensable, was made: viz., that the pus produced by the inoculation can be again inoculated.

"For my part," concludes M. Malgaigne, "I regard the fact that certain persons may acquire an immunity against syphilitic inoculation as incontestably demonstrated. If M. Ricord doubts it, I will engage to produce a young man who believes himself syphilized, and who will defy M. Ricord to produce in him a single atom of pus capable of being again inoculated. M. Ricord shall take his precautions. If he does not succeed, he shall begin again: my patient declares himself ready to allow twelve hundred inoculations to be made upon him: and more, should this not be deemed sufficient." (Vidal, pp. 50, 51.)

Leaving these chivalrous opponents for the present, we come to the consideration of M. Ricord's proposition, "that a chancre is the only origin of syphilis." A very broad line of distinction is here drawn by M. Ricord and his followers, between a chancre and every other form of venereal affection. Primary syphilis is alone, it is said, capable of being inoculated; secondary affections never. Persons may be exposed to the contact of mucous tubercles, to secondary ulcerations, to all sorts of syphilitic eruptions, and will never become infected. The pus produced from these sources will never produce the characteristic pustule of syphilitic inoculation. In cases where apparent exceptions have occurred, either the mucous tubercles have become transformed into real chancres by fresh contact with matter from a primary ulceration, or else real primary sores have been mistaken for some form of secondary affection. "Secondary syphilis is not contagious. Reason would therefore lead us, *à priori*, to infer that it is not capable of being inoculated, and experience confirms this view."*

On the other hand, M. Vidal, Dr. Waller of Prague, and others, maintain that mucous tubercles and secondary symptoms may, under certain circumstances, be communicated from one individual to another. They dwell particularly upon the undeniable fact that a child may be infected in its mother's womb. They consider it well established that a woman who has neither a chancre, nor a syphilitic bubo, but whose constitution is affected, may infect her offspring. That she may bring into the world syphilitic children who will infect their nurses, who will again in their turn transmit the disease to their families.

"Further," says M. Vidal, "that mucous tubercles (see Pustule plâte) may be transmitted, is generally acknowledged. On this point MM. Lagneau, Baumes, Cazenave, Gibert, and all observers whose judgment is not warped, may be consulted." (pp. 55, 56.)

M. Ricord, however, maintains that the origin of the mucous tubercles (said by these gentlemen to have been transmitted) has not been seen, and that the evidence afforded by the neighbouring glands has not been taken into the account. A specific ulcer may at first have been the real disease, and have become subsequently transformed into a mucous tubercle; and he believes that all syphilitic disease said to be communicated by contact with table utensils, with pipes, razors, masks, &c., have no other origin than the pus derived from a primary sore.†

With regard to the transmission of the disease from mother to child, and from the child to the nurse, M. Ricord notices the poverty of the evidence which we have upon the subject.

"I have seen," he says, "nurses, and the children they were suckling, diseased, and both parties accused of infecting the other. Generally, I have been able to trace the origin of the disease to a primary sore in one of the parties. When this has not been the case, I was called too late; five or six months, perhaps, after the child had been with its nurse.

"I have for many years had a number of nurses at the Hôpital du Midi, and I have often given them children to suckle, which were sent to me from the *Maternité*, with secondary affections. Never, as far as my observation extended, were these nurses infected.

"On the other hand, nurses palpably affected with secondary disease have been able to suckle infants which were sent as being affected with syphilis (but who only had simple eruptions of eczema, impetigo, or porrigo), and never were these children, whilst under my inspection, infected." (pp. 100, 101.)

* Maisonneuve and Montanier, p. 24.

† Lettres sur la Syphilis, p. 98.

Thus M. Ricord and his followers, while they admit that a child may be affected independently of inoculation *in utero*, believe, that after the child is born, it can only be infected by contracting a primary ulcer, and that this may happen by some of the virus being carried by the fingers of the nurse, either directly to the child, or first to the nurse's breast, where a chancre is formed, and that from this the child is inoculated. But M. Vidal maintains, that mucous tubercles, and some forms of secondary affections, may be transmitted quite independent of the mode of inoculation which we have hitherto considered. He draws a distinction between contagion in itself and particular means of contagion, and believes that syphilis may be communicated even when it cannot be inoculated, so as to produce the "characteristic pustule." Even this test he expresses himself willing to offer for the "satisfaction of the schools," and distinctly affirms that he has succeeded in inoculating the secondary ecthymatous pustule. (p. 57.)

M. Vidal further believes that certain non-syphilitic affections which are at present considered incapable of being inoculated, might ultimately be found to be so, if the proper moment when they contained inoculable matter were taken for the experiment.

"In the attempts," he says, "which have hitherto been made to inoculate cancer, the ichorous discharge on the surface of the ulcers has been taken, or else the *débris* of the cancerous matter. But are these the products with which the experiments should be tried? ought they not rather to have been performed with the cancer-cell in the early stage of its development? In an experiment performed in this manner by Langenbeck, where fresh cells from a cancer still warm were introduced into the bloodvessels of a dog, cancerous tubercles were formed in the animal's lungs." (Enclycopedie Anatomique, t. iv. p. 279.)

From these and similar facts, M. Vidal argues, that inoculation which fails in one way may succeed in another. The mode of application may have a very material influence upon the result. He instances the fact that the ordinary mode of inoculating syphilitic matter upon animals is not sufficient, and that M. Auzias Turenne found that he succeeded better by removing the cuticle by means of a pair of curved scissors, and keeping the morbid matter for a minute in contact with the excoriated surface, whilst the surrounding part was gently rubbed. In accordance with these views, he maintains, that although the secretion from a mucous tubercle is incapable of being inoculated in the same way as the pus from a chancre, yet that it is nevertheless capable of producing both primary and secondary venereal affections: that if, for instance, a blister be applied to a portion of skin, and the excoriated skin be then dressed with lint soaked in the secretion of a mucous tubercle, the inoculation will succeed, although this same secretion may not be capable of being inoculated by a simple puncture with a lancet.

"It is probable, also, that a simple inoculation with a lancet charged with syphilitic blood would produce no positive results. But if, according to the experiments of Waller of Prague, the skin be first scarified, and the infected blood be introduced with an appropriate instrument into each scarification, there is a good chance that the experiment will succeed, as it did in Waller's hands." (Vidal, p. 60.)

Waller's experiments are so much to the point, and of so much importance, that they cannot be overlooked in the investigation of this subject. Among others he relates the following:

"The skin of a boy who had never had syphilis was scarified with a new scalpel. Some blood was then drawn by means of a cupping-glass from a patient affected with secondary syphilis, and inserted into the scarifications on the boy's skin by means of a piece of wood. Some lint soaked in the same blood was placed over the scarifications and allowed to remain. No immediate inflammation or suppuration followed this operation, and at the expiration of three days the wounds had all healed; but thirty-four days after this experiment, two distinct tubercles appeared on the inoculated part. These were the size of a pea, of a pale-red colour, unattended by suppuration, pain, or itching. On the following day, these tubercles increased in size, so that they became united at their bases. They were scaly upon the surface, and surrounded by a deep-red areola. They gradually increased in size, and assumed a copper hue. The skin and subjacent cellular-tissue which formed the base of the tubercles became hard and dense, while their surface ulcerated, and was covered by a thin brown crust.

"A fortnight after this, an ulcer was developed, with a base as large as a pigeon's egg, surrounded by a red, copper-coloured circle, and covered by a crust similar to that above mentioned. A single tubercle, the size of a pea, now formed on the right shoulder; the surface of this was red, and covered with scales. After the lapse of another fortnight, the patient became feverish and restless.

"A month after the appearance of the first tubercle, an eruption appeared over the lower part of the stomach, on the back, on the chest, and on the thighs. This eruption gradually covered the whole body, and was accompanied by neither itching, pain, nor fever."

Dr. Waller gives the names of several eminent men who recognised this eruption as syphilitic. He concludes that—

"Primary and secondary syphilis are capable of inoculation. The pus of a chancre produces a chancre. The products of secondary affections give rise to secondary affections."*

Of what practical value, then, amongst this conflict of opinions, are we to consider syphilitic inoculation? As a means of diagnosis, we must allow that when the characteristic pustule is produced, it affords satisfactory evidence of the nature of the disease. This mode of ascertaining the nature of the affection we have to deal with, may be of much service where ulcers have appeared in unusual situations, and where, from this or other causes, they have not assumed their common appearances; and to M. Ricord we would accord the full credit of this discovery.

But can we with equal certainty say, that when inoculation fails, the disease is not syphilitic? Certainly not. With the amount of evidence before us, we cannot regard the broad distinctions sought to be established by M. Ricord and his followers as correct. To affirm that all primary chancres can be inoculated because they are syphilitic, and that they are syphilitic because they can be inoculated, is simply to beg the whole question.

Experience proves that we cannot rely upon the negative results of inoculation, and that a primary affection, although not capable of being inoculated in the ordinary way, may nevertheless be followed by secondary symptoms. During the last year, the reviewer had under his care two patients in the Lock Hospital: one of these had a serpiginous sore, extend-

* Du caractère contagieuse de la syphilis secondaire, par Dr. Waller; traduit par M. Axenfeld. (Annales des Maladies de la Peau, et de la Syphilis, Avril, 1851.

ing from the groin, between the thighs, and around the nates; the other had a rapidly-extending phagedænic sore upon the penis, yielding an abundant purulent secretion. Both patients were inoculated. In the first case, the result was positive, and in the latter negative. The first patient continued under observation for nearly twelve months, but no secondary form of disease showed itself; the second patient had a very full crop of pustular eruption in the course of a few weeks.

In these experiments the ulcers were of the non-indurated class, in reference to which we have the concurrent testimony of all writers, that the effects of inoculation are not so satisfactory as in indurated sores.

What, then, is the sum of positive knowledge which we derive from the practice of inoculation as a means of diagnosis? It furnishes us with a ready means of testing the nature of an indurated ulcer, and it will often (when the results of the inoculation are positive) enable us to ascertain the nature of other ulcerations. But upon the all-important questions which continually present themselves to us in practice—namely, Is such a sore likely to be followed by secondary symptoms? or, Will any particular mode of treatment in reference to it prevent secondary affections? inoculation does not pretend to decide; upon these practical points it has hitherto afforded us no information.

But it is maintained by Dr. Sperino, M. Auzias Turenne, and others, that inoculation may not only serve as a means of diagnosis, but also as a means of preserving the system from the action of the syphilitic poison, or of curing the syphilitic disease when the constitution has become infected by it.

This mode of practice is not likely to be seriously entertained in England, and the facts hitherto published with regard to it require confirmation. They cannot, however, be overlooked, and have, at least, this effect, that they tend to disabuse the minds of professional men, and through them of the public, of an idea that has very generally prevailed—namely, that the severity of the constitutional symptoms, and the frequency with which they occur, bear some proportion to the number of primary sores. From Dr. Sperino's book it would appear that in no case were the inoculations which he practised followed with any serious results—a slight surrounding inflammation, or a transient phagedænic ulceration are the sum of the much-dreaded consequences which are said to have followed. Nor can it be said—if his book at all corresponds to his motto, "*non verbis sed factis*" —that he has not given the plan a thorough trial. If very serious results were likely to happen, Dr. Sperino certainly ought to have met with such, for he has been by no means sparing in the number of his inoculations.

Out of 53 cases of primary syphilis, we have 52 treated by repeated syphilization. In these the plan is said to have been successful in 50, and unsuccessful in 2 cases. Out of 43 patients affected with constitutional syphilis, 26 were treated by syphilization, and 25 were cured. In 6 instances the iodide of potassium was used in conjunction with syphilization, and in 8 cases, syphilization, iodide of potassium, and mercury, were all employed. In 3 cases it was found necessary to discontinue the inoculations, and 2 patients died.

Of the 53 patients treated for primary syphilis, only 3 presented themselves subsequently with secondary affections, and in these the disease is said to have been cured by fresh syphilitic inoculations. With reference to the comparative frequency with which secondary affections may be expected after this plan of treatment, we cannot tell how much the popularity or the inconvenience of the mode of treatment may have influenced its apparent results. Whenever a new of plan of treatment has failed, or has been attended with much inconvenience, it is not likely that the patient will again have recourse to it. This remark may apply with peculiar force in the present case, where the plan adopted must necessarily have been met at first with a very general feeling of distrust.* But the result with which we are chiefly struck is, that in the patients who were said to be cured of constitutional syphilis by syphilization, no case presented a recurrence of the disease. This, we believe, is a more favourable result than can ever be expected from any plan of treatment.

A very important point remains to be considered—namely, what length of time was occupied in the treatment of these cases. Excluding the cases treated with mercury, and the cases in which the treatment was interrupted, Dr. Sperino gives us 76 cases in which the supposed immunity against the effects of syphilis, acquired by repeated inoculation, might be observed. Of these the cure is said to have been effected in less than a month in 1 case; in from one to three months in 4 cases; in from three to six months in 7; in from six to nine months in 8; in from nine to twelve months in 8; in from twelve to fifteen months in 21; and in from fifteen to seventeen months in 27. In this analysis we cannot but observe the relation between the gradually increasing number of cases and length of time required for their cure; and we are not surprised to find that no case should have returned after having been treated upon this plan from fifteen to seventeen months.

We are informed that in this mode of treatment the ulcers first produced by inoculation cause some suffering, but that subsequently the patient may attend to his usual business, and appear to be in his accustomed health. The cicatrices produced by the inoculations are said to be, in the majority of instances, small and superficial, leaving very little evidence of their existence. Occasionally, however, the ulcerations have become phagedænic or gangrenous, and then the traces of their existence have subsequently been visible enough.

The practice of syphilization is advocated for primary ulcers, which, it is said, soon lose their characteristic hardness, and begin to cicatrize. Buboes also are beneficially acted upon by this treatment. Vegetations, on the contrary, are not influenced by the practice. But cutaneous syphilitic eruptions, secondary syphilitic ulcerations of the skin or of the mucous membranes, the loss of hair, and the deep subcutaneous ulcerations of the cellular tissue, are all said to be cured by this plan. Syphilitic iritis has also been treated in this way successfully. Periostitis necrosis and caries are likewise supposed to have been brought under its magic spell. But before we can draw any inference from the facts stated, supposing them to be all strictly true, we should require an equal number of similar cases in which no treatment at all was pursued; and we are not prepared to say in

* A gentleman of our acquaintance travelling for amusement in the East, thought he would improve his opportunity, and bring to bear the smattering of medicine which he had picked up in England, and in some places he appears to have gained a considerable reputation among the natives. He had heard that a strong solution of caustic was a good remedy for inflammation of the eyes, and accordingly he adopted this plan of treatment. The result was, that he "cured them all; they never came again."

which of the two classes of cases the disease would prove the most severe, or would last the longest.

In leaving the work of Dr. Sperino, we have only further to remark, that he is a believer in the possibility of the transmission of secondary affections, under certain circumstances, by inoculation. He cites the case of a patient, nineteen years of age, who was admitted under his care on the 21st of January, 1851. This woman had an indurated ulceration at the orifice of the vagina, and a pustular syphilitic eruption on the back and elsewhere. On the 30th of January some pus from one of the ecthymatous spots was inoculated. Six days afterwards a little pustule formed in each of the inoculated spots; these became covered with crusts. On the 10th of February a fresh inoculation was performed with the matter taken from a pustule which had formed of its own accord upon the stomach. On the 17th two fresh pustules had resulted from this inoculation. On the same day some matter was taken from the last-mentioned pustules, and inoculated in two places on each side. Four days after this, two little points were visible on the right side, and one on the left. These formed themselves into pustules, which followed the course of those which had preceded them.

A subject affecting so deeply the sanatory condition of a large class of persons as syphilization, could not long remain without further investigation. M. Auzias Turenne, the reputed author of this plan of treatment, had, in February, 1852, requested permission of M. Pietri, the Préfet of Police, to be allowed to try this system upon the patients in the prison of St. Lazarre. M. Pietri appointed a commission to inquire into the propriety of the proceeding, and we have now before us the report of that commission. Before answering the question proposed to them, they undertook an investigation of the whole subject; and their report is divided into four parts. The first gives an outline of the objects of the commission, and their relation to the inquiries of M. Auzias Turenne. The second furnishes an account of knowledge obtained by the commission on the subject of syphilization in man, derived from the practice of M. Auzias. The third is occupied with an account of an experiment of syphilization made on a monkey, by M. Auzias, in the presence of the commission; and the fourth is the answer to the question proposed to them by the Préfet of Police, as to the propriety of sanctioning M. Auzias' experiments and practice in the St. Lazarre prison.

The commission—composed of Melier, President of the Academy of Medicine; Ricord; Denis; Conneau, First Physician to the Emperor; and Marchal, formerly Professor at Val-de-Grace—entered upon their investigation on the 12th of July, 1852. From this period they met, on an average, once a week, until the termination of their inquiry.

The first part of their report is occupied chiefly with the consideration of points to which we have already referred, and to which we shall not, therefore, again recur.

The second part of the report is that in which we expected to find a detailed and accurate account of the evidence upon which syphilization is sought to be established; but in this we have been disappointed. The amount of information furnished is of the most meagre and unsatisfactory nature.

M. Auzias requested from the commission some assistance in procuring patients upon which he might try his plan of treatment; or, at least, that the commission would give its sanction and approval that he might be furnished with a certain number of beds in one of the hospitals. The commission, on the other hand, declined to say that such experiments ought to be tried, and threw the whole responsibility of supplying the proofs upon which his system was based, upon M. Auzias. This they felt the more justified in doing, as M. Auzias had described his system, in his letter to the *Préfet de Police*, as "approved."

The amount of evidence furnished in the report rests upon seven cases. The first is the case of M. ——, who describes himself as having become quite a new man under M. Auzias' treatment. "I had," he says, "during the first days of syphilization, an extraordinary appetite; and at night I had a repose which I had not known for years. . . . I have no longer any pains, and am full of energy." (p. 39.)

It was desired that this patient should again present himself before the commission, but he never appeared, M. Auzias declaring that since his examination before the commission he had been "ébranlé." Upon an attentive perusal of this case, we must express our very serious doubts whether it was one of syphilis at all.

The second case is one of great importance, and if it stood alone, would make us at least pause before M. Auzias' plan could be seriously entertained. It is that of a student named J——, who was attacked with erysipelas, and died during the progress of the syphilization. The evidence is somewhat of a conflicting nature as to the cause of the erysipelas, and also with regard to the effects of the syphilization upon his general health. M. Mialet (a medical student) states that at the time the erysipelas appeared there were on the left arm eight or ten chancres covered with crusts, and others in the process of cicatrization. The arm was swollen and painful.

M. Auzias, on the other hand, declares that the syphilizing chancres were completely cicatrized in this young man when the erysipelas developed itself.

M. Roby (a medical student) says that M. J—— was feeble, pale, anæmic, and feverish—M. Auzias, that his health was as good as need be wished (pp. 50, 51). M. Auzias adds, that erysipelas was epidemic in the place where this young man lived, and that a woman on the same square was also attacked at the same time, and conveyed to the Hôpital de la Pitié.

M. Guilbert (another medical student) informs the commission that M. J—— appeared satisfied with his treatment up to the last. But from another source we learn, that a fortnight before his death he said, the treatment "fatigues and worries me; I do not mend. The inoculations always succeed. When will there be an end of this? M. Auzias must tell me. I will go and ask him. I am always hungry; I eat like a wolf, and yet I am without strength."

It must be added that this patient had undoubtedly injured himself by excessive work and late hours; that M. Auzias considered his constitution much shaken when he undertook his case; and that at one period he certainly appears to have improved in general health while under treatment.

In the third instance the treatment was adopted for an ulcerated throat, and an enlarged gland in the neck, following a chancre. M. Auzias first made one puncture from the pus derived from the inoculated pustules of the last-mentioned case. A large pustule succeeded, and was followed by a chancre as large as a three-franc piece. The cicatrix which was left was the size of a franc. About three weeks after this first inoculation, some pus was taken from the ulcer which followed, and five punctures were made on the arm, and five pustules resulted. After this, the pus derived from this patient could no longer be inoculated. Some fresh pus was then taken from the last patient, and from other persons. These fresh inoculations always succeeded. After a time, however, some inoculations were again made, which produced no results. M. Auzias then having inoculated the former patient with some fresh pus, which had produced its specific effects, made fifteen inoculations with the pus resulting from this fresh source, and fifteen large pustules followed.

"I suffered much," says this patient. "I had the arm swollen, and much fever, and was obliged to keep my bed for several days. In scratching myself I caused a place to come much larger than any of the rest. The punctures still succeeded, but the last ones in a less degree. I had fifty-two on each arm. I commenced this treatment on the 20th of February, and discontinued it on the 3rd of July, in consequence of the death of the last patient. The summer before the syphilization I had some spots. During the syphilization I had a very peculiar disease of the skin. It appeared in red circles, with the central skin of its natural colour. I showed them to M. Auzias, who told me that it was not a syphilitic affection. My throat became quite well during the time that M. Auzias was syphilizing me. I slept well; I had no pain; I did not lose my hair. I ate enormously, without being able to satisfy myself; since then, however, I have become very thin, and lost my strength; I am now nothing in comparison of what I was. I am soon out of breath. I had a syphilitic eruption on the palms of my hands, and my sore-throat returned." (p. 52.)

The cicatrices on this patient's right arm were red and very apparent, without induration. On the left arm they were much less visible, and some seemed to be disappearing altogether. On examining the throat, a mucous tubercle was seen upon the left tonsil.

M. Auzias maintains that the above account is exaggerated, and that if the patient had suffered, it was from privation and want, and not from the disease.

It is but fair to M. Auzias to state, that when the patient made the above statement, she had placed herself under the care of M. Ricord, and that the subsequent history shows that her health had undergone no permanent deterioration.

The evidence furnished by the remaining cases is not more conclusive than that to which we have above referred; and as we naturally suppose that M. Auzias would bring forward his most successful results, we cannot but see that he has entirely failed to prove his point.

The third part of the report is occupied by the inquiry as to the possibility of transmitting syphilis to animals; but as this adds nothing to the evidence in favour of syphilization, we pass on to the conclusion at which the commission, after much pains and deliberation, arrived.

There appear to have been some shades of difference in the opinions entertained by the five members; but they arrived at the unanimous conclusion, that, from the facts which had come to their knowledge, M. Auzias *ought not to be allowed to try experiments with syphilization in any public institution whatever.* The declaration of M. Marchal (whom we have before seen taking part with M. Ricord's adversaries in favour of syphilization) is remarkable:

"I became a member of the commission," he says, "convinced that M. Auzias was well supplied with facts which he would clearly prove; convinced that the commission would find in those facts sufficient to justify it in recommending a public trial of his experiments; and hoping that from these experiments, made with judgment and followed up with diligence, some truths might result which would be honourable to science and beneficial to the human race. My expectations have not been realized. The facts have not been produced, either in number or kind, such as I might have wished. This to me has proved the greater disappointment, inasmuch as I was to a certain extent committed in favour of syphilization. . . . I believe, from the facts that I have myself seen, that the following questions are still open to investigation by experiment—viz., 1st. Whether inoculations, in small numbers and at different intervals, have not the tendency to cause the induration of the infecting chancre to disappear, and to neutralize the syphilitic diathesis; 2nd. Whether similar inoculations are not capable of exercising a curative influence upon secondary and tertiary symptoms; 3rd. Whether they have not a marked effect upon phagedænic ulcerations, which at present often resist every known method of treatment.

"With regard to M. Auzias, I see with sincere pain that he has not justified before the commission the demand that he had addressed to the authorities." (pp. 88, 89.)

We shall now dismiss the subject of syphilization, and in a succeeding number we shall refer to the important topic of the absorption of the syphilitic poison.

Henry Lee.

(To be continued.)

Review IX.

Practical Observations on Diseases of the Lungs and Heart. By Archibald Billing, M.D., F.R.S., &c., &c.—*London*, 1852. 8vo, p. 138.

The duties of the critic and analyst of medical literature, involving, as they do in all cases, some of the highest trusts which can be reposed in the servant of science, become, if possible, still more enhanced when he is called on to pronounce judgment upon works ushered into the literary field with all the circumstance and influence of a name already honourably known in the annals of science. If, in the case of a new and untried author, who advances bold and original speculations, it requires no small amount of moral courage to lend him support, and encourage him by our commendations, should his views stand the test of a close and rigid criticism; it demands a still higher sense of duty, and an even greater measure of self-reliance and fortitude, to pronounce an unfavourable verdict on the literary labours of one whose former works we, in common with others, have respected and admired. It appears to us, however, that by taking a certain standard of comparison, plain and intelligible to all inquirers, wherewith any given work shall be compared, not only is the course of the critic thus rendered free from all charge of invidiousness, but, moreover, each reader can himself apply the rule, and form his own estimate of the sentence pronounced. In the cause of science the office of the critic is sacred, and he who lightly betrays the trust reposed in him by a hasty or inconsiderate expression of one sentence of unmerited condemnation, is equally base and guilty as he who through fear or partiality withholds censure where censure is due.

In surveying the immense mass of literature which now teems almost daily from the medical press, it is well to bear in mind some of the simple primary canons of criticism, which seem to be lost sight of wholly by some authors, and very largely, if not entirely, by readers in general. The publication of a book on medicine should accomplish one or more of the following objects. The recording and addition to science of substantial facts and observations; the promulgation of original views based on observation or experiment; or, lastly, the exposition of the actual state of knowledge in a particular department of medicine, under which head, of course, may be conveniently arranged systematic treatises, and text-books which, though a less high and ambitious form of medical publication, yet, when well written and composed, answer certain important and valuable purposes, educational and otherwise. These, and these alone, are the ends for which books on medicine have a right to claim admission into the literary field; these, we say advisedly, are the only legitimate ends; but we regret to say that books are written, or rather *made*, to serve other ends and objects.

If we take the above propositions as the basis of our critical observations, and in the case of any particular work, assume as a standard of comparison the actual state of science in the department of which it treats, we shall have at once the simplest, truest, and most impartial grounds for forming our judgment of an author's labours. If his work be original, we shall at once see what and how much he has added to our knowledge in any given branch of medical science; if systematic, how far his treatise embraces all that has been accomplished by previous investigators. Judging the present work of Dr. Billing by the foregoing rules, we are forced, reluctantly, to aver, that in our opinion it in no way fulfils the requirements of a scientific medical publication; and we are yet quite at a loss to understand how a writer of his position, and one already so well recognised, could have been induced to commit so frail a bark to the waters. Dr. Billing is well known as the author of the 'Principles of Medicine'—a work which, if it did not display great originality, yet reflected much credit on the author's erudition, research, and philosophic spirit. In the present work, however, he is neither the exponent of the existing condition of auscultatory diagnosis, nor the promulgator of new or original views on any department of this now very extensive subject. In the brief compass of 138 pages, with an exceedingly diffuse manner, and no economy of space as regards type, he has touched upon most, if not all, of the now recognised forms of disease of the heart and lungs, embracing their pathology, diagnosis, and treatment; but indeed the space actually devoted to these important topics is considerably less than what has been just stated, for we find, on referring to the commencement of the work, that thirty-two entire pages are occupied with discussing the sounds of respiration and circulation, and various theories connected therewith. Considering, then, the extremely curt and superficial manner in which these several subjects are dismissed, we cannot form any idea as to what class of readers (and every author hopes to be read) Dr. Billing's work was designed for. To the student commencing the practice of auscultation, it will be all but useless, for there is no single subject sufficiently worked out to enable him to seize even its salient features; while to the practitioner who has studied physical diagnosis and thoracic pathology from other sources, each page presents only a series of memoranda of what he has elsewhere seen or observed. But, independently of deficiencies of this kind, which are entirely inseparable from a work of such limited dimensions, which attempts to deal with so comprehensive a subject as that of thoracic pathology, every section of which might easily be accorded as much, if not more, type and paper than this whole book contains, there are other defects and errors, both doctrinal and circumstantial, which must be noticed.

In about six pages, one of which is unnecessarily occupied with a figure of a bad stethoscope, Dr. Billing dismisses the subject of auscultation of the air-passages in health, decrying all lengthened description of the natural sounds as unnecessary. Percussion is treated of in sixteen lines. Now we think no idea could be more dangerous to the student than this of the facility and readiness with which correct conceptions may be acquired of the normal phenomena. If there be one thing more difficult than another in the study of the phenomena of animal life, it is, in our mind, the elimination of *the normal constant* (if we may employ the term) from the various sources of error presented by the differences of time and intensity, individual peculiarities, the complicated interferences of age, sex, constitution, and occupation, and numerous other disturbing forces which it is unnecessary to specify. So difficult, indeed, is the recognition of this normal constant of physiological phenomena, that we regard it as perhaps the last, instead of the first, accomplishment of the observer. On this topic in particular, therefore, at the outset, we must caution the student and junior practitioner, and recommend them not to despise full and elaborated details on the natural auscultatory phenomena of the voice, the respiratory murmur, and the heart's sounds—subjects which cannot be studied with too much attention.

Dr. Billing's views on the causation of the cardiac sounds have been long familiar to all acquainted with the history of auscultation. At a very early period (1832), he advocated the opinion that the sudden tension of the auriculo-ventricular valves was the essential cause of the first cardiac sound, and has on repeated occasions since urged this theory, to the entire exclusion of all other sources of sound which may be proved to have a share in producing the phenomenon. Peculiar theories involuntarily lead to the observation of facts in a peculiar manner; to this alone can we attribute the author's statement with which he commences his section on "Sounds of the Heart," in which he says, "The natural sounds of the heart are nearly similar to each other"—an observation which we will only remark is fundamentally at variance with our own results, and those of others on whose faculties of perception we place the greatest confidence. It is no part of our intention here to enter into any discussion of the views now or formerly advanced to explain the cause of the heart's sounds. We will only say that Dr. Billing has not only failed entirely, in our minds, to establish, as a logical conclusion, that certain causes capable of originating sound, and acting synchronously with the first cardiac sound, and in the immediate vicinity of its centre of intensity, have no share in its production; but, moreover, he has not established that the auriculo-ventricular tension-sound, and the normal first cardiac sound, are one and the same, identical in duration, acoustic character, and intensity. He states (p. 28), "Having, from facts established since the time of Haller, deduced my theory by logical argument *à priori,* and upon Newtonian principles, I have never sought for experimental confirmation." Dr. Billing has, we maintain, *no* logical ground for ignoring the existence of certain other alleged causes of the first cardiac sound; and yet, having never himself been in a position to judge of the precise acoustic character of the sound produced by the cause he assigns (tension of auriculo-ventricular valves), he assumes this, to the exclusion of all others, as the only cause of the phenomenon in question. Even on his own strict Newtonian principles, it would be necessary for him to establish, by direct observation, that his assigned cause was sufficient and adequate to produce the sound, which we hold it is not. In support of his views, he appeals to experiments made by two persons independently of any co-operation with himself. The first experiment is that of Mr. Harris Brakyn,* who, by means of a very rude apparatus, consisting of bladders in connexion with a flexible tube inserted into, and flanged on, the left ventricle, thought that he produced a sound similar to the first cardiac sound. As we had ourselves an opportunity of witnessing Mr. Brakyn's experiments, we feel called upon to state, that in our opinion, as far as the heart's action is concerned, they proved nothing at all; that a membranous medium receiving the impulse of a column of air or fluid, and allowed to become tense in air, will emit sound, required no demonstration whatever; but it is needless to say that this case has no analogy with the condition of things in the living heart; it is only necessary further to say, that to our ears the sound produced by the valvular-flap in Mr. Brakyn's experiment was in no way like the first normal cardiac sound. It is unnecessary to go into Mr. Halford's experiment; it likewise proves nothing. The vena cava and pulmonary veins in dogs and donkeys rendered insensible, were compressed between the fingers, and it is needless to say, that the condition of things thus produced is almost as far from the natural state as that in the case just cited.

Basing his principles of diagnosis in disease on the exclusive valvular theory of both normal cardiac sounds, Dr. Billing asserts for this department of differential auscultation a certainty, precision, and accuracy, which we think in no way accord with the experience of other observers who have given great attention to this subject, and which entirely surpass the amount of skill we have ourselves ever succeeded in acquiring. Many instances are given throughout the work, on which, as the data stated appear to us quite inconclusive, we should decidedly withhold judgment; a few occur, in which good reasons, as it appears to us, exist for coming to conclusions different from those of the author.

With regard to the present actual condition of physical diagnosis, notwithstanding the immense advances that have been made, we are not disposed to agree with many late writers, who constitute what we may term

* See Lancet, Nov. 24th, 1849.

a *positive* school of observers, paying entire, if not exclusive, attention to the purely mechanical causation of vital phenomena, and largely neglecting, if not altogether ignoring, the physiologico-pathological elements of diagnosis, which we think can never be with safety omitted from our consideration of any given case. It is hard to say with which of these schools Dr. Billing is to be ranked; in some places he decries all minutiæ of diagnosis and pathology, while in others he dwells with care on, and calls particular attention to, the very smallest shades of difference in the characters of the cardiac sounds, the pulse, and the mechanism by which these differences are produced. In offering an opinion on this class of questions, we do so from a conviction that errors have been and are daily committed in each of these schools of clinical study, not so much by over-attention to its own doctrines and practice, as by neglect more or less considerable of the opinions and modes of observation of the other; and we are convinced that the true method of advancing diagnosis to the highest possible degree to which we may venture to hope it will one day reach, is the judicious combination of the principles and practice of the positive and the physiologico-pathological schools, every possible agency of both being brought into requisition, if it promises to throw only one ray of light on any obscure question in debate.

We have, in the above inquiry, tried the work of Dr. Billing by the standard of the actual state of the department of science of which it treats, and in pronouncing our judgment that he has not adequately represented the actual status of that department of medical knowledge, we have only to add, that we have not come to this opinion without calm and mature deliberation; and in rendering an unfavourable verdict, we have done so conscientiously, and through a feeling of what we owe to the cause of science. When he again assumes the task of authorship, we hope to meet Dr. Billing on ground more worthy the name and position he has already so honourably acquired.

Robert D. Lyons.

Review X.

Handbuch der Diätetik. Von Jac. Moleschott, M.D.—*Darmstadt,* 1850.
Manual of Dietetics. By Dr. Moleschott.

There is annually at Oxford a sermon preached in the vulgar tongue, with the special design of warning those who a few days before have received the higher degrees of the University, against pride in human learning. Few of those present can forget the advantage taken of this anniversary—technically called "Humility Sunday"—by Bishop Wilberforce, when the British Association happened at that time to be holding their meeting on the banks of the Isis. If the hopes expressed in our last number are to be realized, and Oxford become a *bonâ fide* school of physical science, such sermons will be annually more and more anticipated by conviction, and therefore be less and less necessary. If educated persons in general were to be instructed in physiology, and knew the amount of untiring energy, strong will, and zealous hope, which have been expended by minds of large calibre in the endeavour to push scientific research to an union with practical dietetics; if they saw the incapacity for unravelling what we know must be the simplest knot, the daily overthrow of hitherto certain facts in the process of digestion, and the inability to put others in their stead, there would be no danger of their being "puffed up" by such knowledge.

The weakness of the human intellect is more strikingly shown in this branch of learning than any other, because from the earliest times it has attracted the attention of all investigators of nature and appliers of science; and the further we go back the more positive is the profession of knowledge on points of which ignorance is now confessed. So convinced were the chymiatric school of Paracelsus of the value of their "fermentation theory," that they carried it from the physiological process of digestion into the explanation of the whole circuit of life, normal and morbid. We have been witnesses of a somewhat more modest attempt in our own day. Though romance has ceased, and zeal cooled, we still see many lives of animals, and many life-times of men, given almost wholly to this subject—and with what result? That which to-day seems established by the most irrefragable experiment is to-morrow rendered doubtful by equally clear observations. Penelope's fingers are completely outdone. Well indeed may Dr. Lehmann remark, that such experience should more than any other admonish us to be reserved in our judgment upon those results of researches which appear even absolutely certain.* Might not the existence of lactic acid in the gastric juice have been so characterized? Yet has Professor Schmidt "shown" (as chemists say) its absence under many circumstances, and the presence of free muriatic, while we see M. Blondlot coming back again to his former belief in the acid phosphate of lime, though the incorrectness of this has been asserted by the best chemists of the day. Who could have expected, after M. Bernard's recent experiments on the influence of the vagus over digestion, that this influence was to be denied, or at least rendered doubtful? Frenchmen see fat resolved into fatty acids and glycerine by the contact of the pancreatic juice, while Germans can scarcely make out that an emulsion of the two substances takes place. Candidly now, is there among the chaos of different opinions offered by observers, any final cause by which to explain the action and intention of the pouring of bile into the intestines? Who could have foreseen, from the state of our present knowledge, that an isolated coil of intestine, with a little alkali inside it, would be in a condition to digest muscle? In short, the intestinal canal exhibits itself to us as the theatre of a host of most mysterious performances, yet still—

"Before the gate
Our spirits stand disconsolate."

These considerations may fairly prevent us from wondering at the very little advance which the science of Dietetics proper has made, and the little advantage which has accrued to it from the growth of other sciences. A few of the results of old experience have been confirmed, a few rendered doubtful; empirical observation has been sometimes put in the right path; but, in truth, a conscientious writer on this subject is reduced to make a most meagre affair of the real practical part of it. And even then a greater portion of his pages is taken up with pointing out what modern science *might* ascertain, than with what it has actually discovered. Stout books, it is true, are written on the subject, but the smallest part of them is that which is named in the title-page. Take the last hand-book, which Dr.

* Physiolog. Chemie, vol. iii. p. 261.

Lehmann says is indubitably the best that has been written—that whose title stands at the head of this article—Moleschott's 'Handbuch der Diätetik.' It contains 600 pages of letter-press; of these 167 are devoted to general physiology, 337 to the description of animals and plants eaten by man, and 96 to the physiological action of food and the choice of diet. Valuable all this matter is, no doubt, and the more so from having the name and sanction of Professor Tiedemann attached to it as having furnished the greater part of the materials of the first 504 pages to his pupil, Dr. Moleschott; but yet it is not dietetics, any more than a treatise on materia medica is part of the practice of physic. The remarks in the present article will therefore be confined to the latter 96 pages of the volume, which appear very fairly to represent the present state of science referrible to the subject.

The end of taking food being to convert it into blood, the natural division of that part of the work which relates to the "physiological action of food" is according to the *time* it takes to be so converted, the *quantity* of blood which it can make, and the *quality* or specific nature of the blood made. Hence we have the three chapters, "On the digestibility of food," "On the nutritive power of food," and "On the special action of food on particular organs."

On the first head, the classical authors may be considered to be M. Gosse, of Geneva, and Dr. Beaumont. But both these experimenters seem to have viewed solubility as the *whole* instead of merely as a *contingent part* of digestibility. This of itself would vitiate their reasoning; but besides that, no distinction is made between mechanical division and chemical solution. Thus when Dr. Beaumont says that beef suet was "dissolved" in St. Martin's stomach in five and a half hours, he must of course mean broken up into an emulsion, and the same with many of his experiments on compound aliments. Then the quantity of the food taken at once is often omitted to be noticed, and the amount of water mixed with it; so that, in fine, all we gain from their experiments is a rough idea of the solubility of several alimentary matters in a mixture of saliva and gastric juice.

In the defect of empirical information on the true digestibility of different substances in the body, Dr. Moleschott derives the following general rules from chemical reasoning:

"As all the secretions concerned in digestion contain a considerable proportion of water, *the simple alimentary principles must, as a general rule, be the more digestible, the more soluble they are in water.* The chlorides and alkaline salts pass easier into blood than earths, the organic acids and dextrine or sugar, easier than starch, cellulose, or fat; soluble albumen or legumin easier than coagulated vegetable albumen or fibrin.

"With solution is often joined a modification of the dissolved substance; as when starch, cellulose, or pectin, are converted into sugar. And since this modification brings about the special conversion of the aliments into the constituents of the blood, which indeed is to be looked upon as the final goal of digestion, it may be laid down as a further law, that *alimentary principles are digestible in the ratio of the nearness of the relation in which they stand to the last link of the series of allied forms.* In the class of starchy substances sugar is the most digestible, next to sugar comes inulin; dextrin is more digestible than gum, these two more digestible than starch, starch again more digestible than pectin or cellulose."

These laws, of course, as our author remarks, modify one another, so that substances which according to one of them stand low in the scale, in the other take a high rank. For example, our knowledge that starchy substances are convertible into fat, would make us view the latter as the most digestible of the two, till we recollect the difficulty of its solution in the digestive juices. He quotes as an instance of their being both in action together, the indigestibility of vegetable albumen as compared with animal, as being lower in the scale of assimilation, and insoluble too.

The importance of such laws as these, could they be established, is very obvious. In the choice and preparation of food they would be invaluable. But unfortunately, when they come to be tested by experiment, they are found wanting. Gum, for instance, which stands so much higher in the Moleschott scale of digestibility than starch, lies under the imputation of not being capable of absorption at all, or of being converted in any way by the abdominal fluids into a nutritive substance. Dr. Frerichs and Blondlot found that forty-eight hours' exposure of it to the action of gastric juice made no impression at all. Dr. Lehmann obtained similar results after three or four days. MM. Tiedemann, Gmelin, and Boussingault, found in the fæces of a goose and a duck the same amount of gum as had been taken in food, &c. (See Dr. Lehmann's Phys. Chemie, vol. iii. page 286.) This one instance suffices to show the danger of transferring to practical matters provisional rules, based on the imperfect knowledge of the progressive sciences. Not that such rules should be proscribed; they are most useful in giving expression to the stage of science we have arrived at, and serve themselves as tests of its truth; but they must be held light in hand, and let slip the instant that a squall of new empirical knowledge warns us to change our tack.

While inquiring into the various actions of the *separate* secretions upon aliments, it must not be forgotten that the chief point which the dietician wants to know is their *combined* operation. The required sacrifice of animal life would make these experiments expensive and tedious; but the necessity for vivisection is not apparent, as Dr. Moleschott seems to imagine, or one would be cautious in indulging a hope of seeing them carried out. Observations on the fæces, which to the microscopic eye present nothing repulsive, would probably teach as much on this head, and comparative anatomy offers its interesting and promising help. These are both preferable to the author's proposal of dissecting live creatures on a large scale.

The influence of *cookery* on food is a subject of great importance to the dietician, whether he looks on it from a purely philanthropic and economical, or from a curative, point of view. In experimenting on the *chemical* changes effected by its various operations, especially that of heat, the *mechanical* condition of the substance to be cooked must not be forgotten, or we shall find the experience of the *artiste* more valuable than that of the philosopher. A striking example of this mechanical influence is afforded by the truly British operation of roasting a joint. The radiated heat glows out of the open range from the good Wallsend (none of your Yorkshire or Midland coals for our kitchen, they get covered with dust, and do not radiate): soon the outer layer of albumen becomes coagulated, and thus the exit of that which is still fluid is prevented, and it becomes solidified very slowly, if at all. The cellulose tissue, which unites the muscular fibres,* is converted by gradual heat into gelatine, and is retained in the centre of the mass in a form ready for solution. At the same time the fibrin and albumen, according to Mulder, take on a form more highly oxidized, and more capable of solution in water. The fat also is melted out of the fat-cells, and is directly combined with the alkali from the serum of the blood.† Thus the external layer of albumen forms a sort of box which keeps together the important parts of the aliment till they have undergone the desired modification by slow heat; a box, however, permeable in some degree by the oxygen of the free surrounding air, so that most of the empyreumatic oils and products of dry distillation are carried off. These are doubtless in a general way no loss to our stomachs or our palates, but one we may hope is retained in some proportion. This is acetic acid, whose presence would certainly tend to make the muscular fibre, as well as the albumen, more soluble. The case-hardening of the joint may be produced in a certain degree by *rapid boiling*, but the interior albumen seems by this process more hardened and less digestible, perhaps from want of the acid above named. *Stewing* and *baking* retain all the good things, but then they retain also a variety of known and unknown educts, inimical to the stomach's peace; while *slow boiling* makes, it is true, a digestible soup, but converts the muscular fibre into a mass of hard strings, which eaten or not eaten must necessarily be wasted. Roasting, then, is as scientific and wholesome, *and therefore as economical*, a process as it is a palatable one. One great advantage which roasting can boast of is, that it puts a check upon the deception, intentional or non-intentional, as the case may be, of over-preparation. Heat seems to have an effect upon albumen, in some degree proportioned to the period of its application, rendering it more and more insoluble, even after any apparent change can be perceived by the palate. Thus soups and stews which are "kept hot," are wholesome enough during the first three or four hours, *may* be digested at a railway refreshment-room for some hours after, but on the second or third day give the incautious eater of two-franc Palais Royal dinners an infallible diarrhœa.

Another excellent application of the mechanical process of forming the exterior of the joint into a case for the retention of the valuable fluid matter inside, is made by M. Soyer, in his 'Modern Housewife,' (p. 155.) He directs that meat intended to be eaten cold should, immediately it is cooked, be plunged into water of as low temperature as possible, so that the exterior adipose tissue should rapidly solidify, and retain the gravy in the meat by an impermeable coating of fat. As chloride of sodium and acetic acid have both of them the property of rendering albumen more soluble in the laboratory, and perhaps also the fat more digestible,‡ there is every reason to believe that they should not be viewed merely as a means of preserving meat from decay, but of rendering it more completely alimentary at the same time. A most interesting series of experiments

* Not, however, the sarcolemma, which an experiment of Kölliker's seems to remove from the class of substances yielding gelatine. See Kölliker's Microscop. Anat., vol. ii. p. 250.

† G. J. Mulder, on Nutrition in relation to National Disposition, quoted by Dr. Moleschott, who has translated it into German from the Dutch original.

‡ Pereira on Food, &c., p. 173.

might be made on this point, which would have at the same time a calculable commercial value.*

Practical physicians would also be glad to know under what circumstances a mixture of animal and vegetable food is more or less digestible. Most healthy persons seem to thrive best under it, and even some dyspeptics, from whose meals routine practice has excluded potatoes, and peas, and pastry, for a long period, find themselves more comfortable, when in despair they begin to live like other people. Is it that vegetables stimulate the organs of digestion to greater activity and more copious secretion? Such appears to be the case with graminivorous animals as compared with carnivora.

On the whole, it appears probable that more light will be thrown on the subject of the different degrees of digestibility of aliments from a consideration of their mechanical than of their chemical forms. A series of microscopical observations on the condition of food under various circumstances of preparation and digestion, is much wanted. It would both pave the way for further chemical research, and contribute valuable knowledge to the practitioner.

The *nutritive power* of different aliments is directly in proportion to the quantity they contain of the elements of the blood, in a state capable of assimilation. The latter clause is inserted by Dr. Moleschott especially to exclude the valuation of food solely in the ratio of the quantity of nitrogen it contains. This proposal would class perfectly insoluble fibrin, coagulated vegetable albumen, &c., among nutritive substances, besides supposing that the body is increased by thein, caffein, and other similar principles, of which there is no evidence; except, indeed, what is afforded by the resemblance of their chemical formula to that of creatinine, and some of the uninvestigated animal alkaloids. Nutritiveness, then, depends in no slight degree on digestibility, and so far our investigations on this head will receive help from a consideration of the mechanical conditions of the several component parts of the alimentary body. While looking to the more obviously required constituents of food, it must not be forgotten that blood, and that which is to be formed of blood, contains also inorganic mineral elements. Soda, and potash, and chlorides, are required, and the food must possess these, or it cannot be said to be nourishing. Now, the researches of Liebig (quoted in Lehmann's 'Phys. Chemie,' part iii. p. 91) show a very different proportion of sodium and potassium in the extracts of the flesh of different animals. A pursuit of similar researches into the comparative amount of phosphate of lime, mentioned in the same place as being carried on by Schlossberger and Von Bibra, may lead to valuable results. The deficiency of this salt in the flesh of young animals was observed by Liebig and Berzelius, and may perhaps help to account for the inferiority of its nourishing power, which has long been empirically suspected.

Though little positive proof of the reason for the varying nutritive power of the flesh of diverse animals can be given, one negative fact is of considerable importance—viz., that it does not depend on the quantity of nitrogen they contain. This is clear from the analyses made by Schloss-

* Dr. Garrod's observation, that ordinary salt provisions are deficient in the proper amount of potash required by the blood, is suggestive of an improvement in the preparation of meat.

berger and Kemp, which are given in the third volume of Dr. Lehmann's 'Chemistry.' From these it appears—

"That the amount of nitrogen in muscular fibre is throughout the animal kingdom essentially the same. The flesh of fish contains the same total quantity of this important element of nutrition as that of the higher animals; oysters, on the other hand, instead of containing more, as common experience would lead us to conjecture, actually stand lower down in the scale of proportion. So there is a striking difference between being rich in alimentary principles and being good for food."

Besides the necessity for being in a condition capable of absorption, it is probable, also, that there is required in the nutritive principles a certain combination among themselves in order to support life, and that on the approach to perfection in this combination the value of different aliments consists. The diet, as a whole, must contain all the constituents of the body, and we may conjecture, *à priori*, that such diet will be most suitable which contains these constituents as nearly as possible in the proportions and combinations exhibited by the body to be nourished.

Dr. Moleschott is known abroad for the spirited foray on which he has spurred against those who have accepted the theses nailed up before the world by Baron Liebig, in his 'Chemical Letters,' especially those which divide food into "elements of nutrition" and "supporters of respiration." In a recent work, called 'Answers to Liebig's Chemical Letters,'* his energy almost excites a smile. He does not hesitate to pronounce "any extension of pardon, any delay in passing sentence on such a doctrine, an actual sin." For the sentence which he does pass we must refer the reader to the answer to the ninth letter. It is rather too much in the style of "the counsel for the plaintiff," and really argues rather against the Baron's incautious mode of wording his ideas, his fondness for "startling novelties" and bold generalizations, than against what may be supposed his actual opinions. The reckoning of hydro-carbonaceous food as merely fodder for a walking fireplace is very likely as faulty as the answerer of the letters makes it out; but there is good sense in Luther's dictum, *Auf ein grober Klotz gehört ein grober Keil* (a sturdy billet wants a sturdy wedge); and these rough, bold ways of speaking often impress a new truth on the multitude when more correct axioms would fail. On dieticians, medical, agricultural, and philosophical, it certainly has impressed the fact, that for the great outgoings of carbon there must be corresponding incomings; and under the wing of this, we may be allowed to avoid a question which ought to have an article of its own in some future number.

The proper proportions in which the elementary principles of food should be combined cannot be better estimated than by observing what they are in that typical aliment which the Creator has prepared for us before reason makes us cooks, and when the "whole duty of man" consists in growing big and strong. The proportions in milk are, according to Dr. Lehmann, 10 parts of plastic material, 10 parts of fat, 20 parts of sugar, and 0·6 of salts. But then we must remember that this golden age has an end, that other functions and other duties arise; the outer world makes its demands, mental and bodily deeds have to be done, and therefore (have we not a right to say *therefore?*) a different expenditure and a different income must

* Der Kreislauf des Lebens. Mainz. 1852.

be calculated on. This is obvious enough, but as to what the differences are we are completely in the dark. We cannot even guess why different young animals should be supplied, as they are, with different fare—why the calf should be allowed so little sugar and so much fat and casein (unless, indeed, we count butter and cheese dairies as part of the final causes of cows)—why the "poor little foal of an oppressed race" should be treated to as much sugar as his rider, and much more fat. Why should kids and lambs enjoy the sweets which puppies are denied? (See Lehmann's 'Physiolog. Chemie,' part ii. p. 335, for the different constituents of milks.)

"As the founder," says Dr. Lehmann, "after he has assayed the ore, knows how to mix his fluxes in a proportion corresponding to its contents, and suitable to smelting it, so should it be the aim of the physiologist to calculate for a given organism under given circumstances the proportions in which the individual alimentary principles must be combined, so as to ensure a favourable result. In such an interrogation of nature, physiology seems to have the best prospect of keeping regular accounts. From these, again, general formulæ might be constructed, by which he might succeed in predicting, with a high degree of mathematical precision, the effects of a specified agency on the animal frame. Many, indeed, are the vital acts to be taken account of in such a formulæ, and almost countless are the investigations required, ere this goal can be won. But though broad be the field, and great the hindrances, yet rich are the rewards it promises to successful toil—rewards which will not only advance theoretic truth, but become deeply incorporated in everyday life. Dietetics will be grounded on a firm foundation; and perchance the thought is not a vain one, that the healing art also may be thereby made capable of more accurate investigation.*

After discussing the nutritive powers of aliments, Dr. Moleschott enters upon their "specific action on certain organs." In speaking of their influence on the digestive canal, he attributes the good effects of condiments, such as mustard, pepper, onions, tea, and coffee, to the increase in the secretions of the salivary and gastric glands by means of their essential oil. To fruity wines he attributes the same benefit, still further augmented by the alcohol they contain. The admirers of "peaty" small-still whisky will be glad to read of fussel oil (whence its flavour is derived) being included in the same category. The inconveniences arising from the use of maize, lentils, peas, rye, and that "grain which in England is generally given to horses, but in Scotland supports the people" (Johnson), chesnuts, almonds, &c., are due, according to our author, to the great demand they make on the solvent juices. They contain little water, and therefore absorb much of that; while their cellulose so envelops the albuminoid constituents, that these last require a larger amount of gastric secretion to dissolve them than any other that the "dura messorum ilia" can supply. So that an inadequacy of the digestive solvents, that is to say, an inadequacy in respect of the demand, is caused.

The mechanical influence of the unabsorbed parts of eatables is disposed of in rather too summary a manner. That the seeds of strawberries and currants, the skins of small raisins and the like, should act as foreign bodies and excite the peristaltic action, is comprehensible enough. But why syrups, and honey, and oil, and fat, should act in the same way, while emulsin, dextrin, gum, and starch, whose obvious physical qualities are not very different, "clothe the mucous membrane with a defence against irri-

* Lehmann's Phys. Chemie, part iii. p. 448.

tating substances," is not so clear. The chemical changes which the former class are liable to should be taken into account. But, in truth, are the facts quite certain? Figs, dried currants, seedy summer fruits, and so on, are certainly laxatives; but are they so in consequence of their skin and small seeds? Tamarinds, baël jelly, and the insipid mashed-potato-like banana, are much more purgative without seeds; while, on the other hand, canary seed has no effect on the bowels, and pigs eat haws by the bushel without any consequence to themselves, though the haw, when sown, sprouts a year earlier for its semi-digestion. It may fairly be questioned whether indigestible matters, as such, really irritate the intestines, and whether, in some cases, as gum, for example, they do not soothe them.

The direct effect of substances, insoluble as well as soluble, on the mucous membrane of the digestive canal is one of great interest to the practical physician, and he has not received that industrious aid from science applied in this direction which he may justly claim. Experiments on the effects of astringents on mucous membranes, on the lines of distinction between these and tonics, on the methods of guarding them by an envelop of insoluble matter which is adopted in drugs and aliments presented to us by nature, would be of as much use to dietetics as pharmaceutics. The last point especially— namely, the mechanical method of distributing a substance through the intestinal canal by enveloping it in cellulose, deserves great attention. We have all observed the different effects of bark and quinine, between the latter substance given in acid and in a solid form, by the mouth or by enema; and one cannot fail to attribute these variations to the different parts of the mucous membrane, and the different extent to which its action spreads. So, too, in food, the mechanical differences of modes of preparation must offer to the intestinal canal the component parts of the dish in a different order. For example, two sorts of pie-crust are familiarly known as "short" and "puff;" in the former, the butter is thoroughly incorporated with the dough, so as to divide the starch-granules one from the other, and permeate the gluten like a sponge; while in puff-pastry, the dough forms thin but solid layers, like a quire of buttered paper. If the teeth are imperfect, or the mastication careless, the latter is well known to form a solid mass in the stomach, which is very difficult of solution in the upper portion of the intestines; while the easily-broken paste is mixed with the rest of the food, and though formed of the same chemical constituents as its indigestible brother, receives a very different character from its employer.

The changes effected in the blood by a large supply of albuminous and fatty food have not been made the subject of inquiry; the general semi-metaphorical expression, that it is made "richer" by them, is not satisfactory. The author's statement that it is "thinned" by chlorides and neutral salts, is still more vague; for though, truly enough, they would render its albumen more fluid, yet they must also make the albumen of the food more soluble, and so cause the entrance of a larger quantity into the circulation. The same influence is attributed to fruit and vegetable acids, and an awful retribution of dropsies, cachexias, anemias, diarrhœas, threatened to young ladies who endeavour to reduce by vinegar those proportions that Rubens loved. It must be confessed that our author, in spite of the off-hand disrespect which he exhibits in his *l'envoi* (p. 599), for the opinions of nurses and old women, does occasionally fall into their phraseology, as here in his talk about the "richness" and "thinness" of the blood.

The statement of M. Lecanu, that the use of alcoholic stimulants directly augments the fatty matter in the blood, has been employed for moral and social purposes; but now some doubt is thrown upon it by Dr. Lehmann, who says, he cannot find that the change takes place, except where there is a granular condition of the liver preceding.* Dr. Lehmann's own observations as to the increase of sugar after eating starch, and of fibrin from animal diet, are equally important, and deserve further inquiry and confirmation.

The influence of diet over muscular fibre is an important social question, for thews and sinews have always ruled the world both in peace and war in a proportion quite equal to brains. Indeed, it is a question, which the present writer is disposed to answer in the affirmative, whether, *nationally*, muscular and mental energy do not always run in couples, and whether the first is not the cause of the second? It does not appear that any diet, so that there be plenty of it, is incapable of fitting man to get through his daily work; but the best specimens of the species are certainly those who enjoy the greatest mixture. The example of the hunting nations across the Atlantic is given by Dr. Moleschott in evidence that an exclusive diet of mammalian flesh increases muscular development; but Mr. Catlin and the Ioways at Lord's Cricket Ground did not bear out this statement. And the inhabitant of the Pampas, who lives wholly on water and beef, made tender by being rode upon between saddle and back till dinner-time, cannot show, according to Sir F. Head, an inch of calf, though use and necessity develop his arms to an unnatural extent. A Brahmin sepoy, who would as soon eat his own flesh as anything besides rice, would walk him, run him, or knock him down any day; and he again would receive the same treatment from many of us, fed as our fancy leads us. Feeding on fish has, according to our author, a deteriorating effect upon the size and development of the muscles; but he is not very happy in the instances he cites of icthyophagous nations. He mentions the Samoyedes, the inhabitants of the Hebrides and Faröe islands, Greenlanders, and the North-west Americans. Now the Samoyedes are small enough, certainly, but do *not* live on fish, being so noted for their love of warm-blooded meat, that "in the Russian chancellery they are designated *Sirogneczi*, eaters of raw meat."† The present inhabitants of the Hebrides would stare much at the company they find themselves in; nor were matters worse 150 years ago; for we read in Martin's 'Description of the Western Islands' (London, 1716, 2nd edition), that "the diet generally used by the natives [of Skye] consists of fresh food, for they seldom taste anything that is salted except butter. . . . Their ordinary diet is butter, cheese, milk, potatoes, coleworts, brochan, that is, oatmeal and water" (vulgo, *porritch*);—and a similar bill of fare is attributed afterwards in the same work to Tirae and St. Kilda. As to Faröe, their bad habits seem to have been given up now nearly a hundred years; for we read that "we have a remarkable instance of the great effects of diet on the diseases of a nation in the inhabitants of the isle of Ferro. Since fishing has declined among

* Phys. Chemie, vol. i. p. 259.

† Quoted from the Histoire General des Voyages, xxiv., in Pinkerton's collection.

them, and the inhabitants have cultivated corn, and live on other food instead of whale's flesh and bacon, the elephantiasis has entirely ceased among them."* As to North-west America, the public interested in the search for Sir John Franklin have been assured over and over again that an abundance of fowl, and mammalia also, sufficient for human food, is obtainable at all seasons. The excessive improvidence of the natives, and the severe climate, fully account for their miserable condition. On the other hand, large tribes who subsisted at one time entirely, and still in a great measure, without red-blooded meat, are noted for their corporeal development. Who have so often excited the wonder of travellers for their superiority to most of the Pacific nations allied to them as the New Zealanders? Yet they are notorious fish-eaters. Their *carte* is deficient in mammals; dogs and swine are a recent introduction, and man, we hope, was always an occasional luxury; but a programme of their fish-dinners, given us by a chaplain to their enterprising bishop, might almost draw Apicius round the globe. It comprises lampreys, eels fresh and dried, kippered shark, a kind of cod, mullet, whitebait (*inanga*), which is boiled or broiled or baked in small baskets, so as to make a fish-cake, cockles of three sorts, mussels, oysters, and a whole list of Maori names said to belong to dainties of the most refined description. It is true they have birds and vegetables also in considerable variety, and, as before suggested, perhaps this variety causes their superiority; but still the staple of their fare is evidently fish, as observed by Captain Cook. A similar mode of living is attributed by this observant seaman to the Sandwich islanders, of whom he says "the majority were above the middle height," and to the clean, comely Otaheitans, whose frail daughters were fair enough to cause the mutiny of the *Bounty*, before European civilization had altered them. So that we cannot attribute degeneracy solely to the substitution of fish for meat. The truth probably is, that *the mode of procuring food has a greater influence over mind, manners, and muscles, than the nature of the food itself.* He that is satisfied with what he can pick up ready-grown degenerates either into a starved New Hollander, where food is deficient, or into an effeminate creature, like the former inhabitant of the West Indies, where it is abundant; he that seeks only the greatest amount of nitrogenous matter grows up a mere hunter, and becomes a prowling, cruel, passionate, dirty, yelling American Indian (for the "noble savage" existed only in Mr. Cooper's fancy); while a civilized people will be found from the earliest times, like the wise son of Sirach's man of a good heart, to "have a care for their meat and diet." They will have thought about it, laboured for it steadily, investigated nature and advanced science to improve it, and obtained their reward in the search itself.

The articles endowed with most direct action on the *nervous system* are tea, coffee, and alcohol, of which Dr. Moleschott compares the different effects. It is not clear why he omits tobacco, which daily soothes the ruffled spirits of so many, and proves in excess so injurious to nervous energy.

The operation of diet on the *generative organs*, on the secretion of *milk*, on the expired *air, urine, and skin*, are discussed by Dr. Moleschott;

* See Von Troil's Letters on Iceland, Letter xxiv., dated 1776, from Chevalier Back to Dr. Von Troil.

but what has the adipose tissue done that it should not appear among those parts of the person over which food has an influence? That unhappy saying of Hunter's, that "fat is no part of the animal body," seems to be taken much more literally than he probably intended. It is true that to the fat can be assigned no one defined duty, as "motion" to the muscles, "sensation" to the nerves, "preservation of form" to the bones; but without it no one of the other parts can perform their offices, and its total disappearance is synonymous with death. Aliments which most rapidly form adipose tissue are the care of the agriculturist, and it is often of the greatest interest to the physician to know those least likely to produce it.

The succeeding chapter is on "The choice of victuals in a state of health." Modern science allows man in this exercise of volition much more liberty than till lately he claimed from his advisers. If any one desires to see the heavy bonds under which our forefathers groaned when they sought "what to eat, drink, and avoid," let him weigh Sir John Sinclair's tractate 'On Health and Longevity,' in four closely-printed octavos, 800 pages to a volume. Fancy a zealot for antediluvian life trying to act up to the insane quackeries of Lord Bacon! He would from time to time "be let blood," to "evacuate and empty the old moysture of the body;" he would wear clothes of a "watery and oiley substance"—to wit, greasy flannel or baize; he would oil his body daily, or paint it like an ancient Briton, and bathe in a variety of nasty expensive preparations; he would avoid change of air, because it "wastes the body;" he would stir up the sexual affections, but not gratify them, to "strengthen the heate of the spirits;" he would eat nitre with his food; dip musk-roses in his malmsey; take garlic and tobacco as a duty; shun sympathy with his kind, because care, pain, and sorrow shorten the days; lose the seasoning of every pleasure by taking it as a sort of medicine instead of for the pleasure's sake; and live in such a daily state of bewilderment as to whether his food was hot, cold, moist, or dry enough—in such fear lest he ought to be drinking pearls, eating bezoar stones, or taking some similar remedy for immaterial ailments—in such anxiety about the want of hair on his legs, or other equally certain signs of long life, that living would be a task, and "Renew my age" would cease to be his prayer. But we have escaped from this bondage now, and even the rational advice which the Schola Salerni sent to Duke Robert of Normandy in rhymed hexameters, would be considered a burden. These poetical prescribers were long the guides of Europe, and though some lines have a perfectly jovial sound, such as

> "*Ova recentia! vina rubentia! pinguia jura!*
> *Cum similâ purâ, naturæ sunt valitura,*"—

others interfere unwarrantably with personal tastes; denouncing, for instance, one's pet crusts,—

> "*Ne comedas crustam, choleram quia gignit adustam,*"—

and directing the transit of wind through the bowels, and mucus from the nostrils, in a way not to be borne by a free man.

But though the modern dietician does not venture to interfere much with the habits of his species in general, there are certain classes for whom it is incumbent that a provision should be made by others; and the arrangement of the diet of these is one of the most useful employments of science. Masses of our dependents require to be supplied with an amount of food exceeding only slightly that which is necessary to perfect health; and what this necessary amount may be calculated at is of the greatest importance to those who have to regulate the diet of soldiers, sailors, paupers, prisoners, &c. One way of ascertaining the exact quantity which will support life is the direct method of limiting the food till it is evidently insufficient. But this is not a pleasant experiment. Luigi Cornaro tried it, and by dint of living a vegetable life in his palace and gondola, protracted existence on what we should call half a breakfast a day: Dr. Stark tried it, and got on pretty well so long as he had nothing to do besides taking his weight; but when he was obliged to undergo a contested election for St. George's Hospital, it killed him outright—as it would have done Cornaro, St. Hilarion, Simeon Stylites, or any other hero of asceticism. The lowest diet on which a definite individual can manage to exist under special circumstances is not to be taken even as a minimum for others under mixed circumstances. Professor Mulder goes another way to work, and calculates backwards from the expenditure of carbon by the lungs, nitrogen in the urea, and so on, what must be the allowance of each. Dr. Moleschott objects that this is faulty in principle, because it is impossible to be certain from which aliment the particular constituent of the secretion be derived. But why *should* you know? The first thing to ascertain is the quantity of each element required, and its mode of combination may be made the subject of different experiments. On such a point as this it is dangerous to venture upon numbers without going more into detail than would be consistent with the limits of a review.

Another class for whom we are bound to use our reason in providing are children, and above all those whom misfortune has deprived of the ready formed bounty of nature at an early period. The resemblance of ass's milk to human renders it the most proper substitute; though it must not be forgotten that it is more watery, and therefore that a larger supply is required. But its rarity renders it to many unattainable, and it is very desirable to prepare cow's milk in such a way as to make it suitable for the purpose. The latter contains a great deal too much cheese, a slight excess of butter and salt, and is very deficient in sugar. To reduce the excess of casein requires an equal bulk of water to be added to the milk, and the deficiency of sugar is estimated by Dr. Moleschott to be made up if from thirty to forty parts in a thousand of sugar of milk are put in. He is troubled how to compensate the want of butter—would not a little cream settle the matter? Should any ass's milk be obtainable, though limited in quantity on account of the expense, a very perfect imitation of the human secretion is made with two-thirds ass's, and one-third cow's, mixed warm. In regulating the degree of dilution, it must be remembered that during the first days of lactation the milk is at its thickest, gets quickly more watery, and then gradually richer in casein and salts.

In commenting on the choice of food for different sexes and ages, the author states his opinion that the use of tea hurries forward sexual development, and therefore warns us against giving it to young girls, lest the proper time of puberty should be anticipated. He also accuses it of a tendency to cause miscarriage, grounding his statements on an experiment of Mulder's. It is to be hoped he is rash in these opinions.

In speaking of the mode of life as modifying the diet, a popular error is noticed, which supposes that intellectual labour does not exhaust the body. He says that any one may find by his own experience how much the interstitial metamorphoses are hastened, and therefore a fresh supply of food demanded, by mental exertion. Few deny this, but many forget it.

The remarks on the influence of climate are interesting to a people who have so much connexion with foreign parts as the English. A remark is made which we have heard confirmed by several of the junior practitioners from our Indian possessions—viz., that in the present day much more harm is done to European constitutions there by over-eating, than by over-drinking.

The twenty pages designed to contain all that science has contributed towards guiding "the choice of diet in sickness," conclude the book. The limited space shows how little is to be said on this point. The road to truth seems to be that indicated by MM. Andral and Gavarret—viz., the analysis of the blood. Yet at the outset of our endeavours to apply to practice the few facts ascertained, we soon learn to feel the correctness of the observation "that we must not expect to be able to diminish in the blood an abnormally augmented constituent, by removing the corresponding constituent from the aliment." In fact, it is sometimes even added to, as for example, when MM. Andral and Gavarret found the fibrin in a dog's blood unnaturally large, after the entire abstraction of all food. So that the simplest of all direct indications becomes more complicated as we examine into it.

Inflammations, fever, scurvy, drunkard's cachexia, chlorosis, diabetes, rickets, the stone, the gout, and convalescence, are the abnormal states to which sections are assigned. There is nothing that would seem particularly new to the readers of this Review; indeed, they will probably wonder to see omitted much that is familiar to their minds. They will miss allusion to the treatment of consumption, of which even the "*haustus-ter-die*" system has taken a dietetic turn; to the management of diseased hearts, over which muscle, more than all in the body, abstinence seems, by M. Chossat's observations, to exert so much influence for good or for ill; and to the familiar experiments on aneurism. Even diseases of the stomach are alluded to only when malignant, and the indications of liver-disease are not mentioned. Obesity is only inserted as part of the *dyscrasia potatorum* (which it seldom is in England), and the distinction of sthenic and asthenic, so important in the treatment, is omitted. The times of the day, and the frequency with which food should be taken, are learnedly commented upon as respects the healthy, with whom it is rarely a matter of choice, and never of great consequence; while the periods of the sick-bed meals receive no notice. Dyspeptics would not appear to be considered as invalids at all, for they find no place in this chapter. On the whole, little as is the assistance which the present state of physiology shows it to be able to give to dietetics, rare as it is to find in practice any suggestion derived from theory or just analogy, the volume reviewed really does not seem to have made the most of it, or to have set before us the whole capabilities of the subject.

T. C.

Review XI.

1. *Histoire Naturelle des Vegetaux Parasites, qui Croissent sur l'Homme, et sur les Animaux Vivants.* Par Charles Robin, M.D., &c. (Avec un Atlas de 15 planches gravées.)—*Paris*, 1853. pp. 702.

Natural History of the Parasitic Vegetables which grow on Man and on Living Animals. By Charles Robin, M.D. (With an Atlas.)

2. *Recherches sur la Nature et le Traitement des Teignes.* Par M. E. Bazin, Medecin de l'Hôpital St. Louis.—*Paris*, 1853. pp. 152.

Researches into the Nature and the Treatment of Tineæ. By M. E. Bazin, M.D., Physician to the Hospital of St. Louis.

The treatise by M. Robin can scarcely be considered a second edition of that formerly published under the same title. Both as regards matter and manner, it is a fresh work, so widely have M. Robin's researches been extended, and so much has he profited by the labour of the intervening years. It contains a full account of every parasitic plant which has yet been discovered on the bodies of animals, and the descriptions are illustrated by a very beautiful atlas.

The work of M. Bazin is devoted entirely to a consideration of the various diseases, classed by him under the head of Tineæ. All of these are considered to be dependent on the presence and growth of parasitic plants, and therefore, in our review of M. Robin's work, we shall necessarily traverse the ground occupied in M. Bazin's treatise.

The first 252 pages of the 'Natural History of Parasitic Vegetables' are occupied with "Prolegomena." There seems to be a fascination for M. Robin in what he calls the "fundamental questions of biology," and accordingly, as preparatory to a treatise on a small section of the vegetable kingdom, he favours us with a long dissertation on life in general, and on the most complex questions in physiology. If such a preliminary chapter be necessary, it might, at any rate, have been only one quarter of the length; and although it gives M. Robin an opportunity of stating his abstract and somewhat peculiar views on "concrete and abstract biology," we cannot but consider it provoking in no slight degree to be obliged to wade through it, before reaching the pith and marrow of the volume. We will not ask our readers to follow us, but proceed at once to the succeeding portion of the volume.

All the vegetables which grow on living beings are, of course, cryptogamic, and are either low forms of Algæ or Fungi. Under the head of algæ, M. Robin enumerates 14 genera and 38 species; under the head of fungi, there are 16 genera and 48 species. Most of these plants are composed of simple cells, or of cells placed side by side; the unicellular algæ being distinguished from the unicellular fungi by containing chlorophylle or some analogous substance, and usually one or many coloured vesicles. The more highly formed algæ are composed of interlaced filaments (*Trichomata*), simple or ramified, cylindrical or flattened, and containing coloured molecules; and of a reproductive system—viz., vesicles (*sporangia, conceptacles*) and spores (*sporidia*). The fungi are represented by filaments, at first simple, then ramified, and formed by a single elongated cell, or, more rarely, by several cells placed end to end (*mycelium*). The reproductive system is constituted by spores, which are seated on a receptacle, either at once or by the mediation of certain special structures, or are contained in a distinct vesicle (*sporangium*).

It would lead us too far to go into the minute anatomy of these plants, which would indeed be unintelligible without the atlas; nor shall we attempt an account of the various genera and species which are found on the lower animals. We shall content ourselves with enumerating those which are found on the bodies of men.

The conditions of growth of the parasitic plants on human bodies are the same as in all other cases. Whenever the normal-chemical processes of nutrition are impaired, and the incessant changes between the solids and the fluids slacken, then, if the part can furnish a proper soil, the fungi will appear.

The soil on which these plants grow is for the most part composed of epithelium or cuticle, acid mucus, or exudation; acidity, although favourable for their growth, is not indispensable, since some of the cryptogamia grow in an alkaline or neutral ground, as on the ulcerations of the trachea. On the skin, and in the buccal and pulmonary cavities, the plants are exposed to atmospheric air, and many of the fungi absorb oxygen, and emit carbonic acid. In the intestines, the nature of the gas is somewhat different; but some species grow here also. Humidity and warmth are important conditions of growth, and these, of course, are always to be found in connexion with the animal body.

An useful division of the subject for our purpose is afforded by the anatomical seat of the cryptogamia on the skin, or on the mucous membranes of man.

A. Cryptogamia on the Skin.

Ten varieties have been noted in this locality. We shall enumerate them in the order in which they are given by M. Robin.

1. *Trichophyton tonsurans* (Malmsten). — Syn. *Trichomyces tonsurans; Mycoderma of the Plica polonica; fungus of the hairs in Herpes tonsurans; fungus of Porrigo scutulata; Achorion Lebertii; fungus of the Teigne tondante* (Bazin); *Rhizo-phyte* (Gruby).

This fungus was discovered and described in 1844 by Gruby, in the disease called by the brothers Mahon "Teigne tondante;" by Cazenave "Herpes tonsurans;" by Erasmus Wilson "Trichoses furfuracea," (one of the diseases called ringworm and porrigo scutulata in this country.) It exists also, as pointed out by Günsberg, in the Plica polonica, although the two plants were formerly described as different.

The Tricophyton is formed by oval transparent spores, which give rise to articulated filaments. Its anatomical seat is in the *interior of the roots of the hairs.* The hairs and fungi simultaneously increase; the former seem larger than usual, are paler in colour, lose their elasticity, soften and break off when they have risen some one or two lines above the surface of the scalp; in the short cylinder then left the fungus grows still more rapidly, so that the normal structure of the small stump of hair soon becomes indistinguishable. Sometimes the hair breaks off before emerging from the skin, and the fungus, epidermis, and sebaceous matter fill the ends of the piliferous conduits, and form the little prominences which can be seen by the naked eye in this disease, and give the skin a rough, anserrine appearance. The sporules and mycelium of the plants can sometimes be seen, in the form of a white powder, on the roots of the broken hairs; sometimes the cutis becomes congested and thickened, and then the plant is mixed up with scales of epidermis, with fatty and albuminoid granules, with pus, &c.; and crusts are formed of greater or less thickness, in which the growth of the fungus can go on.

MM. Robin and Bazin adopt unreservedly the opinion that the Trichophyton is the cause of the disease known under the various names above given; and each author relates examples of the contagion of the disease by transmission of the spores. Bazin has made the very important observation, that the same disease will attack horses, and can be communicated from them to men. Both Robin and Bazin, however, admit that there is some condition of the hairs (dependent, no doubt, on constitutional causes) which is essential for the growth of the plant, as sometimes the disease disappears—i. e., the fungus dies—without treatment.

The *diagnosis* of this disease is extremely easy. The usually round bald patches, with the little elevations caused by the swollen roots, and the dryish scales of epidermis covering the skin more or less, and accumulating round the elevations, are very distinctive marks. Occasionally, when the cutis is more congested, and the crusts are thicker from abundant cuticle and exudation, some doubt may exist, but then the Trichophyton can be usually found in the crusts.

The *treatment* of this species of ringworm has been long one of the most difficult points in dermatology. Its principles, however, are now well understood, and few cases resist the proper measures. The essential point is to apply to the roots of the hairs a preparation which may destroy the Trichophyton; if this can be done, the disease is cured. It is first of all necessary to remove the hair; this is in part generally accomplished before the case comes under treatment, by the course of the disease; if it has not been sufficiently done, "epilation" can be accomplished by a chemical agent, or by extraction with pincers. M. Bazin recommends the ointment given below,* or the oil of cade, which appears to be the best depilatory known, or with these means epilation with the pincers may be combined.† The removal of the hairs permits a "parasiticide" solution to be applied to the hair-follicles, within which are the prolific spores of the fungus. For this purpose M. Bazin recommends either a solution of bichloride of mercury (1 part to 250 of water), or an ointment of the acetate of copper (1 part to 500 of lard). We have used also, with excellent effect, a solution of the pernitrate of mercury, about 1 part to 30 or 40 of water; this is, however, a very powerful remedy, and is to be cautiously used, as it easily blisters the scalp. We have used also an ointment composed of sulphate of copper (1 part), alum (3 parts), and lard (20 to 30 parts, according to the age of the patient). Probably, however, a better parasiticide

* Lime and carbonate of soda, of each one part, lard 30 parts.

† M. Bazin very justly remarks that the method practised by Mr. Plumbe, of pulling out the hairs with the pincers, has been much too severely criticised by Alibert. If the hairs are pulled out in the proper direction there is very little pain, especially after the sensibility of the skin has been blunted by the use of the oil of cade.

agent than any of these is the sulphurous acid, which we have seen Dr. Jenner employ lately in a case of favus, with astonishing results, and which doubtless would be equally successful in Tinea tondens.* Chlorine water might also possibly answer the same purpose.

With respect to the name of the most common disease in which the Trichophyton tonsurans appear, the term used by Cazenave (Herpes tondens) is extremely unfortunate; no doubt vesicles are sometimes seen, and sometimes the cryptogamic disease succeeds to true *Herpes circinata* of the scalp, but in many cases there are no vesicles at all throughout the whole course of the disease. The term used in this country, *Porrigo scutulata,* is inconvenient, as it is applied with greater justice to favus. The old term of tinea is, after all, by far the best, and the specific affix tondens expresses well one great feature of the disease, the baldness arising from the brittleness of the hairs.†

2. *Trichophyton* (?) *sporuloides* (Robin).—Syn. *Mycoderme of the Plica polonica.*

In addition to the former species, Walther describes, in the Plica polonica, oval or circular flattened sporules, which have been too little studied at present to permit their exact characters to be stated.

3. *Trichophyton* (?) *ulcerum* (Robin).

Lebert has described a fungus in the crusts covering an atonic ulcer of the leg. We refer to Lebert, 'Atlas,' pl. xxii., fig. 7, for a description.

4. *Microsporon Andouini* (Gruby).

This plant has been studied by Gruby, and its existence (though denied by Cazenave) has been confirmed by Robin. It is present in the disease commonly called, after Willan, Porrigo decalvans, or Alopecia circumscripta, or, by Bazin, Tinea achromatosa. It differs from the Trichophyton of Tinea tondens, by its numerous waved filaments, and by the extremely small size of its sporules. It is not found, like the Trichophyton, in the interior of the root, but forms round each hair a little tube; the hair then becomes opaque, softens, and breaks off. The alopecia is rapid, with or without previous vitiligo of the skin; the dermis is not congested, and the epidermis is thin and smooth.

There is an affection which should, we think, be distinguished from the Porrigo decalvans (or Alopecia circumscripta), and which is characterized by a rapid disappearance of pigment from both skin and hair, with or without alopecia. M. Bazin includes it in his "Tinea achromatosa" (Teigne achromateuse), but does not mention the fact that alopecia is not constant;

* See Medical Times and Gazette, August, 1853.

† Bazin has lately proposed the following arrangement of the various diseases called tinea—we add the common synonyms:

1. Teigne faveuse (Tinea favosa—Porrigo favosa—Favus—Porrigo scutulata).
2. Teigne tonsurante (Tinea tondens—Herpes tondens—Porrigo scutulata trichoses).
3. Teigne mentagre ou sycosique (Mentagra—Tinea mentagra).
4. Teigne achromateuse (Porrigo decalvans—Vitiligo).
5. Teigne décalvante (Alopecia idiopathique—Porrigo decalvans).

Exception may be taken to the two last terms; the others are excellent.

he states that a parasitic plant is present, but does not describe it. There must, however, be surely something more than a fungus to cause the total disappearance of pigment from a considerable portion of dermis. Besides, when the hairs return, they are at first quite white, and only gradually regain colour; but if the vitiligo were owing to a plant, they would surely not grow at all. The disease appears to us to be allied to those obscure pigmentary changes which have a much deeper seat than the surface of the body.

5. *Microsporon mentagrophyta* (Robin). Syn. *Mentagrophyte* (Gruby).

This is a plant resembling the preceding, but possessing larger spores and filaments; it was discovered by Gruby in a case of mentagra, and has been since described by Bazin. Its seat differs from that of the preceding, and from that of the Trichophyton; it is between the bulb of the hair, and the follicle in which the bulb is seated, and never extends beyond the surface of the skin.

Mentagra is usually easily cured by epilation, and by the subsequent application, on one or two occasions, of a lotion of bichloride of mercury.

6. *Microsporon furfur* (Robin).

In 1846, Eichstedt discovered a cryptogamic plant in the disease called by Willan, Pityriasis versicolor, and more lately, Chloasma. Soon afterwards, Sluyter* described the same fungus—and lately Sprengler† has described and figured it. It forms, with the epidermic scales, the yellowish brown scurf seen in Pityriasis.

7. *Achorion Schœnleinii* (Remak). Syn. *Oidium Schœnleinii. Mycoderma of Tinea favosa. Porrigo-phyte* (Gruby). *Fungus of Favus.*

Schonlein was the first to suggest that the honeycomb, or yellow favous crusts in the so-called Porrigo lupinosa (Willan), and Porrigo scutulata, were constituted by a vegetable growth. This has been repeatedly confirmed, and many excellent descriptions have been given of the disease, now called indifferently Favus, Tinea favosa, or Porrigo scutulata; but none, we think, better than that which is contained in the work before us.

M. Robin believes he has discovered that the primary seat of the Achorion is in the depth of the hair follicle, against the hair, and, as well as we can understand the description, outside the layer of epithelium which covers the root of the hair, and which forms the "inner root-sheath" of Kölliker.‡ In this observation, however, he has been anticipated by Wedl,§ who has pointed out that by using a concentrated solution of liquor potassæ, to make the parts transparent, the fungus is found in the follicle round the hair at the place where it passes through the epidermis. In

* De vegetabilibus organismi animalis parasitis, ac de novo Epiphyto in Pityriasi versicolore obvio. Diss. Inaug. Berlin, 1847.

† Canstatt's Jahresb. for 1851, Band iii. p. 182.

‡ Dans la profondeur du follicule pileux contre le poil, mais habituellement en dehors de la couche unique de cellules d'épiderme, qui lui donnent l'aspect réticulé en travers, se trouve adhérent le végétal, fait que n'a pas encore été noté. (p. 442.)

§ Ueber Pilze in Favus-Haar. Zeitschrift der k. k. Gesellschaft der Aertze in Wien, 1849.

addition to this, the plant is found in depressions on the surface of the skin, forming the yellow honeycomb-like masses which give the specific name, favus, to the disease; and which, from their frequent buckler-like shape, suggested the term "scutulata." The development of the Achorion in this situation is described by Robin after Remak and Lebert. A cuticular elevation is seen, beneath which is a small favus; when the cuticle is raised, a drop of pus sometimes issues; hence the error of those who have considered this disease always pustular; generally, however (Robin, Simon, and Hoefle), there is no pus or liquid of any kind; the plant grows, and the cuticle over it (supposing it has not been forcibly detached) finally separates, leaving the favus exposed to the air.

M. Robin does not notice the important statement of Simon, that at first there is at the point where the favus is about to form, only an increased secretion of epidermis; he notices briefly the fact, that sometimes the under surface of the favus is coated by cuticle, which separates it from the compressed and attenuated derma.

The structure of the favus is given at length by both authors, but it is scarcely necessary to do more than notice that Robin, in addition to the mycelium, the spores, and the receptacles of the Achorion, describes a finely granular amorphous layer, which forms the external coat of the favus, and is the representative of the amorphous "stroma" which often accompanies the mycelium of algæ and fungi. In the favus, also, as we shall presently see, another and distinct fungus can sometimes be found.

M. Bazin describes the favus under three heads, which are fundamentally identical, and different only in respect of form.

1. Favus urceolaria dissemina: this corresponds to the Porrigo favosa, Favus dispersus, Teigne alveolaire, of other authors.
2. Favus scutiformis: this is the Porrigo scutulata, or Favus confertus.
3. Favus squamosa; a form usually called scutulata, but distinguished chiefly by the irregular distribution of the Achorion, and by the furrowed masses formed by the fungus, the hairs, epidermis, and exudation.

The treatment of favus recommended by Robin and Bazin is epilation, and the application of the corrosive-sublimate solution, or of acetate-of-copper ointment (1 part to 500 of lard), to kill the plant still remaining adherent to the hair follicle. We suspect that the sulphurous acid employed by Dr. Jenner will be found a more effectual application than either of these two.

8. *Puccinia favi.*

The Achorion constitutes, with epithelium and a little exudation, the mass of the favus, but it has been lately (1850) observed by Ardsten of Christiana, that a different fungus, a species of Puccinia, is occasionally also present. Robin considers it to be only an epiphenomenon, and that it is certainly not present in all cases. The Puccinia is easily recognised; it has one extremity (the body), rounded and composed of two cells of unequal size, a superior and an inferior; the other extremity is prolonged into a jointed stem or trunk.

There are still three other plants found occasionally on the skin, which need merely be enumerated.

9. *Mucor.*

In senile gangrene, an ill-described fungus, supposed to be the Mucor mucedo of Linnæus, has been seen on the sloughing mass.

10. *Aspergillus.* (?)

In the wax in the external meatus of the ear, Mayer many years ago described a fungus, and Paccini has lately made a similar observation.

11. *Leptomitus* (?) *of the Epidermis.*

An alga has been seen by M. Gubler in the epidermis of an arm which was irrigated for a long time to keep down inflammation after gunshot wound: no one else has noticed it.

Some of these plants may be held to be of little interest to the practical physician, but this is not the case with others. Not only MM. Robin and Bazin, but Simon, and others of the best dermatologists of Europe, have adopted the opinion that the plants are the actual causes of the diseases in which they are found. The contrary opinion is perhaps generally held in this country, on the grounds that fungi generally are the proofs and consequences of decay, but not its causes; that in the various forms of tinea, a special condition of the skin and hairs appears necessary for the growth of the plant; and that in Tinea favosa (Favus), in particular, a marked feature of the disease occasionally is an hyper-secretion of epithelium, and exudation, owing to an hyperæmic cutis, before any trace of fungus can be found. Nevertheless, these arguments, strong as they are, seem to be overborne by the two grand facts that Tinea tondens, and Tinea favosa, can be communicated by transfer of the plant, and that the disease can be cured with the greatest readiness by the chemical agents which are most destructive to vegetable life. That a special nidus is necessary may very well be admitted by the partisans of this view, since, even in the case of epidemic agents, a predisposition is necessary, yet no one dreams of confounding the co-operating cause with the special and peculiar poison.

It may be desirable to recapitulate the diseases of the skin in which parasitic plants are found.

1. Tinea tondens, in which the Tricophyton tonsurans is present.
2. Tinea favosa, in which are present the Achorion Schonleinii, and the Puccinia favi (in some cases).
3. Mentagra, or Tinea mentagra, which exhibits the Microsporon mentagrophyta.
4. Pityriasis versicolor (Chloasma), in which the Microsporon furfur occurs.
5. Porrigo decalvans (Tinea achromatosa), in which the Microsporon Andouini is found.
6. Plica polonica, in which the Tricophyton tonsurans and Tricophyton sporuloides are present.

B. Cryptogamia on the Mucous Membrane.

The plants forming on mucous membranes, or in the contents of cavities lined by mucous membrane, are of less interest than those which grow on the skin, as in most cases they are decidedly only secondary. We shall merely enumerate them.

1. *Cryptococcus cerevisiæ.* Kutzing. (Syn. Torula cerevisiæ—the yeast-plant), in the bladder, stomach, intestines, &c.
2. *Merismopœdia ventriculi.* Robin. (Syn. Sarcina), in the stomach, intestines, &c.
3. *Leptothrix buccalis.* Robin. (Syn. Alga of the mouth.)
4. *Oscillaria (?) of the intestines.* Farre.
5. *Leptomitus urophilus*, Montagne. (An alga, described as forming in the urine. It has as yet been scarcely studied.)
6. *Leptomitus (?) Hannoverii.* Robin. (Alga found by Hannover in the pharynx and œsophagus.)
7. *Leptomitus (?) of the uterus.*
8. *Leptomitus of the uterine mucus.*
9. *Leptomitus of the eye.*
10. *Oidium albicans.* Robin. (Syn. Cryptogamia of diphtheritis and aphtha.) Aphtophyte (Gruby).
11. *Fungus of the lung.* Bennett.
12. *Fungus in the discharge of glanders.*

The algæ which so rapidly form in the unhealthy mucus, or in the exudation of the mouth, uterus, &c., which M. Robin refers doubtfully to the genus Leptomitus, are too well known to require comment.

The Cryptococcus cerevisiæ (the old Torula or yeast-plant) is well described. Robin does not consider the presence of sugar in the urine as essential.

Robin describes at some length the Sarcina ventriculi of Goodsir, which has been lately considered to be a Merismopœdia, but does not add anything new or important. He shares the now most prevalent opinion, that the Sarcina is perfectly innocuous.

In the description of the Oidium albicans (the alga of thrush and diphtheritis), we find nothing which can be added to the account given by Berg, Gruby, Vogel, Hannover, &c. This historical account of the observations made on the plant is imperfect.

Although we have touched on the part of M. Robin's book which is most useful to practical men, we cannot avoid noticing the elaborate account given of the fungi which form on the lower animals; the description of the Botrytis bassiana, the fungus of the Muscardine, the silkworm disease, is, in particular, extremely interesting.

In the appendix, M. Robin alludes to the so-called cholera fungi, and has copied into the atlas the plates given in the 'London Journal of Medicine.' He has nothing original to say on this point, but appears to agree with the opinions of Baly and Gull (or Sull, as M. Robin chooses to write the name of the latter distinguished physician).

Finally, we can recommend M. Robin's work, not only for the fulness and accuracy of his descriptions, but for the beauty and exactitude (as far as we can judge) of the figures in his atlas.

E. A. Parkes.

Review XII.

1. *A Bill intituled 'An Act to amend an Act passed in the Ninth Year of Her Majesty, for the Regulation of the Care and Treatment of Lunatics.'*
2. *A Bill intituled 'An Act to consolidate and amend the Laws for the Provision and Regulation of Lunatic Asylums for Counties and Boroughs, and for the Maintenance and Care of Pauper Lunatics, in England.'*
3. *A Bill intituled 'An Act for the Regulation of Proceedings under Commissions of Lunacy, and the Consolidation and Amendment of the Acts respecting Lunatics and their Estates.*

The introduction to Parliament of three new Lunacy Bills, containing many penal clauses for medical men who offend against their provisions, appears to justify one more examination into the principles and foundations of our knowledge of insanity.

The collective wisdom of the nation assembled at St. Stephen's makes enactments and statutes respecting the treatment of lunatics, and the disposal of their property, but attempts not to indicate the nature of that condition which shall be called lunacy. This tough subject is one with which Parliament law could never grapple; it has always been left to the judges, and has therefore, like other judge-made law, been developed from a remote and barbarous antiquity, until it has gradually assumed its present shape.

Were the records extant, it would not be less curious than instructive to trace the development of the common law on lunacy from that distant period when the ravings of the madman were listened to as the inspired wisdom of the gods, through those more enlightened and not very remote times when madness began to be recognised as a disease, though demoniac possession was still credited, and poor old women were judicially dismissed from life for the crime of witchcraft. From the decisions of Chief-Justice Hale we would willingly trace the progress of opinion to our own times, but the running to and fro over this ground has been so great, that the onward track is lost; so we must come at once to the highly important authoritative and decisive expression of the law as enunciated by all the judges, in answer to the interrogatories of the Peers after the trial of McNaughten. This consummation of common-law wisdom in lunacy was so perfect that it gave way on the first strain.* The fragments, however, are supposed to do service at the present time.

A great jurist tells us that judges make law for us as we make law for our dogs; they do not tell us what we must not do, but they permit us to do it, and then beat us; so that by observing and remembering what others are beaten for, we may contrive to escape the lash. But some unfortunate dogs are incapable of this amount of inductive ratiocination; therefore they are not responsible; therefore they must not be beaten. It has for some time, however, been professed that legislation ought to be guided

* Mr. Justice Williams' Charge on trial of William Frost.

by the supposed principles of *human* nature; and although some criminal and lunacy laws not very ancient now appear as dreadful and as inconsistent with human happiness as the surgical armamentarium of Ambrose Paré, we may be permitted to think that they also were constructed with the best intentions, according to the amount of knowledge possessed by the workmen of the period: unfortunately, they cannot with equal ease be securely lodged within the glass cases of museums; for in common law every new instrument is but a filing-down or a tinkering-up of some old one. Even these modifications are made in obedience to public opinion; for in a free country law is said to be the practical expression of the information and spirit of the age, though it cannot be denied that in administration it usually lags far behind its ruler and preceptor. But the advancing opinion of the many is formed by the knowledge of the few; 'tis a diffused warmth and light radiated from the altars of science; and a public duty is thus imposed upon the humblest acolyte, to take care that in his particular department of the service this light should be as steady and clear as the materials at his use will permit.

Medical men are and ever have been discoverers. They roam through the wide fields of science unrestrained by the ligatures of mouldy precedent and time-worn prejudice; on all subjects which they investigate with the untiring labour of love, their information and opinions are necessarily in advance of the age; still more in advance, therefore, must they be of the opinions current in tribunals which tardily represent those of the public, and ever reflect more of the past than of the present. Medical witnesses must therefore not expect to advance the rational administration of lunacy law, either by voluntarily airing their theories in the courts, or by permitting them to be drawn out for the amusement and profit of those hair-splitting and adroit word-fencers, the barristers. Theories which may bloom into the fruit of truth under the favouring shade of retired study, will inevitably wither, die, and be blown into the "outermost limbo" of nonentities, by the ungenial blasts of our courts of law. We would ourselves never venture in the Courts to speak of insanity otherwise than as farmers talk of fallows and crops and bullocks; for so long as a man keeps to what he positively knows, his position, even in the witness-box, is inexpugnable; but if he commences to show reasons, it is not certain that he will be able to prove that two and two make four.

No theory or explanation of insanity, therefore, will have a chance of vitality in the courts unless it is introduced there under the ægis of public opinion; to gain this protection it must be consistent with the reason and feelings of mankind, simple and uniform in its construction, and not liable to part amidships under any strain to which facts or sophistry may be able to subject it. Do those whose duty it is to lead opinion possess such a theory and explanation?

The only definition of insanity which will embrace all its forms appears to be this,—that it is a general term used to express that mental condition which is opposed to sanity. We see no objection to comprehending in this manner the delirium of drunkenness and of fever with the more recognised forms of insanity: these states are essentially forms of insanity, although differing in their cause and progress from others.

Probably such a definition would not be deemed wholly satisfactory and sufficient by learned cross-examining counsel, but no other could be offered either with exactitude or safety.

We apprehend that the strict meaning of the term definition is, fixing verbally the limits or boundaries of some subject or matter beyond which it does not exist; as we may say the land is defined by the sea, or the sea is defined by the land, though in coral districts the encroaching land, and in alluvial flats the encroaching sea, may sometimes leave the boundaries indistinct. If, however, it is not a definition, but a description, which the learned gentlemen require, in order that they may swoop down upon some unfortunate defect or redundancy, we will not, in lieu of such, propound any fascinating theories; we will honestly and fully detail all the observed phenomena, only we nourish a secret hope that before commencement the court will adjourn to refresh.

But suppose the definition accepted, the fair rejoinder would be, What is this condition of mind which defines insanity? what is sanity? Against this it would be puerile to seek refuge in an alternating definition. A short description must be hazarded. The following appears to us simple and compendious; one less liable to objection could scarcely, we think, be devised, although even this affords an opening to the perplexities of necessity and responsibility. Sanity, then, is that condition of mind which enables a man to discharge his duties to himself, his neighbour, and his God. The judges place sanity and responsibility of man in his knowledge of right and wrong; we place it in his ability to *do* the right, and to *avoid* the wrong. We do not believe that knowledge is always power, even over oneself. If it is not, how can it confer responsibility? But so long as we retain the power of *doing* or *avoiding*, we are truly answerable for our actions. When we lose this power, we become the puppets of circumstance; while we possess it, we are sane, moral agents; when it is gone, we are insane.

Here is the fundamental question of free agency, of voluntary action of conscience, the whole philosophy of ethics, which we must not pursue, but merely point to as the basis of mental pathology; for the laws of deviation cannot be determined until the laws of healthy action are established. Pathology does not precede physiology, although it may sometimes reflect back a ray into some dark corner of the parent science. A true theory of insanity cannot therefore precede, but must follow, and be auxiliary to, a true philosophy of the human mind; although to some extent the phenomena of insanity may serve to elucidate and to verify the deductions of philosophy, as the disturbing causes of planetary motion served to prove and to confirm the great laws to which that motion was subject.

Hence it follows, that the system of philosophy being given, its legitimate offspring in the theory of insanity may be inferred. Sensationalism will refer all cases of insanity to somatic disease; idealism will incline its disciples to suppose a perverted action of the soul; the ethical school of utilitarianism will place insanity and irresponsibility in want of knowledge of right and wrong; its opponents will place them in the want of power.

If the partial truth contained in these theories be mistaken for entire and universal truth, its pursuit, conducted with logical fidelity, will not fail to lead to conclusions, whose incorrectness and absurdity will be apparent.

The trail of either sensationalism or idealism thus followed, without lateral deviations, will conduct to those morasses of scepticism where "Strauss may shake hands with Diderot." In mental pathology, the one will lead to the repulsive doctrines of Heinroth, and the other to those of Mr. Atkinson and the psychology of Miss Martineau's cow.

A witty author says, that "in 1763, Bishop Berkeley destroyed this world in one volume octavo. And a few years later, Mr. Hume did the same for the world of mind; so that with all the desire of destruction remaining, there was nothing to exercise it upon." In a manner somewhat similar, the psychists have disallowed the influence of the brain in the production of insanity, and the somatists have repudiated the interference of the mind, so that between the two we see not what remains, except what Oken calls "the essence of nothing."

The only safe path in these difficult regions appears to be that of a discriminating eclecticism, which will permit the psychologist to recognise the instruction derived through the senses, and the light afforded by the reason, without forgetting the arbitrary power exercised over the conduct by the will and the emotions. This philosophy will also leave him at liberty to recognise the full instrumentality of the cerebral organ, although it will not forbid him to entertain the belief, that the servitude of the mind to its organ may be not dissimilar to that which the mighty genius of Aladdin's lamp paid to a paltry vessel for the convenient combustion of oil and cotton.

Most curious and instructive would it be to trace the opinions which, in various ages and countries, have been held upon the nature of insanity; most of them, indeed, containing a certain leaven of truth, but spoiled, obscured, and overgrown by the heterologous formations of systematic dogmatism.

It is said that the Medicean effigies of female beauty, which shows "what mind can make when Nature's self would fail," was constructed by selecting the perfect portions from imperfect forms, and joining them together in a consummate whole. If a similar operation could be accomplished with the theories of insanity, a true and complete system might be expected to result. Until the hour and the man have arrived for this work, it will be useful employment for more humble artists to sharpen and prepare the tools and rough-hew the block.

Most medical men entertain on this subject some theory or other of a character sufficiently decided and explicit; they find such a bond necessary to connect the groups of phenomena they observe; besides, at the present day, the bias of the medical mind is set strongly against empiricism, and indefatigably strives to found its practice upon reason. Here and elsewhere we use the term theory, not in its unfavourable sense of something speculative and visionary, but as a philosophical explanation of phenomena founded upon principles which have been established by independent evidence. In this sense, therefore, and for these reasons, medical men entertain theories of insanity. In Germany, men are thus divided into *psychists-somatists* and *somato-psychists:* in this country, we apprehend that the first and most philosophical of these sects has scarcely a place in the profession; the two latter sects divide us; a large proportion go still further than the somatists, and frame their notions according to the phrenological formulæ. Not that any considerable proportion of the profession are professed believers in the so-called science. Were this so, it would indicate that the facts upon which it is built are more evident and uniform than they are generally acknowledged to be. But men who have not time or opportunity to investigate the intricacies of metaphysics, on which, notwithstanding, they feel themselves compelled to form opinions, are apt to content themselves with the most plausible, the most simple, and the most familiar explanation which presents itself; and thus the phrenological theory comes to be accepted as a royal road to mental science. Sir James Mackintosh ascribes much of the long popularity of Epicurus "to that mental indolence which disposes the mind to rest in a simple system comprehended at a glance;" and we apprehend that much of the popularity of Gall's system must be attributed to the same cause.

It is astonishing with what tenacity a simple notion, whether it be true or false, adheres to the mind; let the staunchest materialist constantly hear that a certain place is haunted, and in vain will he endeavour to exorcise the ghost from his imagination, whenever he happens to be in that place. By a similar association of ideas, the ghost of phrenology intrudes itself whenever we think of the human mind. How difficult is it to realize the fact that "the great globe itself" is not surrounded by those zonular, equatorial, equinoctial, and other lines, which its pigmy representatives exhibit for the use of schools; sometimes even grave writers are caught in describing their existence;* and, though no believers in the art of phrenology, we not unfrequently discover ourselves tracing out organography on the bald pate of some subject placidly unconscious of the operation.

But in addition to this tinge which phrenology gives to the mind of the incredulous, a certain free and easy, but not very respectful, belief in its doctrines is sufficiently common; and in this diluted form they are accepted as the basis of opinions on the nature of insanity.

Very many persons, whose observations have led them to refuse any faith in the details of the system, yet profess to believe in the great regions. Thus Guislain says, "Si je regarde comme très problematiques les indications anatomiques que Gall a fournies, sur les compartments cérébraux, j'admets néanmoins avec une profonde conviction l'existence de certaines zones fonctionelles dans la masse encéphalique." (p. 173.) He admits, indeed, at p. 165, vol. iii., that "dans son application à la pathogénie mentale, le système phrénologique ne nous guide ni quant à ses détails, ni quant aux éléments anatomiques indiqués." The world is full of such half-and-half believers on all subjects, but phrenology cannot admit them within its pale: because, if it is anything, it is a science constructed from the detail of simple facts, as language is constructed from words and signs. However, these regionalists, who are numerous in the profession, admit the science far enough to accept its formula as the basis of their views on insanity.

Again, the medical profession forcibly influences its members to look upon man, in all his relations and bearings, physiologically; and of systems of mental philosophy, that of Gall is the only one worthy of the name, which founds itself exclusively upon bodily organization and function: add to this, that it explains the most occult phenomena with which we are

* See Miss Carpenter on Juvenile Delinquents, 'Athenæum,' March 19, 1853.

cognizant with a completeness and perspicacity most attractive to those whose attention and studies have been fixed upon the objective world, and who are for the most part unfamiliar with the trains of thought suggested by the subjective realities of existence. These considerations taken together will account for the prevalence of ultra-somatic notions of insanity among the members of the profession.

If phrenology be true, it will afford a solid resting-place for the foot weary with floundering in metaphysical quagmires. But order and simplicity are not always synonymous with truthfulness and reality; the olla-podrida of the metaphysicians contains real nutriment, while the regular spread of the phrenologists may be but a Barmecede feast after all; the apparently solid and orderly arrangements of phrenology may be but a mirage, "the baseless fabric of a vision," while out of the chaos of metaphysics the divine force of genius may be destined to evoke a real world.

We have no intention to enter, in this place, upon a formal description of this subject; we have for many years carefully investigated the facts of the so-called science, as they presented themselves to us, not in the spirit of hostile criticism, but in that of a student willing and wishing to believe. The opinions we have been compelled to form may briefly thus be stated. In healthy individuals a fair-sized and symmetrical head appears to be the result of healthy cerebral growth developing itself *equally in every direction*, under the most favourable circumstances. These circumstances are also, in the great majority of instances, favourable to the most perfect development of the intellectual and moral faculties of man. In the majority of instances, therefore, a well-shaped head will be coincident with a well-developed mind: but it will require observations collected with more discrimination, and recorded with more accuracy, than any which the phrenologists can at present adduce, to show that this coincidence takes place more frequently than a similar one between the shape of the countenance, of the hand, or of the human figure, and that of the mind. Long before Gall's time, no less a person than Lord Verulam observed that crooked persons were generally even with nature, by the crookedness of their dispositions.

This view of the matter may, therefore, be applied to deviations from true symmetrical shape: thus, from hereditary tendency, or the influence of circumstances, a man's bodily frame and organization may develop themselves in the lateral mould, or that best adapted for strength and resistance; the chest may become broad and capacious, the bones thick in proportion to their length, the hands short and muscular, and the cerebral organ will also itself be developed laterally. The same circumstances which tend to produce this type of physical man, tend also to form the corresponding mental character; with vigorous appetites, great strength of will, and of the pathematic functions of the mind. Vigorous functions are not usually easily moved or highly sensitive, therefore in this type of character the intellect is often slow, it is frequently also untrained by exercise, the other portions of the character inducing this kind of individual to avoid study, and to engage in the active turmoil of life. Reverse all the conditions, and we have the highly intellectual man, with emotions quick and vivid rather than strong, with elongated hands and frame, and an elongated brain. In these co-existences of certain physical types of human form with certain mental types of human character, we see ordinary examples of coincidence of effects; any connexion more intimate we have hitherto been unable to recognise. In the dead house we have not unfrequently found circumscribed atrophy of the convolutions, which had not been pointed out during life by any decay of the supposed cerebral function, to which the atrophied convolutions should have subserved; and in far more numerous instances we have in vain sought for the atrophy, hypertrophy, anæmia, or hyperæmia of particular convolutions, in which one or other of these conditions ought to have existed in correspondence with some remarkable and pronounced mental affection. We have thus tried the truth of the doctrine in both scales of the inductive balance; and both by the method of agreement and by the method of difference, it was found wanting.

The accomplished advocates of phrenology would doubtless be able to shift off the burden of these objections, as they did when the skull of Swift was examined at Dublin, and found to be very wrong; they escaped by affirming that the shape of the poor Dean's head had been greatly changed by his long insanity. But if in argument they slip through the ordinary rules of logic with such adroitness, they must not be surprised if their adversaries sometimes resort to the tropes of rhetoric, and use a little vituperative dirt, as fishermen take gravel in their hands to lay hold of eels.

The doctrines of phrenology are irreconcilable with the received doctrines of the responsibility of man for his actions; and the witness who, in a court of law, assumes this mental philosophy to be the true one, assumes the folly, injustice, and worthlessness of the whole judicial system. The judge might say to him, "If you are right, my occupation is gone: it would be equally useless and unjust to hang that man for the consequences produced by the large cerebral organ behind his ear; do you take him, and endeavour by scientific treatment to remove the cerebral substance exuberant in that locality to the part where conscience and benevolence dwell. As for me, like Prospero, I will bury my wig and drown my commission; the criminal law is a delusion and a snare; all right practice in law, polity, and religion, consists in scientifically operating on the organs of the brain."

In Simpson's 'Criminal Jurisprudence,' the doctrines of phrenology are applied in an honest and faithful spirit, and with a pitiless logic, to prove that the responsibility of a murderer is no more than the responsibility of a man with the gout:

> "The doctrine of responsibility, which appears to me to be alone consistent with reason, religion, and morality, is simply this,—that so far from the Creator having sent into the world some beings who are responsible, and others who are exempt from responsibility, there is in fact no exception whatever; and that every human being is alike responsible—responsible (according to the degree of his departure, either in mind or body, from that degree of sanity necessary to the proper discharge of his social duties) *to undergo the painful but benevolent treatment which is requisite for his cure.*"

This is the responsibility of the phrenologists, who appear to know that it is often easier to explain away a thing than a name, but it is not the responsibility of the common law, and is in fact quite a different thing. If, therefore, the opinions of medical men on subjects of mental science, and on insanity, have been formed, if not in accordance with the whole details, at least with a leaning towards the doctrines of that system which results in this kind of responsibility, can we be surprised if they have found themselves in direct antagonism with the great functionaries of the law, the very essence of whose office assumes that man is answerable for, and punishable for, his actions in quite a different sense. We believe in the common-law responsibility of man, though we do not use it as an argument against phrenology. If the latter be true, it will prevail, and when we find two truths apparently contradictory to each other, we can only, as moral observers, wait patiently for a new apocalypse to reconcile them. This corollary, however, we will venture to educe; that if judges, lawyers, and medical witnesses, could form similar, or at least not antagonistic, opinions on general questions of mental science, responsibility, free-will, and the like, criminal lunacy trials would become much more satisfactory than they have hitherto been.

The next important modification of the lunatic theory is that which, without in any degree committing those who entertain it to the phrenological doctrines, refers all cases of insanity to some disease of the brain. It will be observed that this theory differs from the former one, not only in excluding the element of phrenology, but also in admitting that of *constant disease.* Insanity, according to the former, may sometimes be occasioned by unequal development of the organs, and consequent loss of balance without disease; according to the latter, an abnormal condition in the solids or fluids of the brain is essential. This opinion is so well supported by facts, by the authority of writers for whom we entertain the highest respect, and undoubtedly offers a true explanation in so many instances, that we question its universal truth with cautious reluctance. We doubt whether it can be either satisfactorily established or refuted, until we know something of the relationship existing between mind and matter. The physical and vital laws to which the cerebral organ is subjected, and the laws of mental action, come within the reach of our investigating faculties; but of the nature of their union we are entirely ignorant: their ultimate relation is "the adamantine wall against which the human intellect in vain beats itself."*

It is in vain that we attempt to solve this problem as it bears upon insanity, by the records of post-mortem examinations. Our powers of observation do not always enable us to discover any alteration in the brain, even when insanity or death has been occasioned by the most obvious physical causes; by blows, insolation, or poisoned blood; and the testimony of most experienced pathologists goes to prove that the brains of insane persons present no uniform appearances dissimilar to those of persons dying from other chronic diseases, or from old age. This was Esquirol's opinion. Pinel thought that the anatomical lesions which he found, were merely the results of the malady, not, we suppose, as a solid lung is the direct result of inflammation; but as the wasting of a fakir's arm, which he has held behind him without using for a dozen years, is the indirect result of his mental condition.

It may be urged that if insanity occasioned by known physical causes leaves behind it no physical lesions with which we can identify it, but the

* Whewell.

existence of which lesions we are bound by the laws of the mind to admit; so also insanity produced by moral causes may invariably leave behind it cerebral lesions, which we are unable to detect on account of the imperfection of our senses.

Our want of necroscopic knowledge on these points, therefore, scarcely tells against the opinion that insanity is always a disease of the brain. There is a want of evidence on both sides. The only facts which appear decidedly opposed to the doctrine, are those not very unfrequent cases of insanity which, without any cause external to the mind, are gradually developed by the overgrowth of some predominant passion; as pride, fear, envy, ill-temper, vanity, generosity, or even the religious sentiment. Such cases are not unfamiliar to physicians experienced in the treatment of mental disorders. In such cases it may be possible to refer to some distant period, at which the patient was undoubtedly sane, but the mental change has been so gradual and insidious, that it is impossible to say when the insanity commenced. As in Bayles' ingenious paradox of how many grains of barley make a heap, or how many drops of wine will make a man drunk; we may, if incautious, be compelled to the absurdity of allowing that one small increment of emotion can make the difference between sanity and insanity, between a healthy and a diseased brain.

In such cases it is as impossible to fix the point at which sanity merged into insanity, as it would be to decide the precise time when a man has come under the influence of some ridiculous personal habit. Indeed, these cases of insanity may be looked upon as instances of mental habits which have passed beyond the influence of the reason and the control of the will.

An individual or his friends may perfectly remember the time when he was entirely free from some personal or mental habit. The time may also be well remembered when the tendency to the formation of the habit existed, but when no difficulty and but little pain was experienced in checking its action and forbidding its indulgence, by exercising the coercive power of the will. And a third period may be easily fixed upon, when the man has become the abject slave of the habit—locked into it and carrying it about with him as a Chinaman does his neck-stocks. Contrary to the opinion of the Rev. Sidney Smith, it appears to us extremely difficult to fix the period when the habit is formed—whether, in the case of a personal habit, for example, it may be said to have established itself at the fourth, the fortieth, or the four thousandth repetition. This author has also perverted the habitual use of the term, in applying it to those deep marks left in the mind by the rude and rapid inroads of emotion, so humorously illustrated by him in the formation of Dr. Isaac Barrow's habitual dislike of dogs; proceeding from a tussle between the mathematician and a mastiff, to which it is added that "the adventure gave the doctor a strong habitual aversion to dogs, and I dare say, if the truth were known, fixed in the dog's mind a still stronger aversion to doctors."

This application of the term does not appear to be in accordance with its conventional usage. Locke, referring to confidence in speaking, says "which power or ability in man of doing anything when it has been acquired by *frequent* doing the same thing, is that idea we name *habit;* when it is froward and ready on every occasion to break into action, we call it *disposition.*" (b. ii. c. 22.) Butler, who uses the term in a wide sense, and speaks of habits of virtue, moral and religious habits, &c., does not extend it to comprehend single but permanent impressions, like Dr. Barrow's canine aversion. He uses it in a manner consistent with our views of its bearing on pathology; thus:

"For distinctiveness, we may consider habits as belonging to the body or the mind, and the latter will be explained by the former. Under the former are comprehended all bodily activities or motions, whether graceful or unbecoming, which are owing to use; under the latter, general habits of life and conduct, such as those of obedience and submission to authority, or to any particular person; those of veracity, justice, and charity: those of attention, industry, self-government, envy, revenge. And habits of this kind seem produced by repeated acts as well as the former." (c. v.)

Whether the alteration of temper and character produced by the habitual despotism of some particular emotion or set of emotions be capable of causing insanity, can be determined only by observation of instances. According to our own experience, insanity thus produced is by no means unfrequent; and many cases may be cited, from Esquirol and other authors, which are clearly deducible from defective education, and the compelling force of some bad habit of mind. We believe that a sound, moral, and religious education would not only prevent an immense amount of crime, but also a great proportion of that insanity, whose prevalence stigmatizes an uneducated civilization. Like Kavanagh's schoolmaster, we may fairly speculate "on what Lady Macbeth might have been, if her energies had been properly directed."

Some of the meaner emotions are peculiarly liable, like dry-rot, to destroy the stability of mental health. We have observed excessive vanity, selfish timidity, propensities to anger, envy, and covetousness terminating in this manner, by overgrowing and preventing the free actions of the other faculties. Habits of mind of a passive nature lead more directly than active ones to this melancholy result; thus the disposition to be unduly moved by the trivial annoyances of life, if it causes the resistance and the habit of peevishness, never exceeds a certain limit, which, although it may render the subject of it very ineligible for a club or a mess-room, does not entitle him to the benefits of an asylum; whereas, if the same disposition exists without causing resistance, but occasioning only disquiet and low spirits, it is in a fair way to lead its victim to helpless dejection, melancholy, and insanity.

Our active habits are bounded and limited by the external resistance they meet with; our passive habits are unlimited, and, like the tub of the Danaides, can never be filled. It should be observed, that excessive emotions thus overgrowing and dominating the mind, cannot correctly be said to *cause* insanity, as sudden fright or grief will cause an attack of mania. Properly speaking, they *constitute* the disease. Thus, checked perspiration causes rheumatic fever; but an excessive quantity of blood, or an accumulation of fat, constitute abnormal conditions of the human frame, which impede the due performance of the functions, and are practically recognised as serious diseases.

If, therefore, it be admitted that the emotions and passions, gradually acquiring an inordinate power, and carried to an undue extent, are capable of constituting insanity, it is clear either that insanity may exist without disease of the brain, or that disease of the brain must exist whenever any of the emotions are in excess. The latter assumption resolves itself into an absurdity, or into a mere quibble, on the meaning of the word disease.

Having thus briefly referred to the general principles of the somatic theories, we cannot at the present time examine the subdivisions or separate articles of these creeds, or discuss whether anæmia or hyperæmia of the cortical substance, or meninges, or poisoned blood, or peripheral irritation, or "want of tone," are the most potent causes. We must proceed to give such consideration as our limits will permit, to the most important psychical views on this question.

John Charles Bucknill.

(*To be continued.*)

Review XIII.

Norsk Magazin for Lægevidenskaben. Heft 1—10. 1852.
Norse Magazine for Medical Science. Parts 1 to 10.—*Christiania*, 1852.

This medical journal is, we believe, the only one published in Norway, and appears in monthly parts, under the sanction of the Medical Society of Christiania. Each part contains some original articles, followed by extracts from various Continental and British journals, and concludes by a résumé of the proceedings of the monthly meetings of the Medical Society. Notices are likewise given of all changes in the laws affecting the medical profession in Norway; and to judge from their number and variety, the healing art is deemed of more importance to the welfare of the state in Scandinavia than in our own land. We purpose here briefly to analyze the more important original communications in this journal, as they afford much insight into the diseases and the sanitary condition of the remote portion of Europe to which they relate. Some of these papers, however, will hardly admit of satisfactory condensation, and there is a large amount of interesting matter contained in these numbers, which must, for want of space, be omitted in our notice.

Dr. Danielsen's report of the cases that have been treated in Lungegaard's Hospital in Bergen, during the years 1850 and 1851 (Nos. I. and VII.), is an able document, not only for the diligence with which the various diseases there occurring have been observed, but for the new modes of treatment which have been cautiously yet perseveringly pursued.

The hospital in question is principally devoted to the reception of that form of elephantiasis termed in Norway "Spedalskhed," and which occurs under the name of "liktraa" in Iceland, and in more southern climes is known as Elephantiasis Græcorum. As this malady has been more than once described at considerable length, in the pages of this Review,* we shall here only notice the further researches of Dr. Danielsen on this interesting subject, and the novel modes of treatment that he has pursued with varied success.

In the year 1850, 95 patients were treated in this hospital, 70 of whom laboured under the disease in question. Fifty of these patients were affected

* See review of Danielsen and Boeck on Spedalkshed, in vol. v.

with elephantiasis-tuberculosa, 11 had elephantiasis-anæsthetos, and 9 suffered from both forms of the malady. The remaining 25 were cases of syphilis, or of ordinary skin diseases, and most of these left the hospital cured. So favourable a result was, however, not obtained in regard to the patients affected with spedalskhed. Out of the whole 70 cases, only one was cured, and in him the malady can hardly be said to have developed itself; 3 left the hospital improved in health, 5 more went out without having experienced the least amendment of their condition, and 4 died. Many of the patients, it appears, reside in the hospital at their own expense, and are consequently obliged to leave it when their scanty means are exhausted. Of the 3 who went out improved in health, 1 was a girl of 21 years of age, affected with hereditary spedalskhed. After ten months of treatment, the tuberculous masses, which covered, more or less, the whole cutaneous surface of the body, had almost entirely disappeared; but as she still exhibited other symptoms, which showed that the morbid diathesis was not entirely eradicated, Dr. Danielsen did not place her on the list of cures. The second, a young man of 20, had the disease to such a degree, that every part of his body was covered with tuberculous masses, which in many places had united together, so that the surface was enormously swollen and disfigured. In seven months, these symptoms had, under appropriate treatment, almost entirely disappeared; he left the hospital to all appearance well, and remained at home for six months after in good health. In December, 1850, he made a voyage in an open boat to Bergen, and suffered much on that occasion from exposure to cold; the disease again broke out, and he re-entered the hospital.

Of the 4 who died, only 1 can be said to have fallen a victim to the disease itself. In this individual a great many of the subcutaneous tubercles softened in succession, and formed large ulcerating surfaces, which gradually exhausted his strength. Two others died of chronic diarrhœa, which came on about a fortnight after the protracted employment of tartrate of antimony in gradually increased doses; but no pustular eruption was found on the mucous membrane of the intestines on dissection. The fourth patient died from ulceration of the mucous membrane of the bowels; and in all, the tuberculated form of elephantiasis existed, though in two it was complicated with anæsthesia of the extremities. The first of these two individuals had been for four years a sufferer from the malady in its tubercular form, and large tracts of the skin were infiltrated with tuberculous matter. A few days before his reception into the hospital, he began to complain of intense pain in the right eyeball, with diminished vision and some intolerance of light. The other eye was soon after affected in like manner; and about the same time, severe pains were felt in the hands and feet, extending to the arms and legs, with excessive sensibility of the skin of these parts. Antiphlogistic treatment relieved these symptoms for a time, but the affection of the eyes returned with increased violence, and the intolerance of light became extreme. The morbid sensibility of the extremities now disappeared, and was replaced by complete anæsthesia of these parts. Death at length ensued from softening and ulceration of the tuberculous masses beneath the skin. On examination of the head, it was found that a thick, firm exudation existed in the arachnoid membrane, directly over and pressing upon the decussation of the optic nerves. The ulnar and tibial nerves were carefully dissected out, and in certain parts of their course they were observed to be swollen and hard to the touch, and the sheaths of the nerves were of a reddish colour from the distended bloodvessels they contained. On making a transverse section of these nerves, it was observed that here and there yellowish-white tuberculous masses were infiltrated between the nerve-tubes, by which the size of the nerve was considerably augmented. Dr. Danielsen observes that amaurosis, which occurred in this case for some time before death, has been found, by Dr. Turk of Vienna, to arise often from a chronic-exudative process of inflammation, by which the mass of the optic nerve is infiltrated, compressed, and rendered unserviceable. Here, however, the nerve was unaltered, but the deposit on the arachnoid probably compressed it, and produced the same effect.

In the second case pain was felt in the right ear, and the hearing on that side was totally lost. On dissection, a neuroma was found situated on the right auditory nerve, immediately before it enters the temporal bone, and compressing the fibres of the nerve so as to reduce them to an atrophied condition. The same appearances as before described were observed in the nerves of the extremities.

Dr. Danielsen believes that where anæsthesia of the extremities comes on in the course of spedalskhed, it is the result of inflammation of the sheaths of the nerves, indicated by the excessive sensibility of the skin, and this is followed by the deposit of the whitish yellow albuminous matter between the fibrils of the nerves, compressing them, and at length producing total loss of feeling in the parts to which the nerves are distributed. When, as sometimes happened, the cutaneous nerves were inflamed in this manner, they could be felt as hardened cords beneath the skin; and where there was hyperæsthesia of the extremities, as for instance of the hands, the ulnar nerve at the elbow was so extremely sensible, that the patient almost fainted with pain when it was slightly compressed. Dr. Danielsen regards these symptoms as of great importance in a therapeutic point of view, as the favourable moment for early and effective treatment soon passes away; and the attention of the practitioner should be sedulously directed to the subduing of the inflammation, so that the deposit of albuminous matter in the nerves, and the consequent anæsthesia may not be produced. For this purpose local treatment by cupping, &c., is absolutely necessary, and the results obtained by these means have been very encouraging. Local treatment has, however, no influence on the morbid diathesis which constitutes the disease.

Dr. Danielsen has satisfied himself that in spedalskhed there is an excess of albumen and fibrin in the blood, and against this as the *materies morbi*, his treatment has been chiefly directed. With this view the diet of the patient was restricted chiefly to vegetables; and to excite the secretions of the skin, steam-baths were given every second day. Internally, phosphoric acid, tartrate of antimony, iodide of potass, chloride of gold, and chlorine, were given, with variable results. Phosphoric acid, largely diluted with water, produced no good effects, though its use was long persevered in. Tartrate of antimony, especially when it was had recourse to in an early stage of the malady, diminished and rendered more regular the superficial circulation, but when long employed, it caused, almost invariably, obstinate diarrhœa. During its use, it was always remarked that the tubercles of the skin diminished in size, and became of a paler colour. Iodide of potass was given chiefly in the tubercular form of elephantiasis; iodide of iron was principally used in the anæsthetic variety, and no medicines produced results so favourable as these.

"Where the nodose (tuberculated) form exists to a high degree, and the system has suffered for years from the altered condition of the blood, the iodide of potass must be given with caution. It is liable, after having been administered for several weeks in succession, to cause great congestion of the skin, which becomes then of a deep crimson colour, swells, and is very painful; and if you then pass the hand over the surface of the patient's body, you feel, deep below the skin, many hard masses of various dimensions, which seem to have been formed within a day or two. The older tuberculous masses become redder, and swell up, and a general febrile exacerbation accompanies these symptoms. Great pain is now felt along the course of the ulnar and tibial nerves, extending downwards to the fingers and toes. The above-mentioned nerves are found on examination to be hard, swelled, and extremely sensible. The eyes are often much affected, congestion of the conjunctiva, or exudation of lymph into the pupil, frequently occurs. All these symptoms, however, rapidly disappear if nitre be given in large doses (gr. xv. ad ʒss) every second or third hour, and at the same time blood should be taken by cupping. Upon the cessation of these untoward symptoms, it is generally found that the tubercular masses have greatly diminished in volume, that the skin has become soft and elastic, and more nearly approaches its normal condition. Occasionally the iodide of potassium is well borne for two or three months together, and during this time the tubercles decrease and assume a bluish colour, while most profuse perspiration ensues, and continues for three or four weeks, without materially impairing the patient's strength, and during this time the amendment advances as before."

The iodide of iron acts more slowly; it is particularly useful in those cases where anæsthesia is present.

Dr. Danielsen often allows an interval of two or three months to elapse, and then resumes his former mode of treatment; and he has remarked that the second course of the iodides of potass or of iron is less frequently injurious, and produces more beneficial effects than the first.

The chloride of gold was tried both in 1850 and 1851, but with completely negative results. In 1851,* 85 patients affected with spedalskhed were admitted into the hospital; 57 of these were males, and 25 females. 52 of the whole had the tubercular form of the disease, 11 the anæsthetic, 8 the mixed form, 13 had maculæ-elephantoideæ, and 1 had only the prodromata of the disorder. 3 were cured, 3 more were improved in health, 18 left the hospital in the same condition as when they entered it, and 2 died. All the 3 cases that were cured, presented the disease in its earliest stage. One of these patients was treated with arsenic in small doses; but Dr. Danielsen does not speak favourably of this potent mineral poison as a remedy in spedalskhed. Indeed, in advanced cases, he considers it to be positively injurious, producing, gradually, its poisonous effects without destroying the disposition to elephantiasis. In the tubercular form, however, where the deposits in the skin have a dark brown or black colour (morphea nigri), the employment of arsenic is often beneficial.

The only new remedy tried extensively in 1851, seems to have been oxalic acid. Dr. Danielsen was led to its employment by its asserted

* See No. 8.

effect on the constituents of the blood, and on the process of digestion. We are not yet informed of the results produced by this remedy, as regards the cure or amelioration of the disorder for which it was administered; but Dr. Danielsen's experience of it has enabled him to correct many of the erroneous ideas entertained of its poisonous properties. It has generally been stated, that in doses of ʒij. and upwards, its poisonous effects are rapidly produced on the human system, and that it causes then paralysis of the heart, with almost immediate death.*

Dr. Danielsen dissolved pure oxalic acid in $10\frac{1}{2}$ parts of water, and began his treatment by administering ten drops of this solution in a table-spoonful of water every two hours. Every five days, five drops more of this solution were administered, until 150 drops were taken every second hour. One patient took this large quantity without the addition of any water for upwards of a month, and not till after that time did she complain of any burning sensation in the fauces, while this was speedily removed by her taking the medicine in a little gruel. Eleven patients, aged from 10 to 50 years, were treated in 1851 with oxalic acid, and no particular effects were observed in any of them until they took 40 or 50 drops, or 7 to 8 grains of pure oxalic acid, every two hours; when in some the pulse sank down to seventy beats per minute; but they complained of no uneasy sensations. One girl, aged 15, continued the use of the remedy for eleven months, and during the last three months of that period, she took daily ʒij. to ʒijss. of pure oxalic acid, without the slightest injurious effects. During this time, the appetite continued good, and the bowels were perfectly regular, while her general health was much improved. The great affinity of oxalic acid for lime, and the consequent formation of a compound insoluble in water, made Dr. Danielsen fear that such a combination might be formed within the body, and that thus the operation of the remedy might be rendered abortive. He therefore submitted to careful analysis the whole of the urine passed during three days, by the patient who was taking daily ʒijss. of pure oxalic acid. During the three days that the urine was collected, this patient had taken very nearly an ounce of the acid, but only two grains could be discovered in the urine, or about $\frac{1}{103}$ parts of the whole. Nor was the acid passed in any state of combination by stool, for the fæces of two days yielded hardly one grain of oxalic acid.

Dr. Danielsen confesses that the progress hitherto made in the treatment of spedalskhed is far from encouraging; but he is evidently pursuing the right course of careful observation and experiment; and we may hope that this will eventually be crowned with success. The most serious obstacle to effecting a perfect cure in these cases of spedalskhed, is the length of time required for the employment of remedies; most of the patients are poor, their scanty means of payment are exhausted, and they must then return to their homes, as the institution does not appear to possess funds for their gratuitous support. We rejoice, however, to learn that the Stor-

* We believe that half an ounce is the smallest dose that is known to have proved fatal to the human subject; but the case related by Dr. Scott, of Cupar (Edinburgh Medical and Surgical Journal, vol. xxiv. p. 67), where a girl swallowed about a wineglassful or nearly two drachms of oxalic acid, proves that even in that dose it acts most powerfully on the system. It is, however, by no means certain that a larger quantity was not swallowed in this instance. We are inclined to doubt the truth of the assertion of Henke and Most, that oxalic acid is most dangerous when it is diluted with large quantities of water. When concentrated, it acts first as an irritant poison; when much diluted, its effects are exhibited on the nervous system, but not till after some time.

thing of 1850 voted funds for the building and support of a large hospital at Bergen.

There is an able article in the first number by one of the editors of this journal, Dr. Faye, on the relative value of the separate and of the silent systems (Auburn and Philadelphia systems) of prison discipline. Dr. Faye has studied the operation of these systems in England and elsewhere, and he is evidently inclined to prefer the separate or Philadelphia plan to that where silence is enforced; but the prisoners work in a common room. He is of opinion, however, that term of seclusion should not be too much extended; that no exertion should be spared to excite during this period the moral and religious feelings of the criminal; and above all, that means should be devised for enabling the reformed prisoner to regain, in part at least, the social position he has forfeited. We have not space for any lengthened analysis of this sensible paper, but at its conclusion, we learn what has been done in the north of Europe towards a reform of prison discipline in accordance with these views.

In Denmark, the American system was adopted as a principle in 1842; and it was determined that the separated plan of prison discipline should be followed for a brief term of punishment, and the congregated but silent system for a longer period. The new prison at Horsens is to be fitted up on the above plan, but our author does not know if it be already in operation.

In Sweden, the prisons for the different provinces (*Länsfaengsler*) have been provided with cells for the separate system, and this plan of discipline will be chiefly adopted for the suspected criminals (*Ransaknings-fangar*), so as to save them from the deleterious society of the older and more hardened criminals.

In Norway, a different mode of proceeding has been adopted, and a large prison was erected at Christiania, on the separate system, in 1852. The shortest period of solitary confinement in the building is to be three months, the longest four years.

In the second and fifth numbers of the journal, Professor W. Boeck presents us with a well-written retrospect of the more remarkable cases of skin-disease, and of their treatment, in the two wards allotted for this purpose in the Christiania hospital. Dr. Boeck adheres to Cazenave's nomenclature and arrangement, though he confesses that the natural system, founded on anatomy and physiology, and promulgated by Erasmus Wilson in London, and by Hebra in Vienna, is the more scientific, and will, in the end, be generally adopted. In scabies, for example, the insufficiency of the old artificial system is very apparent. Scabies is there placed amongst the vesicular diseases, because the acarus scabiei most frequently produces vesicles; but the disease may exist without a single vesicle, and papulæ, impetiginous pustules, ecthyma, and furunculi, may also be present. Dr. Boeck details a curious case of scabies in a young girl of 15, where the inner surfaces of the hands and fingers were densely covered with thick, whitish-grey, shining crusts, so closely agglomerated, that they could be peeled off like a piece of bark. The same condition of the skin was observed on the soles of the feet, on the nates, the elbows, and on portions of the back. Flat, brownish-red scabs were observed on the shin-bones; on the upper surface of the arms there were many vesicles; and here and there on the extremities a few pustules. The disease had lasted for about two years. When the patient was put into a warm bath, the crusts fell off, and the moist, red, uneven skin appeared. On examining these crusts with the microscope, they were found to consist almost entirely of sarcoptes scabiei; there were entire insects, with their eggs, shells, and excrements; but only one living specimen could be detected. The malady was highly contagious, and the nurses and the patients inhabiting the same room were all more or less affected. After warm baths had been employed for some time to loosen the crusts, the Vienna ointment (pulv. cretæ alb. ℥ss., flore. sulph. et picis liquid āā ʒvj., sapon. virid et axungi. āā ℥jss.) was applied to the whole body. The crusts now rapidly disappeared; but about three weeks afterwards, a pustular eruption appeared over the whole body, and even on the face, while around these pustules small crusts were formed like those before described. On submitting them to the microscope, it was found that they consisted, externally, of a shining layer of epidermis-cells, and beneath this was a grey-coloured layer of the remnants of sarcoptes scabiei. A further use of the Vienna ointment, with warm baths, freed the patient entirely from this disgusting malady. The rest of this long article is occupied by a clinical disquisition on favus and its varieties, and on the necessity of microscopical examination to determine the true character of these diseases of the hairy scalp. Dr. Boeck has always found true favus to be characterized by a fungous parasitic growth, by the sporules of which he believes that the malady is propagated. He considers the eradication of the diseased hairs to be an essential requisite for effecting a permanent cure.

In the continuation of his clinical remarks in the fifth number, Dr. Boeck treats chiefly of gonorrhœa and syphilis. He believes that radesyge, a disorder so frequent in Norway, and more than once described in this journal, is nothing more than an after product (*Af-födning*) of syphilis; and he maintains that the disorder was first introduced into the country by the sailors of a Russian ship-of-war, about the year 1709. The historical details he has collected to corroborate this opinion are extremely curious, and we do not remember to have seen them alluded to in any of the numerous works that have been published on radesyge.

From a report on typhoid fever in the Christiania hospital, by J. W. Randers, contained in the third number of the journal, we learn that the well-known lesion of the glands of Peyer is generally present in this disease as it occurs in Norway; but that cases of well-marked typhoid fever are not unfrequently met with there, where these glands are totally unaffected. In the same number, Professor Boeck details an interesting case of the cure of aneurism by compression, after the method practised by Mr. O. B. Bellingham and others. After trying to effect adequate compression of the femoral artery by the ordinary means without success, owing to the pain thus produced, he at length obtained the apparatus proposed by Mr. Bellingham, and found it to answer his most sanguine expectations. A small stream of blood was allowed to flow through the aneurism; the pulsation in the tumour diminished, first in the upper part, and then in the lower, when the patient was attacked about the tenth day with inflammation of the right lung, and the compress was laid aside on the twelfth day, at his urgent request. At this time, pulsation could still be distinctly felt in the lower part of the tumour; but on examination the following day, it could not be detected; nor was there any movement in the femoral artery for several inches above the tumour, or in the tibial arteries below. Three days after, the compress was again applied, and in four days more the cure seemed to be completed. Tubercular disease of the lungs now appeared, and the patient died, after the lapse of six weeks from that time. A careful examination of the limb was made after the arteries had been previously injected. No collateral circulation had been established; the smaller arteries were not dilated, but the circulation had been carried on through the femoral artery by a very narrow canal, which passed through the much diminished aneurismal swelling.

A partially successful case of Cæsarean section is recorded in No. 6 by Dr. Faye. The pelvic diameter was narrowed by two large fibrous tumours depending by long stalks from the sacro-iliac fossa on either side into the cavity of the pelvis, and reducing the space for the extraction of the infant to little more than an inch. Each of these tumours was as large as the head of a seven-months' fœtus. The operation was delayed for a day or two, in the hope that some softening or displacement of these obstacles might ensue, and because the pains were not severe, but at length it was determined to extract by the Cæsarean section, as the child was still living, and the os uteri could not be reached, for it was pushed high up towards the brim of the pelvis by the adjacent tumours. Even if the os uteri could have been reached, the extraction of the child piecemeal would have been impracticable through the narrow space of one inch that was available. The operation itself presented no difficulties, chloroform was used, and a living healthy child was brought into the world. Symptoms of peritonitis set-in on the following day, and the patient died thirty hours after the operation. Dr. Faye thinks that it would have been impossible to push the tumours upwards into the cavity of the abdomen, or to reach the neck of the tumour from below, so as to be able to excise them with safety. Considerable hæmorrhage ensued after the placenta was removed, but it was immediately controlled by compression of the aorta through the abdominal parietes. The advantages of this mode of arresting uterine hæmorrhage have been, as Dr. Faye remarks, much discussed of late, both at Paris and at Brussels. It has been maintained that the hæmorrhage arises chiefly from the uterine veins, and that the vena cava is thus emptied by a reflux of the blood it contains, while the "arteriæ ovaricæ" may still continue to supply blood to the uterus, as they originate above the point where compression is generally applied. As the origin of these arteries is on the right side, generally from the arteria renalis, and on the left from the aorta itself at about the same level, Dr. Faye has always thought it necessary to compress the aorta some distance above the umbilicus, instead of directly over that point.

The Cæsarean section has been performed in Norway four times within the last ten years. In two of these cases it was had recourse to in the last extremity, and after the head of the child had been already perforated; and in both these instances the mothers expired shortly after the operation, completely worn out by their previous sufferings. In the third case, and in this present instance, the children were saved, but the mothers perished.

In the same number (6) we find given at full length the extraordinary trial for poisoning with lobelia inflata, which took place a year or two ago at the Cumberland assizes, before Judge Patteson. Our readers will probably recollect the humiliating treatment experienced by the medical witnesses, from the judge and from the counsel for the defendant (Serjeant Wilkins); and the incapacity of a jury of Cumberland farmers to decide such a question is here strongly insisted upon by the reviewer.

The seventh number contains some valuable remarks on puerperal fever, by Dr. Faye. He adopts, in its fullest signification, the modern doctrine, that puerperal fever in all its forms is to be regarded as a phlebitis, or an inflammation of an unhealthy character, and insists strongly on the highly contagious nature of the disease. In 1850 an epidemic of puerperal fever prevailed in the lying-in wards of the Christiania hospital, but this abated rapidly after careful prophylactic measures against contagion were adopted. The utmost cleanliness on the part of the attendants was enjoined, the hands of the operators were constantly washed with chloride of lime, and even the oil used for examinations was mixed with disinfecting substances. (Would it not have been better to have employed fresh oil for each examination?) To strengthen the constitution of the patients, and to preserve them from liability to infection, sulphate of quinine was given in full doses at an early period. The spread of this malady, like many others, by a process analogous to that of fermentation, from a small amount of poison introduced into the system, is corroborated by Dr. Faye's observations. His treatment of this zymotic disease is grounded on the hope of producing a total change in the condition of the blood, by large and repeated doses of calomel, after the method proposed by Ritgen and others; but he considers this practice to be only advisable where the treatment is commenced on the first appearance of symptoms of the disease. Twenty grains of calomel were given once or twice daily, and followed by the exhibition of ℥ss. of castor oil, every three or four hours. In 1850 Dr. Faye had hesitated to employ purgatives until at least two days had elapsed from the period of delivery; but he found, from bitter experience, that if purgatives were not employed at the beginning, the favourable time for their administration soon passed by, and the bowels became so paralyzed by peritoneal inflammation, that they could not be induced to act by the strongest evacuants. Quinine and opium were likewise given along with calomel in large doses, from the first day that the fever showed itself. Dr. Faye refers to Mr. Garlick's treatment of puerperal fever as corroborating his practice of giving laxatives at an early period in this disorder. He remarks, too, that although so large a quantity of castor oil was given, the secretion of milk was rather increased than diminished, and he suggests that this oil may really have the property of augmenting the lacteal secretion, as is stated by Dr. McWilliam, in the 'Monthly Journal of Medical Science,' for October, 1850. We are glad to observe that at every meeting of the Medical Society of Christiania, the "constitutio epidemica" of the town, and of the country in general, is made a subject of discussion and report.

At the end of the tenth number a list is given of the subscribers throughout Norway to the fund for a monument to Jenner. The sum that has been thus collected (240 Sp. D.) will, perhaps, appear small; but

Norway is not a land abounding in wealth, while the medical practitioners are but indifferently remunerated for their labours. Still, it is pleasing to observe that, out of their scanty means, our Norwegian brethren of the profession have contributed so large a proportion of the whole amount that has been raised for this purpose throughout that kingdom.

It will be seen, from the brief analysis we have here given of some of the more generally interesting papers, that the 'Norway Medical Journal' is carried on with considerable spirit, embracing every department of the profession, and discussing its various subjects with an earnestness and impartiality well worthy of imitation. We could indeed wish to receive more frequently separate works or monographs of particular disorders, from our Norway brethren; but next to this we rejoice to observe that the discoveries of other countries are readily received and appreciated by the members of the Medical Society of Christiania.

Edward Charlton.

REVIEW XIV.

1. *A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula.* By HENRY ANCELL, late Surgeon to the Western General Dispensary, and Lecturer on Materia Medica and Medical Jurisprudence, &c. &c. London: 1852. 8vo, pp. 779.
2. *The Nature, Symptoms, and Treatment of Consumption: being the Essay to which was awarded the Fothergillian Gold Medal of the Medical Society of London.* By RICHARD PAYNE COTTON, M.D., Member of the Royal College of Physicians, and Assistant-Physician to the Hospital for Consumption and Diseases of the Chest. London: 1852. 8vo, pp. 286.

IN an article contained in the twenty-first number of this Journal (January, 1853) we classified and gave an abstract of the views entertained by various pathologists as to the structure and pathological origin of tubercle, and stated the question at issue as to what disease or diseases should be signified by the term phthisis. The authors whose works are placed at the head of this article belong to that section of pathologists which, as we then stated, has for its most recent exponent, Lebert.

Tubercle is with them a secretion, or rather, a developed secretion—for the tubercle-blastema is in reality the secretion, and the tubercle the same blastema, organized to a certain extent by the development in it of tubercle-corpuscles. The tubercle-blastema is derived directly, like all other secretions, from the blood; and it is in this fluid that Mr. Ancell looks to find the primary, the essential disease. Tuberculosis is, he states, "neither a cachexia nor a diathesis, but an idiopathic malady affecting the blood." And again: "Tuberculosis will in this volume be regarded as an idiopathic blood-disease." Dr. Cotton speaks more vaguely as to the primary affection, observing merely that it is constitutional; still he evidently holds that the blood must be diseased before tubercular matter is formed, for he speaks of this matter being, in a large majority of cases, merely separated from the blood. "Tubercular matter is formed in two ways: 1st. By a gradual and uninflammatory separation from the blood; 2nd. By inflammatory action ending in a morbid scrofulous secretion, instead of a healthy fibrinous one.

The first of these is by far the most common." The constitutional state and the blood-disease to which it necessarily leads are designated Phthisis, or Consumption, by Dr. Cotton. But he does not always use these words in the sense he has himself assigned to them at the outset of his work; for at p. 12 he uses the word phthisis to signify the local disease, while at various parts of his essay he appears to use phthisis and consumption indifferently to express the general constitutional state, the blood-disease, and the one of the local lesions which is the consequence of the blood-disease—i.e., tubercles in the lungs.

The Fothergillian gold medal of the Medical Society of London was awarded to Dr. Cotton's essay. It is divided into three parts; the first of which treats of the *nature* of consumption; the second, of the *symptoms* of consumption; and the third, of the *treatment* of consumption. It contains the expression of his own opinions as to the first of these points, and the result of his own observation at the Brompton Hospital—expressed, with one or two exceptions, in the most general terms—in regard of the last two.

Mr. Ancell's work is an epitome of all that has been done and said in reference to scrofula and tubercle; excepting those views of Virchow, Van der Kolk, &c., which were analyzed in the late numbers of this journal.

Mr. Ancell considers his subject under eight heads:

1. Tuberculous predisposition.
2. The signs and symptoms of tuberculosis.
3. The history of tuberculous deposits.
4. The special pathological anatomy of tuberculosis.
5. The causes of tuberculosis.
6. The general pathology and essential nature of tuberculosis.
7. The forms and varieties of tuberculous diseases.
8. The treatment of tuberculosis.

To each of these subjects a chapter is devoted.

Of the Tuberculous Predisposition.—That condition of the system, or rather those individual peculiarities of form which are ordinarily said to indicate a predisposition to tubercular disease, are considered by Mr. Ancell to have their origin in, and to be the result of, the blood-disease which he calls tuberculosis. Holding that tuberculosis always precedes the deposit of tubercle, and that this blood-disease is the primary departure from health in every case in which a deposit of tubercle occurs, Mr. Ancell opens his work with an attempt to determine from the observations of others, in regard of the general physical, the microscopical, and the chemical characters of the blood in the very outset of tuberculosis, what is the essential deviation from health in that affection. Thirteen pages are devoted to this subject; but the greater part of the so-called facts thus collected together are opposed to each other, and the opinions of the writers quoted are almost as numerous as the writers themselves. Some—as Baumes and Denis—affirm that the clot in blood drawn from scrofulous individuals is large and soft, and coagulation rapid; others, on the contrary—as Dubois and Andral—that the clot in such cases is small and dense, and that coagulation takes place slowly. As to the microscopical observations hitherto made on the blood of scrofulous subjects, they are utterly worthless. Appearances which may be produced at will in the blood of the most healthy are set down as signs of disease, and no evidence is afforded that the various sources of error, so difficult to avoid as to deter the most accomplished microscopical observers from entering on the subject, were shunned.

As to the chemical constitution of scrofulous blood, L'Héritier states, that in scrofula the earthy salts are diminished; Fricke, that they are increased; the fat elements are by some held to be above the average, by others to be below the mean of health. The fibrine has been found to be very nearly as often in excess, as deficient in quantity; and the only points that appear established by the many chemical analyses made of the blood of scrofulous and tuberculous subjects—of the blood, that is to say, in tuberculosis—are, that the blood-discs are deficient in number, and that the albumen is increased in quantity. But the fibrine and the albumen are defective in quality; their vitality is diminished, in the opinion of Mr. Ancell; and as this defect in quality of the fibrine and albumen is the centre-point of all Mr. Ancell's opinions—the basis of his pathology of tubercle—we shall give, in his own words, his explanation of the term.

"In living matter the degree of vitality is measured by the sum of vital actions, comprising, not only chemical and physical changes produced under the influence of the vital force, but actions referrible to the vital properties of irritability and sensibility. The healthy constitution of man corresponds with a physiological range of vital actions in the blood and the living tissues, but especially in the blood. These actions, as respects the blood, consist in all the molecular changes which take place in hæmatosis. They comprise those which result from the dynamic property of endosmose and exosmose in the corpuscle, as well as those which mark—its organic contractility—the assimilation of old and new matter to the form of blood—the formation of red corpuscles and liquor sanguinis—the consumption of red corpuscles—and the waste of liquor sanguinis in nutrition. When the sum of these molecular actions is within the physiological range, and they are in harmony with each other, a sufficient number of corpuscles being formed and wasted, and all the constituents of the liquor sanguinis being atomically complete and of normal proportion, and consumed and renovated in proportion to the requirements of the living structures, the blood has a healthy degree of vitality; but in tuberculosis the sum of these actions is below the physiological range; and, moreover, the proportions and qualities of the blood are deranged. This marks a low degree of vitality, which, as we shall see, is consistent with all the phenomena of the tuberculous predisposition, and with the signs and symptoms of tuberculosis in its various forms." (p. 15.)

For our part, after giving this paragraph as attentive a consideration as we are able, we must own that some parts of it lie beyond our comprehension, and that we are unable to see, adopting Mr. Ancell's own definition of diminished vitality in regard of the blood, that in tuberculosis the *blood is deficient in vitality;* for the only constituent of the blood deficient in quantity is the red blood-discs, while the albumen is in excess. And with reference to the blood-discs, although deficient in number, there is no evidence before us to prove that they are defective in quality; for the microscopical observations are, we repeat, judging from their intrinsic evidence, absolutely worthless; and we may add, our observations, more numerous than those he has quoted, fully justify this *à priori* conclusion.

Of the condition of the *lymph* and *chyle* in tuberculosis, literally nothing is known.

While certain physical deviations from a healthy standard are said by many to indicate a predisposition to tubercular deposition — e. g., the smooth, soft, delicate, and transparent skin, through which the bloodvessels are perceptible to the eye—Mr. Ancell, who holds these to be evidence of the existence of tuberculosis, of an already established blood-disease, regards these deviations from healthy structure as the result of the evolution of blastemata of defective vitality. He argues that, the blood being healthy, healthily-constituted blastemata are formed by it, which develop into normal tissues. But in tuberculosis the blood is diseased; hence the blastemata poured out from that blood are diseased; and as a consequence, the structures formed from them are defective in constitution.

Mr. Ancell then passes in review "the cutaneous system" and its appendages, the mucous membranes, the teeth, the vascular system, the serous membranes, the osseous tissue and the skeleton, the muscular system, the viscera, the lymphatic glands, and the organs of special sense, as these are severally modified in structure by the circulation of tuberculous blood; and describes the deviations from their normal functions manifested in the digestive, respiratory, circulatory, secretory, muscular, sensorial, moral and intellectual, and generative organs. Dr. Todd's opinions in reference to strumous dyspepsia are adopted by Mr. Ancell. With reference to the relation which exists between deformities of the chest and tubercular disease, Mr. Ancell remarks:

> "The contraction of the chest, and the defective structure and functions of the lungs, have been properly regarded, as long as the tuberculous state of the blood exists, as a real predisposing cause of the development of tubercles in the lungs; but they have also been regarded, without any conclusive evidence, as the cause of the tuberculous cachexia—that is to say, of the disease of the blood. This subject will have to be considered in a future chapter; it is sufficient here to observe, that the mal-nutrition of the lungs, and of the bony parietes of the chest, is of the same nature as that which is liable to pervade any other part of the system; that tuberculosis pulmonalis is but one form of tubercular disease; and that if we were to admit this principle of etiology, we must also, by a parity of reasoning, admit, that where tuberculosis meningitis exists without any pectoral predisposition or disease, the irregular formation of the skull and mal-nutrition of the membranes of the brain constitute the cause of the disease of the blood." (p. 42.)

The *muscular powers* are deficient in those suffering from tuberculosis. This loss of power is witnessed not only in the voluntary, but also in the involuntary muscles. The latter is exhibited in the diminished power of the heart, the feebleness and distensibility of the muscular coats of the intestines and bladder, and especially in the action of the iris. The habitually dilated pupil depends, Mr. Ancell says, upon the diminished power of the iris.

> "It has been remarked," he continues, "that it is a better measure of the tone of the muscular system than the touch; since want of exercise alone would relax, for instance, the arm of a delicate female as compared with that of a labourer; but the iris is employed alike by rich and poor. Whatever the physiological explanation of the action of the iris, the habitual dilatation of the pupil, as it often occurs in tuberculous subjects, is an important sign of the general constitutional debility." (p. 47.)

It is a fact that an extreme degree of tuberculosis may exist, and yet the intellectual faculties be highly developed. All tissues save the nervous may in a given case suffer; whence, then, comes this exemption of the nervous centre from structural change? This question is met by Mr. Ancell thus:

"Although the nerves are developed from cells, the dependence of the medullary matter upon the proper material of cell-growth appears to cease at a very early period of embryotic existence, and the medullary portion of the nervous structures becomes a secondary deposit in the cell-membrane. The materials constituting the proper substance of nerves, with all other materials employed in building-up and sustaining the animal economy, are equally derived from the blood; but they differ *in toto* from the materials required for the nutrition and growth of cellular structures. The nervous tissue differs from other tissues in that it never can, like them, be formed at the expense of the cellular tissues. This being the case, it by no means follows that a special morbid condition of the blood, which furnishes a morbid blastema to the cellular, gelatinous, and fibrous tissues, should necessarily furnish morbid materials for the nutrition of the anatomical nervous system. In many of the most unequivocal cases of the tuberculous constitution, the anatomical structure of the nervous system is perfectly intact." (p. 49.)

An explanation which will not be found very satisfactory by those who are not already of Mr. Ancell's opinion.

The second chapter is devoted to the signs and symptoms of tuberculosis. As Mr. Ancell regards the tuberculous predisposition of other writers to differ in degree only from established tuberculosis, he retreads in this chapter the ground he has gone over in his first. "The morbid condition of the blood," he writes, "in tuberculosis consists, in an aggravated degree, of many of those deviations from a healthy state enumerated in the preceding chapter." The fact that the fibrine is so very often found to be in excess in this stage of tuberculosis, and the clot small and firm, and covered with a buffy coat, is considered by Mr. Ancell to result from the frequency with which local inflammations occur in the progress of tuberculosis. "The blood," he says, "has rarely, if ever, been examined in its simply tuberculous state. The albumen is *probably* further increased in proportion; and the albumen and fibrine are *in all probability* more vitiated in quality." (The italics are our own.) The blood-discs certainly continue to decrease in number as the disease advances.

The atrophy which is so marked a feature in advanced tuberculosis—

"Is not produced by a deficiency of blood only—a very common cause of atrophy—nor by fever, nor by excessive evacuations, nor by any defect of primary digestion, for in many cases neither of these circumstances have existed. . . . The emaciation of tuberculosis is attributable to a specific vitiation of the blood, producing a corresponding vitiation of the blastema, and mal-nutrition of cells, the vital power of which being below the healthy standard, there is a consequent excess of the disintegrating over the integrating processes." (p. 70.)

The atrophy, even in cases of phthisis pulmonalis, is attributed by Mr. Ancell directly to the diseased condition of the blood. "There can be no doubt," he says, "that it is, from the beginning to the end, symptomatic of the general disease." The anæmia is explained in a similar way.

Hæmorrhage from the lungs and other parts, Mr. Ancell thinks, is often the result of attenuation of the pulmonary vessels, and occurs frequently before there is any deposit of tubercles—nay, is not unfrequently the primary exciting cause of the local disease. On this point, however, it does not appear to us that the evidence is very satisfactory. Diminished breathing capacity, and short, dry, hacking cough, may be observed as the result of the blood-disease when no local affection of the lungs is present.

A section of this chapter, on Febricula as a result of Tuberculosis, contains some very interesting matter. The febrile state which often precedes the deposit of tubercle is, we are satisfied with Mr. Ancell, frequently overlooked or misinterpreted; and the practitioner is astonished to see his patient cut off a few days only after his attention has been seriously arrested on the case; and then to find, after death, grey granulation scattered through the lungs or the pia mater. This febrile state is thus described by Mr. Ancell:

"It consists at first of a general sense of uneasiness, languor, and feebleness, aggravated by the slightest exertion or irregularity of diet. There is a slightly increased temperature of the body generally, with a tendency to cold, and a great susceptibility to the action of external cold; the skin being frequently warmer, and acrid or arid, except at the extremities, which are colder than natural; there is slight thirst, a suppression of natural perspiration; but after accessions of the feverishness, and during sleep, the skin becomes pale, soft, perspirable, and cold to the observer. There is great susceptibility to cold, and tendency to slight rigor, this chilliness in connexion with the feverish heat being a very constant symptom, and of considerable importance in a diagnostic point of view." (p. 105.)

The breath is heated and fetid, the bowels are irregular, and the digestive organs generally deranged; the urine deposits a whitish sediment; and the patient more or less rapidly loses flesh. Sometimes this febrile state at its outset observes "an irregular periodicity," coming on after meals, or in the evening or morning; but after a time it becomes continued. Mr. Ancell points out the danger of confounding this febrile tuberculosis with that "feverishness of digestion observed after meals in many persons who have in no respect the slightest indication of tuberculosis." With reference to its diagnostic value, Mr. Ancell says, "When it occurs simultaneously with, or supervenes on the anæmia, emaciation, and debility, we may be quite sure that the case is verging upon the deposition of tubercle."

"The feverishness of tuberculosis," continues Mr. Ancell, "is of more positive value as a diagnostic sign than either of the other symptoms—than the anæmia, the emaciation, or the debility. No subject of the disease has this symptom in a well-marked form, with the chilliness I have mentioned, without being at least on the verge of the deposit of tubercle. Since this feverishness also passes by almost imperceptible degrees into hectic fever, when it exists we must always entertain the suspicion of a deposit of tubercle." (p. 107.)

Still, Mr. Ancell observes, these general symptoms may occur "without any local deposit whatever."

The third chapter is on Tuberculous Deposits, and includes a consideration of the several modes of origin, seat, physical and microscopic characters, chemical constitution, and phases of metamorphosis of scrofulous pus, of tubercle, and of the black colouring so often found in company with the latter. Having in a future article to discuss the subjects considered in this and the succeeding chapter on Special Pathological Anatomy of Tuberculosis, we shall pass them by, merely remarking that they contain most full accounts of the observations of various pathological anatomists on the subjects treated of in them. Two hundred pages are devoted to the fourth chapter alone. Mr. Ancell's materials are drawn chiefly from the writings of Laennec, Cruveilhier, Andral, Carswell, Louis, Hasse, Rokitansky, Rilliet and Barthez, Lebert, Legrand, Clark, Todd, Rainey, Clendinning, and Boyd.

The fifth chapter is a very valuable one; it is on the Causes of Tuberculosis. Mr. Ancell commences this chapter with the apt remark, that "volumes have been written on the causes of those diseases of which tuberculosis is the parent, and very little that is satisfactory can be elicited from them." The causes of the local affections have been strangely commingled with the causes of the general disease. "Sir James Clark made the distinction," he continues, "very clearly, but statistical writers have very little regarded it." The importance of this distinction, practically as well as theoretically considered, must be admitted by all who have had their attention directed to this subject; and we are surprised that writers on the etiology of phthisis should, after it was once distinctly pointed out, have disregarded it.

To attempt an analysis of this chapter would carry us far beyond the limits assigned to this article. Mr. Ancell introduces it thus:

"On a careful consideration of the whole subject of causation, it appears that the specific morbid condition which constitutes tuberculosis can only result from hereditary transmission by the blood of one or both parents, or be produced by a modification of the blood of the individual. The causes of tuberculosis as we recognise them in practice are accordingly—

"1. The hereditary transmission from parent to offspring of the predisposition to the disease, or the disease itself.

"2. The causes of the production of the disease in the individual.

"I propose to treat of these two totally different subjects in two divisions, but in order that their scope may be somewhat more fully before the reader at the outset, I may here state that the causes which have been assigned of the original production of the disease in the individual, admit also of a twofold division into—

"1. The cause of acquired tuberculosis in the embryo and fœtus.

"2. The causes of acquired tuberculosis after birth.

"The latter again are divisible into—

"A. The predisposing causes.

"B. The exciting causes.

"This arrangement differs *in toto* from any that has been employed hitherto by those who have treated on the etiology of tubercular disease." (p. 373.)

With reference to the still vexed question of the hereditary transmission of tuberculosis, considered as a general disease, and in regard of its local manifestations, Mr. Ancell, after giving the statistical evidence bearing on the subject, (by the way, how could Dr. Walshe's model paper on this subject have escaped Mr. Ancell?) concludes that—

1. Accidental or acquired tuberculosis in the parent may be transmitted to the offspring, either as a predisposition, or as the disease in its active state. At the same time, the frequency of hereditary transmission is probably less than some authors state.
2. The child may inherit tuberculosis from either parent.
3. Tuberculous parents do not necessarily beget tuberculous children.
4. Parents suffering from scrofula—i.e., the early stage of tuberculosis—transmit an analogous condition of blood to their children, but rarely transmit tubercle; whereas those suffering from tubercle may transmit indifferently scrofula or tubercle, or both, to their offspring.

5. A robust mother rarely gives birth to a tuberculous child, even though the father be tuberculous.
6. The tuberculous quality of the blood may be latent in the parent at the time it is transmitted to the child.
7. The more intense the tuberculosis in the parent, the more likely is the child to suffer.
8. The influence of the mother is greater in transmitting tuberculosis than that of the father.
9. The parent may recover from a local manifestation of tuberculosis, and yet the blood-disease may not have been eradicated; so that the blood may remain tuberculous to a sufficient extent to transmit the disease to the offspring.
10. "The cessation of the hereditary transmission of tuberculosis in a few generations," proves that, "however general the tuberculous constitution may be in the human race, it does not constitute a new species."
11. The blood-disease may be so slight at birth as to constitute only a proclivity to the disease.
12. If hereditary transmission proceeds from one parent, any cause referrible to the other parent by which the constitution of the child is enfeebled, will increase the pernicious consequences of the tuberculous disease.
13. As tuberculous disease may be developed without the action of any known anti-hygienic influences in those who are hereditarily disposed to it, it is an error to affirm that the hereditary tendency is a mere pathological susceptibility to an ultimate and indeterminate disease.
14. Hereditary tuberculosis developes itself in the form of phthisis at an earlier period of life than the disease does when acquired.
15. Hereditary transmission is so much the less to be feared as the appearance of the disease in a family is removed to a distant generation.
16. When tuberculosis is transmitted by the blood hereditarily to several members of the same family, the local development may be different in each. One may have hydrocephalus; another, white swelling; and a third, phthisis. In other cases, however, the tendency to a special local development is transmitted.
17. At the time the parent transmits the disease he may appear to be healthy, although subsequently he may die of phthisis.
18. The chances of transmission increase with the number of children born, so that it frequently happens that the younger children die before the elder.
19. If a tuberculous husband marries two wives, both equally exempt from the disease, the children of the last wife will be more liable to the disease than those of the first.

One hundred and seventy pages of his work are given up by Mr. Ancell to a consideration of the causes by which tuberculosis may be produced in the individual. The result of his researches may be thus summed up:

The most efficient causes in inducing tuberculosis are, a vitiated atmosphere, insufficient muscular exercise, and the depressing passions; the next in importance is defective diet. There are many other subordinate ancillary causes, as cold, a dense and humid atmosphere; those influences which diminish or retard the hepatic or digestive functions, or lead to congestions of the chylopoietic viscera; whatever exhausts the nervous power and produces debility. To these heads most of the so-called exciting causes of phthisis may, Mr. Ancell thinks, be referred.

The sixth chapter is on the Essential Nature and General Pathology of Tuberculosis. In it the question of antagonism is fully entered into, and the various opinions of writers quoted. The following observation by Dr. Cotton seems to us to be almost absolutely true, judging from the evidence at present before the profession on the subject: "Consumption has no antipathy or antagonism to any other disease, beyond that which is common to every morbid condition." The forms and varieties of tuberculosis are disposed of by Mr. Ancell in thirty-five pages.

The last chapter in both the works before us is on the Treatment of Tuberculosis; only, in Dr. Cotton's work attention is especially given to the pulmonary development of the general disease. In both works this part gives evidence of having been written by men practically well acquainted with the subject on which they were writing; and both chapters deserve attentive consideration.

Dr. Cotton and Mr. Ancell add their voice to the evidence in favour of the value of cod-liver oil. Dr. Cotton made some experiments to test the comparative value of cod-liver oil and other animal oils, and vegetable oils –viz., train, spermaceti, neat's foot, linseed, almond, and olive oils. The common train oil was given by Dr. Cotton to fifty patients in different stages of phthisis pulmonalis, and notes of the cases were carefully preserved.

"Except in ten instances, it was not continued longer than a month, for within this period its inferiority to the cod-liver oil became too manifest to justify further experiment. In these ten cases it was taken—and for a considerable time—with good effect, especially in those which had reached the third stage: there was in each an increase of weight, the cough was lessened, some of the most urgent symptoms were relieved, and the health improved. Upon the whole, however, the benefit appeared less than might have been expected from the cod-liver oil." (p. 276.)

The value of the spermacetic oil was also tried on fifty patients suffering from phthisis pulmonalis. In four or five of these fifty cases only did the patients increase slightly in weight. "In none was the advantage of this remedy either so conspicuous or long continued as that of the train oil." Neat's-foot oil was given to twenty patients. Some gained slightly in weight; "but the majority appeared little, if at all, improved by its employment, so that this oil, like the others, ultimately gave place to its more trustworthy competitor."

"The oils of linseed, almond, and olive, may be included under one head. They were given respectively in about thirty cases, in all of which there was either no improvement whatever, or it was so slight as to render it difficult to determine whether or not the oil deserved any of the credit. The cough, however, was generally diminished by their influence, but neither the patients' appetite nor strength was materially increased, whilst the olive oil occasionally produced a disinclination for food and slight diarrhœa. It was singular, indeed, to observe the rapid improvement which often followed their exchange for oleum aselli. Upon one occasion, after the linseed oil had been taken for nearly a month with no success, the cod-liver oil completely restored the patient's strength, and added to his weight one stone and one pound within six weeks; and in another example, after having prescribed the linseed oil, apparently with signal success, the health being improved, and the weight greatly increased; and whilst imagining that at last an instance had occurred of my expectations being realized, I discovered that, the hospital supply having become exhausted, the patient, dissatisfied with his improvement, had been taking of his own accord oleum aselli. I had also," Dr. Cotton adds, "many similar illustrations in reference both to the almond and olive oils."

Dr. Cotton's experiments, then, afforded very different results from those made by Dr. Duncan and Mr. Nunn:* they gave almond oil in the place of cod-liver oil in more than 250 cases, and found it of the same value. One of their patients gained two pounds weekly in weight, and another, four pounds, while taking it. Dr. Cotton tried the combination of oil and iodine recommended by the same gentlemen, and found that "in many instances it diminished the patients' appetite; and in none did it afford any obvious benefit." Mr. Ancell thinks the fact of the Colchester Hospital occupying an elevated site on a dry soil may account for the differences in the results obtained there and elsewhere, in regard of the changes ensuing on the administration of almond oil; while Dr. Cotton remarks that Dr. Duncan and Mr. Nunn gave the almond oil chiefly in scrofulous diseases, and "not to any extent, at least in cases *purely phthisical*." A mixture of sugar with ox-gall was employed by Dr. Cotton, at the suggestion of Dr. Thomas Chambers: "in no case was it productive of good, and several times it caused diarrhœa."

Naphtha was prescribed in upwards of fifty cases "in different stages of consumption." In many of these cases the patient was made much worse by the drug. The cough was aggravated, and more or less dyspepsia produced, accompanied by a distressing feeling of constriction across the chest; not "the slightest amelioration of the tubercular symptoms being produced." In two instances only did Dr. Cotton see any improvement follow its employment; and in both these cases the patients were put upon an improved system of diet at the same time that the naphtha was prescribed, and one of the two was sent into the country.

"Notwithstanding an early prejudice," Dr. Cotton says, "in favour of this medicine, I am compelled, after a fair trial of its effects, to pronounce it of little or no value in the treatment of phthisis." (p. 283.)

Dr. Cotton's essay will be found useful to the student and young practitioner, as a guide in the diagnosis and treatment, especially, of cases of phthisis pulmonalis. Mr. Ancell's work is remarkable for the care with which he has collected the facts and opinions of other writers on the subject of tuberculosis; for the fairness and clearness with which he has stated those facts and opinions; for the skill he has displayed in arranging his heterogeneous materials so as to construct a systematic treatise embracing the whole subject of tubercle, and in bringing all to bear in support of his theory of tuberculosis and tubercular deposits. Mr. Ancell's own opinions are put forward as by a man thoroughly satisfied of their truth, of their importance, and of their practical bearing; and yet the opinions of others are always treated by him with due respect. His treatise on tuberculosis ought to be in the library of every practitioner of medicine.

W. Jenner.

* Provincial Medical and Surgical Journal, March, 1850.

PART SECOND.

Bibliographical Record.

ART. I.—*The Transactions of the American Medical Association.* (Instituted 1847.) Vol. V.—*Philadelphia*, 1852. pp. 939.

THE American Medical Association is a society which may be compared to our own Provincial Association. In their volume of 'Transactions' are published the proceedings of the society, and various papers contributed by the members. Among the latter we notice a valuable paper by Dr. Austin Flint, 'On the Variations of Pitch in Percussion and Respiratory Sound, and their application to Physical Diagnosis," which we shall take an opportunity of considering at length. Dr. Dickson communicates a paper on the blending of types in fever, which does not appear to us to be likely to lessen the confusion already existing on this subject. Dr. Adams reports, in the name of a committee, on the action of water on lead-pipes, and on the diseases arising from lead thus introduced into the system. Some very interesting cases of lead-poisoning are recorded. The committee deny the various statements which have been made, respecting the protection given to lead by a coating of salts. They allude especially to the "Edinburgh doctrine," as taught by Christison, to the "Boston doctrine" (that the lead is protected by a deposit of suboxide), and to the "London doctrine," promulgated by Hoffmann, Graham, and Miller, that the carbonate of lead is deposited, and prevents the further action of the water. They also state, that in the Croton water, which supplies New York, and which is drawn from leaden hydrants, put down ten years since, lead can be detected.

We shall take occasion to refer to this important paper when the subject of lead-poisoning again comes under our notice. Mr Hayward reports on the permanent cure of reducible hernia; and Dr. Pope has a paper on the topical uses of water in surgery. The whole of the remaining part of the volume, consisting of nearly 700 pages, is occupied with reports on the epidemics of New England and New York, and on the medical botany of the United States. These are also extremely valuable papers.

ART. II.—*Anatomical and Physiological Bibliography, from* 1849 *to* 1852 *inclusive.* By G. E. DAY, M.D., Professor of Medicine in the University of St. Andrew's. (From 'Goodsir's Annals of Anatomy and Physiology.')

THIS is a very complete bibliography of all anatomical and physiological papers published in periodicals and 'Transactions' from 1849 to the end of 1852. The names are arranged in alphabetical order. We have looked through some foreign journals, and compared them with the 'Bibliography,' without at present detecting a single error.

We would suggest, however, that in future bibliographies (and we hope this is not to be the only one), it would be well to have a list, not only of names, but of things, in alphabetical order. Reference could then be made with great ease, whereas at present we can learn at once what has been done by any particular *person*, but we cannot learn without great trouble what has been done in any particular *subject*.

We hope that this 'Bibliography' will meet with a good reception. It deserves to be encouraged, and a proper sale would enable Dr. Day to make future numbers more useful still for reference.

ART. III.—*Lectures on Surgical Pathology, delivered at the Royal College of Surgeons.* By JAMES PAGET, F.R.S., lately Professor of Anatomy and Physiology to the College.—*London*, 1853. Vols. I. & II.

MR. PAGET'S lectures on inflammation and atrophy have been virtually so long before the public, and have been reviewed by us at such length on various occasions, that scarcely anything now remains for us to do, as regards the greater number of the lectures, but to express our pleasure at meeting them in their present shape. They will now form one of the standard works of English Medicine, and will become a landmark, when the flood of progress has concealed from view less lofty and stable productions. The large audiences who assembled in the theatre of the College of Surgeons, to hear Mr. Paget's eloquent lectures, will derive scarcely less pleasure from reading them. The style is so easy, the illustrations are so apposite and so graphic, and the expression of facts is so pointed, that it is impossible not to see that Mr. Paget possesses, in an unusual degree, not only the art of observation, but the still more uncommon one of exposition.

The second volume demands from us more than this passing notice. It treats of tumours, and is in many parts a new work. We shall take an early opportunity of carefully reviewing it, and in the meantime will merely say, that although we differ from it on some points, it is equally distinguished by the two great qualities of range and clearness of thought.

ART. IV.—*Researches on the Primary Stages of Histogenesis and Histolysis.* By ROBERT D. LYONS, M.B., &c. (From the 'Proceedings of the Royal Irish Academy.' Vol. V., Part III.)—*Dublin*, 1853. pp. 16.

THIS is an important paper, and will be read with interest in connexion with the elaborate review on the cell-theory, which we have given in a former page. Dr. Lyons has fully recognised the inadequacy of the cell-theory of Schwann to explain *per se* the formation of organic structure. From the examination of the yelk of the egg, of the germinal membrane, and of the plasma poured out on a fresh wound, and from a consideration of the experiments of Ascherson, Panum, and Melsens, he has satisfied himself that "structures can originate under conditions when we cannot suppose any vital organic influence to be present, but when such forces as attraction, cohesion, fusion, endosmose, and exosmose, and the mutual reaction of elements differing in physical and chemical characters, are in full operation." (p. 7.)

The structures thus formed under simply physical or chemical conditions are as follow: organic granules, isolated or aggregated, and then forming granular corpuscles, granular stroma, hyaline membrane, and hyaline vesicles. These elements are considered to be, by Dr. Lyons, the "rubble-work of the organic edifice," but to be in themselves incapable of generating the higher tissues which occur in the animal frame. These higher tissues, originating in a mode of which we have no very clear conception, are plastic corpuscles, fibres, cells, and definite tissues.

We are inclined to think that Dr. Lyons might have advantageously developed this portion of his subject a little more, as it is one of immense importance in minute anatomy. We are convinced that the cells forming in albuminous fluids from physical conditions merely, and to which the writer of this notice has been accustomed to apply the term "corpuscular albumen," are frequently referred to, as evidence of the last remains of life and vitality in a plasma or exudation; as the proof, in fact, of the dying and ineffectual effort of the organism to form a tissue.

In the second part of his paper, Dr. Lyons enters on a consideration of the changes which the developed tissues undergo in their progress towards decomposition—that is to say, in their passage from complex into simple forms. For this process he proposes the convenient term "Histolysis," as an antithesis to Histogenesis. To trace the histolytic changes in the animal tissues is a department of physiology or pathology of the highest interest, but unfortunately of no less difficulty; and very little is known of the microscopic changes which a tissue—as muscular fibre, for example—undergoes in its progress from its perfect to its effete condition, when, after having played its part, it vanishes from the body. Dr. Lyons narrates briefly the microscopic changes in integument, subcutaneous structure, blood, and muscle, decaying when removed from the body. He sums up his results thus:

"In considering the chief results arrived at in the study of the process of putrefaction, I am led to believe—

"1st. That, concurrently with the first order of chemical changes, a certain order of morphic changes takes place before the final dissolution of organic structures, by the action of chemical and physical forces.

"2nd. That this series of changes may, under normal conditions, take place very slowly, so that, at the end of many months, and probably of even much longer periods, we are still enabled by the microscope to recognise and identify structures of great delicacy, such as elementary muscular fibre, and that this knowledge admits of important applications.

"3rd. That in this process of histolysis, the first changes consist in the softening, disunion, and separation from each other of the morphic constituents of the tissues, each of which is then subjected to a process of disintegration.

"4th. That granules and granular corpuscles appear at an early period, arising probably from recombinations of the particles of the organic fluids. Animalcules appear at this stage.

"5th. That granules, corpuscles, vesicles, cells, and granular masses of various kinds and sizes, may form in fluids and tissues undergoing histolysis, in which no such elements exist when in their normal states.

"6th. That generally in the progress of histolysis, structures very similar to those which are arranged under the first group, or the a-plastic elements of histogenesis, form at different stages, and that they exhibit the same modes of growth and development, but, like them, are incapable of producing higher forms.

"7th. That these morphic elements of histolysis pass gradually into lower forms, exhibiting occasional instances of endogenous fissiparition, granular disintegration, and other changes, and that the cellular and corpuscular elements, by forming media for endosmose and imbibition, may aid in the disintegration of contiguous structures.

"8th. That certain elements may pass directly into a state of molecular disintegration.

"9th. That certain corpuscles of peculiar characters, and not identical with any known normal elements, are occasionally formed.

"10th. That a period arrives at which chemico-physical forces prevail, which is evidenced by the passage of certain elements into crystalline forms, others passing off by volatilization, solution, &c., and that in this way the final dissolution of a tissue is accomplished, the several morphic changes which take place probably facilitating and preparing the way for the action of chemical forces."

In addition to this, we need only notice, that Dr. Lyons has observed blood-crystals to form in one case in twenty-six hours, and in another (the blood of the duck) to be present two years after removal.

We trust Dr. Lyons will continue his important researches.

ART. V.—*The Harmonies of Physical Science, in Relation to the Higher Sentiments; with Observations on the Study of Medical Science, and on the Moral and Scientific Relations of Medical Life.* By WILLIAM HINDS, M.D.—*London*, 1853. 8vo, pp. 195.

THE latter half of the title of this book expresses the nature of its contents better than the first half. It is a treatise on medical life in its moral relations, with incidental discussions on the physical sciences as bearing especially on moral and physical training. We have read it with great pleasure: it is written very clearly and agreeably, and bears in every page evidences of thought, without anything very striking or original. We should say that the author commenced his work without a definite idea of its scope and limits, but being urged on merely by the knowledge, that he had thought over and had something to say on a good many closely-connected subjects. In some parts it is like Paley, in others like Perceval; now we seem to be reading a chapter on the evidences of design, and now we turn to a discourse on medical ethics. We do not quarrel, however, with this discursiveness; the book is not, perhaps, the less interesting on that account.

In the last chapter we find some very useful remarks on the "Profession of Medicine and its Relation to the Community." Dr. Hinds points out clearly the manifold influences which bear on the medical practitioner, and which act with such different effect on variously constituted minds. After mentioning the practical and scientific elements which, in just proportion, should constitute the basis of the practitioner's professional character, Dr. Hinds continues:

"On this foundation there must yet be a superstructure of qualities and qualifications of a different kind; and this brings us to a brief consideration of the *motives* which should influence a man in adopting and pursuing the profession of physic as a calling. If these motives were confined to the mere choice of a profession, at the commencement of life, their influence would be of less value, inasmuch as the choice is often made either by proxy, or before the agent is thoroughly capable of taking enlarged views, in a moral and professional sense. This, however, is not the case. The motives and springs of conduct which should actuate the practitioner of physic affect the whole of his career, and must be kept before his mind as a great and enduring principle of action. In their nature, these motives must not be sordid. The man who pursues his profession with no higher considerations than those which arise out of a love of wealth, will not only often fail to accomplish his aims, but will almost inevitably degrade the high and noble calling in which he is engaged, and bring dishonour upon a profession *the proper pursuit of which is truly calculated to ennoble the sentiments, and to raise the relation between practitioner and patient to something higher than a mere sordid or business relation.* . . . . . It is a truth to which society has given an unreserved adhesion, that a practitioner wanting in humanity and not self-sacrificing is not in his proper sphere. A pure and elevated humanity, and self-denial, with a love of knowledge, are amongst the great aims which should be found in the bosom of every man who undertakes the duties and responsibilities of the practitioner of physic. Without this moral heroism, as a mainspring of his conduct, he will not ennoble his calling, nor be at peace with its requirements." (pp. 159—161.)

This is perfectly true, and we will venture to say that no profession can offer more numerous instances of men constantly acting under the influence of these noble sentiments, than our own. But there is another side to the question; if we have our duties, the public have theirs; our exertions have their value, to claim which is not avarice, but justice. Let us quote again from Dr. Hinds a few pages further on.

"In the career of practice, great forbearance is often needed on the part of medical men, and frequently nothing but an appeal to high principle will enable them to bear with composure the injustice which both individuals, and sometimes even bodies, will inflict upon them. It will accord with the experience of nearly every medical man to have been habitually subject, at some part of his career, to acts of injustice the most flagrant from individuals who have sought his professional aid; and indeed the mass of our profession, at all points of their professional career, are more or less visited by the same reckless want of thought and injustice. While the community in general hold it cruel and shocking for a medical man to exercise a discretion when he is called to any case supposed to require immediate aid, and while universally medical men themselves freely and fully recognise the claims of the suffering, there are multitudes of persons to be found above the classes of the poor who will resort to shifts the most dishonourable to avoid remunerating the medical attendant for his services. Not a word could be said if these cases were confined to the really poor, whose poverty is well known to be, with nearly all medical men, a sufficient justification for them to seek such services without incurring responsibility. Unfortunately this is far from being the case. Persons in respectable positions, and with ample means, will practise the same injustice. The law courts alone will testify to the fact, and perhaps not a ten thousandth part of such cases ever gets into these courts. Moral responsibilities for medical services, of no ordinary kind, are for ever being sought to be got rid of *in toto*, by some amongst all classes of the community, on mere technical grounds. A gentleman has a servant attacked with apoplexy or epilepsy, and, in the alarm and excitement, orders the nearest medical man to be summoned. The servant, after a recovery from a long illness perhaps, removes to a distant part of the country, and the gentleman, thinking the servant responsible, allows the medical man to remain unpaid. A traveller on a railway meets with an accident, and has a limb fractured, and is carried insensible to a place of rest; while the railway official, or any one with sufficient humanity, seeks the nearest medical man, who leaves his home and bed, perhaps in the middle of the night, to perform an operation. The traveller recovers, and leaves the medical man, who was called-in by the railway official, to appeal to the law to ascertain who is responsible for payment. Thousands of instances, in great variety, and of frequent occurrence, might be enumerated, of injustice to medical men, more or less resembling these. . . . . . . The too frequent perpetration of acts of heartless injustice with which he meets in his career, appeal strongly to his conservative views of self, and to the duties he owes to his personal interest, and tempt him with the necessity to relieve suffering and save life only where there is a distinct comprehension of responsibility for his remuneration. Heaven forbid that any medical man should be ever found to yield to the temptation, and carry this inhuman principle into practice. That he does not, however, is not attributable to the generosity or justice with which he often meets during his professional experience." (pp. 163—166.)

We are not quite sure that we thoroughly comprehend how Dr. Hinds would solve this somewhat knotty point. No one could wish our profession to lose its liberal and benevolent character, in a too earnest desire to obtain an equivalent for its services; on the other hand, there cannot be a doubt that great harm has resulted from a too lavish charity on its part. It appears to us that the proper mode of viewing the case is exactly the same as that in which almsgiving is now universally considered. The medical practitioner, after giving his advice, is entitled to receive an equivalent; if he gives up this equivalent he is bestowing an alms on the receiver, and it is as much his duty to determine that the receiver is a proper person to receive alms, as if he gave money, instead of money's worth. He is guilty of indiscriminate almsgiving, and encourages exactly the same evils as the man who maintains a corps of idle beggars in the streets of London, if he is not quite clear on this point. He does, however, more harm even than this, as he injures also his brother practitioners and the whole profession. That which is easily got is seldom highly esteemed: and in addition, the public has been so accustomed to disregard remuneration to medical men, that absolutely a man who is merely contending for the rights most justly his, is looked upon by too many in the present day as illiberal and money-seeking. We must blame ourselves if the public are slow to perceive what are *our* just rights, and *their* proper duties. We admit that there may be a difficulty in knowing always who are the poor, and then we may safely err on the side of charity; but every practitioner must know that there are hundreds of cases in which he has given gratuitous services to persons who ought to have returned an equivalent.

We quite agree with Dr. Hinds, that in a case of emergency a medical practitioner should not think of assuring himself, first of all, that his remuneration is secure. His first duty is clearly towards the patient, whose life may be in danger; but this immediate crisis being past, we are at a loss to know why he should not then attend "to the duties he owes to his personal interest." In so doing, he cannot lay himself open to the charge of avariciousness or sordidness; he is performing an act of simple justice, both towards himself and towards those who are practising the same calling.

At a subsequent page, Dr. Hinds has some pertinent remarks on the behaviour of the members of the profession towards each other, with a quotation from which we conclude our notice of this pleasing work.

"If a Christian liberality and justice be imperative as a rule of conduct between the medical man and his patients and the community, it is equally imperative amongst his fellow-practitioners. Perhaps nothing produces such lamentable impressions upon the community, in reference to the medical body, as a selfish rivalry and ill-nature amongst themselves. . . . .

"Feuds, and selfish jealousies, and vindictive rivalries, amongst distinguished members of our profession, are injurious to our moral and social status, if they do not even outrage the proprieties and laws of polite society. They degrade us below a first-class moral and social rank, and lower our standard, even in the eyes of the less-refined and less-cultivated portion of the community. It is a truth which we shall all readily acknowledge, that every feeling opposed to a frank and genial intercourse, and entire good faith amongst the members of our body, is totally unworthy of us as a profession It is not to be denied that this high standard in our mutual ethics can be attained and preserved only by means often of the most firm and determined forbearance. Through the inadvertence or whims or misrepresentations of patients, medical men are not seldom brought painfully into opposition or collision with each other. In all such cases, the only complete safeguard is a reliance upon the honour of each other, and a firm determination to allow no circumstance, however irksome and trying, to induce a departure from a true and gentlemanly forbearance. Every member of our body, before he can assure himself of his perfect fitness for this task of excellence, must be prepared to make sacrifices of self interest; for though a mere sordid personal interest is not the most prevalent of the causes of ill feeling, and the other allied evils complained of in our profession, yet sacrifices of personal interest are occasionally nevertheless demanded." (pp. 187—190.)

ART. VI.—*Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands, from the Battle of Rolica in* 1808, *to that of Waterloo in* 1815. By G. J. GUTHRIE, F.R.S. Fifth Edition, revised to 1853. pp. 603.

ALTHOUGH a fifth edition, this book may justly claim something more than a mere announcement. We shall therefore review it in a future number, and shall now merely state that this edition is much improved; it is full of details and illustrative cases, and the commentaries upon them have been carefully re-considered. Many of the cases are of a most extraordinary kind, and illustrate the wonderful recoveries which sometimes occur after apparently the most desperate wounds. They are related in a very vivid manner; and in reading them, we have had constant occasion to admire the way in which the prominent points of the case are brought out. We defer our criticism on the surgical doctrines till a future occasion.

ART. VII.—*On the Nature and Treatment of the Deformities of the Human Frame; being a course of Lectures delivered at the Royal Orthopædic Hospital, in* 1833. With numerous Notes and Additions to the present time. By W. J. LITTLE, M.D.—*London*, 1853. pp. 412.

DR. LITTLE's treatise on the 'Nature and Treatment of Clubfoot,' has received much attention from the profession, and has decidedly been the means of making widely known some great surgical improvements. The present work contains the substance of the former one, and much original matter, and is certainly the most complete treatise we possess on the subject of deformities. The descriptions of the distortions are clearly given, and are illustrated by very good woodcuts. The only omission we notice in it is, that there is no account of the operation lately proposed by Mayer for knock-knee* (*genu valgum*). In speaking of lateral curvature, Dr. Little enumerates the various means adopted for its removal under eleven heads—viz.

1. Improvement of the general health.
2. Mechanical supports, or scaffoldings.
3. ———— compression of lapsed parts.
4. ———— elongation of the whole trunk.
5. Surgical division of contracted muscles.
6. Recumbency.
7. Gymnastics.
8. Automatic exercise of particular muscles.
9. ———— rectification of equilibrium of the column.
10. Topical application of medicines.
11. Electricity.

The remarks made under each head are judicious. Mechanical supports are neither too much discountenanced, nor too much lauded. The effect of gymnastics, however, is too shortly discussed, and the "Swedish exercises" are too decidedly condemned. We cannot too strongly discountenance the quackery which has been, and is, associated with these exercises; but that some of the practice might be properly incorporated into our mode of treatment of spinal curvatures, we cannot doubt.

We recommend Dr. Little's book as a most useful one; every page bears marks of the good sense of the writer, and of his profound acquaintance with the subject.

ART. VIII.—*On the Use of an Artificial Membrana Tympani in cases of Deafness, dependent upon Perforation of the Natural Organ; to which is added a paper, entitled, Ought the Tonsils to be Excised in the Treatment of Deafness?* By JOSEPH TOYNBEE, F.R.S.—*London*, 1853. pp. 46.

THE first of these two papers is divided into the following sections:

1. On the Structure of the Healthy Membrana Tympani.
2. On the Functions of the Membrana Tympani, Tympanum, and Eustachian Tube.
3. On the Formation and Use of an Artificial Membrana Tympani.

The third section alone requires any notice. The artificial membrana tympani is made of a thin slip of gutta percha or vulcanized india rubber, placed between two thin and small plates of silver, or between two delicate silver rings, smaller in diameter than the gutta percha. At the time of application, more or less of the excess of gutta percha may be cut off. The artificial membrane is then moistened and passed down to the remains of the membrana tympani; it cannot pass beyond into the cavity of the tympanum without causing pain, which should be the signal for its

* Die Osteotomie, als neues Orthopædisches Operations verfahren, von Mayer. (Würzburg Verhandl., vol. ii. p. 224; and vol. iii.)

withdrawal. It can be withdrawn from the meatus by means of a silver wire attached to one of the plates. The cases in which this apparatus has been found useful, appear to be similar to those which are temporarily so much improved by the use of moist cotton—viz., perforation of the membrane from otitis after scarlet fever, measles, &c., or idiopathic inflammation and ulceration of the membrane. Nine satisfactory cases are related in proof of the efficacy of this method.

The second paper is a protest against the practice of excising enlarged tonsils for the cure of deafness.

ART. IX.—*Habit, Physiologically considered.* (*A Lecture delivered at the Bristol Literary and Philosophical Institution.*) By JOHN ADDINGTON SYMONDS, M.D. (Reprinted from the 'Psychological Journal,' for July, 1853.)—*London*, 1853. pp. 28.

THIS is a very clever lecture on a subject of which it must have been somewhat difficult to treat before a mixed audience.

The first fourteen or fifteen pages are taken up with illustrations and descriptions; it is not till the seventeenth page that we come to the all-important question—Why should mere repetition dispense with volitional exertion?

"To answer this fully," says Dr. Symonds, "would require a long and abstruse discussion; I must content myself with a summary statement of what seems to be the explanation of the fact."

This explanation is, that

"In volitional movements, the action of the commissures between the sensational and their related motor centres is excited in the first instance by the will. But the repetition of more or fewer of these incitements appears to suffice for the growth and development of the commissure to a degree of strength and activity adequate to the performance of its functions independently of the will."—(p. 17.)

This strikes us as a very improbable hypothesis.

In the latter part of the lecture Dr. Symonds slightly touches on what he terms the automatic action of the brain in intellectual processes, the self-evolution of thought, what has been termed by Dr. Carpenter, "unconscious cerebration." This is a most curious and interesting subject, scarcely yet explored, and likely to lead to some modification of psychological doctrines. We would suggest to the directors of the Bristol Literary Institution, that they should request Dr. Symonds to enter on this subject in a second lecture; it will, we are convinced, amply repay them.

ART. X.—*Remarks on Hysteria, in connexion with Hydrophobia and other Convulsive Affections.* By JOHN DALZIEL, M.D.—*Edinburgh*, 1853. pp. 32.

THIS paper was written by the author more than twenty years ago, and was sent for publication to the 'Glasgow Medical Journal,' in which an abstract of it was published in 1830. The author has now published it *in extenso*, in order to show that his views on hysteria and hydrophobia correspond with those lately promulgated by Dr. Marshall Hall. The following quotation expresses his opinions:

"1st. The *globus hystericus*, as well as the similar affection of the throat in hydrophobia, occasioned by the idea &c. of liquids, is a spasmodic stricture of the muscles of the throat (*glottis*), whereby respiration is obstructed.

"2nd. *Obstructed respiration*, whether *suspended* or *impeded*, occasions cerebral congestion, as well as that feeling of *general uneasiness*, designated *sensation of suffocation*, which attends the paroxysmal exacerbation in both the diseases under consideration.

"3rd. Cerebral congestion, and the sensation of suffocation, separately or conjointly, may, especially in an irritable habit, occasion *convulsion*." (p. 6.)

With respect to hydrophobia, we may quote one or two passages which explain clearly enough the author's meaning:

"It is my express object to show that the convulsion is to be attributed entirely, though indirectly, to *obstructed respiration*, and not to the accompanying circumstance of the local spasm, or pain attendant on the spasm. The competency of obstructed respiration to occasion convulsion will be fully made out by establishing the second and third propositions; but the second term of the second proposition, and the first of the third, are also admitted. The only link awanting, then, to complete the chain, is the proof that obstructed respiration produces cerebral congestion." (p. 8.)

"If a cure for so horrible a malady is ever to be discovered, there is a rational ground for hope that the operation of *bronchotomy* will at least make one step of approximation towards it; and it will, at all events, afford the disease ample opportunity of fully developing itself, in its undisguised essential character, and place the patient in circumstances more favourable for the salutary operation of medicine." (p. 29.)

Certainly the similarity between these views and those of Dr. Marshall Hall is singular. How far they are correct we shall not stop now to inquire.

ART. XI.—*Reports by* NEIL ARNOTT, M.D., *and* THOMAS PAGE, Esq., C.E., *on an Inquiry ordered by the Secretary of State relative to the Prevalence of Disease at Croydon, and to the Plan of Sewerage.* (Parliamentary Paper.) 1853.

IN both a medical and a social light, the fever which prevailed at Croydon after the new plan of sewerage had come into operation, is deserving of very careful study. As we have reason to believe that other medical statements will yet appear, we shall not now consider the very able paper before us at any length. Both Dr. Arnŏtt's and Mr. Page's reports are excellently done; the former, treating chiefly the medical and sanitary problems presented during this outbreak; the latter, the engineering operations.

The disease at Croydon was, we are informed, in the main, well-marked "typhoid fever," this term being used in the sense in which it is employed by Louis and Jenner, but other anomalous forms of disease presented themselves, as in the case of Dr. Chalmers (Report, p. 8), such as low erysipelatous inflammation of the mouth and fauces. Although exact records are yet wanting, we believe that one of the most interesting problems in etiology will be solved by this epidemic, and that the arguments to show that the effluvia from drains can produce a specific fever, capable, subsequently, of propagation from person to person, by contagion, will be made complete. On this point Dr. Arnott has some illustrative cases in his interesting general remarks on the origin of fevers, published at the conclusion of his report.

Mr. Page's report is, of course, an engineering statement, but is made with such clearness and precision, that there is no difficulty in following it throughout, and in giving assent to all the conclusions.

Mr. Page adds another able protest against the engineering mania of the General Board of Health; a mania which has already been unequivocally condemned by the united voice of the engineers, as expressed in the discussion at the Institute on the small-pipe drainage. But he also shows that the peculiar difficulties incident to the drainage at Croydon were quite unprovided for, and that the General Board of Health made everything worse by a number of ill-advised directions.

ART. XII.—*Small Pox and Vaccination. Copy of Letter from Dr. Edward Seaton to Viscount Palmerston, with inclosed copy of a Report on the state of Small Pox and Vaccination in England and Wales and other countries, and on Compulsory Vaccination, with Tables and Appendices, presented to the President and Council of the Epidemiological Society by the Small Pox and Vaccination Committee.* (Parliamentary Paper, 3rd May, 1853.)

ALTHOUGH we do not propose at present to enter on a critical review of the subject of vaccination, we are unwilling to omit the earliest opportunity of expressing our admiration of this admirable Report. The Small Pox and Vaccination Committee of the Epidemiological Society consisted of Messrs. Grainger, Marson, Hunt, Waller A. Lewis, Kesteven, Aikin, and Seaton; the latter gentleman being the honorary secretary. These gentlemen put themselves into communication with the several official bodies, and with numerous private practitioners in this country, and, by means of the British ambassadors, with the various institutions on the Continent. The result is a most complete account of the prevalence and mortality of small pox in different countries, and of the means taken to guard against it through vaccination. The general result of the investigation is, that the protective influence of vaccination is affirmed, and that the virtual obliteration of small pox is proved to be a matter of easy accomplishment for those who will adopt the means of doing so which Jenner pointed out. These conclusions are deductions from the largest mass of statistical evidence which has ever been brought to bear on the question. The Registrar-general's returns, the documents of the Poor-law Board, official statements from Mr. Wilde for Ireland, from Dr. Stark for Scotland, and from more than twenty foreign governments, form the staple of the Report. It would be extremely difficult for us to present an abstract of the tables, but the following statement will give their main features.

1. To prove the influence of vaccination in England:

Out of every 1000 deaths in the half-century from 1750 to 1800, there were of small pox	96
Out of every 1000 deaths in the half-century from 1800 to 1850, there were of small pox	35

Either small pox has become naturally milder, or is treated more successfully, or the mortality from it has been diminished by vaccination. That the last is the true cause can be proved from independent evidence.

2. To prove the influence of vaccination on the Continent:

In various German states, sufficient evidence can be obtained to show that, before vaccination was used, out of every 1000 deaths, there occurred from small pox	66·5
After vaccination there occurred	7·26

3. To prove that in countries where vaccination is most perfectly carried out, small pox is least mortal:

(*a.*) In this country, where vaccination is voluntary, and frequently neglected—

	Deaths from Small Pox.		Deaths from all causes.
London	16	...	1000
Birmingham	16·6	...	„
Leeds	17·5	...	„
England and Wales	21·9	...	„
Paisley	18	...	„
Edinburgh	19·4	...	„
Perth	25	...	„
Glasgow	36	...	„
Dublin	25·66	...	„
Galway	35	...	„
Limerick	41	...	„
Connaught	60	...	„
All Ireland	49	...	„

(*b.*) In other countries where vaccination is more or less compulsory—

	Deaths from Small Pox.		Deaths from all causes.
Westphalia	6	...	1000
Saxony	8·33	...	„
Rhenish Provinces	3·75	...	„
Pomerania	5·25	...	„
Lower Austria	6	...	„
Bohemia	2	...	„
Lombardy	2	...	„
Venice	2·2	...	„
Sweden	2·7	...	„
Bavaria	4	...	„

That the immense difference apparent in these two tables is rightly attributed to vaccination, there can be no question.

The Report enters at some length into the comparative use or neglect of vaccination in various parts of England, and shows, as might be expected, that in many places the majority of the population are altogether unprotected.

These statements lead, of course, to the conclusion that the only proper course is to make vaccination compulsory. Whatever may be the defects of Lord Lyttelton's bill in some respects, it is undoubtedly one of the most important measures of the session. We are at a loss to know why it has met with so much opposition. Its defects will be easily amended; and if they were twenty-fold, it must be better to put up with them, rather than delay for another year the benefits of this wise measure.

PART THIRD.

Original Communications.

ART. I.

The Blood—its Chemistry, Physiology, and Pathology. By THOMAS WILLIAMS, M.D. Lond., Extra-Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea.

THE fluids and the solids in all biochemical inquiries should rightly be studied in conjunction. They interblend, substantially and dynamically, with such intimacy, that to indicate a partitional boundary were to divorce what nature has united. They reciprocally cede and accept. They mutually act and react. Fluidity and solidity are the first conditions of all organized matter. The aëriform state has nowhere a single illustration. Every gas must become a fluid as the fundamental term of union with the elements of the living body. The fluid is the active condition; the solid, the passive. The latter is sedentary; the former, locomotive. The former is the scene of rapid molecular transformations; the latter is distinguished for its stability: between these two extremes there exist manifold intermediate conditions. In the category of the nutritive fluids, unmixed fluidity is not known: solids are introduced. Fluids are not self-productive; they require either the direct or the catalytic agency of solids. The fluids even of the vegetable organism bear floating corpuscles. In the vegetable economy, it has hitherto been falsely supposed that the cells of the fixed tissues traversed by the fluids effect that change which, in the instance of the animal fluids, is accomplished by the self-borne corpuscles. The formative capacity of a fluid, in the abstract, is limited to a low standard. Albumen cannot mount into the condition of fibrine in virtue of any self-originated and self-sustained inherent molecular activity; the agency of a third body must be interposed. This is the "doctrine of the schools."

The blood is a wondrous epitome of fluids and solids. In complexity of composition it has no parallel. He who would resolve it into its components, must in himself unite the qualifications of the chemist and the microscopist, the anatomist and the physiologist. The chemistry of organized beings has now assumed the exalted rank of a rich and varied science. The microscope, at first a costly toy, has grown into its inseparable handmaid. Micro-chemistry is opening upon the mind like a vast panorama, opulent in wonders and boundless in limits. The microscope conducts the eye to the confines only of visible form; the formless eludes its scrutiny: it is, emphatically, an instrument fitted to define the geometric properties of bodies. Into the world of fluids it cannot penetrate. Fluidity is a condition of matter which it cannot appreciate. Of solids, it takes cognizance only of the state of quiescence; it does not reveal the "forces" impelling them to activity; it is an apparatus of inquiry into the statics, not the dynamics, of matter. In micro-chemical inquiries, its peculiar value consists in empowering the eye to read and register the changes of form and colour wrought by test-fluids upon solids: the knotted tangle of a multiplex compound is thus gracefully unwound; it is in this elegant manner that the mind arrives at a knowledge of the chemical constitution of bodies —even those so minute in mathematical dimensions as to occupy the remote frontier-line which divides the fluid from the solid state, the extreme verge of morphological substance.

From the arena of zoochemical research, in these unsuperstitious days, all substanceless divinities must be exterminated. The era of the *anima mundi* has become historic. The "essence of life," *vis vitæ*, *vital principle* (Prichard); *nisus formativus* (Blumenbach); *organic force* (Müller); *organic agent* (Prout); *vis medicatrix naturæ* (Hofman and Cullen); now number amid the effete jargon of an extinct nomenclature. The "illuminating orb" of a new mental life has arisen above the edge of the eastern horizon. "Abstract terms" are as luring as "golden calves;" they demand the homage of believers. The "*principle* of motion," the "*principle* of gravitation," the "*principle* of attraction," are the relics of an entombed terminology. The "*life*" of the blood is an ontological expression which belongs to a former phase of science. No *divinæ particula auræ* swims in this fluid. Modern philosophy displays a tendency to refer all phenomena to the *laws* of matter—*directions* of action first implanted by the Deity. A "law" in its ontological sense is a *deus ex machinâ;* it sounds as though endowed with an existence distinct from the body—as something superadded to the organism—like magnetism to iron, heat and light to a luminous body (*sic*). The word "*principle*" is characteristic of a less advanced state of science; it should *now* be used only as the final letters of the alphabet are employed by the algebraist—to denote an unknown element, which, when thus indicated, is more conveniently analyzed. It is customary, even now, to speak of the "principle"—the agent, of heat, light, electricity, magnetism, gravitation, &c.—as signalizing severally the undetermined causes of familiar phenomena. When these phenomena, in the progress of science, fall under a more just interpretation, they come to be referred to the primary properties of matter; they then may be deduced by demonstrative reasoning, like geometric theorems from the postulates on which they are founded. But in the science of physiology the term "law" has been employed in a less justifiable acceptation; a "principle," a "force" has been personified, and invested with a spontaneity of action. As an expression descriptive of the "conditions," the "circumstances" under which the actions and reactions of the material elements of the organism originate and proceed, it may be tolerated as a convenient phrase; the physiologist, however, idolatrously animates it with a creative and directive power, in virtue of which it acts upon matter, removing its particles from the pale of physical and chemical laws, transforming them into organized tissue, endowing these tissues with novel properties, prompting them to action, opposing resistance to injurious influences, defining the cessation of these acts of the organic body as synonymous with the departure of the "vital principle." A "law" ought not to be defined as altogether resident in the mind of the observer. The mind only links certain acts into a certain order of occurrence; this *order of occurrence* is coeval with matter; it moves, "displays its forces," "exerts its energy," only in a given direction. This *mode* of occurrence is the *law;* it is the final impress of the will of Him who made matter; it is an ultimate fact beyond which philosophy cannot penetrate. The task of the organic chemist, then, lies not in a search after an *ignis fatuus*, a *vital principle;* he is required only to define the terms of events, the conditions of phenomena, as they occur in the living organism. *Words* must be used to indicate events. The exigencies of language will oblige the use of such expressions as vital force, chemical force, affinity, property, &c. It will, however, be now understood within what latitude such expressions are to be limited.

Two new ideas have recently taken birth in science. The mutual convertibility of the "forces" is an accepted doctrine, and the transmutability of matter (Dumas, Faraday) is no longer held as an alchemic extravaganza. The facts of allotropism, established by the genius of Schönbein, and the late prophetic speculations of M. Dumas* on the chemical, electrical, and mathematical *progression* traceable through the properties of isomeric or conformable bodies, awaken in the mind of the modern chemist a reverence for the disentombed manes of alchemy. The transmutable bodies group themselves in nature in triads, or ternary series, thus: chlorine, bromine, and iodine; sulphur, selenium, and tellurium; calcium, strontium, and barium; lithium, sodium, and potassium. The members of these triads, severally, are capable of replacing one another in chemical compounds. "When three bodies having qualities precisely similar, though not identical, are arranged in succession of their chemical powers, there will be also a successive arrangement of mathematical powers, indicated by the respective atomic numbers of the substances, and amenable to every mathematical law." "That this symmetry of chemical with mathematical function points to the possibility of 'transmutation' is unquestionable, yet not transmutation in the sense of the old alchemical philosophy." Chemists see no manifestations of a tendency of being able to convert lead into silver, or silver into gold. These metals are not chemically *conformable;* one cannot take place of another by substitution; they do not form an isomeric group.

The preceding illustrations are drawn from inorganic bodies. Chemists have long believed that certain *organic* compounds display, in their properties, a close resemblance to metals. Of this kind are the three organic radicals—

$$C_2\,H_3\,O$$
$$C_4\,H_5\,O$$
$$C_6\,H_7\,O$$

which may be regarded as the three several oxides of an isomeric triad, bearing analogy to those already adverted to in the inorganic world. With reference to these radicals (omitting the oxygen) it is found, as in the case of the inorganic triads, that the sum of the atomic weights of the extreme bodies divided by two is equal to that of the intermediate body.

* See Chemical Record, July 12, 1851; and Lectures on the Non-Metallic Elements, p. 160, by Faraday.

The discovery of allotropism has bereft these speculations of impossible extravagance. The allotropic modifications of which sulphur, phosphorus, oxygen, and carbon, are susceptible, suggest the thought that the countless array of organic compounds which now bewilder the chemist, may prove only modified forms of one unchanging radical. Like those of isomerism, the phenomena of allotropism inspire the zoochemist with new hopes.

Solidified albumen and fibrine are allotropic conditions of liquid albumen and fibrine. Physically the former differ remarkably from the latter, chemically they are identical. "There was a time when the doctrine which supposed the convertibility of metals was opposed to known analogies; it is now no longer opposed to them, but only some stages beyond their present development." (Faraday.) These discoveries have been accomplished, not by the magic touch of the philosopher's stone, but by the touch of genius.

Coincidently with these recondite speculations as to the transmutability of *matter*, have been projected novel thoughts as to the "mutual correlation and convertibility of '*forces*.'" (Grove.) Heat, light, electricity, magnetism, motion, &c., are severally interproducible "forces." (Faraday). "Vital action," cell-growth, nerve-force, muscular-action, *are the* physical imponderable forces modified in manifestation by passage through an organic material substratum. (Carpenter.) Heat becomes vital force by passing *from* without *into* the egg. Such expressions imply the locomotion of an entity. Against the vague use of this hypothesis-involving-language, the student of organic chemistry should jealously guard. He must be warned once more, lest he confound the *ego* with the *non ego*—the conditional with the absolute and unconditioned. All general ideas are born of abstraction. They should be permitted in science only as intellectual guides. The "forces" of matter are the "properties" of matter. The quality cannot be separated from the body. The word "property" in chemistry should be accepted in a phenomenal, not entological sense. The dynamics of matter cannot be studied apart from the statics. Though, however, the student be warned against the idolatrous worship of all bodiless essences; though it be affirmed that the physical philosopher is in pursuit, not of fictions, but facts, not of subjective creations, but objective realities, it must be understood that the intellectual "aid to pursuit" to be drawn from an intelligently conceived theory is not to be contemned. A "theory" is not an illusive palace raised by fancy; it assists in two ways; it guides the mind in the marshalling of novel phenomena under known analogies—and it dictates method, suggests conditions, devises experiments. It ought not to trammel the freedom of research, nor circumscribe the range of just speculation. It is a mental light, an intellectual instrument of inquiry. It is the bright path projected by forethought into tracts of research yet unsurveyed by the eye of science, and uncatechized by experiment.

"Chance" may have imparted motion to the classic chandelier in the Cathedral of Pisa; but if chance also suggested to Galileo the laws of the pendulum, it must have belonged to that multitudinous order of casualties by which ideas are ordinarily propagated in fit and fertile minds. Theoretic previsions had qualified the mind of the philosopher to evolve an immortal principle out of an accidental event, to generalize a "law" on the basis of an accident. "Facts," uninterpreted by "theory," are inert, incoherent verities.

A *vital fluid* is different in no single particular from any cosmical compound fluid. The constituent elements of an organic fluid completely inter-blended in the living organism, manifest the same laws and the same properties with those distinctive of them while yet within the region of inorganic matter. In the body they acquire no *new* properties. The *old* are only modified, intensified, neutralized, co-ordinated with, or subordinated to, "conditions" of action known nowhere but in the living body. An organic substance may be composed of five different elements. Each element may be present in a great number of equivalents. The number of the elements added to the number of equivalents explains the complexity of organic compounds:—thus,

	Carbon. Atoms.	Hydrogen. Atoms.	Nitrogen. Atoms.	Sulphur. Atoms.	Oxygen. Atoms.	Total. Atoms.
Albumen . . .	216 ...	169 ...	27 ...	2 ...	68 ...	482
Caseine . . .	288 ..	288 ...	36 ...	2 ...	90 ...	544
Fibrine . . .	216 ...	169 ...	27 ...	2 ...	68 ...	482

A "force" exerted in *one* direction—i. e., between *two* elements—unites them in the closest and strongest manner. The result is a compound which is gifted in the highest degree with the power to resist the causes of perturbation. To this compound if one more element be added, the uniting force is reduced to one-third—it is now exerted in three different directions; if a fourth constituent be added, each line of force will be four times less than the sum of the power by which the first two elements were joined. If this reasoning be prosecuted to the limit of 500 atoms, it admits of no doubt that the power will remain the same in *nature,* though multiplied in *directions* of action. The centre of force remains unchanged, though the radii therefrom may diverge to numerous points of an embracing circumference. Where, then, is the limit to be inscribed between vital and cosmic chemistry, between organic and non-organic affinity? Is it at 8, or 10, or 20, or 500 equivalents? Is it at the fifth element, or the fiftieth? Is it at carbonic oxide ($C_2\ O_2 = 28$) and cyanogen ($C_2\ N = 26$), or at starch ($C_{12}\ H_{10}\ O_{10}$) and sugar ($C_{12}\ H_{11}\ O_{11}$)? Where between oxalic acid and those composite products containing sulphur and phosphorus, can the chemist mark the bound beyond which the physical force does not ascend? In the formation of chemical compounds, "organic principles," it is quite certain that the supposed line cannot be drawn which is said to divide physical from vital affinities. Thus, then, the instability, the ready decomposableness, of organic compounds, is a property which flows from the multiplied directions in which the uniting power operates upon the constituent atoms. In an atom of sugar the attraction radiates in 36 different lines—in one of olive oil, in many hundreds. But this denotes merely a difference of *number* in the combining elements. Is there no difference of *kind,* of arrangement? Without adding or withdrawing any element, we may conceive the 36 simple atoms of which the atom of sugar consists to be arranged in a thousand different ways; with every alteration in the position of any single atom of the 36, the compound atom ceases to be an atom of sugar, since the properties belonging to it change with every alteration in the manner of the arrangement of its constituent atoms. It will be subsequently proved that whatever may be the groupings of elementary atoms, no *new* force will be required to explain their altered disposition. There is, however, in the study of organic structures, a point at which the evidences of a *new* power are encountered. It is in accordance with analogy to suppose that albumen, fibrine, gelatine, &c., owe their formation to the play of purely chemical affinities. The power which moulds a cell, configurates a fibre, shapes the tubularity of a vessel, transcends the conceptions of the pure chemist. It cannot be experimentally catechized, it cannot be imitated by any artificial arrangement. It is a force *sui generis*. The corpuscles of the blood assume the discal figure, not mechanically, in virtue of a rotatory or elliptic motion like the planets in the orrery. In the latter case the *figure* may be predicated from a calculation of the attractive and centrifugal forces. The oblate spheroid is a *necessary* figure. In the attempt to explain the *form* of organized solids, all mathematical and mechanical conceptions utterly fail. Wherefore the discoidal shape of the blood-cell? why the elliptical in the fish? *This is the vital force;* chemistry compounds the materials, vitality chisels them into form. Chemistry unites the elementary atoms into composite groups; vitality utilizes these compound groups, and no other, in the fabrication of the solids. The floating cells are the only parts of the fluids which, in this sense, exhibit the evidences of a vital force. The hæmatosine and globulin and cell-walls are chemical products. "Vitality" gives to these products a special arrangement; but no other "principles" could be shaped, even by the vital force, into corpuscles. It requires conditions of action as well as every other force. There is, therefore, some mysterious relation between the *shape* and the *substance*, between the *material* and the *form*. The cells of the blood, though the constructive principles were provided, can be formed nowhere but in the blood—no other locality offers the required conditions. Muscular fibre can only be formed out of previously prepared compounds, and *then* only in a particular place. Thus obviously governed by "conditions," the *vital* force, as much as any other force, becomes a legitimate object of investigation. Allied intimately to the chemical force, it discovers a mechanical and mathematical mode of operation. In this inquiry the difficulty has hitherto proved insurmountable, of ascribing effects to their true causes—of explaining the share which belongs to each "force" in the arrangements which constitute life.

The organic *fluids* fall exclusively under the dominion of chemical laws. Under chemical circumstances, the atoms of bodies are more inclined to unite in simple than complex numbers. These combinations are always regulated by definite proportionality. Organic compounds do not always obey this law. The union of mineral bodies with organic principles is often governed by no proportionality. Phosphate of lime in union with osteine is regulated by no limits. It is possible to separate a large part of the non-metallic element contained in the protein compound without destroying the distinctive characters of the principles. It is not yet certain whether these elements in albumen and fibrine are chemical or mechanically mixed. The protein compounds have not yet been separated and demonstrated in a chemically pure state. Albumen is intimately blended with the salts of soda. That they are chemical compounds *at all*, cannot, in the present state of organic analysis, be affirmed with confidence. The elements may be held in union by a homogeneous, not heterogeneous force. They may yet prove to consist of peculiar mixtures, rather than chemical compounds.

In the arena of organic chemistry the *elementary bodies*, as such, enact only a subordinate function. Oxygen, in its unmixed form, cannot be "organoleptically," histogenetically, employed under any circumstances. Nature fabricates with compound atoms, not with simple. The simples first unite to build compounds—*then* they are qualified. The composite atoms react upon composite atoms, and these upon others. With each complication the chemical force is replaced more and more by another, which undoubtedly *grows out of*, *is directly developed from*, the former. *Chemical* force is inversely as the atomic weight; *vital*, directly. This is a beautiful generalization. The chemistry of organized beings is the chemistry of compound bodies, not of simple. The solids can only be constructed by means of compounds. In the fluids the *elements* are arranged into compounds. The chemistry of the fluids concerns itself chiefly with mineral bodies and their unmodified properties, that of the solids exclusively with peculiar compounds. The individual atoms of bodies are *in motion* in the organism from the instant of entrance to that of exit. The momentum acquired by such a motion impels them to that *mode* of union which is distinctive of organic substances. Vital force appears thus as the higher evolution of the chemical. Whether, with Newton, Guyton, Morveau, and Buffon, chemical affinity be regarded as due to "universal attraction;" or with Berthollet, to an attraction of "a peculiar nature;" or with Berzelius and Sir H. Davy, to the electrical properties of bodies,—it is certain that chemical substances unite and disunite, move and fix themselves, *in* the organism and *out*, in obedience really to the same laws.

The fluids of the higher animals are more complex than those of the lower; the solids are so also. The non-proportional constitution of albumen, as will afterwards appear, favours the ready renewal of the fluids in the case of the lower invertebrata. In the blood of the higher animals, albumen, fibrine, and the oils, by endowing this fluid with a thickness and viscidity, diminish the rapidity of molecular movements, and the freedom with which, if swayed only by their chemical affinities, the mineral constituents would interchange places. *Viscidity* becomes thus a mechanical condition capable of imparting a certain measure of permanency and stability to the composition of the blood. This quality is due much more to the fibrine than to the albumen. The fluids of the lower invertebrata are almost entirely devoid of fibrine; they are much thinner, less viscid, and of far lower specific gravity. The lower animals *live faster* than the higher; the operations of life are more rapid, and briefer in duration. Complex compounds demand for their production more time than the simpler. Hence the harmony between the physical characters of the organic fluids respectively in the lower and higher animals, and the *rate* of the nutritive operations, the durability of the resulting products, and the "term of life."

Organic fluids differ from inorganic in a marked manner in the greater frequency of the phenomena of contact or catalysis. A feeble acid will change starch into glucose; it excites without participating in this mutation. Hydrogen and oxygen left to themselves, even in presence of strong sulphuric acid, will not combine—spongy or leaf platinum will instantly initiate that combination; this is a phenomenon of simple contact, catalysis. Pure sugar in solution will remain unchanged—added, dilute sulphuric acid, it appropriates water in the proportion required to form glucose; the acid remains unaltered. Caseine or gluten placed in a solution of glucose, the latter will transform itself into lactic acid, without taking anything from the former. Sulphuric acid mixed with alcohol produces sulpho-vinic acid—*this* distilled, gives ether and sulphuric acid—the acid is unchanged; this is *not* catalysis (Liebig); the acid takes a chemical part in the process.

All azotized substances exposed to the air will absorb oxygen, and reject carbonic acid; if mixed with sugar in solution, the latter will be transformed into alcohol and carbonic acid; the azotised matter in a state of change cedes nothing *material* to the sugar—but to its atoms it gives *motion*. Berzelius contends that the passive contact of the ferment initiates the fermentation; Liebig holds that the motion of the atoms of the ferment propagates itself to those of the fermentescible body. Catalytic excitants may cause the union as well as the *dis*union of elements. Oxide of silver *de*composes per-oxide of hydrogen; platinum determines the *com*bination of oxygen and hydrogen; the word catalysis is, therefore, not strictly applicable. Faraday has recently projected the splendidly simple thought, that the part of the catalytic agent is really *electrical;* it acts as a conductor of electricity between the two uniting bodies; thus in science has genius dissipated another arcanum. The theory of catalysis finds frequent illustration in the metamorphoses of organic compounds; its application will be hereafter shown.

The very essence of zoochemistry may be thus stated *in summa, like produces like*—organic principles produce organic principles. This may be called homologous catalysis; organized beings do not, cannot create, they cannot transmute elements: they can only undo, make anew, construct compounds out of pre-existing simples. The vital fluids, unlike inorganic solutions, never subside into the quiescence of chemical and molecular equilibrium; the union and disunion of the elements are ceaseless; the play of affinities is complexly various; the blood is, nevertheless, an organic unity. No single element can be withdrawn without destroying that unity. Then how is this wonderful fluid to be studied? If dried and burnt in total substance, and the *ultimate* products collected and weighed, the whole might readily be tortured into the fantastic shape of a chemical equation; or by an analysis less extreme, all the bases, and acids, and organic compounds, might first be determined, and then an empirical formula might be constructed, expressive of their relative positions: little advantage would result from either of such methods. An exclusively chemical or analytic investigation will prove alike unproductive; the purely physiological is no less inadequate; assistance must be drawn from both departments of study. The inquiry, then, must embrace the following questions:—1. What are the *elements?* 2. What are the *groups* which are formed by the union of these elements? 3. How are these groups (compounds) related among themselves? 4. *How* and where do they become parts of the solids? 5. In what manner do they return into the current of the fluids, and finally escape as *excreta?*

It is indispensable to the intelligent pursuit of this inquiry, in this place to demand by what *names* are the constituents of the blood to be distinguished; their *chemical* designations will of course remain; but is albumen to be called an "element" or a "principle"? An element is an undecomposable, indivisible substance. Albumen, though a chemical compound, is an organic atom; take *any* part away, and it ceases to be albumen. Is oxygen a "principle," or an "element"? It is no real part of the blood until it enters into chemical union with some one of its constituents; it must surrender the gaseous and assume the fluid form. In the state of gas, it is literally an extraneous body; it is only in readiness *to be* used; it nowhere constitutes an anatomical part of an organized substance; it is in itself nothing; it must first merge its individual properties in those of the compound of which it is to form an element; it enters into chemical union with almost every constituent of organized beings. But is it, because it can be detached again by violent means, extracted with all its original properties, to be called a "principle"? If it be denominated a "principle," it will lead to endless confusion to call also a "principle" the compound, of which it is but an element. What is sulphur out of its place in the protein compounds, whether there oxidized or not? How different its organic value in the sulphates! What is nitrogen out of its position in the azotized groups? In the language of Chevreul* it would be called a "*mediate* principle," and the compound into which it enters, an *immediate* principle. In the recent work of Robin and Verdeil,† these acute authors distinguish both the element and the compound as "*immediate principles.*" On what ground? On that, that *all* substances which can be extracted from the animal body "without decomposition," are entitled to be classed as immediate principles! Thus the chemically undecomposable, and the organically undecomposable, are alike to be called "immediate principles!"

Oxygen is not a histogenetic substance, it expends its force upon the fluids. Here the organic compounds are formed, from which the solids are afterwards constructed. These observations apply with undiminished force to the *salts* concerned in the production of the organic compounds. Let it be supposed that oxygen, hydrogen, nitrogen, and carbon, were so grouped as to form an "organic radical;" it could not exist as such in its pure and unmixed state; it must be fixed either by chemical union or mechanical alliance with an inorganic body. Lime is united to osteine in the bones; potash to musculine in the muscles; soda to albumen in the blood; phosphorus to neurine in the brain, &c. The living body is thus not only "a superstructure of physics on chemistry" (Arnott), it is a fabric raised of organic compounds on the basis of mineral bodies. Then, are the elements, oxygen, hydrogen, carbon, and nitrogen, which constitute the very substance of a protein-compound, the compound itself, as well as the mechanically allied mineral body, indiscriminately to be ranked as "immediate principles"? Are creatine and urea, which are *out*going, and albumen and fibrine, which are *in*going substances, alike to be termed "immediate

* Récherches sur les Corps Gras d'origine Animale. Paris. 1803; and Considerations sur l'Anat. Organique. Paris, 1824.

† Traité de Chimie Anatomique et Physiologique Normale et Pathologicale, &c. Paris, 1853. Though I may lament the diffuse verbosity of this work, I would yet signalize it as one which I have studied with the greatest advantage. Less cautious than Lehmann, in their very original work Robin and Verdeil discover more genius.

principles"? Is the answer, that they acknowledge a diversity of source, that they discover differences of properties, and tend towards unlike destinations, of valid avail? What in the living organism is *not* of mineral origin? Is the oxygen inspired, in breathing, directly from an inorganic source, forbidden to form part of the protein-compounds in the blood? If not, why should not the albumen of the serum be indicated as a "principle of mineral origin"? It is augmented by an element immediately drawn from without. Oxygen, hydrogen, nitrogen, and carbon, are really the only elements essential to the production of "organic principles." The acids, the earthy and alkaline bases, and the non-metallic bodies, are accessory, though important constituents.

Let, then, the word "element" retain its legitimate chemical signification; let the oxides, acids, and salts be denominated "mineral compounds;" and let all adipose and nitrogen products—whether progressive or regressive; whether crystallizable or not—be ranked in the chapter of "organic *principles*:" what, then, does the blood contain?

1000 *parts of blood-corpuscles contain*		1000 *parts of liquor sanguinis contain*	
Water	688·00	Water	902·90
Solid constituents	312·00	Solid constituents	97·10
Specific gravity 1·0885.		Specific gravity 1·028.	
Hæmatin	16·75	Fibrin	4·05
Globulin & cell-membrane	282·22	Albumen	78·84
Fat	2·31	Fat	1·72
Extractive matter	2·60	Extractive matters	3·94
Mineral substances (without iron)	8·12	Mineral substances	8·55
Chlorine	1·686	Chlorine	3·644
Sulphuric acid	0·066	Sulphuric acid	0·115
Phosphoric acid	1·134	Phosphoric acid	0·191
Potassium	3·328	Potassium	0·323
Sodium	1·052	Sodium	3·341
Oxygen	0·667	Oxygen	0·403
Phosphate of lime	0·114	Phosphate of lime	0·311
Phosphate of magnesia	0·073	Phosphate of magnesia	0·222

This tabulated view of the quantitative relations of the principal constituents of the blood is founded upon the analyses of Lehmann* and Schmidt.†

All the constituents of the solids must once have existed in the fluids: the analysis of the former would not, however, express the composition of the latter. In the solids, the organic and inorganic compounds enter into different and new combinations. Though in ultimate analyses these two parts of the animal body present a striking resemblance, in proximate they widely contrast. Many of the saline compounds discoverable in the living fluids exhibit the same properties here as in lifeless solutions; they obey the same chemical affinities. The chlorides of sodium and potassium; the

* Physiological Chemistry, translated by Day, vol. ii. p. 160.
† Characteristik der Cholera, quoted by Lehmann.

carbonates and bicarbonates; sulphates and phosphates of lime, soda, potash, magnesia, and ammonia, though always in solution while in the organism, may yet be separated from the fluids by crystallization and volatilization. That chemical or organic compound which admits of extraction by either of these proceedings *without* decomposition is entitled to rank as a component "principle" of the fluid or solid from which it was derived. With Robin and Verdeil it is important to distinguish between those bodies which are drawn ready-formed from external sources, from those which are products of the chemistry of the body: to the latter class belong the urates, hippurates, lactates, oxalates, isonates, pneumates, creatine, urea, sugar, &c. They exist only in organized bodies, to the agency of which they owe their formation, and from which many of them are rejected externally, while others are resolved into their elements while yet in the organism; they sink by decomposition into simpler and simpler bodies; they are on their passage out of the organism.

The living fluids comprehend another class of constituents, to the presence of which the former owe their distinctive qualities—viz., albumen, fibrine, and the fatty principles. It is from these that the organic bases of all the solids are derived; osteine, musculine, globuline, cartilagène, &c., are really the former, modified by altered chemical conditions. In the organism they exist in the fluid or semi-solid form; they are neither crystallizable nor volatile without decomposition; in chemical composition they are non-proportional; they constitute the essential substance, or material substratum, of all organized bodies; organized beings alone offer the conditions essential to their formation. The act of solid nutrition may be almost defined as that of the transition of these principles from the fluid into the solid state.

Under the head of the fatty principles of the fluids will come hereafter to be examined the saponifiable fats (margarin, olein), the phosphorized or non-saponifiable fats, and cholesterine and serolin. The analyses of Becquerel and Rodier have proved that these exist in the blood. Berzelius has shown, what Lehmann has confirmed, that the phosphorized fats (analogous to the glycero-phosphoric acid discovered by Gobley in the yelk of egg) accumulate principally in the blood-cells. The cells of arterial blood are poorer in fat than those of venous. Fat also exists in the liquor sanguinis, united intimately but mechanically to albumen and fibrine. In the fluids of the invertebrata the adipose elements are present in far greater relative amount than in those of vertebrated animals. Professor Quekett has affirmed the general principle, that the "adipose cell" does not exist in invertebrate animals. Neither of the solids nor of the fluids is this quite true. The floating cells abounding in the chylaqueous fluid of the acalephæ, echinoderms, and especially of the annelids, are *chiefly* laden with fat; they *secrete fat* as literally and obviously as the liver-cells of the cod. "Fat certainly does exist in insects, crustacea, and mollusca, but no true adipose cell is ever present; it could not be nourished without its accompanying bloodvessels, and these are not found in invertebrata. The tissue resembling adipose tissue usually belongs to the liver or other glandular organ, and the fat exists in its cells in the form of oil."* From this passage it is evident that Professor Quekett has incorrectly observed and erroneously interpreted the mechanism of nutrition in the invertebrated animals; in

* Lectures on Histology, by Professor Quekett, p. 182.

them the solids are penetrated and pervaded as intimately by the fluids as in the example of the vertebrated animals. The adipose tissue in insects is bathed and traversed by fluid; the walls of these areolæ *secrete* the fat from the circumambient fluid; though they do not constitute "*closed* cells," they notwithstanding perform the functions of adipose cells; but it is quite extraordinary in what abundance the adipose principles accumulate in the *hepatic* cells of the invertebrata! If these cells were found floating in the fluids they would be undistinguishable from those morphic elements which form the normal constituents of the latter. Fat, then, enacts a constant and prominent office in the secernent agency of cells, alike fixed and free, in the invertebrata. Upon the basis of these facts, developed under the guidance of logical analogy, the argument may be reposed which contends that the cells of the blood, though not sedentary, should physiologically rank as true glandular agents. In the invertebrata, fat is transferred in great part from the solids to the fluids—*fibrine* and its modifications from the fluids to the solids. Schultz and Schmidt have recently supposed that fibrine occurs as a *separate* principle in the following manner: "As the blood escapes out of the body, an acid albuminate of soda which is dissolved in it becomes disintegrated into its component parts in such a manner that a less acid, neutral or basic albuminate of soda remains dissolved, while the other atom of albumen separates under the form which we name fibrine, and fibrine subsequently contracts to the smallest possible volume—just as freshly-precipitated silica, alumina, and phosphate of lime, gradually contract."* The researches to be subsequently noticed in these papers will constrain the physiologist to adopt one of two conclusions: either that in the fluids of the *lowest* invertebrata fibrine has no existence as a separate principle, or that, on the hypothesis of Schmidt, those chemical conditions do not occur on which the separation of fibrine as such from the fluids depends—a conclusion which implies that fibrine and albumen are essentially identical bodies.

Insurmountable difficulties will oppose the study of albumen as a "principle" isolated from the saline ingredients of the serum. They are inseparably united; they accompany one another in their respective physiological acts. Scherer has shown that the apparent properties of albumen vary according to the different proportions of alkali or salts with which it may be combined, the organic group of atoms in the albumen remaining unchanged. The neutral, basic, and acid albuminates of soda are marked by differential chemical properties. The several forms in which albumen coagulates will be found to be due to the varying amount of alkali with which it may be combined. Albumen stands in a far more intimate relation to the floating solids of the fluids than any other principle. *It is never* absent; it plays in the chemistry of organized being a primary and fundamental part. Albumen is remarkable in this respect,—that it is capable of entering the blood of even the higher animals unchanged by the agency of the gastric chemistry. Albumen-*peptone* is more like albumen than fibrine-peptone is like fibrine. To this fact extreme interest will attach in the study of the origin and composition of the nutrient fluids of the inferior invertebrata.

The *mineral* constituents of the *serum* of the nutritive fluids occur in

* Lehmann's Physiological Chemistry, vol. ii. p. 196.

the animal kingdom under remarkable diversities. In those inferior animals in which the circumfluent element is admitted *directly* into admixture with the nutrient fluid, the saline ingredients of the serum are identical with those of the surrounding medium. In the blood of the vertebrata the phosphates and potash salts predominate in the corpuscles—the soda salts, and especially the chloride of sodium, in the serum. The alkaline sulphates and carbonates exist in simple solution in the water of the serum. In the blood a considerable amount of carbonic acid exists in combination with alkaline bases. The methods of analysis used by Professor Rose have led him to the conclusion that the various compounds of sodium, potassium, calcium, iron, phosphorus, and sulphur, exist pre-formed in the organic substances in an *unoxidized* state, and take an important part in their metamorphoses. The carbonate and phosphate of lime are important ingredients in the fluids of the invertebrata. The polypidoms of the Polypifera, and the shells of the Mollusca, are composed almost exclusively of carbonate of lime; the calcareous casing of the crustacea, on the contrary, are formed as exclusively of the phosphate. It will be interesting, subsequently, to ascertain whether the fluids respectively in these classes present corresponding diversities.

The ash of all animal fluids abounds in the *phosphate of magnesia;* it is present in large quantities in the cerealia; hence its predominance in the blood of the herbivora. The intestinal concretions of herbivorous animals are composed of this salt. In the horse they consist of the triple phosphate. Like carbonate of lime, the functions of phosphate of magnesia in the organism are probably mechanical.

Chloride of sodium is an important constituent of the blood. To this salt albumen owes much of its solubility. It is capable of impeding the coagulation of fibrine, and of acting as a solvent for caseine. Distinguished from other inorganic bodies, its combinations are regulated in the organism by definite proportionality. Its proportion in the blood, under normal circumstances, is *constant;* in disease it varies. It is the source of the hydrochloric acid of the gastric juice. Boussingault has determined that the presence of this salt in the blood facilitates the deportation of excrementitious substances from the body.

The *alkaline carbonates* abound in the blood. In that of the higher animals those of soda preponderate; in the lower, those of lime. The bicarbonate of soda is capable of exerting a special power over albumen, augmenting strikingly its solubility—of impeding the coagulation of and chemically dissolving fibrine. Soda supplies a base for the acids formed in the fluids. The carbonates do not reach the blood as such; they are formed from the lactates. Upon their presence chiefly depends the alkalinity of the blood. This property of the nutritive fluids knows no single exception in the animal kingdom. The fluidity of the blood flows from its alkalinity; the walls of the bloodvessels are impermeable to an albumino-alkaline fluid. The free alkali acts as a resistance to many causes which, in the absence of the alkali, would coagulate albumen. Liebig has proved that the more alkali the blood contains, the higher the temperature at which albumen coagulates. The oxides of iron, and other metallic oxides, are rendered readily soluble in the blood by the action of alkali. The latter also exert upon the former the extraordinary influence of depriving them of their characteristic colours, such that a perfectly transparent solution results. This interesting fact explains the *colourlessness* of the fluids in the invertebrata. To this rule the blood proper of the annelida *only* is exceptional. The chylaqueous fluid of zoophytes, acalephæ, echinoderms, and annelida, contain iron; they are yet devoid of every trace of colour. The blood of molluscs and crustaceans is perfectly transparent; it yet affords ready indications of the presence of iron. In the *fluids* of the invertebrata, it may be stated as a rule, that the metallic oxides and their compounds exist under a colourless form; the cells of the *solids* exclusively are endowed with the pigment-forming power.

The blood of the vertebrate animal contrasts with that of the invertebrate in the abundance of its hæmatosine. This is the most palpable difference; other less striking, but not the less essential distinctions, divide the fluids of these two great kingdoms. One may be here stated by anticipation: in the fluids of the invertebrata, *one* order of floating corpuscles only exist; in the blood of the vertebrata, *two* are readily distinguishable.

The free alkali of the fluids serves also to promote the combustion of organic compounds, which in its presence acquire a power of combining with oxygen, a property which they do not possess at ordinary temperatures; thus, milk-sugar and grape-sugar, in presence of a free alkali, and with the aid of a gentle heat, deprive even metallic oxides of their oxygen. It is by the alkalinity of the blood that the metamorphosis of the malic, citric, tartaric, and other organic acids, used as food, is promoted; and the same influence is exerted even over uric acid, which, when introduced into the system from without, is speedily resolved into urea and oxalic. If, on the other hand, there be not an adequate supply of alkali in the blood, some of the vegetable acids (such as the gallic and tartaric) pass through it unchanged, and reappear in the urine: this is especially the case in carnivorous animals, whose food abounds more in the alkaline phosphates than that of herbivorous or omnivorous animals.*

The *alkaline phosphates*, especially that of soda, exist in considerable relative quantities in the blood. The analyses of Rose render it doubtful whether the phosphates are present really as such in the blood; they are probably products of incineration. Liebig maintains that the alkalinity of the blood depends more upon the existence of the phosphate than the carbonate of soda. The solubility of carbonic acid in the blood is remarkably increased by the presence of the phosphate of soda. Phosphate of potash does not possess this singular property; when phosphate of potash is brought into contact with the chloride of sodium, phosphate of soda is formed; this remains in the blood while the potash salt is appropriated by the muscles. Thus Liebig explains the constancy with which the amount of phosphate of soda in the serum of the blood is maintained under all variations of food. Potash also exists in the fluids in small proportion. In the blood of the fowl it was discoverable by Liebig in large quantities; in that of mammals it is inappreciably minute. Ammonia does not exist in the fluids; it is the characteristic of the excreted matters. The *alkaline sulphates* play an insignificant part in the composition of the fluids. The iron of the blood will be afterwards studied in connexion with the corpuscles.

* Researches on the Chemistry of Food, by Baron Liebig, pp. 115, 116.

Thus a brief sketch has been presented of the history of the protein-compounds, oleaginous principles, and inorganic bodies, which conspire to form the living fluids. At a subsequent stage of this inquiry, each of these divisions will be studied in detail; at this, general statements only are required; they will serve to bound the field of research, and to indicate the path of investigation.

The *floating cells or corpuscles* in chemical and proximate composition, differ remarkably from the fluid by which they are borne; this fluid may be indifferently distinguished as "the *fluid*," the *liquor sanguinis*, the *intercellular fluid* (Lehmann). The cells are the floating solids of the fluids; they are determinately organized structures, as truly so as any other living solid. Deriving from the liquor sanguinis their elementary parts, with it they yet contain scarcely a single principle in common. Though thus contrasted in proximate composition with the sustaining fluid, to the normal existence of the latter they are yet essentially necessary. Between these two elements of the blood there obtains an intimate mutuality of action and reaction. No single real example occurs in the whole animal kingdom of a nutrient fluid totally destitute of morphological elements. This rule is ultimate and absolute.* The origin, and office, and destiny, of these elements are time-honoured enigmas in science, which physiology has attempted in vain to unriddle. A new method of investigation will consist in modestly interpreting the experiments practised by Nature's own hand. Let the mind contemplate first the incomplex fluids of the humblest organisms; it discerns living fluids of the simplest chemical composition, and of less entangled relations to the organized fixed solids. These are circumstances which favour the solution of the refractory problem which relates to the mode in which the morphic elements of the fluids arise, grow, and disappear. These are questions which science has never answered. Theories are confusingly numerous; demonstration is utterly wanting. A correct apprehension of the conditions which determine the origin and propagation of the moveable organized solids of the blood, will lie at the root of all future discoveries in histogenesis. The causes inciting to *their* formation must prevail at the birth of every other organized solid, sedentary or free. The act of organization implies the transition of a soluble substance into an insoluble, of a liquid into a solid; the material is first precipitated, *then* shaped into

* It recently occurred to me, while prosecuting certain inquiries into the mode in which the corpusculation of the animal fluids takes place, to imagine that *vegetable* fluids would exhibit this process under terms of greater simplicity. Reducing hypothesis to demonstration, my first observations impressed me with unbounded surprise. I found at once *that the milk-sap of different plants was charged with floating globules of different size and conformation, that those of different plant-fluids are variously affected by the same reagents.* I claim no novelty for these observations. Schultz (Die Natur der lebenden Pflanze, 1823—28, and Sur la circulation et sur les vaisseaux laticifères dans les Plantes, 1839; and also, Die Cyclose des Lebensaftes, 1841) has diligently laboured in this field: I wish only to raise the subject to the eminence of a new point of view. Schultz saw in the globules-bearing milk-sap of vegetables the counterpart of the blood of animals. Hugo von Mohl ridicules this reputed analogy. The ridicule may yet redound upon the scoffer. The recent researches of MM. Melsens and Panum on the corpusculation of artificially-prepared fluids, render it almost certain that the plant-fluid *globulates* under the government of the mechanical conditions of *pressure* and *motion*. (For an account of these researches, see the admirable report, by Dr. Lyons, which appeared in the April number of this Review.) Since the globules vary in the fluids of different species of plants, a diversity in the chemical composition of these fluids is implied. In a subsequent number of this Review I will state at length the results of my inquiries into the history of the *corpuscles of the vegetable fluids.*

form; chemistry and physics preside over the scene. The constituent parts of the nutritive fluids, when traced from the base to the summit of the animal series, occur in the following order of frequency:—(*a*) water, and the saline substances dissolved; (*b*) albumen; (*c*) corpuscles; (*d*) oleus principles; (*e*) fibrine. Albumen is the first *organic* principle which is superadded to the inorganic saline solution, constituting the basis of *all* vital fluids. Corpuscles appear next, then the fatty principles, and lastly the fibrinous. If the corpuscles were literally an evolution of the fibrine in the line of a supposed progressive metamorphosis, *their* presence would imply necessarily that of the fibrine; the converse will prove to be true. In a limited number of instances, albumen occurs *without* corpuscles, these latter *never* without albumen. The history of the fluids in the animal kingdom will be prosecuted with more exactness and success, if first that of the floating cells be made the subject of separate and special investigation.

HISTORY OF THE MORPHOTIC ELEMENTS OF THE NUTRIENT FLUIDS IN THE ANIMAL KINGDOM.

It were here superfluous to enunciate the physiological canon, that in the lower organisms the processes of fluid and solid organization are less complex than in the higher. The elements concerned are fewer in number—of the fluids this remark is especially true. The more simple the fluid, the more comprehensible the reactions which occur between its constituents. Problems too involved to admit of solution through the study of the blood of the highest animals, assume an intelligible, because less complex, expression in that of the lowest. This is the ground on which it is preferred to commence the present inquiry at the lowest link in the chain of animal life. Among the invertebrated animals, *two orders* of fluids occur, which are distinguished from each other in several striking respects. The first is the *chylaqueous fluid*;* the second is the *blood-proper*. The chylaqueous fluid is always contained in the great splanchnic cavity, which is bounded on one side by the tegumentary parietes of the body, and on the other by those of the alimentary canal. It moves freely in this capacious chamber, and penetrates into all the dependent cells and channels of the organism. In zoophytes, actiniæ, and medusæ, this cavity openly communicates with that of the stomach, of which it forms the apparent prolongation. In the echinodermata and annelida it constitutes a perfectly-closed chamber, opening directly neither externally nor into the alimentary canal. A totally different rule applies to the blood-proper: contained in a closed system of vessels, it moves in a determinate circulatory orbit. The motions of the chylaqueous fluid are those of irregular oscillation, excited in part by ciliary, but chiefly by muscular action. The chylaqueous fluid presents gradations of composition; in its simplest form it is a very dilute solution of albumen in sea-water. The "corpuscles" have scarcely yet appeared; there prevails always a direct proportion between the amount of albumen and the number of the corpuscles. It is only in the highest or most

* I would here desire to explain that in the paper 'On the Chylaqueous Fluid and Blood-Proper of the Invertebrated Animals,' published in Part ii. 1852, of the Philosophical Transactions, I have given at length the history of discovery upon this subject, and that, although the *present papers* are founded upon *entirely new* researches, they prove essentially confirmatory of the views and facts stated in the essay to which I have referred.

developed grades of the chylaqueous fluid that *fibrine* occurs, and *then only in very small comparative amount.* True blood differs from chylaqueous fluid in this important particular—that the former never occurs in the animal series without fibrine; the latter *almost* always. The blood-proper of every known animal is corpusculated but that of the annelida. The blood of annelids is almost invariably coloured; pigment therefore does not necessarily depend upon the agency of corpuscles. The morphotic elements of the chylaqueous fluid are, for the most part, distended with highly-refractive oil; in those of the blood-proper this principle *never* assumes a visible form. Albumen, corpuscles, and fat, are thus intimately associated in the simplest type, under which the nutritive fluids of the animal organism occurs. These definite and incomplex facts are prophetic of discovery. The chylaqueous fluid and the true blood of every animal, alike fresh-water and marine, afford an alkaline re-agency. The organized solids are always acid. An acid nutrient fluid is not known in the animal kingdom. Facts of wide-spread application are "*laws.*" Here, then, are some of them. Sea-water is a rapid solvent of albumen. It is a circumstance of singular interest, that the vast multitudes of the lowest forms of animals are inhabitants of the sea; salt-water constitutes the bulk and basis of their nutritive fluids. How wondrous the adaptation between animate and inanimate nature!

The *fluids* have yet received no attention in those unicellular types of animal life—i. e., the gregarinæ, the amæba, the proteus, and the actinophrys sol, &c.—which stand below the zoophytes in the scale. The semi-fluid contents of these simple cell-structures must of course form at once food and blood. Under this type the porifera must be ranked. The animalized covering of the sponge consists of a congeries of cells, which are distended with a non-corpusculated albuminized salt water.

Polypifera.—Three types of structure prevail among zoophytes—the hydroidal, the asteroidal, and the helianthoidal.* The first includes the true zoophytes and the hydræ; the second, the alcyons; the third, the familiar actiniæ. In the zoophyte, the stomach opens directly by a sphincteric orifice into the fleshy axis of the polypidom; this is alike the case in the solitary and composite species. The transparent horny walls of the polypidom are separated from the fleshy axis by a very distinct interval, which is occupied by a hyaline areolar substance. The body of the polype, properly so called, consists of a stomach embraced by a tegumentary wall; these two cylinders are separated by an open space filled with a transparent fluid. The axes of the tentacles are the prolongations of this space; they are penetrated by the fluid contained in the latter; this fluid bears no visible floating globules. The fleshy axis of the polypidom is tubular in all hydroidal polypes; it is filled with a rapidly-moving corpusculated fluid. The motion of the corpuscles is most distinctly perceptible through the walls of the polypidom and the fleshy tube itself. The globules whirl in every direction—sometimes advancing, sometimes receding; now revolving in the same place, and now coursing spirally along the sides of the tube. This motion was studied by Lister; its existence has been confirmed by Sharpey and Van Beneden. The globules are moved by cilia: this fact is now unquestionable. Neither Lister, nor Sharpey, nor Van Beneden, could, how-

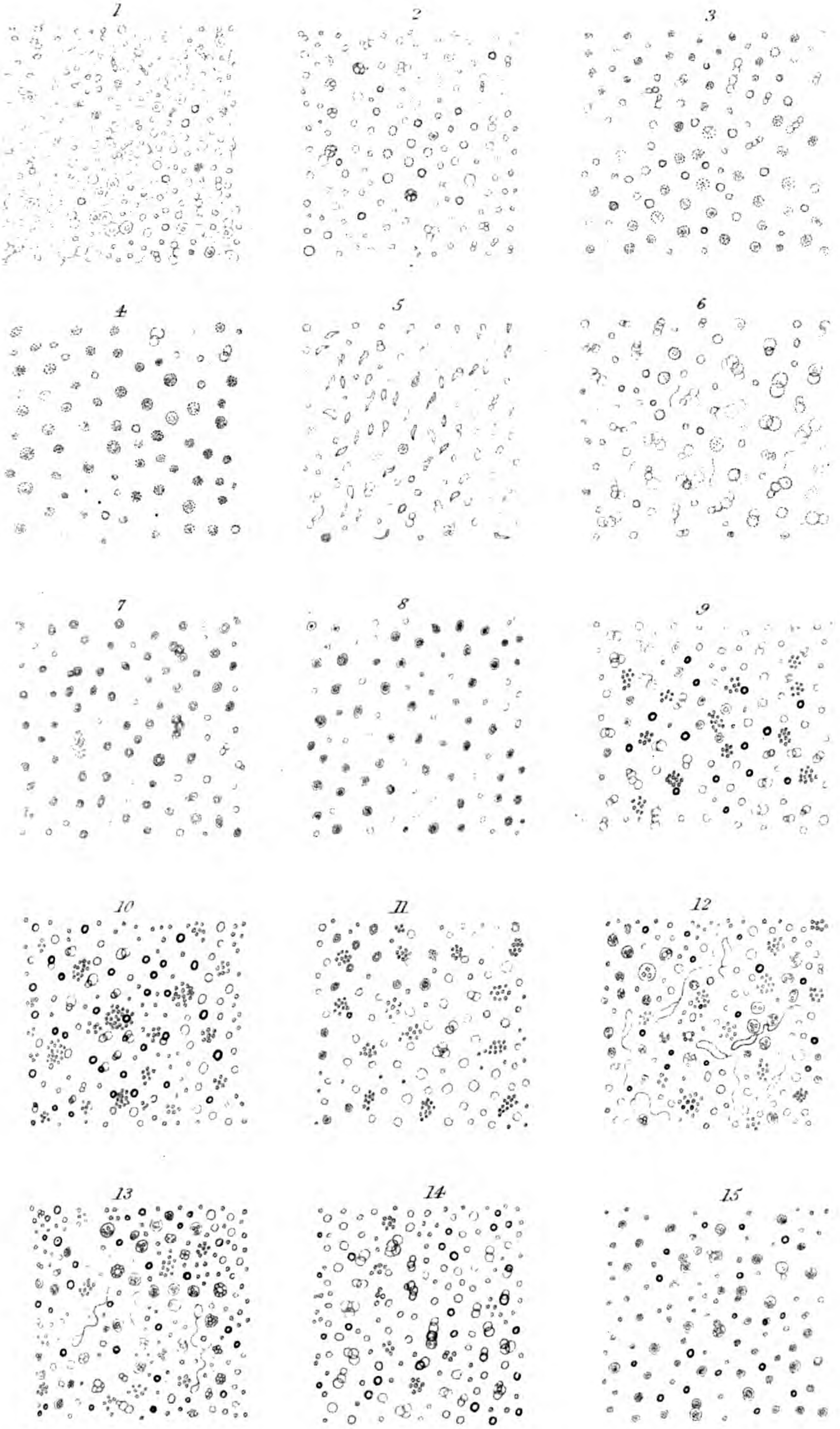
* History of British Zoophytes, by Dr. Johnson, 1842.

ever, succeed in demonstrating their presence. In the stems and branches of Laomedea geniculata they are convincingly conspicuous.* The floating-corpuscle so obviously present in the fluid, occupying the axes of the polypidoms everywhere, passes upwards into the space between the stomach and tegumentary parietes only when the polype fully distends itself: it is seldom seen in the axeal channels of the tentacula. The splanchnic cavity in some species appears to be divided from the polypidal tubes by a cribriform septum; it is then only the fluid parts which filter through; the corpuscles remain behind. This foreshadows the *first* attempt of nature to form true blood out of a chymous fluid. Here the corpuscles are no agents in the respiratory process; they do not penetrate the organs dedicated to this function. Neither the true splanchnic chamber, the interior nor the exterior of the tentacles, are ciliated in any hydroid anthozoa. The fluid in these processes differs in no sensible particular from that in the axes of the polypidoms but in the absence of corpuscles; they cannot, therefore, be distinguished as two distinct and independent fluids. There is a mechanical reason in some species against the admission of the globules into the canals of the tentacles—they are too large. It is probable that nature has imposed a certain limit of bodily dimensions on the cells of fluids, below which they cannot discharge the office for which they are designed. The illustrations of corpuscles accompanying the present communication were drawn as faithfully as possible, as they were seen in motion in the axes of the polypidoms. They constitute only varieties of one type. Figs. 1 to 8, Plate I., exhibit one general character; they are thick-walled vesicles, commonly spherical, sometimes compressed, bean-shaped, frequently nucleated, and now and then granular. They are in some cases intermingled with highly-refractive oleus molecules. These corpuscles bear the general character of inferior organization; they arise in an albuminous solution of salt water; they are invariably present; the conditions determining their production are therefore constant. It will not dissipate the mystery to affirm, with Ascherson, that they arise mechanically, as though by the contact of two homogeneous fluids (oil and albumen); or with Melsens, that the presence of certain salts precipitates the albumen; or with Gluge, that pressure and agitation will account for their origin. They present a definite typal character in all hydroid zoophytes. There is a speciality in the process of histomorphosis to which they owe their formation. Fig. 1 represents these bodies as they occur in the fleshy polypidom of Clava multicornis; fig. 2, Hydractina echinata; fig. 3, Sertularia polyzonias; fig. 4, Sertularia pinaster; fig. 5, Plumularia falcata; fig. 6, Plumularia catherina; fig. 7, Laomedea geniculata; fig. 8, Campanularia integra; all magnified about 400 diameters. These corpuscles may be noted as distinctive of the zoophytic type.

In the *hydræ*, it is easy to demonstrate the existence of a fluid charged with delicate pellucid globules, in the *cells* of the space between the stomach and the tegumentary walls. In the hydræ this space is not divided by parallel longitudinal septa as in the actineæ, but by intercrossing septa. At

* I have examined the greatest number of the species abounding along the coast between Swansea and Tenby. In none have I failed to detect cilia in the fleshy axes of the polypidoms. In Tubularia, Clava, Hydractina, and one or two more species, the walls of the polypidom are too dense and obscure to render the observation of the cilia satisfactory. In others, their existence cannot be disputed.

Plate I.



Williams del. ad nat.

J. Basire sc.

the roots of the tentacles the moving globules may be readily observed; they do not appear to traverse the axes of the tentacles. The perigastric space in the hydræ, as in the hydroid anthozoa, is destitute of ciliary epithelium. The corpuscles partake more of the character of oil-globules (figs. 9 & 10). In hydra viridis, they consist of a few granular spherules (fig. 9), intermixed with oily molecules. Those of hydra vulgaris discover the same character, abounding more in oil. In the *asteroid polypes*, the stomach of each polype is prolonged into canals in the polypidom. No corpuscles can be seen moving in the tentacles when they are being protruded by the limpid fluid of the splanchnic cavity. A thin section made by aid of Valentine's knife, parallel with the canals of the polypidom, and placed carefully under the microscope, will display the motion of cilia.

The corpuscles, as seen moving under ciliary action in the channels thus observed, are represented in fig. 11. They correspond with those of other zoophytes. They are little organized; they consist of spherical vesicles, mingled with oil-particles. Some few are nucleated, while others are charged with minute granules. It is difficult to conceive the nature of the office which morphic elements so simple are capable of discharging.

The tentacles of asteroidal polypes are said to be perforated at the extremities. This is scarcely possible. They are capable of expansion, through injection of fluid from the perigastric space into their axes.

The fluids in the *Actiniæ* admit of ready demonstration. Selecting the more transparent species, or young individuals, the following facts may be verified. While the little object lies on its side, the fluid contained in the chambers between the stomach and integuments may be seen every now and then escaping into the stomach, through an orifice situated at the bottom of this organ. The dissections of Dr. Cobbold* have recently confirmed the existence of this opening. The converse movement of the fluid may also be easily traced. The surrounding water enters the stomach, wherein it briefly sojourns, then passes through the opening at the bottom into the great cavity of the body; in this cavity it remains for a variable period; it now injects the tentacles. Corpuscles now arise in the fluid; it becomes thicker in consistence, through increase of its albumen; it is no longer pure, lifeless, salt water; it is a corpusculated, chylaqueous fluid; it is competent to subserve the ends of solid nutrition. Whence do the floating cells proceed? what produces them? Certainly no solid organ; neither liver nor spleen can in this case interpose its agency. Then, is it possible that there can inhere in albumen a mysterious histomorphotic power, in virtue of which it transmutes itself from the liquid into the solid condition? This were only a mode of enouncing "the theory of spontaneous generation."

Let *facts* accumulate. The *true* theory of the genesis of the floating corpuscles will in time arise.

In Actinia mesembryanthemum the corpuscles of the chylaqueous fluid are complex in comparison with those of zoophytes. They contain secondary cells and nuclei; they are interspersed with oil-globules and sperm-cells (fig. 12); they are not numerous in proportion to the bulk of the fluid. The cavity of the body and the interior of the tentacles, in all helianthoid zoophytes, are richly ciliated; in this particular they are dis-

* Annals and Mag. Nat. Hist., Feb. 1853.

tinguished from the asteroid and hydroid orders. The entire bulk of the fluid in these animals, when irritated, is emitted through the mouth. This faculty is peculiar to the Actiniæ. In zoophytes, the contents of the polypidom are not expelled externally on the retiring of the animal into its cell. The surrounding element is drawn again into the body with equal facility in the instance of the sea anemone. The marvel is, that fluid thus ingested and rejected so readily should prove adequate to the purposes of solid nutrition—that free cells should develop themselves in it with so much rapidity. The fluid contained in the splanchnic cavity of the Actiniæ (that is, if it have remained sufficiently long in that cavity) acquires in a very short time an impregnation of albumen; it becomes turbid on the addition of nitric acid.

The invertebrated animals generally are distinguished from the vertebrated in this respect,—that in the former the solids are more completely saturated with the fluids; the cells of the solids are larger, and more adapted to *retain* fluid than in the latter. This is more strikingly true of those classes, the Zoophytes, Medusæ, Echinodermata, and Annelida, in which the chylaqueous fluid constitutes more or less completely the liquid medium of nutrition. The inference is irresistible, that the fluids undergo a preparatory change in the interior of, or between, the cells of the solid structures; the fluids are *then* qualified for the work of histogenesis. This supposition explains the simplicity, the low, almost inorganic, type of their composition; it assigns a reason for the comparative absence of corpuscles from these fluids while yet within the great cavities of the body; it accounts for the non-development of a blood-vascular system in these animals; it renders intelligible what was before inexplicable—the mechanism of nutrition in these incomplex organisms. As nearly as it is possible to express in lines and points objects so delicate, fig. 12 shows the corpuscles of the chylaqueous fluid of Actinia mesembryanthemum; fig. 13, those of Actinia crassicornis; fig. 14, of Actinia bellis; fig. 15, of Lucernaria auricula: all magnified about 400 diameters. In morphic characters they fall *within* the zoophytic type.

The *Acalephæ* are constituted on the pattern of zoophytes. From them they differ in having no common splanchnic cavity. The stomach is prolonged into canals ramifying in the solid substance of the body; the canals receive their contents *directly*, through open orifices, from the stomach. Sea-water, with the organic matter, living or dead, which it may perchance hold in mixture or solution, is first drawn into the stomach; it sojourns there over the time required to subject it to the agency of the gastric secretion. It is then in great part admitted into the gastric canals; here it circulates to and fro, driven by vibratile cilia. Definitely organized corpuscles now evolve themselves; albumen increases in proportional amount, and the fluid becomes a nutritive compound. In these canals it is submitted to atmospheric agency; from them it passes by exosmosis into the cells of the solids. This fluid, in passing from the stomach into the canals, experiences the influences of no glandular organ: unless those cells constituting the walls of the containing canals. It signifies little whether the alimentary system be furnished with one or two openings; the character of the fluid contents remains the same. In the *pulmograde* orders it may be readily collected for examination: Rhizostoma Cuvieri is familiar along our coasts. In this species the corpuscles of the chylaqueous fluid are large and abundant (fig. 16); in their mature state they consist of compressed spheres; they bear two or three bright nuclei. The whole cell is filled with minute yellowish granules; other, simple, non-nuclear vesicles are interspersed. The parietes of the containing canals are internally villous; they furnish a digestive secretion, which promotes the chemical changes involved in sanguification.

The corpuscles of the chylaqueous fluid of the Medusæ depart strikingly from the zoophytic type; they are much larger; they are more laden with granular and adipose contents. They present a yellowish tint, indicating the dawn of a pigment-secreting property. They are furnished, in the mature state, with a dense involucrum; the "molecular base" is abundant. In the genera Aurelia, Pelagia, Chrysaora, Cassiopæa, and Cyanæa, the digestive canals are cæcal, as in Rhizostoma. The *ciliograde* acalephs present a modified arrangement. Here the digestive system is provided with an outlet as well as an inlet; the contents of the system coincide with those of the ciliograde orders. The little Velella limbosa is common in the bays of our seas; it exemplifies a cirrhigrade medusan; in it the entrance to the stomach is situated at the extremity of a sort of flask-shaped proboscis, the base of which opens into a cavity of somewhat cylindrical form, which lies in the direction of the length of the body, and subdivides at each end into two prolongations, from which a system of canals appears to pass off into other parts of the body. The corpuscles of the chylaqueous fluid in this beautiful medusan, like the animal, exhibit a slightly bluish tinge (fig. 17). In microscopic characters they do not materially differ from those of Rhizostoma. In Willsia stellata, a less common acaleph, the stomach is campanulate, and opens widely by four scarcely undulated lips. The surrounding ovaries impart to the colour a yellowish colour. From the stomach proceed six gastro-vascular canals, which subdivide as they descend to the margin of the subumbrella, and terminate in the tentacles. These canals are lined with a ciliary epithelium, and filled with a fluid thickly corpusculated. The floating cells are medusan in type (fig. 18); they consist of cells gorged with comparatively large secondary cells; they are tinted yellowish blue. Many are simple non-nuclear vesicles; the molecular base is abundant. In Turris neglecta, the floating cells of the fluid contained in the gastro-vascular canals are essentially similar to the former; they are, however, distinguished by the greater dimensions of the secondary cells; they dance amongst themselves, as though mutually repulsive. In Geryonia appendicula, the remaining example to be here cited, the cells of the chylaqueous fluid fall under the description already given; the involucrum is dense, the secondary cellules large and oleus, the vesicles and granules numerous (fig. 20).

The *acalephæ* mark the upper boundary of that division of the nutritive fluids in which the latter communicate openly and directly with the external medium. The surrounding water, salt or fresh, enters immediately into the splanchnic cavity, *the* cavity always in which the chylaqueous fluid is lodged: this fact constitutes a fundamental article of distinction between the fluids of the lowest animals and those of the higher. In the acelephs, this splanchnic cavity assumes the shape of canals—the gastro-vascular. The chambers and channels in which the higher grades of the chylaqueous fluid and the blood proper are included, are everywhere and invariably closed; neither the contents can escape, nor the circumambient element enter, in an immediate manner. From the arrangement, peculiar to, and distinctive of, the two lowest classes of invertebrata, the inference may be drawn that in *these* animals the whole *interæ* of the body may be penetrated by the external inorganic medium; no injury results from this penetration; contrarily, it forms the first condition of existence. But one striking additional fact should be stated as characteristic of this lowest division of chylaqueous fluid; *it contains no fibrine;* no coagulum forms in it; its corpuscles, which are chiefly impregnated with oleine, are the products of the histomorphosis of *the albumen;* they secrete in these degraded organisms no hæmatosine. This colouring and coloured compound is manufactured by the cells of the solids, not those of the fluids. The fact of the absence of fibrine from the fluid of the splanchnic cavity, which in these animals is the *only* nutritive fluid of the organism, there being present no trace whatever of a blood-vascular system, is corroborative of the theory, formerly stated, that the fluids acquire their final *histogenetic capacity* in the interior of the cells of the solids. It will be afterwards demonstrated that the semi-fluid contents of the floating corpuscles as they exist in the true blood of the higher mollusca and crustacea is a self-coagulating principle; if it be *globulin* in a colourless form, *then* globulin, like fibrine, is spontaneously coagulable. The free-cells of the fluids of zoophytes, acalephæ, are filled only with *colourless* principles; the involucra are also devoid of colour; the eye reads in their interior nothing but oleine. Ether extracts no adipose matter from the filtered intercellular fluid. The office of the floating corpuscles of the fluids in these inferior forms of life, may therefore be defined as that of converting the saccharine into the adipose principles—principles which, next to albumen, are most important in the requirements of solid organization. But let the question be now emphatically pressed—In a fluid thus constituted—of water, a few salts, albumen, corpuscles, and oleine—in what manner, and where, are the floating-cells generated? The fluid enters the stomach, sojourns briefly in contact with its *living* surface, proceeds into the interior of the peritoneal cavity, therein to oscillate, impelled by muscular and vibratile action; this is its history. In what manner, *but* through certain chemical relations into which the albumen enters, with the other principles present; where, *but* in the fluid itself, seeing that it receives the vital impress of no solid organ, can these bodies arise? Philosophic circumspection forbids at present the further flight of speculation. All that can with propriety be admitted in the glimmering light which immature science as yet affords, is, that under certain undefined conditions of development, of chemical combinations, and in presence of a living body, albumen *does* acquire the spontaneous power of passing from the fluid into the solid form. This limit bounds abruptly the light of chemistry. *Why* this "solid form," thus acquired, should, in different classes, orders, genera, and species, be differently configurated, is a question before which the genius of modern science lies powerless.

(To be continued.)

Art. II.

Decennium Pathologicum; or, Contributions to the History of Chronic Disease, from the St. George's Hospital Records of Fatal Cases during Ten Years. By Thomas K. Chambers, Physician to St. Mary's Hospital, London.

Part IV.—Diseases of the Heart.

The design of the set of papers, of which this forms one, is to employ for the fixing of pathological data a consecutive series of cases examined during the ten years ending December 31, 1850, at St. George's Hospital. The analysis is not carried on to the present time, principally because the more recent post-mortem books are in such constant request for clinical lectures and other purposes, that to borrow them for the necessary period would occasion much inconvenience. Moreover, as some day must be appointed for winding-up accounts, none seemed more appropriate than the conclusion of the decennium and the half-century; the definiteness of which date may, perhaps, induce the resumption of the task by the author or others on its future recurrence.

The whole number of bodies examined was 2161 in 2539 fatal cases; of which the 378 not opened formed a perfectly promiscuous class, as is known both from the habits of the hospital, and the fact that the proportions of their ages and sexes correspond closely to those of which an autopsy was made. The influences of age and sex on the whole number have been fully entered into in the 'Medical Times,' (July 24th and August 7th, 1852,) and will be repeated here only when the argument demands such quotation.

Of the same 2161 cases, those will here be made the subjects of analysis and comparison which exhibited after death some lesion acute or chronic of the central organ of circulation.

The condensation of materials which a course of argument by statistics allows, gives an opportunity for introducing into one paper the whole series of cardiac lesions, by which a picture of them is presented very conducive to the practical interests of truth. For nothing, I conceive, leads more straightly to the gaining those broad physiological views of chronic ailments, which aid us to manage them successfully, than the union, in the mind first, and then upon the paper which is its index, of diseases which have the same real nature; while the scattering of a subject in crumbs to the winds, or the tearing it limb by limb in the volumes of a dictionary, cannot but give rise to narrow views and pedantic treatment. The careful writer and the forewarned reader may and do guard themselves, but there is always this danger to be apprehended from the current literature of medicine, otherwise so beneficial to science.

The chief object of the arrangement to be adopted is, convenience of perusal, but at the same time, a presumption that such is in most instances the order of the succession of morbid phenomena gives it a certain degree of natural order.

A. *Malformations of the Heart.*

The proportionate frequency of occurrence of several of the commoner forms of *arrested development* in the heart, is probably fairly represented in the following table; but it is almost needless to say, that no implicit trust must be placed in statistics where the numbers are so small.

TABLE I.

Malformations of Heart.	With disease also of Valves.	With altered muscular walls of Heart.	Heart otherwise normal.	Total.
Foramen ovale and ductus arteriosus open	...	...	1	1
Foramen ovale open	...	1	4	5
Ductus arteriosus open	1	...	...	1
Aortic valves irregular	10	...	1	11

Narrow as is the field of observation here presented, it is sufficient to render probable these *Deductions:*

1. That patency of the foramen ovale has little, if any, influence for ill over the action of the heart.

2. That irregularly formed valves are peculiarly liable to become diseased.

3. That of all parts of the organ, malformation is most frequent in the aortic valves.

B. *Tubercular Deposit.*

This was found in 2 instances in the substance of the heart, in both of which tubercle existed also in the lungs in an unsoftened condition. The respective ages of the patients were 4 and 26; in the former, no change in the structure of the organ was observed; in the latter, there was a certain amount of *hypertrophy* and *dilatation* of the muscle, with *œdema* of the valves on the left side, very probably the consequences of the presence of a foreign body.

The pericardium also was affected by *tuberculosis* in the first of the above-mentioned cases.

The pericardium alone was affected without the heart in 5 other cases. In 3 of these there were *tubercles* also in the lungs; in 2 there were not. In one of those, where there were tubercles in the lungs, there was *tubercular peritonitis,* and in two, *tubercular arachnitis.* The respective ages of those 5 last cases were 3, 18, 20, 26, 44.

Deduction.—The whole number of cases of tuberculosis was 566 in the 2161 autopsies, so that the occurrence of this disease in the heart is clearly a rare occurrence, only provoked by the strongest diathesis, and at an age when general tuberculosis is most prevalent.

C. *Fibrous Tumour.*

Fibrous Tumour is recorded as having been found attached to the pericardium in one instance.

D. *Malignant Disease.*

In 179 cases of *malignant disease* which were examined, and at least 20 not fully examined, the heart was affected by the morbid growth in 7 instances.

In 3 of these, the substance of the muscle was implicated; in 1, the mitral valve; in 3, the pericardium only.

Of the first 3, in No. 1, the foot had been amputated for the same disease, and the inguinal glands were affected; in No. 2, the peritoneum participated in the cancerous growth; in No. 3, the thyroid gland, the adjoining parts of the neck and mediastinum. With the mitral valve were joined the mamma, the axilla, and the recti muscles.

Of the 3 where the pericardium chiefly exhibited the malignant condition, in No. 1, the pleura, omentum, ovaria, and skin were similarly diseased; in No. 2, the liver, uterus, and ovaria; in No. 3, the stomach, pancreas, the kidneys, and abdominal glands.

In 3 of the 7 cases of malignant disease, *pericarditis* was induced by the lesion.

Deductions.—I. As malignant disease must rapidly prove fatal whenever it attacks the heart, it may be presumed that the other organs found simultaneously diseased were its primary seat, and that its occurrence in the centre of circulation alone, is in the highest degree improbable.

II. It is somewhat more likely to fix in the heart than tubercle is.

E. *Adipose Tissue in Excess.*

Hypertrophy of the adipose tissue, that is to say, an augmented amount of *normal* fat, was found,

With other chronic disease of the heart, 32 times.
Without chronic disease of the heart, 17 times.

In the following table are divided the *concomitant chronic diseases of the heart.*

TABLE II.

Chronic lesion of Heart associated with hypertrophy of the adipose tissue of the organ.	Valves diseased.	Valves healthy, aorta diseased.	Valves and aorta healthy.	Total.
Dilatation	5	6	7	18
Hypertrophy	4	1	1	6
Dilatation and hypertrophy	4	...	1	5
Soft muscle, or probable fatty degeneration	...	1	2	3
Total	13	8	11	32

One case where there was no chronic disease of the heart had a deposit of *recent fibrin* on the valves, the kidneys being small and granular.

The chief remark to be made on the above table is, that it affords a proof that excess of adipose tissue is in itself a cause of dilatation of the heart. A comparison of it with Table XX., at the conclusion of this paper, will show that in the whole number of dilated hearts, those which were so affected without valvular or aortic lesions are only half as numerous as those where the valves were diseased; whereas here, with excess of adipose tissue, they exceed them in number.

We cannot be surprised at this frequency of dilatation of the heart, when we reflect on the great increase of work which is thrown on the chief organ of circulation by the increased area of capillaries through which the blood has to be propelled in fat people. The addition of several stone to the weight in fat, requires certainly a very large, though not perhaps a proportionate, addition of blood and bloodvessels to nourish it, yet the same heart has still to undertake this extra labour. The balance then between the systemic and pulmonary circulation must be destroyed, and the lungs be unequal to the secretion of so much more carbon than they were made for; hence the blood becomes more venous, more liable to form congestions, and to dilate the cardiac cavity by its retarded pace.

The hypertrophy of the cardiac fat was sometimes associated with general corpulence of the whole system, and sometimes not. The proportionate frequency of these two occurrences, together with the particular state of heart in which they were individually observed, is shown in the following table.

TABLE III.

State of Heart associated with excess of adipose tissue.	In corpulent persons.	In persons not corpulent.	Total.
Dilatation	13	5	18
Hypertrophy	...	6	6
Dilatation and hypertrophy	2	3	5
Softening or atrophy	1	2	3
Healthy state	4	13	17
Total	20	29	49

In this table may be remarked the much greater frequency with which a healthy state of heart was joined to excess of its adipose tissue when the rest of the person was normal, than when it was corpulent; an evidence that *general obesity* exercises a remarkable influence over the circulation, equally with the growth of fat on the heart itself.

With respect to the 29 cases mentioned where excess of adipose tissue was found on the heart in patients of normal size, it may be observed that there is no proof that the fat there was not the remains of a general obesity, which had been reduced by the disease that próved fatal, or by other agencies. For the fat is removed from the base of the heart with greater difficulty than almost any other situation. As an evidence of this, it may be mentioned that one of the 29 cases was an old man of 60, emaciated by pulmonary consumption of old date, ulceration of the rectum, and abscess, yet still retaining an abnormal amount of fat in this place.

The whole number of corpulent persons examined was 104 (67 of decided corpulence, 37 "stout"), of which we see 20, or 219· per cent., had excess of fat on the heart.

The whole number of persons not corpulent was 2057 (i.e., 2161 − 104), of which 29 at most, or 1·4 per cent., had excess of fat on the heart.

Deduction.—Over-development of fat on the heart is, as a general rule, part of an universal fatty hypertrophy.

These last remarks are not incapable of practical application. We must not forget, in treating corpulence, that fat on the heart, whether it be the last to come or not, will surely be the last to go; and we must therefore carry on the plans which we have found beneficial beyond the diminishing of the abdomen and limbs, if we would relieve the overburdened centre of circulation.

F. *Adipose Tissue absent.*

In 340 cases of *vomicæ in the lungs*, emaciation existed in 299, but in one only of these last was the fat absorbed from the base of the heart. Here it was replaced by a quantity of serous fluid effused in the interstices of the areolar tissue, so that the form of the organ and its mechanical relations to surrounding parts were in a great measure preserved. This is a further exemplification of the note before taken of impediments existing to the absorption of fat from this situation.

G. *Acute Inflammations.*

(a.) *Inflammation of the Muscular Structure.*—There were 3 cases of *abscess* in the muscular parietes of the heart, all arising from *purulent infection of the blood* by distant chronic diseases—viz., twice by diseased joints, and once by a bubo in the groin.

(b.) *Endocarditis.*—Marks of recent inflammation were found in the valves in 43 instances; in 2 evidenced by *œdema* alone, in 41 by a *deposit of fibrin.**

In the 2 former there was *hypertrophy* and *dilatation* of the heart also; in one, *phthisis pulmonalis.*

In the 41 where there was *recent fibrin* deposited, the immediate causes of the inflammation may be divided as follows:

* In many cases the existence of very extensive old disease of the valve renders the description of the acute lesion doubtful; these are omitted.

TABLE IV.

	Cases.
Uræmia from Bright's disease of kidney	12
Dropsy from diseased heart	2
Acute rheumatic fever	9
Chronic ditto	2
Chorea	1
Ditto and chronic rheumatism united, &c.	(?) 1 *
Anæmia from starvation	1
Typhus	2
Pyæmia	1
Pneumonia (in one case supervening on phthisis)	3
Pleurisy	2
Malformation of valves	2
Old disease of valves	1
(?)	2

In one case the part of the endocardium just above the mitral valve was rough from *recent fibrin*, without any of the valves themselves being affected. The heart was dilated, but no other obvious cause of the endocarditis is recorded.

Deduction.—It is clear, from the list of causes enumerated above, that endocarditis, as seen in the dead body, is either a manifestation of those diseases in which the blood is known by analysis to be the most altered in composition, or a consequence of mechanical interruption to the motion of the valve.

In 30 of the above-named 44 cases the *muscular structure of the heart was chronically affected*—viz.

TABLE V.

Hypertrophied and dilated	15, or 34·0	per cent.
Hypertrophied	8, or 18·1	,,
Dilated	7, or 15·8	,,

In 2117 cases free from acute endocarditis, it was similarly diseased in 475—viz.

TABLE VI.

Dilated	202, or 9·5	per cent.
Hypertrophied	129, or 6·0	,,
Hypertrophied and dilated	124, or 5·8	,,

Deduction I.—The numbers affected with disease of the heart show its probable influence in determining inflammation to the valves.

Deduction II.—The comparative rank in the per-centages which hypertrophy and dilatation hold in the two last lists, show that it is the former condition particularly which has this influence.

The influence of kidney-disease, or of that state of blood which is one of its conditions, in determining valvular inflammation, is sufficiently apparent from the table of cases, and has been illustrated in the paper on the subject

* In this instance it is doubtful how far the disease was recent or chronic.

by the present author in the April number of this journal; so no more than a reference thither is here necessary.

The particular valves were affected in the following proportion—viz.

TABLE VII.

	Cases.
The mitral and aortic simultaneously	21
The mitral alone	14
The aortic alone	7
The tricuspid alone	1

In 3 of these cases there was chronic disease of the same valve; in 4, chronic disease of the other valves.

Deduction.—We have here evidence from acute inflammation of what will be made more certain by higher numbers from chronic states—viz., the tendency of both sets of valves to be affected simultaneously, and of the greater liability of the auriculo-ventricular valves.

This last remark would be the more strikingly exhibited, if we took only the constitutional cases into consideration: for one of the mechanical causes of the inflammation, malformed structure, has been lately shown to be almost peculiar to the aortic valves, and by adding to the number of instances of localization in that part, diminishes the prominence of the mitral.

(c.) *Pericarditis.*—Marks of *recent inflammation*, consisting of pus or soft fibrin, were found in the pericardium in 135 instances; in one case, the inflammation had only caused reddening and vascular injection of the membrane.

The probable direct *causes* of the inflammation were as follow:

TABLE VIII.

	Cases.
Rheumatic fever	18
Ditto, with diseased heart and kidneys	1
Uræmia from diseased kidneys, either alone or with diseased heart	36
Diseased heart and dropsy	18
Pyæmia	18 (or 17)
Erysipelas	4
Petechial, or typhus fever	3
Abscesses after fever	1
Pneumonia	10
Peritonitis from accidental causes	3
Pleurisy, or empyema	5
Vomicæ in the lungs	8
Malignant disease, in actual contact	2 (or 3)*
Ditto in the neighbourhood	1
Neighbouring abscess	2
Slough of œsophagus	1
Aneurism of aorta	2
Fracture of sternum, &c.	1
?	2

* In one case a general development of malignant disease coincided in time with pyæmic affection of the blood in producing inflammations after an amputation.

In this list an attempt has been made at a natural arrangement of causes, by placing as near together as possible those which appear to have the greatest likeness to one another, and by making no defined division into classes. At the beginning are placed those which are most decidedly constitutional, and the most clearly local come at the end. The great preponderance of the former is sufficiently evident to require no comment.

As respects the ages of those who were affected with acute pericarditis, there is, of course, a considerable difference according to the cause from whence it arose.

Of the 18 *rheumatic fever cases*, there were, up to the age of 21 years, no less than 12—viz., 4 males and 8 females. Of the remaining 6, 3 had *suppuration of the joints*—viz., 2 males, aged 25 and 45, and 1 female, aged 29; 1 male, aged 35, had also a *slough* on the sacrum; the remaining 2 were a male of 27 and a female of 30.

Deduction.—Rheumatic pericarditis is peculiarly a disease of youth, and when it proves fatal after 21, it is usually from some complication with other inflammations consecutive on the fever, and perhaps is in some degree allied to pyæmia.

It seems a subject not unworthy of clinical inquiry, but which I do not think has received investigation, whether the tendency to acute inflammations, as a consequence of rheumatic fever, is not developed principally in those persons who have disobeyed the warning to keep quiet, which the pain caused by motion so feelingly preaches to them. Our young patients, who will not be controlled, and our poor patients, who are unwilling or unable to give up work, are certainly those in whom we most commonly find the various degrees of these inflammatory lesions. My own experience, limited as it is, of rheumatic fever among the upper ranks, certainly makes me fear pericarditis less in them than in the frequenters of hospitals, which has led me to think that the "*comfortable classes*" are little liable; while life insurers, who may be taken as a type of the "*prudent classes*," when they confess to having had rheumatic fever, scarcely ever have any sure symptom to relate of chest-affection, or any marks to exhibit of having been treated for it. The power, then, and the will to keep quiet at the commencement of acute rheumatism, seem to me the great preventives of secondary inflammations.

Though it does not come strictly under the present head, I cannot forbear drawing attention to the frequency with which this same tendency (to become locally inflamed in consequence of motion) is exhibited throughout the whole persons of those affected with rheumatic fever. The localization of the rheumatic action in one or more joints, and the putting on of sthenic, and then of disorganizing inflammation, occurs by far the most often in those who, through wilfulness, ignorance, or necessity, have made the greatest efforts to war against pain, and keep about in spite of it. I believe most practical observers agree with me in this; yet, strange to say, it is scarcely noticed by writers on the subject. We find great varieties of treatment recommended, by which we flatter ourselves we somewhat reduce the duration of the distemper; but we never feel quite happy while employing any, for each has, at some time, received the reproach of doing harm while it relieves the immediate pain. The pupils of Sydenham used to feel great comfort in seeing their sick easier after bloodletting, till Dr. Todd told them that they increased thereby the liability to pericarditis; colchicum made both patient and practitioner joyful, till a suspicion was started, that the anæmia which so often occurs in gouty and rheumatic constitutions, was due to the soothing drug. And so on, throughout the list. We use them all, but we use them as two-edged tools. Not such is rest: it cannot injure, and I believe does more to prevent the ill consequences of rheumatic fever than any other treatment. One who would spend time in making this humble anodyne and prophylactic generally known, would deserve the gratitude of society. Recommendations of it *ad clerum* are, it may be hoped, superfluous; but unless we inquire, we should hardly guess how wide-spread among the laity is the idea, that all pain, especially gouty and rheumatic pain, should be fought against to the utmost, and that rest is a temptation to be resisted.

H. *Remains of former Pericarditis.*

There is this distinction between the lesion now under consideration and those which will follow—viz., that it represents only the former existence of an acute inflammation which has passed away, whereas the morbid conditions of valves to be enumerated next in order are a continually augmenting evil, in all probability, and may fairly be called a chronic disease, as well as a chronic lesion.

1. *Adhesions.*—There were 86 cases of *old adhesions* in the pericardium, by which the opposed surfaces of the serous membrane were united. Of these 86 internal adhesions, 51 were *universal*, completely obliterating the serous sac; 4 *nearly universal;* 29 *partial* bands, of various length; and 2 insufficiently described.

Of the 55 cases of universal and nearly universal adhesion, the *ages and sexes* were known in 54:

The mean age at death of 37 males was 34·6 years.
„ „ „ 17 females was 34·8 years.

Of the 29 cases of partial adhesion, the *ages and sexes* were known in 28:

The mean age at death of 20 males was 42·9 years.
„ „ „ 8 females was 41·5 years.

Deduction.—The consequences of partial adhesion are much less rapidly fatal than of universal adhesion.

It would have hardly been worth while to notice this, had not mechanical grounds been taken for an idea that universal adhesion is the most favourable termination of pericarditis. The above-quoted numbers are satisfactory proof that it is not so. They are, indeed, not quite so large as might have been wished, but the similarity of the mean ages of men and women in both classes shows that there exist no disturbing causes of importance to invalidate the correctness of the average.

2. *Incidental consequences of adhesions.*—In 5 of the cases with partial adhesions, there was a quantity of *serum in the sac*, varying in amount, but in one case only was it very large.

The *mean age* of 4 of those where the age was known, was 43·0 years, so that the chance of fluid in the sac does not add much to the danger.

In 5 cases out of the 86 a formation of *bony matter* had taken place in the old fibrin.

In 1 case it contained *tubercles*.

In 1 case the old adhesions were *œdematous* with serum.

3. *Thickening, &c.*—There are 23 other cases where the pericardium, though not adherent, exhibited traces of *former inflammation*—viz.

15, where it was adherent to external parts.

6, where it was thickened.

2, where there were unattached fibrinous filaments, possibly the remains of bands set loose by absorption of one extremity.

4. *White patches on the Pericardium.*—Much doubt has, with good reason, been thrown on the assumption patronized by the late Dr. Hope, that white patches on the heart are an evidence of former pericarditis. However, as that opinion is still entertained by many persons, this would seem the proper place to speak of the lesion.

It was found altogether in 160 cases.

It was found associated with adhesions of the pericardium but 3 times; in two of which there was fluid contained in the pericardium, and in 1 some recent inflammation. With the more ordinary cases of adhesions, it does not seem to have been conjoined.

It is, I think, rendered probable from these facts, that though the exudation of fibrin from an acute inflammatory condition, may in some few cases confine itself to a circumscribed place, and form these spots; yet in the great majority of instances they are due to a process different in kind, and affecting different states of system. Whether this process is a sort of localized hypertrophy of the membrane, or a union of it with the cellular tissue beneath, so as to become opaque and thick, will receive more evidence from the microscope and the scalpel than from statistics. We ought to distinguish more accurately than is the habit at autopsies, whether the opaque patch is capable of removal, leaving the serous membrane beneath; or whether it really forms one continuous structure with it. And even then it is probable that the naked eye requires a certain course of instruction by the microscope, before it is able to decide without it on the nature of these membranous structures.

I. *Chronic diseased states of the Valves.*

During the ten years there were 367 cases in which the valves were *chronically diseased*—viz.

Thickened or contracted in 156.
With morbid deposits in 211.

The *particular valves* affected were as follow—viz.

TABLE IX.—*Thickened or contracted.*

	Times.
The mitral and aortic simultaneously	50
The aortic alone	48
The mitral alone	37
The mitral and tricuspid	8
The mitral, aortic, and tricuspid	5
All four sets	4
The tricuspid alone	1
The tricuspid and aortic	2
The aortic and pulmonary	1

TABLE X.—*With chronic morbid deposits.*

The mitral and aortic simultaneously	71
The mitral alone	59
The aortic alone	59
The aortic, mitral, and tricuspid	6
All four sets	5
The mitral and tricuspid	2
The aortic, mitral, and pulmonary	2
The aortic and pulmonary	2
Doubtful	5

That the thickening of the valves above enumerated may be fairly considered under the same head as the chronic deposits of atheroma in various degrees of hardening, of warty growths, and calcareous formations, is assumed from our knowledge of the graduated shades by which these appearances run into one another in individual instances. Another reason for the assumption may be drawn from statistics of the association of these deposits in other parts, with thickening of the valves in the vicinity. Thus we find *atheroma of the aorta* noticed:

In the 156 cases of thickened valves, 62 times, or in 39·1 per cent.
In the 211 cases of chronic deposits, 90 times, or in 42·6 per cent.

It is possible that where atheroma, or any congenial state of the valve had been noted by the observer, it was in some cases considered superfluous to state also that the aorta was in a like condition; and therefore that the 42·6 per cent. is somewhat under the mark. But still the very frequent conjunction of atheroma of the aorta with thickened valves, is in itself a sufficient argument for their being parts of the same disease, even if we allow that there is a fallacy in the apparent showing of the above table, by which it seems that simply thickened valves are nearly as often associated with atheroma of the aorta as atheromatous valves are.

Deduction.—Thickening of the valves is a lesion arising under similar conditions as chronic deposits of atheroma, earthy matter, &c., in the same parts.

From the above tabular details, it would at the first blush appear, that chronic disease of the *aortic* is at least as frequent as that of the *mitral* valve, and that the assumption drawn from our statistics of acute endocarditis, as to the greater liability of the latter, was erroneous. But if we consider the frequency of morbid deposits in the coats of the great vessel as the first commencement of the degeneration, and the likelihood for the neighbouring valves to be thereby affected, we shall see, that for the auriculo-ventricular orifice to be *as often* diseased, shows in it a greater individual aptitude than its colleague suffers from.

Thus we find that in the 107 cases where the *aortic* valves were alone diseased, the aorta was atheromatous 55 times, or in 51·4 per cent.

In the 96 cases where the *mitral* alone were diseased, the aorta was atheromatous 31 times, or in 32·2 per cent.

And of these 31 times, 9 instances occur in the early part of one year, with the signature of a different curator; so I think there is fair reason for suspecting an idiosyncrasy on his part for including the aortic valves under the generic term "aorta," and therefore omitting to notice separately disease of the latter.

Deduction.—As in acute, so in chronic disease, the most common case is for both sets of valves in the left heart to be affected simultaneously; and of the two, the mitral would appear to have the greater individual liability, though the proximity of the aorta, and its frequent diseased condition, actually put the aortic in most danger.

Now if atheroma of the aorta is part of the same disease as thickened and atheromatous valves, it is fair to associate all three together in the view we take of their frequency in the dead body, and in the inferences we thence draw concerning the living. I will therefore add to the instances of diseased valves those where the aorta was affected with chronic deposit, without the valves having been partakers in the injury.

In the 1794 cases *without* chronic valvular disease, we find the aorta noticed as being in that state 195 times, or in 10·8 per cent.

Adding these 195 cases to the 367 of affected valves, we get 562 in the whole 2161, or 26 per cent., as a symbol of the probable frequency of these sister lesions in the sick.

Deduction.—Chronic degeneration of the valves on neighbouring aorta is at least equally common with tuberculosis, which affects 25·4 per cent.*

Questions of Age and Sex.

The tables which follow are designed to illustrate the proportionate frequency with which the lesion in question affects different ages and sexes in those classes of patients which become the subjects of observation at metropolitan hospitals.

* See Decennium Pathologicum, Part I. No. III., in Medical Times, August 14th, 1852.

TABLE XI. — *Ages and Sexes of those affected with Chronic Degeneration of the Valves and neighbouring Aorta.*

AGES.	Thickened valves.		Chronic deposit in valves.		Chronic deposit in aorta, with healthy valves.			Total.		Total.
	Males	Fem.	Males	Fem.	Males	Fem.	Sex unknown.	Males	Fem.	Both sexes.
From birth to 15, inclusive	3	0	1	0	0	1	0	4	1	5
From 15 to 30, inclusive .	12	12	23	13	17	14	1	52	39	92
From 30 to 45, inclusive .	35	25	41	14	54	13	0	130	52	182
From 45 to 60, inclusive .	27	15	50	16	44	24	0	121	55	176
Above 60	12	8	27	15	14	6	0	53	29	82
Age unknown	7	0	7	4	5	1	1	19	5	25
Total of all ages . . .	96	60	149	62	134	59	2	379	181	562
Column	a.	b.	c.	d.	e.	f.	g.	h.	i.	j.

TABLE XII. — *Per-centage in total Deaths of all Diseases, where an Autopsy was made.*

	Thickened valves.		Chronic deposit in valves.		Chronic degeneration of aorta with healthy valves.		Total.		Total.
	Males	Fem.	Males	Fem.	Males	Fem.	Males	Fem.	Both sexes.
In 154 cases from birth to 15, inclusive—viz., 94 males, 60 females	3·1	0	1·0	0	0	1·6	4·2	1·6	3·2
In 636 cases from 15 to 30, inclusive—viz., 377 males, 259 females	3·1	4·6	6·1	5·0	4·5	5·4	13·7	15·0	14·4
In 651 cases from 30 to 45, inclusive—viz., 472 males, 179 females	7·4	13·9	8·6	7·8	11·4	7·2	27·5	28·6	27·9
In 438 cases from 45 to 60, inclusive—viz., 299 males, 139 females	9·0	10·7	16·7	11·5	14·7	17·2	40·4	39·5	40·0
In 167 cases above 60—viz., 109 males, 58 females	11·0	13·7	24·7	25·8	12·8	10·3	48·9	50·0	49·1
In 111 cases of unknown age—viz., 74 males, 37 females . .	9·4	0	9·4	10·8	6·7	2·7	25·6	13·5	22·5
In total of 2161 cases—viz., 1425 males, 732 females, 4 ? sex	6·7	8·1	10·4	8·4	9·4	8·0	26·5	24·7	26·0
Column	A.	B.	C.	D.	E.	F.	G.	H.	I.

Deductions.

From Columns G, H, and I, in Per-centage Table— Table XII.

I. The tendency increases with the age.

II. The rate of the increase is, however, in an inverse ratio to the age.

Thus in the cases analyzed it was—

From 15 to 30, as 450 to 100 more likely that a person would be so affected than under 15.

From 30 to 45, it was as 191 to 100 more likely than from 15 to 30.

From 45 to 60, it was as 144 to 100 more likely than from 30 to 45.

Above 60, it was as 123 to 100 more likely than in the preceding fifteen years.

The above deductions do not at all lead to the conclusion which is sometimes formed, from the fact that the liability increases with age—viz., that the degeneration is, in truth, *old age itself.* Were this the case, the rate of increased liability would be either regular, or would increase in a direct ratio with the years. The inference rather is, that it is a disease incidental to development of the body, and from a variety of circumstances, which may be surmised, still more incidental to riper life, but by no means a part of the physiological course itself of life.

III. The liability of males and females differs but little.

This may be inferred *from columns G and H*, in which the difference of men and women may be fairly suspected to arise from the narrowness of the field of observation not excluding accidents. It is very much under that which was observed in tuberculosis, where the per-centage of males was 27·2; of females, 21·7; a difference entitling us to a decided opinion of a real excess of liability in the former.* Such a conclusion would not be legitimate from the data before us, especially as we see that the higher the figures, the more nearly do the proportionate number of the sexes approach one another.

But when we come to examine the *earlier columns*, a difference is apparent which seems to demand remark. It is to be observed, that up to the age of 60, the cases of thickened valves without irregular deposit (*Columns A and B*) exhibit a decided preponderance on the side of the females; *but* that the roughening by any of the various grades of morbid formation (*Columns C and D*) is more frequent among males. Chronic degeneration of the aorta (*Columns E and F*) without diseased valves shows no variable proportion.

IV. Though the liability of males and females differ as a whole, yet that is not the case in the different forms, *thickened valves* being oftenest seen among females, chronic deposits among males.

May not the greater tendency of males to form these solid matters bear some relation to what was formerly observed about tubercle†—viz., the

* Decennium Pathologicum, Part I. No. III., Medical Times, Aug. 14th, 1852.

† Cretaceous matter was found in the pulmonary tissue in 64 cases; of whom 45 were males, and 19 were females. Decennium Pathologicum, Part I. No. IV., Medical Times, Aug. 28th, 1852.

greater tendency in that sex to its conversion into chalky substance? Has it any connexion with man's tendency to gout and its consequences? It is easier to ask these questions than to answer them.

J. *Association of Disease of the Muscular Walls with Disease of the Valves and Aorta.*

The conditions of the muscular walls of the heart, which were found coincident with the previously enumerated cases of diseased valves or aorta, are divided in the ensuing tables.

TABLE XIII.

In the whole 562 *cases,*

	Times.		Per cent.
The muscular walls were healthy . . .	214	...	38·0
,, ,, diseased . .	348	...	61·8

In the latter 348 cases, they were—

Dilated	140	...	40·2
Hypertrophied	97	...	27·8
Dilated and hypertrophied	104	...	29·8
"Enlarged"	7	...	2·0

TABLE XIV.

In the 367 *cases of Chronically Diseased Valves,*

The muscular walls were healthy . . .	113	...	30·8
,, ,, diseased . .	254	...	69·1

In the latter 254 cases, they were—

Dilated	87	...	34·2
Hypertrophied	74	...	29·1
Dilated and hypertrophied	89	...	35·0
"Enlarged"	4	...	1·5

TABLE XV.

In the 156 *cases of Thickened Valves,*

The muscular walls were healthy . . .	43	...	27·5
,, ,, diseased . .	113	...	72·4

In the 113 latter cases, they were—

Dilated	44	...	38·9
Hypertrophied	34	...	30·0
Dilated and hypertrophied	33	...	29·2
"Enlarged"	2	...	1·7

TABLE XVI.

In the 211 *cases with Chronic Deposits in the Valves,*

The muscular walls were healthy . . .	70	...	33·1
,, ,, diseased . .	141	...	66·8

In the 141 latter cases, they were—

Dilated	43	...	30·4
Hypertrophied	40	...	28·3
Dilated and hypertrophied	56	...	39·7
"Enlarged"	2	...	1·4

TABLE XVII.

*In the 195 cases, where the Aorta was hardened, there being no Disease of the Valves,**

The muscular walls were healthy . . .	101	...	51·8
" " diseased . .	94	...	48·2

In the 94 latter cases, they were—

Dilated	53	...	56·4
Hypertrophied	23	...	24·5
Dilated and hypertrophied	15	...	15·9
"Enlarged"	3	...	3·2

To compare with these, we find the following 157 cases *where the muscular walls were diseased without any lesion of the valves or aorta—viz.*

TABLE XVIII.

	Times.		Per cent.
Dilated	69	...	43·9
Hypertrophied	40	...	25·4
Dilated and hypertrophied	35	...	22·2
"Enlarged"	13	...	8·2

Deductions.

Hence it appears that—

I. *If degeneration exists in the great vessels or valves—*

That the muscular walls will also be diseased is rather more than 3 to 2.

If the valves are diseased—

That the muscular walls will also be diseased is rather less than 7 to 3.

If the valves are healthy, but the aorta diseased—

That the muscular walls will also be diseased is not quite an even chance.

II. *In thickening (which we may fairly view as the first stage of degeneration) of the valves—*

Simple dilatation is the most common condition of the muscular walls.

Where the disease has advanced to chronic deposits—

Dilatation with hypertrophy is the most common.

Where the valves and aorta are healthy, and the muscular walls diseased—

Simple dilatation is (as in thickening of the valves) the most common; but instead of being one-fourth more common, it is nearly twice as common as any of the other states.

* From this and the following tables cases where recent fibrin only was found on the valves are excluded.

Where the muscular walls alone are diseased—

The order of frequency of the form of disease is as follows: dilatation; hypertrophy; dilatation joined to hypertrophy.

But where the valves are diseased (especially in the most advanced state)—

Hypertrophy joined to dilatation is the most usual form of lesion. *Simple lesion of the aorta* follows the same rule as disease of the walls alone.

Corollary.—It is probable, therefore, that valvular disease, by its continuance, tends particularly to generate hypertrophy of the heart; and that other causes more usually produce dilatation.

A general view of the comparative *frequency of occurrence of the several lesions mentioned in the foregoing tables* may be obtained from the list which follows.

TABLE XIX.

	Times.		Per cent.
Diseased valves, with healthy heart	113	...	5·2
Diseased valves, with diseased heart	254	...	11·7
Atheromatous aorta, with healthy valves and healthy heart	101	...	4·7
Atheromatous aorta, with healthy valves and diseased heart	94	...	4·3
Diseased heart, without any of the above lesions . .	159	...	7·3
Heart free from the chronic lesions, included in the above enumeration	1440	...	66·7

K. *Disease of the Muscular Walls.*

A general view of the frequency of occurrence of the different forms of diseased muscular walls is given in the following table:

TABLE XX.

Form of Cardiac Disease.	Valves chronically diseased.	Aorta diseased; no disease of valves.	Both healthy.	Total.
Dilatation	87	53	69	209
Hypertrophy and dilatation	89	15	35	139
Hypertrophy	74	23	40	137
"Enlargement" (form not specified) .	4	3	13	20
Total	254	94	157	505

The openings for fallacy in the foregoing table are the following:

In some cases of diseased valves, "hypertrophy" may have been intended to include "dilatation" also.

Some few cases of tonic contraction of the ventricles may be included under "hypertrophy, with valves and aorta healthy."

How many times this may have happened it is impossible to estimate; but the general character of the curators is a guarantee that it was not often, and the following conclusions are probably not thereby affected.

As a general rule, the most common diseased condition of the muscular walls is dilatation.

With diseased valves, dilatation with or without corresponding hypertrophy, is about equally common.

With diseased aorta, dilatation alone is most common.

Where both valves and aorta are healthy, simple dilatation is *by far* the most common.

L. *Association of Emphysema of the Lungs and Diseases of the Heart.*

TABLE XXI.

—	Altered muscular walls, without valvular or aortic disease.	Altered muscular walls, with chronic valvular disease.	Altered muscular walls, with atheroma of aorta, but no chronic valvular disease.	Chronic valvular disease, without alteration of muscular walls.	Atheroma of aorta, without alteration of muscular walls.
In 258 cases with emphysema of lungs	30	58	15	18	19
Per cent. . . .	11·6	22·4	5·8	6·9	7·3
In 1903 cases without emphysema of lungs . . .	127	196	79	95	75
Per cent. . . .	6·6	10·2	4·1	4·9	3·9

From this table we may make the following deductions:

I. Emphysema has a very marked tendency to be connected with all forms of disease in the heart, or in its valves and vessels.

II. The tendency is most marked in cases where the valves are diseased, as well as the muscular walls.

It may be seen that the per centage of emphysematous cases bears to the non-emphysematous cases a ratio of 219 to 100 where the valves are diseased with the muscle—a ratio of 175 to 100 where the muscle alone is diseased.

III. Valvular disease alone, and aortic disease alone, have some tendency to be connected with emphysema, and the latter more than the former.

The last clause, perhaps, is in a great measure due to most of the aneurisms and marked dilatations of the aorta being included in the atheromatous cases.

It does not seem likely that emphysema should produce valvular disease, and therefore—

IV. Where cardiac disease is associated with emphysema, it is most likely that the former is the primary lesion.

The subjoined table (XXII.) shows that—

V. There is no very marked difference between simple thickening and chronic morbid deposit in the valves, as respects the above lesion, but it is *most* marked in the latter.

TABLE XXII.

——	Altered muscular walls, with simple thickening of valves.	Altered muscular walls, with chronic deposit in valves.
In 258 cases with emphysema of lungs . . .	26	32
Per cent.	10·0	12·4
In 1903 cases without emphysema of lungs .	87	109
Per cent.	4·5	5·7

M. *Association of Pulmonary Tubercle and Disease of the Heart.*

In the first part of the 'Decennium Pathologicum,' No. VII., published in the 'Medical Times and Gazette' of November 6th, 1852, I have at some length discussed the question propounded in this section. It is unnecessary, therefore, to repeat the statistics there given. The conclusions drawn are—

I. That cardiac disease is less usual in cases of tuberculosis than in mixed cases.

II. The inclination of the evidence is to the view that cardiac disease does not so much render tuberculosis improbable, as tuberculosis renders cardiac disease improbable.

It may be mentioned that the latter conclusion is in opposition to the conjectural statement of Professor Rokitansky, that disease of the heart confers an eminent immunity on its possessor against tuberculosis.

N. *Association of Hepatization of the Lungs and Disease of the Heart.*

I do not feel sufficiently assured that throughout the ten years accurate distinctions have been made between acute pneumonia and chronic atrophy of the pulmonary tissue, to make it safe to draw any statistics from the post-mortem records on this point. *A priori*, conjecture would lead us to expect that the former was likely to be most usually a consequence, and the latter a cause, of cardiac dilatation or hypertrophy.

The following comparison is probably only an approximation to truth, and gives little information:

In 505 cases of *diseased heart*, hepatization of the lungs occurred 96 times, or 19·0 per cent.

In 517 cases of *tubercle of the lungs*, it occurred 218 times, or 42·1 per cent.

In 1139 *other cases*, 231 times, or 20·2 per cent.

O. *Association of Renal Degeneration with Heart-Disease.*

This has been fully discussed in the April number of this journal (Decennium Pathologicum, Part II.), and the remarkable frequency with which it appears to be the cause of the cardiac lesion is there exhibited.

Art. III.

The Internal Surface of the Uterus after Delivery; its Analogies, &c. By J. Matthews Duncan, A.M., F.R.C.P.E., Lecturer on Midwifery, Edinburgh, &c. &c.

The internal surface of the uterus after delivery may be divided into three principal parts: 1. The inner surface of the cervix; 2. The site of the insertion of the placenta upon the inner surface of the body of the uterus; and 3. The rest of the inner surface of the body of the organ.

The first of these parts is entirely excluded from discussion in the present paper. The cervix of the uterus is now known to differ so materially in its anatomy and physiology from its body, that it may with propriety be described as almost a separate organ. The researches of M. Stoltz have been confirmed by numerous authors, and seem to show that the cervix uteri does not become developed so as to form part of the cavity of the organ in the way that it was formerly believed to do. More recent researches by anatomists and obstetricians, too numerous to be mentioned, have shown the entire difference, anatomically and structurally, as well as functionally, of the mucous membrane of these two parts. And, in more immediate relation to our present subject, it may be stated, that there is little variance of opinion as to the inner surface of the cervix being provided with a mucous membrane after as well as before parturition.

The other two parts of the internal surface of the uterus after delivery form the subjects of our present communication: and I may here be permitted to state, that my object in this paper is to attempt to point out correct doctrines in regard to this most important subject. With this I shall content myself at present, leaving the numerous practical deductions from it for another occasion.

Every attentive student of obstetrics must have been struck with the manifest paradoxes implied in the history given of the termination of parturition, and the restoration of the inner surface of the uterus to its pristine condition. Obstetricians now teach that with the expelled ovum there passes away the entire decidua, leaving the muscular tissue of the uterus bare over its whole inner surface; and, in contradiction at once to observation and analogy, describe the formation over this surface of a false membrane, and afterwards of a new mucous membrane—all this process being, even in the healthy female, an inflammatory one, and, like superficial inflammations in such structures, accompanied by the secretion of a quantity of pus which they describe as constituting the essential part of the lochia. To adduce evidence of this statement here is, perhaps, not necessary, as the description must be readily recognised by almost every one; and in a subsequent part of this paper I shall return to these errors, and dwell at some length upon them; at present I may merely remark, that it appears wonderful that so many physiological paradoxes should have been so easily adopted by the profession, and that so much of what is essentially morbid should have been invoked to aid in the performance of a natural function in a healthy body.

In the early months of pregnancy, the mucous membrane of the cavity of the uterus is very highly developed, forming a rich, soft lining to its inner surface, and contributing greatly to the increased thickness of the parietes of the organ at this time. When the walls of the uterus are examined in advanced pregnancy, the thickness of this structure is found to be greatly diminished, even when the whole structures between the chorion and the muscular tissue of the organ—that is, the proper mucous membrane, or decidua vera, and in addition, the decidua reflexa—are included in the measurement. If, in a woman who has died in advanced pregnancy, the membranes are separated from the uterus, the mucous membrane is left adhering to the walls of the womb; only its surface is lacerated and irregular.* I have witnessed the hurried post-mortem examination of a woman dying during labour and before rupture of the membranes, where the ovum was almost completely separated by hæmorrhage extravasated between the membranes and uterus. In this case the uterine fibres did not appear to be anywhere denuded; but the examination was not sufficiently minute and satisfactory.

In parturition a similar process takes place, with this difference, that after the removal or expulsion of the ovum, the uterus is reduced by its contractions to dimensions very small compared to those it had whilst expanded. The result of this contraction, upon the mucous membrane,

* These facts I have verified by means of several dissections preserved in the late Dr. Campbell's museum. Albinus, in his Atlas of Anatomical Plates, describes the same results of a similar dissection. See Uteri Gravidi, Tab. VII. His words of description are, " Ovum exemtum . . . . . Uteri pars interior, mollis, tenera, veluti spongiosa, canosaque: cui ovum molliter adhæret, involucro suo membranaceo." See also W. Hunter, Anatomical Description of the Gravid Uterus, Ed 1843. With his admirable accuracy, W. Hunter describes (p. 47) the adhesion of the decidua to the muscular fibres of the uterus as being " rather stronger than the adhesion between its external (decidua vera) and internal stratum (decidua reflexa), which we may presume is the reason that in labour it so commonly leaves a stratum upon the inside of the uterus." And he elsewhere makes the general statement, that " one stratum of the decidua is always left upon the uterus after delivery." See, also, J. F. Meckel, Descriptive Anatomy (Eng. Trans.), vol. ii. p. 596.

resembles its effect on the muscular tissue of the organ. In both, the diminution of superficial extent is the result partly of the expulsion of the large mass of blood contained in their very large vessels, but chiefly of the assumption of a greatly increased thickness of wall. After parturition, the mucous membrane of the uterus is rough and irregular on the surface, and covered over with blood and adherent coagula.* As the uterus diminishes in size, its thickness increases. In a preparation in my possession, taken from a woman who died soon after delivery, where the os uteri is still largely dilated, and the utero-placental veins open, the length of the uterus is about seven or eight inches; its thickness, including all its parts, is less than one inch. Its internal surface is covered by a thin membrane. This latter is thicker and more prominent at the seat of the placental insertion. Judging from the extent of surface occupied by open-mouthed veins, the placental site is reduced to a circular or rather oblong space, of about three inches in diameter. In another preparation, the uterus is of about the same size, above an inch in thickness; the site of the placenta is evident from the prominence and softness of the part, the uterine veins not prominent and gaping, but apparently closed, and the mucous lining thicker and soft. This uterus is evidently that of a woman who has died some time after delivery. In another case, where the mother died on the fourth day after delivery, I had, through the kindness of Dr. W. T. Gairdner, an opportunity of examining the uterus. It measured about seven inches in its greatest length, and the thickness of its walls was about three quarters of an inch. The whole inner surface of the organ was manifestly covered by a mucous membrane; lacerated at the site of the placental insertion, a surface of between three and four inches in diameter, a number of clots of blood were entangled in the venous openings. Elsewhere, the mucous membrane was distinct. It was covered by the lochial secretion. On scraping the surface, the lochia and epithelium were easily removed, laying bare the fibrous structures of the mucous membrane beneath. In another preparation, which I examined lately with Dr. W. T. Gairdner, which was procured from a woman who, although engaged nursing a child, denied having been recently delivered, and where the woman died of phthisis, we found the lining membrane of the uterus very thick, forming nearly one-third of the entire thickness of the walls of the organ, but thinning rapidly as it approached the cervix. The site of placental insertion was still marked by the prominence of the lining membrane, by its numerous elevations and depressions, covered with sanious fluid, and by the large veins which were easily seen in a cross section, proceeding through the mucous tissue to the very surface of the membrane,—making it evident that the mucous membrane is not developed as a new production over the venous orifices. Between the placental insertion and the cervix, a punctuated

* In his Researches on the most important Diseases of Women, p. 36, Dr. Robert Lee thus describes the interior of the uterus after labour in the healthy state: "For several days after delivery, where no disease of the uterus has supervened, its lining membrane is coated with a yellowish-brown, dark-red, or ash-grey coloured layer, of no great thickness, which seems to be formed chiefly of the fibrine of the blood with small portions of deciduous membrane . . . . Where the placenta had adhered, numerous dark-coloured coagula of blood are found to seal up the orifices of the uterine sinuses in the inner membrane, and frequently to extend a considerable distance into these veins. The clots of blood, one extremity of which hangs loose within the cavity of the uterus, are often connected with a large fibrinous coagulum, which entirely fills the fundus uteri, and everywhere firmly adheres to the inner surface of the organ."

appearance, produced apparently by the openings of the follicles of the membrane was seen. In this case the uterus measured about five inches in length, and the woman had passed the period of confinement at least four weeks. In the uterus of a woman dying the day after a difficult delivery, I found its inner surface covered by a copious soft membrane, the inner surface of which was very dark in colour, covered with blood, and almost gangrenous in appearance. The woman died from peritonitis, the result of hæmorrhage into the peritoneal cavity from a rupture. In addition, I may state, that all authors, even those who assert that the muscular fibres of the uterus are denuded after delivery, as Cruveilhier, Fergusson, and others, yet describe their dissections as displaying an inner or lining membrane covering the inside of the uterus.*

It is, then, found, that after delivery the muscular fibres of the uterus are not laid bare, but are covered by a mucous membrane. This membrane is undoubtedly the remains of the uterine decidua, a mucous structure having the peculiar characters of the uterine mucous membrane. When examined at this time, it is found, as in early pregnancy, to be thicker at the site of the insertion of the placenta than elsewhere. In that site its surface is diversified by numerous elevations and depressions, and by the open mouths of the uterine veins, which have been, as it were, cut across by the separation of the placental mass.

The cotyledonary placenta in the living cow, and in many other species of quadrupeds, is formed, like the human placenta, of an umbilical or fœtal portion, and an uterine or maternal portion. But the process of separation of the placenta in parturition is conducted very differently in these two animals. In the cow, the placental mass is not entirely separated from the uterus. The fœtal part alone is discharged with the membranes; the maternal part is left attached to the mucous membrane of the uterus, of which it is merely a part highly developed for a special object. There is no wound left upon the surface of the uterus of the cow; no vessels are divided in the process. But although there is no wound, yet the cotyledonary surfaces are left in a denuded condition, these parts not being covered by the ordinary epithelial structures. In woman, the separation of the placenta is very different. The after-birth of the human female consists of two parts—a fœtal and a maternal portion. The essential part of the maternal portion is the development of the inner tunic of the uterine vessels, which is prolonged into the after-birth, and springs from the numerous vascular canals which traverse the decidua at the site of the insertion of the placenta, and which are cut across, as it were, by the separation of the placenta. The after-birth of woman thus contains, besides the fœtal structures, the important maternal venous structure just mentioned; and also the layer of decidua which covers its surface, and is described as throwing processes into its substance. The separation of the placenta thus involves the cleaving of the mucous or decidual structure at the line of separation, and the section of the serous internal tunic of the vascular system of the mother at numerous points. And when we con-

* See the cases recorded in Livraison xiii. of Cruveilhier's Atlas of Pathological Anatomy; also the cases in Dr. Fergusson's Essay on Puerperal Fever.

sider the extremely soft and fragile nature of both these structures, we shall not be astonished at the great facility of separating the placenta.*

After the separation of the after-birth the internal surface of the uterus at its former site is left covered by a thicker layer of the decidua than elsewhere. The internal surface is at this part more rough and irregular, and more prominent, than elsewhere. These vascular openings are numerous, and spread over a surface which is, from the contraction of the entire organ, much smaller than the original surface occupied by the placenta.

But, even in woman, the anatomical imitation of the process, as it takes place in the cow, is not impossible. W. Hunter succeeded in performing this operation in a conception of four months. At this time, he says, the union of the two constituent portions of the placenta is less intimate, and they may both be preserved very entire, like the vascular chorion and fungus in the quadruped.†

The membrane is easily distinguished from the muscular tissue of the uterus by its softness, and by difference of colour in a cross section. It is in some cases so soft that its surface can with facility be almost completely rubbed off or brushed off the subjacent tissue. And if this manipulation be practised upon it, the investigator will assuredly find no difficulty in discovering the fibrous tissue to be bare, like the muscles in an amputated stump. This softness and friability is undoubtedly one of its characters which has given rise to the erroneous opinions of authors, for we frequently find them speaking of removing a soft membranous or *flaky* structure, in order, as they imagined, to display the real internal surface of the womb. For example, W. Hunter, in describing‡ a dissection of the uterus of a woman who died at the end of the ninth month without being in labour, states that, finding the internal surface of the uterus everywhere covered with a thin stratum of decidua, he rubbed off the *tender* membrane with a cloth, in order to expose the subjacent muscular structure. But numerous later investigators have not so correctly appreciated, as W. Hunter did, the nature of the structure they removed in a like manner, and which they believed to be effused lymph, false membrane, bloody coagula, or patches of decidua having no necessary existence *there*, and ready to be discharged or rubbed off in order to expose the muscular fibres, which they erroneously believed came to be exposed after parturition. We thus find that here, as elsewhere,§ W. Hunter's accounts of his dissections are still in our day true in almost every particular. But whilst there can be no doubt of the truth of W. Hunter's anatomical description of this part, exception may very justly be taken to the opinion he expresses as to this residuary decidua. He states his belief that "most of it dis-

* "The separation," says W. Hunter, "of the placenta from the uterus, is commonly practicable with the least imaginable force."—Anatomical Description of the Human Gravid Uterus, Ed. 1843, p. 37.

† In regard to this dissection, Hunter adds, "I wished to give a figure of it, but the processes were so irregular and so changeable, while floating in the water, that the painter could not express them; and when taken out of the water they collapsed into a smooth membranous appearance."—Anatomical Description of the Human Gravid Uterus, 1843, p. 36.

‡ Ibid. p. 26. In his paper on the Muscularity of the Uterus, Sir C. Bell writes thus: "Upon inverting the uterus, and brushing off the decidua, the muscular structure is very distinctly seen."—London Medico-Chirurgical Transactions, vol. iv. p. 341.

§ See, in regard to the decidua in early pregnancy, Monthly Journal of Medical Sciences for April, 1853, p. 326.

solves, and comes away with the lochia."* Now there is every probability that this takes place in a very different way. The residuary decidua forms the mucous lining of the uterus, and may pass away from the uterus, not in mass, but in the regular insensible exfoliation of such structures, or be removed by the vessels in its substance in the ordinary course of nutrition and absorption.

Moreover, when we inquire into the real nature of the discharges from the uterus after delivery, we shall find that they are of a nature and character quite antagonistic to the notions entertained as to the denudation of the muscular fibres, and the formation of a new mucous membrane after the deposition of a false membrane over the supposed wound, and under the influence of an inflammatory process. No doubt, the authors who hold these views state that, in accordance with them, the lochia is essentially a puriform fluid, at first mixed with blood, and afterwards giving place to a serous discharge;† but it is impossible to understand whence these authors have derived their information as to the purulent discharge which they describe the lochia to be.

To the most ordinary observer, the lochia in the healthy female always presents an appearance far removed from that of purulent discharge from an extensive superficial wound, like that described to exist in the uterus at this time. The lochiæ have been frequently and correctly described as presenting three different appearances in correspondence with three different stages in the condition of the internal uterine surface after delivery. These have received the names of *lochia cruenta*, *lochia serosa*, and *lochia alba, vel mucosa, vel lactea*;‡ this last is sometimes also called *purulenta*, but this more from theoretical notions in regard to it than from any resemblance it shows to purulent discharge. No doubt, pus may often be observed in greater or less abundance in the lochia mucosa, proceeding probably from patches of inflamed surface on the uterus or vagina, or from healing lacerations; but purulent discharge is not found in any quantity, if at all, in the healthy lochiæ. After the blood has disappeared from the lochia, it is generally observed to be a more or less clear viscid fluid, of a whitish, brownish, or yellowish colour, and wanting the rich yellow colour, creamy consistence and appearance of pus discharged from a healthy wound. Under the microscope§ it presents blood-corpuscles, entire or breaking-up, epithelial cells, of various forms, and abundant detritus of the same; and along with these, some mucous globules. When pus becomes mixed with the lochia, as is not unfrequently the case, it is discovered by its different appearance and characters.

Of late years,‖ it has been customary to compare the internal surface of the body of the uterus after delivery to a great wound, or solution of continuity. This fallacious analogy was in modern times originated by Van

* Loc. cit. p. 47.

† "Ordinarily," says Cruveilhier, "this false membrane is thrown off with a purulent discharge, which is the lochia."—See Fergusson, loc. cit. See, also, Velpeau, Traité des Acc., Ed Bruxelles, p. 518; Litzmann, Aussatz über Schwangerschaft, Wagner's Handwörterbuch, iii. 2, § 135.

‡ See Maygrier, L'Art des Accouch., tom. ii. p. 218; Boivin, Mém. de l'Art des Accouch., p. 446; Chailly, Traité Prat. des Acc., 1853, p. 395; Jacquemier, Manuel des Acc., tom. ii. p. 584; Churchill, Theory &c. of Midwifery, second edition, p. 188; Naegele, Lehrbuch der Geb. für Heb. p. 186; Scanzoni, Lehrbuch der Geb., p. 267.

§ See Gruby, Morphologia pathologica, p. 20, for some remarks on this subject.

‖ Rigby, System of Midwifery, p. 269.

Swieten; but it is due to him to state, that his notions on this subject were, in many respects, more true and correct than those of Cruveilhier and his followers in our own day. Thus, Van Swieten points out,* that after the separation of the chorion and placenta, the inside of the uterus is left covered with the remains of the *tunica cellulosa*, or *substantia cellulosa*, which separates the chorion from the proper tissue of the uterus. He does not describe the uterine muscular fibres as being laid bare, but states that, in addition to the rupture of numerous large vessels, there is†—in his own words—*soluta cohæsio, recens, cruenta, partis mollis; id est vulnus;* and then he goes on to point out the particulars in which this solution of continuity differs from an ordinary wound. In a subsequent paragraph he states his belief that the fever commonly called *febris lactea* does not arise from the afflux of milk to the mammæ so much as from the superficial suppuration which he describes as taking place on the internal uterine surfaces.‡ In his excellent treatise on child-bed fevers, Dr. Kirkland§ takes opportunity to corroborate the statements of Van Swieten on this subject.

M. Cruveilhier is the anatomist of our own times who has been most explicit in this matter; and it is to his misstatements that are due many of the anatomical and pathological errors in regard to it which have found currency among distinguished teachers and authors in our own and other countries. Cruveilhier's views are detailed in his great work on pathological anatomy;‖ and his words are quoted with approval by Drs. Fergusson,¶ Rigby,** and numerous others.

In the passage alluded to, one of the first statements, in regard to the inner surface of the uterus, is to the effect that the "whole of the mucous membrane has been altered by the inflammation of which it has been the seat."†† It is impossible to reconcile this quotation with what follows it almost immediately, to the effect that "except just at the inner surface of the cervix uteri, there is no mucous membrane at all; but the muscular tissue of the uterus is everywhere exposed."‡‡ It is quite needless to attempt to make these statements tally, seeing that in the act of parturition no inflammatory process goes on whatever. Cruveilhier's assertion is totally unsupported by observation, and is quite inconsistent with the general principles of physiology, which do not admit of any morbid process, such as inflammation, acting a primary or subordinate part in any natural and healthy function. In natural parturition, as in all other healthy actions of the body, inflammation interpolates itself only as a cause of derangement or destruction of the function. Not very long ago, the

* Commentaria in Aphorismos, &c., Lugduni, 1764, tom. iv. p. 604, [1329]: "Cum ergo uteri superficies . . . . laceræ tunicæ illius cellulosæ reliquias habet."

† Ibid., p. 604.

‡ Ibid., p. 605: "*Videtur hinc valde probabile esse, febriculam illam, quæ in puerperis lactea solet dici, non tantum a lacte ad mammas delato nasci, sed etiam a depuratione uteri per blandam talem, et superficiariam, suppurationem.*"

§ A Treatise on Childbed Fevers, and on the Method of Preventing them, &c. &c., 1774, p. 75.

‖ Livraison xiii.

¶ Essays on the Most Important Diseases of Women, part 1, p. 77.

** System of Midwifery, p. 270.

†† Essays on the Diseases of Women, by Dr. Fergusson, p. 77. The words in the original are—"*La muqueuse intérieure n'existe plus ou plutot ses éléments se sont dissociés, modifiés par l'inflammation dont elle a été le siège.*"

‡‡ Ibid., p. 77. The words in the original are—"*On ne trouve de debris de muqueuse que sur la face interne du col uterin et quelquefois autour de l'orifice des trompes; partout ailleurs le tissu propre de l'uterus est à nu et partout il doit être recouvert d'une cicatrice.*" See also Heschl, on the Human Uterus after Delivery, 1853, p. 6.

history of the origin of the decidua, or modified mucous lining of the uterus, was involved in obscurity: and John Hunter, in attempting a theory for it, founded on erroneous and imperfect observations, was driven to suppose that in healthy pregnancy a sort of inflammation was set up on the internal surface of the uterus, producing there a layer of coagulable lymph, which he imagined came to form the decidua. Into a similar error M. Cruveilhier and his followers have fallen. There is no evidence whatever of the existence of inflammation of the internal surface of the uterus in natural delivery. The only process like inflammation in this part is at the seat of the insertion of the placenta, where the closure of the disrupted vessels may be said to take place by a sort of inflammatory process.*

M. Cruveilhier goes on to state, that after parturition "the muscular fibres of the uterus are everywhere exposed."† But of this he gives no evidence whatever. On the contrary, the dissections he describes, although they are morbid, give some evidence to the contrary effect. In fact, the examination of this part after death overthrows this statement of Cruveilhier, Fergusson, and others. The actual observations of these authors form a curious commentary upon their expressed opinions. It is unnecessary to refer to and quote these observations, where the inner membrane of the uterus covering its muscular fibres is incidentally mentioned or described at length.

Finally, did there exist after every delivery a wound of the enormous dimensions of the internal surface of the uterus—dimensions not inferior to those of the wound produced in amputation of the thigh, it is difficult to conceive how parturient females should escape the frightful mortality succeeding that operation, or the like. It would be difficult or impossible to explain why, instead of one in every three or four dying as after ampu-

* It would be wrong to admit the process of closure of these vessels to be an inflammatory or morbid process. The closure of the foramen ovale in the fœtal heart by adhesion or cohesion of the valve to the opposed border of the opening does not involve an inflammation of the inter-auricular septum or of any of its parts. The inflammation of the inner surface of the uterus, as described by Cruveilhier, is undoubtedly a very different process, and undoubtedly a morbid one, resulting, as he says it does, in the formation of pus and of false membrane.

† In an interesting paper on the Dysmenorrhœal Membrane, published in the Monthly Journal of Medical Science for Sept. 1846, Dr. Simpson states his belief that the actual absence of the mucous surface of the uterus has been often ascertained on dissection, and adds, "I lately saw a case where the patient died six weeks after delivery, and still, at that late date after confinement, the mucous lining of the uterus was not yet regenerated." This case is not given at sufficient length to allow of any judgment in regard to it, and as it stands it is quite unsatisfactory. Further, Dr. Simpson states, that "the absence of the mucous lining of the uterus in persons who have died after delivery, or who have been previously subject to membranous dysmenorrhœa, may have given rise to the strong opinions expressed . . . . in regard to the human uterus not being normally provided with a mucous membrane." But it has yet to be shown that at any time the uterus is denuded of mucous membrane. And certainly there is no reason to believe that, as Dr. Simpson states, "the proper mucous tissue of the uterus itself may, within the compass of a menstrual period, form, enlarge, separate, and again be reproduced; and further, that all this may occur and continue regularly for a succession of months, or, as sometimes happens, for a succession of years." The discharge of the dysmenorrhœal membrane (or, as it may be called, inflamed menstrual decidua) does not involve the removal of any part of the entire thickness of the mucous membrane. A comparison will illustrate my meaning. The cuticle is, in the healthy state, constantly and insensibly exfoliating in dusty particles. In like manner the mucous membrane of the uterine cavity undergoes an insensible exfoliation. But let the skin be inflamed, as by a blister, and then the cuticle is detached in mass and separated as a membrane; the proper tissue of the skin is, however, not removed. In like manner, when the mucous membrane of the uterine cavity is inflamed, as in dysmenorrhœa, the thick and rich epithelium is detached in mass and separated as a membrane; the proper mucous tissue of the cavity of the uterus is, however, not necessarily removed.

tation of the thigh, there should be only one in every two or three hundred. It can scarcely be asserted that the shock produced, and the circumstances of the supposed uterine wound, are a whit more favourable to recovery in the obstetric than in the surgical patient. The explanation lies in the fact that the chief analogy of the internal uterine surface after delivery, is not with a stump, so far as it consists of incised and denuded tissues, but only in both surfaces presenting numerous open veins liable to become inflamed, or to absorb the obnoxious materials which may be brought into contact with them. And it is to this anatomical circumstance that are traceable most of the cases of death in childbed.

Art. IV.

Historic Data, &c., in reference to some points of Infantile Pathology. By W. Hughes Willshire, M.D. Edinburgh, senior Physician to the Royal Infirmary for Children, &c.*

No. I. — Pneumonia.

Notwithstanding the attention which has been paid in this country, during late years, by many practitioners to the peculiarities and effects of diseased action in the child, and after making full allowance for the extension of sound and advanced opinions since the publication of the lectures of Dr. Charles West, yet it cannot be denied that there are still many (some of whom are well acquainted with the pathology of the adult frame) who are unaware of the litigated and unsettled position which certain questions respecting the pathology of children have occupied, and even occupy at the present time. Were such questions relative only to rare or unfrequent diseases, or, though connected with affections of common occurrence, disputed merely by men of limited knowledge or experience, the author would not think it advisable that they should be discussed in these pages; but inasmuch as some of them refer to things of every-day frequency, and are vigorously combated by very high authorities, he has deemed it might not be out of place to draw the attention of such to them, who, though not desiring to enter deeply into the minutiæ of recondite points in *pædiatrics*, may yet feel interested in some important features of common forms of disease. As the pages of a serial are often read when those of a systematic treatise are neglected, it may happen that the following analytic sketches (which are all that can be attempted by the author) shall be an inducement to some to seek in such treatises a more extended account of the subjects therein touched upon. It is intended that pneumonia, *in some of its bearings*, be first illustrated, but only, it must be remembered, in a very general way; the landmarks are pointed out which may indicate the path to those who wish to explore it more minutely; in the words of Orfila, "Il est toujours utile d'essayer de frayer la route quand même elle sérait imparfaitement tracée."

Whatever weight may be attached to the knowledge presumably possessed by the more ancient writers, or to that indicated by the allusions of

* It is proper to state that nearly the whole of this paper was in our hands before the article on Bronchitis, Pulmonary Collapse, and Emphysema, in our April number, was received.—Editor.

Stoll, Sydenham, Morton, Rosen, &c., concerning a general idea of inflammation of the respiratory organs in children, and as leading them to make a distinct separation between the affection attacking the upper air-passages and the parts within the thorax, it may be affirmed, that it was not until the time of Reil, in 1792, that distinct proof was given that the differential lesional signs of bronchitis and pneumonia had been recognised. This we may assume to have been done, from the account given in the 'Memorabilia Clinica' of the appearances found after death in some of those who died during an epidemic of small-pox, raging at Hallé in 1791.* The redness of the mucous membrane, even in the fine ramifications of the bronchia, was noted; and also a change in the parenchyma of the lung, which was said to be due to inflammation of the latter. For more than twenty years from this period there seems to have been a strong tendency to neglect the study and more perfect elucidation of the differential signs of the two diseases; in particular, losing sight of pneumonia, or merging it as much as possible in bronchitis, the history of which, since its establishment by Reil, was progressively advancing. This fact the writings of Thomann, Cheyne, Cuming, Badham, Kersig, and others, sufficiently testify, in which the terms bronchitis, peripneumonia, paralytic peripneumonia, and asthma paralyticum, are employed as designative of the affection of which they treat. It will be found, on reference to Meissner's 'Grundlage,'† that in 1820 an inaugural dissertation was published in Berlin, "de pneumonia infantum;" but what this dissertation especially defines as pneumonia, or what demarcation is established between the disease of the bronchia and of the pulmonary parenchyma, we are, unfortunately, unable to say. Under these circumstances, we cannot dispute the claim made by MM. Rilliet and Barthez in favour of M. Lèger,‡ as having been the first to describe—in 1823—as a distinct affection, the pneumonia of children, both in acute and chronic forms of the malady. M. Lèger was soon followed (1825) by M. Lanoix;§ and the latter by Berton,|| Bergeron,¶ Burnet,** De la Berge,†† Seiffert,‡‡ and others, by whom not only were the distinctions between bronchitis and pneumonia affirmed as established, but the essential lesional changes characterizing the latter asserted to be so satisfactorily demonstrated, that it could be laid down not only that pneumonia was a common disease in children, but that as it attacked them it was different from the pneumonia invading adults. The nature of this difference, and the special lesions attendant upon the disease in children, were attempted to be formally defined. Not alone, however, was an advance thus made, but steps were being taken for distinguishing between the effects of pneumonic inflammation, and particular conditions met with in the lungs of new-born children and of children at the breast; and also for establishing a difference between the pneumonia of the latter and the pulmonic inflammation of more advanced childhood. In illustration of the above remarks, the labours of

* Memorabilia clinica Medico-practica. Vol. ii. fasc. 1. Fieber lehre. Halle, 1792.

† Grundlage der Literatur der Pädiatrik, &c. &c. Leipz. 1850, p. 95.

‡ Essaie sur la Pneumonie des Enfants. Paris, 1823. Thèse.

§ Pneumonie des Enfants comparée à celle des Vieillards. Paris, 1825. Thèse.

|| Sur la Pneumonie Lobulaire ou Partielle. Paris, 1828. Thèse.

¶ La Peripneumonie des Enfants. Paris, 1828. Thèse.

** Journal Hebdomadaire, &c., 1833, t. xii. pp. 129—197.

†† Journal Hebdomadaire, 1834, No. 26 (Schmidt's Jahrbuch, Band v.)

‡‡ Die bronchio-pneumonie der Neugebornen und Sänglinge. Berlin, 1837.

Billard, Cruveilhier, Gerhard,* Rufz, Iörg, Valleix, &c., may be referred to. But we now arrive at a time when many workers were in the field, and literature upon the subject so prolific, that it becomes difficult to allot to their originators many observations which were becoming common property. In 1838—40, MM. Rilliet and Barthez appeared before the profession; but before carrying on the present department it is necessary to draw attention to the following observations in connexion with the general anatomic lesions of the pneumonia of adults:

> Leaving out of consideration the more exceptional forms of this disorder, whose symptoms and lesional changes undergo more or less modification by the dyscrasic and other states of the patient's system, it may be borne in mind that the morbid anatomic alterations generally characterizing the primary or idiopathic pneumonia of adult life are, in broad terms, the following: More or less of a continuous extent, or lobar portion of the lung, is of a dull or brownish red colour, varying in tone in different parts, and giving rise in a variable degree to a somewhat marbled appearance of the pulmonary parenchyma. The latter when pressed wants the springiness of health, and much of its natural crepitation; and when cut, a frothy semi-sanguineous fluid quickly exudes from it. If the disease has progressed to its second stage, the diseased portion becomes condensed throughout; crepitation is quite wanting; the colour somewhat lighter than before; the part much softened, and presenting more or less of a granular appearance when a section is made; and which combination of characters constitutes the stage of "hepatization," or "red softening." In a still more advanced degree of the disorder, the hepatization is the same, but the softening is greater; the colour of the lung of a rather dirty light-grey or yellowish tint, according as this stage—that of purulent infiltration, or grey hepatization—has extended. In whatever state or stage the pneumonic lesions are found, it will be remembered that their course is, under ordinary circumstances, from below upwards, invading more or less of an entire lobe of the lung, and not attacking it in circumscribed, discrete, definite, or scattered patches. The second stage may commence centrally or circumscribedly in the parts invaded by the first stage; but the latter itself does not commence so in healthy parenchyma—on the contrary, it is diffusedly continuous.†

We now revert to the position which the subject of pneumonia in children was attaining to at the time of the appearance of MM. Rilliet and Barthez, drawing attention to the fact of the observations and investigations supporting this position having been made and carried on for the most part in the large foundling hospitals of the Continent. It was observed (Gerhard, Billard, Guersent, &c. &c.) that whilst certain cases of pneumonia presented the same lesional characters as attend the disease in the adult, the same engorgement, red and grey hepatization, the same progress from below upwards, and involvement of more or less of the whole lobe of the lung—in others, and by far the majority of cases, the cadaveric appearances were very different. The disease appeared to be of an isolated or discrete form: instead of attacking a continuous extent or lobar portion,

* American Journal of Medical Sciences. Philadelphia, 1834.

† Hasse's Pathological Anatomy: Organs of Circulation and Respiration.

it invaded the lung in disseminated circumscribed spots, or *lobules*, which were surrounded by parenchyma apparently healthy, and the "three stages" in general still preserved this isolated character, this *lobular* appearance, although in some instances the more perfect demarcation of the latter was lost, in consequence of the affected lobules becoming coalescent. In many of these latter cases such coalescence offered no hindrance to the detection of the primary discrete nature of the disorder, whilst in others it was so intense, or perfect, as to cause a simulation of the *lobar* pneumonia of adults, and for which indeed it was frequently mistaken. Hence, in general terms, we may say that two forms of the affection were now recognised as attacking children—a *lobar* and a *lobular* form; but that "lobular pneumonia" was their pneumonia, *par excellence*. Whilst this position was being wrought out and established, certain other doctrines were being evolved, which it is essential we should now notice.

In 1811, Schenk reported a case in 'Hufeland's Journal,' in which a particular condition of the lungs of a child dying four days after birth was remarked upon. The lungs appeared never to have been perfectly inflated, were condensed, and sank in water; they admitted of artificial inflation, however, and then seemed to be quite healthy. According to Mauch,* Wilbrandt, in 1816, Eberhard, in 1817, and Lucae in 1819, drew attention to a like condition, which the latter two writers regarded as arising from "the *musculi-bronchiales* assisting in the production of respiration not being as yet sufficiently accustomed to the rhythmic order of the respiratory acts."† The state of the pulmonary tissue alluded to by these pathologists was soon recognised by Dugès, who, in 1821 (in a thesis), remarked upon the caution necessary to be taken in not confounding with "peripneumonic condensation a state of the lungs of some weak new-born children, in whom respiration had not been completely established." In this state "the lungs are violaceous, deprived of air at certain portions, sinking in water; in fine, dense but flabby, soft, flexible, coriaceous, slightly developed, and but imperfectly filling the corresponding side of the thorax." "M. Chaussier (continues M. Dugès), who has likewise observed the above-mentioned state, regards it as the effect of congestion ulterior to birth. In spite of the weight of such authority, I cannot coincide in his opinion. I have found too much analogy between what I have witnessed in infants who have died before birth and that which I have seen in those who have died several days later." In 1823, Schallgruber,‡ in discussing the causes of death, and the post-mortem appearances found in the bodies of infants supposed to have been suffocated in bed by the mother or nurse, alluded to the unexpanded condition of the lungs, which swam with difficulty in water, and remarked that most new-born children who die, do so apparently from want of, or incompleteness of, pulmonary expansion. It is true that Schallgruber (nachtrag zur Thymus druse) regarded an enlargement of the thymus gland as the

* Die Asthmatischen Krankheiten der Kinder. Berlin, 1853.

† Mauch, op cit. p. 164.

‡ Abhandlungen im Fache der Gerichtsarzneikunde. Grätz, 1823.

main cause of the deficient expansion; yet it is clear, from what he states, that he had formed a good notion of that condition we shall presently find to have been called *Atelectasis*. In 1832 the younger Iörg, following out some views of his father, published a distinct treatise* on a particular condition of the lungs of new-born children found after death, in cases "where the first act of breathing had been imperfectly accomplished, either because they were puny and feeble, or because they had been hurried into the world before placental respiration had been altogether suspended, and the necessity for pulmonary respiration became sufficiently potent to stimulate all the muscles of inspiration."† In 1835 the subject was more fully worked out‡ by the same author, and the term *atelectasis* applied to the condition in question. In the same year, also, M. Rufz§ drew attention (in a note to one of his Memoirs) to an alteration (carnification) of the pulmonary tissue, which he affirmed to be distinct from hepatization, but of whose entire symptomatic value he was ignorant. The lung was described as being very dense, of a violet colour, sinking in water, non-crepitant, and looking like a portion of the lung of a child that has never respired. M. Rufz suggested that it might be simply the result of compression of the pulmonary tissue. To this writer, indeed, is generally awarded the credit of thus linking, as it were, the congenital non-expansion, or atelectasis of Iörg, to the assumptory collapse of the lung afterwards (as we shall presently find) playing so important a part in the hands of MM. Bailly and Legendre. But such credit is misplaced, for even if we cannot go the length of admitting this to have been done by Dugès, we cannot avoid believing that our countryman, Dr. Alderson, had forestalled M. Rufz. In a paper by Dr. Alderson, upon Pertussis, published in 1830,‖ a lesion of the pulmonary parenchyma was commented on, which was said to differ from the hepatization of "peripneumony." The individual lobules were more dense, of a dull red colour, devoid of air, and sinking instantly in water. Such a condition, too, was found uncomplicated with any evidence of pleuritic inflammation, the lung being dense and contracted, as if the air had been expelled, and the sides of the air-cells agglutinated together. Following M. Rufz, MM. Rilliet and Barthez, in their 'Memoir,' published in 1838,¶ described a "carnification" *not the mechanical result of effusion*. This carnification resembles, at first sight, the lung of a fœtus which has never respired, or more nearly simulates the effects of inflammatory action where no engorgement is left behind. From this latter circumstance it might be considered, according to these writers, as a result perhaps of chronic pneumonia. Finally, upon this point we may remark, that in 1839 Dr. A. Rees, in a paper published in the 'Medical Gazette,' on

* Dissertatio de morbo pulmonum organico ex respiratione neonatorum imperfecta orto. Lipsiæ, 1832.

† Hasse, Pathol. Anat., vol. i. p. 253.

‡ Die Fœtus-lunge im gebornem kind für Pathologie Therapie und Gerichtlicke arzneiwissenschaft geschildert. Grimma, 1835.

§ Journal des Connaissances, Medico-Chirurg., Paris, 1835, p. 101.

‖ Medico-Chirurgical Transactions, vol. xvi.

¶ De la Pneumonie chez les Enfants. Paris, 1838.

"Deformity of the Chest in Young Children," described a condition of the lung in which the latter was "converted into a dense, firm texture, sinking in water, of a purple colour, resembling most nearly lung having suffered from compression," and which condition was considered to be the result of "chronic pneumonia." Bearing these facts—parenthetically, as it were—in mind, we now recur to the position of *lobular pneumonia* becoming established as that form of pneumonitis most frequently met with in children.

The memoir of MM. Rilliet and Barthez, before alluded to as published in 1838, received high encomiums, and was particularly adopted by MM. Sestier* and Grisolle.† In 1843 appeared the great systematic work of the former writers,‡ in which the chapter on pneumonia was (to use their own expressions) to be considered as a second edition of their earlier monograph. The high authority of MM. Rilliet and Barthez succeeded, notwithstanding the opposition on some points of MM. Berton and Barrier,§ in pretty widely establishing the following doctrines. Pneumonia in the child may become developed during perfect health, or in the course of some other malady. The former or primitive pneumonia, though frequent in the adult, is rare in children, particularly during infancy. In children the disease is generally of a secondary, whether of an acute or cachectic, character. Both lobar and lobular pneumonia occur, but the latter is by far the more common form. Lobular pneumonia is observed under three phases: first, *mammelated,* in which the diseased parts are perfectly circumscribed, and have a nucleus of hepatization, whose colour and appearance are strictly defined from those of the surrounding tissue, and readily seen to be distinct, even when the latter is in a state of engorgement; secondly, *partial,* in which the affected lobules are less distinctly separated or parted off than in the previous variety, their circumferences becoming insensibly confounded with the surrounding tissues, so that no perfect demarcation can be demonstrated; thirdly, *generalized,* in which the size of the inflamed patches becomes large, irregular, and prolonged, the centres of the lobular patches being in the second stage of pneumonitic changes, the circumferential parts in the first; these latter coalescing, it results that a considerable portion of a lobe is found pneumonitic, presenting intermixed characters of the first and second degrees of the disorder; this variety has been confounded by pathologists with lobar pneumonia. It is admitted that in some instances particular varieties of generalized lobular pneumonia very closely simulate the lobar form, so closely, indeed, we may remark, that it appears to us MM. Rilliet and Barthez have failed to afford an easy solution of the difficulty—a difficulty not less apparent to M. Barrier, who makes use of the designation *pseudo-lobar.* It is not denied that both the lobar and lobular varieties may exist in the same patient, but such cases are affirmed to be rare. All the varieties of lobular pneumonia are not of a like frequency at a like age. Mammelated is much rarer above six years of age than partial, whilst generalized pneumonia is yet rarer than mammelated from six to fifteen.

* Leçons de Clinique Médicale du Profess. Chomel. Art. *Pneumonie.*

† Traité Pratique de la Pneumonie aux differens âges.

‡ Traité Clinique et Pratique des Maladies des Enfants, tomes i.—iii. Paris, 1843.

§ Traite Pratique des Maladies de l'Enfance. Paris, 1842.

In bringing these doctrines to perfection, the minute anatomy of the lesional changes which the inflamed lobules underwent, were of course patiently investigated, and a distinction drawn between hepatization and the *carnification* of M. Rufz, which latter was nevertheless linked in a very indefinite way to inflammation, and therefore reduced in the long run to a pneumonitic lesion. Considering the high authorities propagating these views, or particular modifications of them (unnecessary at present to dwell upon), and the vast opportunities offered by the continental hospitals, apparently for their substantiation, it might have been supposed that the nature of pneumonia in children was now fairly established. But the truce was soon to be broken. Several pathologists, more particularly in Germany, recalling to mind the observations of Billard, Cruveilhier, Dugès, and Valleix, in connexion with new-born children, the case of Schenk, the views of Schallgruber, the note of Rufz, and the memoir of Iörg, intimated their doubts as to the matter being by any means settled. Cruse* and Hasse,† in particular, asserted that some great mistake existed, and that neither "hypostasis" nor "inflammation" fully explained some of the peculiar lesions found after death in much of the so-called lobular pneumonia of new-born children; that even perfectly demonstrable *atelectasis* could be shown to have been confounded with pneumonia; and that the whole subject required further elucidation. Hasse concluded his fourth chapter on the organs of respiration, by affirming that "the greater number of cases of pulmonary disease occurring at the earliest period of infantile life, and set down as pneumonia, may be looked upon as cases of atelectasis. The last assertion is, however, to be taken with some reserve, inasmuch as in the vast lying-in and foundling hospitals (Kiwisch) pneumonia is apt to become epidemic with new-born infants, and under these circumstances to attain a numerical preponderance over atelectasis." In 1843, in our own country, Dr. West‡ maintained the frequent confusion of the symptoms and effects of atelectasis with those of pneumonia, the much more frequent occurrence—at least in Britain—of idiopathic or primary pneumonitis than the French writers admitted of on the Continent, and also the tendency of the lobular form of it to become generalized, to be greater than they allowed. Dr. West also maintained the frequency of the true lobar variety. Nothing less than a complete unsettlement of the whole subject as related to new-born children and infants seemed inevitable, in consequence of the views thus being enforced concerning atelectasis. A sort of compromise, however, was attempted by some: it was admitted that a "fœtal condition" of the lungs frequently occurred in children who died a short time after birth, and in whom the acts of respiration had been very imperfectly performed: and that, of course, mere atelectic symptoms and lesions were not those of inflammation. But it was at the same time sought to be established, that if, under such circumstances, life had been prolonged to some extent, an inflammatory condition supervened, and then after death the effects of the two conditions were so intermixed, as to baffle all solution. As to "carnification," some of its analogies to the

* Ueber die "Acute Bronchitis" der Kinder und ihr Verhältniss zu den verwandten Krankheitsformen. Königsberg, 1839.

† Pathological Anatomy: Organs of Respiration.

‡ Clinical and Pathological Report on the Pneumonia of Children as it appears among the Poor in London: British and Foreign Medical Review, vol. xv. p. 543.

atelectic state were admitted, one or two regarded it as a result of chronic pneumonia, whilst others denied the explanations thus offered, but could not afford any solution of the matter themselves.

We have thus stated sufficient to carry the subject onward to the year 1844, in which appeared the memoir of MM. Legendre and Bailly,* causing quite a revolution in the history of "lobular pneumonia." They introduce the subject by remarking upon the fact of the various writers on pneumonia, during the previous twenty years, having occupied themselves with a special alteration, called *splenisation*, *carnification*, &c.; and that, with the exception of M. Rufz, they are quite in error about its nature; and not only this, but such is the confusion and contradiction existing, though the idea of inflammation has been common to all, that one cannot help rising from the perusal of what has been written, with the paradoxical impression that "the pneumonia of children essentially differs from that of adults, in so far that it is not an inflammation." It is then affirmed that, whatever it may be, the authors had convinced themselves "of the absence of all inflammatory action in the indurations found in the lungs of a large number of children who die from pulmonary catarrh, and often in the course of other maladies foreign to the lungs—indurations which had been regarded up to the present time as the disseminated nodules of lobular pneumonia." Having found a certain analogy between these alterations and the condition of the lungs of a child which has never respired, they were led to enquire if the pulmonary tissue of an infant could, under particular circumstances, reassume the anatomic state presented before respiration was established; this their investigations satisfactorily proved to them could be done: and upon this single but important fact are based the views they would promulgate on the question. These views they preface by an account of the lesions found in the lungs of children dying from "pretended pneumonias," and which are now asserted to be analogous to the normal condition of the pulmonary organs in the fœtus. Great stress is laid upon the fact, that insufflation can restore the affected lobules, "*à l'état physiologique.*" This character necessarily excludes in the tissue which possesses it all idea of inflammation with veritable lesion. How is it possible to admit that such could disappear, and the organ reassume its normal condition, under the influence of this single operation?" "Besides, a lung truly hepatized permits of no penetration of air by insufflation." This "fœtal state" [of post respiratory assumption] is then minutely described, and two varieties of it laid down—viz., a simple and a congestional variety. The former is met with in children of from five months to five years of age, as a complication of the larger number of diseases, particularly of bronchitis, and embraces "carnification," and "marginal pneumonia;" whilst the latter is the only form met with in new-born children, and includes the majority of instances recorded by writers as examples of simple or double lobular pneumonia. The circumstances inducing the assumption of this "fœtal state" are those which by any means hinder the free access of air into the pulmonic cells; general debility, rachitism of the thoracic walls, exhaustion from disease, inflammation of the bronchia, &c., may do this, and the consequence will be, that "a sort of retraction of the pulmonary tissue is produced, which thus

* Archives Générales de Médecine, Jan., Fev., Mars, 1844.

being deprived by default of air of its functional activity, gradually collapses and becomes condensed." In the production of the "congestional" variety, another force is superadded—viz., "sanguineous congestion of the vascular network enveloping the cellules, a congestion having as its effect the compression of the latter." A very common affection in children is "inflammation of the bronchia and of the vesicules:" to this combination the term of *catarrhal pneumonia* is appropriate; as an attendant upon or a likely contingency of the disease, is found occlusion or compression of the pulmonary vesicles, in other words, the assumption of the "fœtal state" in certain portions of the lungs. The above, with its modifications, constitutes for the most part the disseminated and generalized lobular pneumonias, mucous pneumonia, capillary bronchitis, and the suffocative catarrh of different pathologists. MM. Bailly and Legendre, after resolving the greater mass of cases of lobular pneumonia in the way thus indicated, ask the question—if there really does exist such a disease as lobular pneumonia? This is answered in the negative, as its affirmation would involve a perversion of terms; and, moreover, the pneumonia of children is always parenchymatous, not having its *point de départ* in the mucous tissue, but in the cellulo-vascular, and in the cellular interstices separating the ultimate bronchia. It would, therefore, not be reasonable to retain the term lobular as applicable to any parenchymatous pneumonia, however circumscribed the latter might be. Parenchymatous lobar pneumonia occurs in children, and gives rise to two kinds of partial hepatization, "one, irregular in its form, is nothing but ordinary simple pneumonia, only of limited extent;" "the other is mammelated or truly circumscribed." Thus far is it necessary to go with MM. Bailly and Legendre. The opinions they have advanced have been adopted by some in their totality, by others but partially; many suspend their judgment until after further enquiry; whilst some are entirely opposed to them, and maintain much the same views previously held regarding lobular pneumonia.

In 1845, Dr. West remarked, in his "Report,"* that he had repeated the experiments of the above writers, and could fully substantiate the correctness of their statements as regards the effects of insufflation. In 1852,† he coincided with their more important views, believing that by the simple experiment of inflation more light had been thrown on the affections of the lungs in infancy and childhood than by all the writers of the previous ten years. Dr. West also appealed to the observations of Baly, Gardner, and Louis, illustrative of collapse and carnification of the lung in adults, &c., as tending to establish the main truth involved in the argument. Though formerly coinciding in the opinion that an unusually congested state of the reticular vessels had power in compressing the air vesicles, he did not agree in this now, considering the congestion to be rather a secondary and accidental occurrence. He also believed that a form of pneumonia now and then occurs which may justly be called lobular; whilst lobar pneumonia is often met with in early life, both as an idiopathic and secondary affection. In 1845, Traube, in his 'Contributions to Experimental Pathology and Physiology,' described "red induration" as rather to be regarded as atelectasis

* On the Progress of Practical Medicine in the Department of Midwifery and Diseases of Women and Children: British and Foreign Medical Review, vol. xx. p. 525.

† Lectures on the Diseases of Infancy and Childhood. London, 1852, p. 168.

than as the result of inflammation, and as produced by the accumulated mucus in the smaller bronchia. "We stand here, as it is seen, on the confines of a territory that can only be conquered for science by sufficient clinical and pathologico-anatomical investigation, combined with systematically-ordered experiments." Unfortunately for Dr. Traube, as Fuchs remarks,* this territory had already been invaded a year before the latter made his pretensions to it; and all that was required was, that existing literature should have been more carefully consulted. In 1846, Dr. A. Rees published a paper in the 'Lancet' upon "Carnification," prefaced with a reclamation against MM. Rilliet, Barthez, Legendre, Bailly, and Dr. West, and affirmed that to him was due the honour of first noticing "carnification" in young subjects, unconnected with effusion into the pleura. Dr. Rees rejected the idea of its being a result of chronic pneumonia, and stated there to be considerable difficulty in assigning its causes.

M. Legendre, after the lapse of a short time, collected his separated essays, and published them as a substantive work.† In 1846, it was reviewed in the 'Archives Gén. de Médecine' (t. xii. 4th série, p. 379), the review bearing the signature of V——x [Valleix?]. On the conjoint labours of MM. Bailly and Legendre regarding our present subject, it was stated, that it was believed these two investigators had generalized too far; that without doubt they had indicated an important fact in showing that in certain forms of carnification of the lung no connexion with true pneumonia existed; and this they had in particular shown to hold good as regards the greater number of cases of lobular pneumonia supervening in the course of a severe malady. But they had not proved that the same held good as respects the lobar pneumonia of new-born children; for whoever has seen the lungs affected with this disorder will scarcely comprehend how their primitive lightness and permeability can be restored to them by insufflation; nevertheless (as it was afterwards remarked by M. Valleix, in the 'Bulletin de Therapeutic,' vol. xxxvi.), a condensed state of the pulmonary tissue disappearing on insufflation may very often be found in very young infants. Still it is an error to suppose that all the cases usually described as infantile pneumonia are of this nature. In such cases, even a denser hepatization than is met with in the adult may be found, the lung rapidly sinking in water, and being quite impervious to insufflation. Again, many of the cases described by MM. Bailly and Legendre would not, from their symptoms during life, be by good observers considered as those of pneumonia.

In 1847, the writers under the direction of M. Fabre,‡ in commenting upon the views of MM. Legendre and Bailly, remarked, that the anatomic details upon which they are founded are too decidedly based or dependent upon the necessity of admitting a particular structural condition of the lung, and one by no means so generally held to be correct as these pathologists seemed to assume; and M. Bourgery, who has most occupied himself with this point, by no means agrees with the doctrines of Reisseisen. On the other hand, Dr. Fuchs§ in 1849 remarked, that it is perfectly immaterial whether the structure of the lung be such as is maintained by Reisseisen

* Die Bronchitis der Kinder. Leipzig, 1849, p. 111.

† Récherches Anatomiques et Cliniques sur quelques Maladies de l'Enfance. Paris, 1846.

‡ Bibliothèque du Médecin-praticien: Maladies des Enfants, t. i. p. 185, sous la direction du Docteur Fabre.

§ Op. cit. p. 68.

or by Rainey. A most determinate opponent of MM. Bailly and Legendre is found in M. Bouchut:* he affirms (1845—52) that the common practice of dividing the pneumonia of children into many varieties, based upon the innumerable forms of its anatomic lesions, has the great drawback of "infinitively dividing" a subject which, on the contrary, demands greater simplification. He opposes the views under consideration, and maintains that no confidence is to be placed on the experiment of insufflation; for "in the child as well as in the adult, in lobular as in lobar pneumonia, in the first as also in the second and third stages, as likewise in that of grey hepatization, the insufflation of the lung is *often possible;* the air may penetrate into the pulmonary vesicles, and permit of the parenchyma floating upon water." According to M. Bouchut, lobular congestion and inflammation of the lobules constitute the *point de départ* of nearly all the pneumonias of young infants; and the lobar form of complete and uniform hepatization of the pulmonary parenchyma is very rarely met with in such patients; when it does occur, also, it scarcely ever presents the same anatomic characters which are generally allotted to the *pneumonie franche* of adults. It is in reality so little different from confluent lobular pneumonia, that a question of *degree* alone divides them. Two forms of lobular pneumonia are laid down by the author—viz., a discrete and a confluent. According to Friedleben,† true lobar pneumonia is one of the most frequent diseases of childhood; whilst the lobular form is a very rare one, very uniformly of secondary origin, and when primitive, occurs only within the first year of life. We may now consider the general bearings of the subject exposed and carried forward to 1849—50, when the work of Dr. Fuchs, from which we have before quoted, appeared. The main points therein inculcated are as follows:—True pneumonia never occurs in children below five years of age, though in them "catarrhal inflammation of the pulmonary vesicles" is frequently met with at the posterior margins of both lobes, and connected with inflammatory congestion of the parenchyma. But this should be regarded rather as a consequence of death than as a contingency of life, and does not deserve the importance attached to it by M. Legendre. Whatever may be the nature of the "nodular," "red," and "blue-grey indurations" met with in the lungs of young children, they are not due to inflammation of the pulmonary parenchyma; they have their *genesis* in bronchitis. The vesicles becoming devoid of air, a portion of the lung fails in, or is deprived of, its function, and it becomes atrophied. "Legendre has rightly grasped the matter; but his appellation [fœtal state] leads to confusion, since it is held as synonymous with atelectasia." . . . . "Under the term atelectasia that condition of the lung is implied which exists before birth and before commencing respiration, and after birth and after respiration. In the former case it is a normal condition; but in the latter, where air should and must penetrate the lung, it becomes an abnormal, even a diseased one: the cause of the latter does not lie in the pulmonary parenchyma itself. The term of 'atelectasia,' as applied to it, has not been selected with due circumspection." . . . . "I designate with the term *apneumatosis* that condition which ensues when a lung previously containing air becomes void of

* Traité Pratique des Maladies des Nouveaux-Nés et des Enfants à la Mammelle. Paris, 1852. Première édition en forme de "Manuel." Paris, 1845.

† Archives für Physiologische Heilkunde. Heft 2.

it." . . . . "A lung in which the respiratory process has commenced and the minor circulation began; into which air has penetrated, and in which the vessels of the minor circulation have become enlarged, and contain more blood, does not retrograde into the fœtal condition, nor does it reassume the quality of the fœtal lung." But after all, according to Dr. Fuchs, M. Legendre has only described the lesion up to the beginning of its second stage, and therefore his views are wanting in completeness. The result of the investigations of Dr. Fuchs appears to be, that that which many pathologists have denominated lobular pneumonia in young children is a diseased condition of the mucous membrane of the bronchia, combined with a contingent lesion of the pulmonary vesicles, which latter exists normally in the fœtus [atelectasia, atelectasis, fœtal condition or state, &c.], or may be afterwards acquired [apneumatosis] from various causes, bronchitis being one of them; the apneumatic lesion presenting certain modifications, according to circumstances, we need not dwell upon. It was natural to expect that much interest should be evinced by those specially prosecuting the study of pædiatrics in awaiting the judgment of MM. Rilliet and Barthez upon the peculiar views promulgated by MM. Legendre and Bailly. In 1851 that judgment was given,* of which we must lay before our readers some details. The anatomic investigations of the latter pathologists receive from MM. Rilliet and Barthez that credit which they so fully deserve; but it is believed that the influence of *congestion* has been exaggerated far beyond that of parenchymatous inflammation—or, in other words, the frequency and importance of the inflammatory element have been too much limited. MM. Legendre and Bailly have also laid far too much stress on what they have termed the prolonged or chronic congestive form, and have mistaken active pulmonary congestion for it. With these provisos, however, they agree to separate entirely lobar pneumonia from lobular pneumonia, which is the catarrhal pneumonia of MM. Legendre and Bailly, and the broncho-pneumonia of themselves. This they are led to do, seeing that their farther investigations in the track so ably indicated by the above writers have, without affording identical results, yet so far agreed as sufficiently to prove the necessity of the separation; though this be necessary, however, no important changes as regard the diagnosis, prognosis, ætiology, and therapeutics, will have to be made to what relates to those points of the maladies in question as laid down in their systematic treatise.† It is in the section on pathologic anatomy that the changes must be made. The labours of their colleagues, and their own verified results, lead to quite a new view of pulmonary inflammation—a view indeed which even MM. Legendre and Bailly have not completely seen through; for in their, for the most part, mere hypostatic and scarcely inflammatory congestion of the vessels, "a real inflammation of the lungs is present, which, moreover, is essentially distinct from hepatization, and is by no means only a gradation of this so common anatomic change."

It is an error to reduce all forms of pneumonia to a single type of anatomic alteration, regarding a difference only as existing in degree and extent. There is at least a form of pulmonic inflammation that cannot be

* Einige Bermerkungen zur Geschichte der Broncho-pneumonie der Kinder. Von E. Barthez, &c., und Rilliet, &c. Journal für Kinderkrankheiten. Band xviii. Heft 5 und 6, 1851, p. 329 et seq.

† Traité Clinique et Pratique des Maladies des Enfants, tom. i.

regarded as only a grade of hepatization, and which even can no more pass into true hepatization, than erysipelas can be metamorphosed into acne. Attention must be directed to two forms of pulmonic inflammation—viz., lobular congestion and partial hepatization. The term *lobular congestion* is used, merely for want of a better one, to distinguish it from true hepatization: it is not what is usually understood by engorgement; there is a very essential difference between them. The term "fœtal condition" is not employed, because it rejects all idea of inflammation, and because it has been employed to designate two textural alterations which cannot be associated together—viz., carnification of the lung, or, rather, collapse of the pulmonary lobules, and lobular congestion. The latter is very frequently met with; it may be said to be one of the most common pulmonary affections, excepting bronchio-pneumonia, and where it has been present, traces of changes in the bronchia are almost sure to be found. Though it may be possible that "lobular congestion" may arise without the influence of pulmonic catarrh, yet it rarely does so; and it must be maintained that lobular congestion is a consequence, or at least an accompaniment, of catarrh. The lung in this affection suffers collapse of the walls of its vesicles, as in the fœtal state, and as shown by Legendre and Bailly. The vesicles are devoid of any contents like plastic lymph, or any other fluidity; the diseased parenchyma, from its turgescence and swelling from congestion, requires more space; the air is thus driven out of the cells, and the walls of the latter become appressed. In many cases after death, air cannot be made to penetrate all the vesicles by insufflation; this occurs, probably, when the parenchyma between them is thickened or engorged. This engorgement is to be regarded "as the result of an extra-vesicular and extra-vascular secretion, and must be viewed as the second degree of a true inflammation of the cellular tissue." It is difficult to conceive how this "lobular congestion" is always the result merely of hypostasis, or identical with that arising in the *agony*. That the conditions favouring hypostasis favour the production of the other may be true; but it is to be maintained, that very frequently, if not always, a truly active and even inflammatory process has been in operation at the same time. There may be three causes of production in operation—inflammation, hypostasis, and an hindrance to the entrance of air into the bronchia; and according to the intensity of either, so may the pulmonary congestion present somewhat different characters. But this pulmonary lobular congestion, under any circumstances—partly from its specific nature, from its seat, and the condition of the bronchial mucous membrane—has but very slight tendency to give rise to plastic effusion in the pulmonic vesicles, and which characterizes lobular and primitive hepatization; nevertheless, there cannot absolutely be denied to exist a certain relationship between the two pathologic changes, and the possibility of an apparent transition of one into the other in the following way: "It may happen, that under the influences of a strong constitution, of age, of the nature of a previous disease, &c., the active congestion, with its effusions, may assume a more inflammatory character, and evince a plastic tendency, so that a true extra-vesicular hepatization may follow on the lobular congestion."

It would be beyond our purpose to enter more fully into the details of MM. Rilliet and Barthez' elaborate memoir; we shall therefore conclude our analysis of it by the following quotation, which terminates the essay:—"Lobular congestion is, as well as partial and generalized hepatization, in genetic relation with pulmonary catarrh. Upon this point the results of post-mortem investigation, in connexion with the evidences of auscultation and the progress of the symptoms, leave not the slightest doubt. In rare cases, some isolated nuclei of hepatization are met with in the lung, and which cannot be ascribed to catarrh; but this is an exception which cannot enforce the complete separation of partial hepatization and catarrhal pneumonia. The same reasons enforce the coalition of capillary bronchitis with congestion and hepatization. The one cannot be separated from the other; the only difference lies in the less frequent occurrence of hepatization." (p. 350, *op. cit.*)

In 1852 M. Hervieux, in some remarks* upon the "effect of prolonged horizontal posture in the production of the great mortality in foundling hospitals," asserted that such posture produces sclerema, &c., from which some children die, whilst others succumb "à ces pretendues pneumonies qui ne sont autre chose que des stases sanguines." M. Bailly has since† replied to this statement of M. Hervieux, and recalled attention to the fact of his having already, ten years since, asserted that the pneumonias so fatal to young children in hospitals are due to the stasis of blood consequent upon prolonged decubitus. Since then, M. Bailly, having witnessed disease under different circumstances, has not met with those numerous examples of bronchial catarrh generating fatal pneumonia, or of children dying from sclerema, and with their lungs gorged with blood. It may be doubted, indeed, whether such affections are not entirely generated in hospitals, and also that much of what is contained in works on Pædiatrics, deduced from hospital investigation, is of any applicability to private practice. Finally, we would remark, that at the commencement of the present year, Dr. Gairdner, in an able article on 'Bronchitis, Pulmonary Collapse, and Emphysema,'‡ expressed his opinion that the physical condition of the lung in the so-called "lobular pneumonia," is a form of condensation having no connexion whatever with any of the ordinary types of inflammation or hepatization; but "that it has the closest possible analogy, if not identity of character, with the unexpanded condition of the lung of the infant just born." In the opinion of the writer, neither the views of Legendre nor of Rilliet afford a satisfactory explanation of the forms of pulmonary condensation under review. "The *état fœtal*, or collapse of the air-cells, when occurring in a lung that has been once expanded, is in all probability a secondary lesion, and dependent, in the majority of instances, on a catarrhal condition of the bronchial tubes"—a conclusion, as we have already seen, arrived at by Fuchs. Dr. Gairdner also maintains the opinion, "that, in some exceptional instances at least, collapse of the lung, even in its lobular form, is a disease of adult life," and that the reason of the great liability of the child to pulmonary collapse, as a consequence of bronchial obstruction, is chiefly seen in the want of resistance in the bones of the infantile chest preventing the muscles from acting on individual portions of it with the force necessary, in some cases, to overcome bronchial obstruction, and in the frequent coexisting debility

* L'Union Médicale. Paris, 1852, Nos. 139, 140. † Ibid., No. 149.

‡ British and Foreign Medico-Chirurgical Review, No. xxii. p. 452, April, 1853.

and dyscrasia which are such strong predisponents to the occurrence of the lesion under review.

We should not feel satisfied that we had sufficiently developed the history of infantile pneumonia without drawing attention to certain views very lately (1851) promulgated by M. Baron, of Paris.* It is true that they may be said more especially to relate to "œdema of the lungs" than to pneumonia; but as the author is so intimately concerned with "carnification," and which we have already found to be such an object of dispute, it is necessary to allude to the views in question. Previous to doing so, however, it may be advisable to remark, that Billard, Gardien, Berton, Rayer, Gregory, Rilliet, and others, had drawn attention to œdema of the lungs in children, as Laennec had to the affection existing in adults. "Carnification" was spoken of by some as accompanying it; Berton thought the œdema depended upon inflammation, and of the lungs more frequently than of the bronchia. Billard imagined there might exist some connexion between the cause of œdema of new-born children, and the serous infiltration of the lungs; whilst Gardien opined that this infiltration might be idiopathic, or at least as offering no evident cause for its production. By some, fluid was stated always to exude when the lung was cut or pressed, which fluid was believed to have been effused into the cavities of the pulmonary cells. In 1843, MM. Rilliet and Barthez pronounced judgment, based upon the observations of 77 cases. It was affirmed that the causes of the pulmonary œdema were those common to all dropsies, and only special as regards the lungs in certain very rare cases. That in all probability there existed a vesicular and an interlobular œdema, but that this is a very difficult point to settle, and one in reference to which no special investigations had been instituted. "On section of the lung an abundance of serous fluid escapes"—"placed in water, the pulmonary tissue always swims, either wholly or in part"—"the fluid is mixed with air—when the œdema coincides with pneumonia, or with carnification, but then these two affections merit the appellation of œdematous." In 1846, MM. Legendre and Bailly discussed the matter relative to the affection occurring after scarlatina, and stated it to be a dropsy of the interlobular cellular tissue, and not to be intra-vesicular œdema. By this interlobular cellular infiltration the air-cells become compressed, giving rise to the dyspnœa experienced by the patient. Puncture of the lung allowed the fluid to escape, and insufflation was practicable, and caused the pulmonary parenchyma to reassume its natural tension, crepitation, and colour. In 1847, Dr. West stated his opinion that he had no doubt M. Legendre was correct in his opinions. In 1851, appeared the elaborate memoir of M. Baron "On Carnification of the Lungs in Children, and on its distinction from Hepatization." The author commences by remarking upon the fact of the former lesion having been by some considered as equivalent to the latter; by others, (Valleix and Rufz,) as different to ordinary hepatization, but as yet undetermined in its nature; and by Legendre and Bailly, and their school, as the consequence of the non-penetration of air in a given portion of the lung, which thus retrogrades, to a certain extent, to the fœtal condition.

* Gazette Médicale de Paris, 1851, Nos. 46, 47, 48, 52; also, Journal für Kinderkrankheiten, Band xviii. Heft 3 und 4, from which we quote.

This latter theory, although capable of application to a large number of cases, is as little applicable to the explanation of all as the theory of inflammation. Hitherto it has been the case, that the subject of carnification has been developed only from its occurrence in the pulmonary organs. But it occurs elsewhere, and it can only receive its proper development from its general consideration, the more particularly as its character and causes are everywhere identical. M. Baron then gives the details of 31 cases, and comments upon them. The youngest patient was five days, the eldest 73 years old; the majority of the patients were children. Carnification is held to be most common in new-born children; after the fifth year it is comparatively unfrequent in occurrence; whilst in adults it is very rarely met with; but in aged people again becomes more common, though still less so than in early life. The characters of this carnification of M. Baron differ, however, from those given by M. Legendre as illustrative of his "fœtal state," both simple and congestional. From "engorgement" it also differs, as well as from hepatization; nor does it include the discrete and confluent pneumonias of various writers. In nearly all the cases it was found in connexion with the remains of serous effusion, either in the carnified organ itself, or in other parts of the body. The lesion has its genesis in the effects of such effusion. The fluid infiltrated into the organized tissue ceases by degrees to be isolated or distinct from such tissue, but gradually becomes identified or in intimate connexion with it. By this the tissue loses its normal character, becomes hardened and carnified, whilst the infiltrated fluid loses by degrees its normality. When the carnification has attained its maximum development no more fluid can be pressed from the part. The character last attained to is that of *induration*. In a very small number of cases whilst there was carnification of one lung there was evident inflammation of the other. Were such cases more frequently met with, considerable ground might exist for believing in the inflammatory nature of the lesion in question, or at any rate for thinking that inflammation had some sort of causative connexion with the latter. But at present we must assume such connexion to be but of slight intensity. *Splenisation* depends (as does carnification) upon the infiltration of fluid, and its development is favoured by conditions differing very slightly from those favouring the latter. Between splenisation and commencing carnification there is such similarity, that it may be often difficult to say which lesion we have before us. Still there are distinctions. In pulmonary carnification the walls of the air-cells, though appearing to be so closely appressed as to destroy vacuity, are not so in reality, as insufflation can prove. The contrary is the case in hepatization,—the cells cannot be inflated; in the latter, the changes are internal, so to speak, whilst in the former they are external, in regard to the air-cells.

Art. V.

On a Case in which the Sarcina Ventriculi, seu Sarcina Goodsirii, was found in the fluid removed from the ventricles of the human brain. By William Jenner, M.D., F.R.C.P., Professor of Pathological Anatomy, University College; Physician to the Hospital for Sick Children, &c.

The sarcina Goodsirii has been found, not only in adults and children, but also in the lower animals—viz., the rabbit, the dog, and the tortoise.* It is by no means rarely present in the matters ejected from the human stomach and intestinal canal. In 1847, Heller† described a case in which he discovered sarcinæ in the urine; subsequently, Dr. Mackay‡ detected them in the same fluid; and Virchow§ and Zencker‖ in the lungs.

To all of the last-mentioned cases it has been objected that the sarcinæ were in reality derived from the stomach; that when found in the urine, some of the vomited matters had been accidentally mixed with that fluid after its escape from the bladder; that when found in the lungs, the sarcinæ had passed into them during the act of vomiting. The more recent observations of Heller¶ have, however, placed beyond a doubt the fact that sarcinæ Goodsirii are occasionally present in the urine when that fluid is free from admixture with vomited matters. In one of the two cases last described by Heller, the urine was examined for many weeks in succession, and a sediment in it of an inch in depth sometimes observed, composed of sarcinæ only, or of sarcinæ mingled with a little carbonate of lime. The following case lends support to the foregoing evidence in favour of the opinion that the stomach and intestinal canal are not the exclusive seats of the development of the sarcinæ Goodsirii.

A boy aged four years was admitted under my care into the Hospital for Sick Children, June 24th, 1852; he died July 3rd. The child was of fair complexion, well-made, and moderately stout. His illness was reported to have commenced on June 17th, with pain in the head. On the 19th, he was taken as an out-patient to King's College Hospital; subsequently to that time he never complained of headache, only he seemed heavy. He did not keep to his bed till the 23rd. After his admission into the children's hospital, the most prominent symptoms were, drowsiness, talking in sleep, an irregularly diffused scarlet rash on the skin, redness and swelling of the tonsils, with a white patch on the left tonsil, frequent pulse, sordes about the teeth, and some dryness and brownness of the tongue. On the 30th my notes say, "Almost incessant grinding of the teeth; seems quite sensible when awake; asks for the cold wash to be applied to his head."

Between the 30th of June and the day of the child's death,—i.e., July 3rd,—restlessness, strabismus, inequality of the pupils, and redness of the

* Canstatt's Jahresbericht, Dritter Band, 1850.

† Eine eigenthümliches Harn sediment. Archiv für physiologische und pathologische Chemie und Mikroskopie, Heft 4.

‡ Lectures on Clinical Medicine, Dr. J. H. Bennett, July, 1851.

§ Schlossberger, Die Sarcina, Württemberg, Corresp. Bl. N. 26.

‖ Henle's Zeitschrift, Band iii.

¶ Archiv für physiologische und pathologische Chemie, &c., 1852, p. 30.

conjunctivæ. General convulsion commenced about four A.M. on the 3rd, and continued till the child's death at half-past four A.M.

The examination of the body was commenced ten and a half hours after death. The vessels of the dura mater were more filled with blood than is usual. Numerous semi-transparent grey granulations were seated *on the arachnoid lining the dura mater*, and a few similar granulations on the visceral arachnoid. The arachnoid itself was dry; the cerebral convolutions were flattened; the minute vessels of the pia mater, on the surface of the convolutions, were abnormally injected with blood. In the grey matter of the cerebrum were about fifteen masses of yellow tubercle—the largest was oval, half an inch by a quarter of an inch—the smallest about the size of a very large pin's head. The pia mater, dipping between the convolutions, was studded with grey granulations. Four ounces of colourless serosity were removed from the lateral ventricles—that which first escaped on opening the ventricles was transparent, that which flowed towards the last was turbid. The fornix and septum lucidum were white and of a creamy consistence. Viewed from within, the floor of the third ventricle was highly vascular. The membranes covering the base of the brain were opaque, tough, and loaded with serosity. The fluid removed from the ventricles was alkaline and albuminous; after standing twenty-four hours the deposit of albumen constituted one-seventh of the fluid tested. In the cerebellum were several masses of yellow tubercle.

Microscopical Characters of the softened Fornix and Septum Lucidum.—There were no granular corpuscles, no free fat-granules, detected in the softened septum lucidum and fornix. In the cerebral substance, adjacent to some of the tubercles, were numerous large granular corpuscles.

The peritoneum was studded with grey granulations; it was abnormally vascular. Grey granulations studded the pleuræ, pericardium, and lungs; the bronchial glands were stuffed with tubercle; the liver was dotted throughout with small transparent, grey granulations; yellow tubercles in considerable number were found in the spleen and kidneys; there was an ulcer on either tonsil. The small and large intestines were extensively ulcerated; the edge and floor of some of the ulcers were covered with tubercles.

July 5th, 11 A.M.—The fluid removed from the lateral ventricles of the brain was examined more particularly than it had previously been. After its removal from the cerebrum, on the 3rd, the fluid had been kept in an open glass vessel, in a large, light, and airy room. It turned turmeric paper brown; its odour was sickly, brain-like, not ammoniacal; it seemed as if just beginning to decompose. There was a little sediment at the bottom of the vessel.

A drop of the fluid containing some of the sediment was examined with a magnifying power of about 200 diameters; the following objects, *and those* ONLY, were contained in it:

1. A considerable number of spherical bodies $\frac{1}{4000}$th of an inch in diameter, of a pale yellowish colour, (blood-discs altered in form?)
2. Square bodies, each side of which measured $\frac{1}{2000}$th of an inch; some were a little larger than this, others a little smaller. The surface of each body was divided by cross lines into four equally-sized compartments, and each of these quarters was again divided into four.

In some of the bodies the lines producing the secondary quarterings were well, in others imperfectly, marked; while in others these secondary quarters were themselves divided by cross lines into four parts. The angles of the bodies were somewhat rounded. Some of these bodies were, my notes state, " as well-formed sarcinæ as I have ever seen."

3. Oval bodies about $\frac{1}{5000}$th of an inch in breadth, and $\frac{1}{2500}$th of an inch in length, distinctly divided by a transverse line into two equal parts; each of these two parts being very obscurely divided by a longitudinal line into two other parts.

The fluid removed from the pericardium had been standing side by side with that removed from the ventricles of the brain; it was alkaline, but contained no sarcinæ.

It was a question when this child came under observation, whether or not he was suffering from scarlatina. The fatal termination was evidently the result of acute tuberculosis. Unfortunately, the fluid from the ventricles of the brain was not examined for forty-eight hours after its removal from the body. The questions, therefore, arise—Were the sarcinæ in this case developed after the fluid in which they were found was taken from the cerebral ventricles? Were they developed after death, but within the body? Did they exist in the ventricles while the child was yet living? To whichever of these questions the answer be in the affirmative, the occurrence of sarcinæ under the conditions mentioned is remarkable.

The fluid in which sarcinæ have been found in the stomach has been on all occasions acid; and in the same situation vinous fermentation has been their constant concomitant. So invariably have the torulæ cerevisiæ, and sarcinæ Goodsirii been found together, that Simon of Halle* has maintained that the latter are merely an advanced stage of the development of the yeast plant. In the case I have detailed, as in one of Heller's cases, the fluid in which the sarcinæ formed was alkaline, and in it there was no trace of torulæ, and no evolution of gas. Supposing the sarcinæ to have been developed in the fluid after its removal from the body, then this case stands alone, inasmuch as they have never heretofore been known to form, except in the interior of the bodies of animals.

As to the stages in the development of the sarcinæ, they would appear from this case to be briefly these. A simple cell is divided into two parts by a transverse line; each of the two cells thus formed are again divided into two by a longitudinal line; each quarter of the primary cell subsequently experiences the same changes as the primary cell itself. This description of the mode of development of the sarcinæ differs *in toto* from that given by Simon of Halle,† who states that the primary cell increases its size and changes its form by the formation of nuclei in its interior; and as widely from that given by Pockel,‡ who affirms that the increase in size of the primary cell is partly the effect of endogenous cell-formation, and partly of gemmation, while comparatively it differs but little from that given by Frerichs.§

* Ueber die Entwicklung der Sarcine aus dem Hefenpilze. Virchow's Archiv, Band ii.

† Loc. cit. ‡ Nonnulla de Sarcina Goodsirii, Wirceburgi.

§ Ueber Sarcina Ventriculi. Häser's Archiv, Band x.

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF MICROLOGY.

By Robert D. Lyons, M.B., T.C.D., M.R.I.A.
Honorary Professor of Anatomy to the Royal Dublin Society, &c. &c.

Part II. — Pathological Micrology.

HISTOLYSIS, OR DECAY AND DISSOLUTION OF TISSUES.

Intermediate between the normal conditions of the tissues in health, and the changes which they undergo in disease, a class of structural alterations may be placed, to which as yet no distinct position has been assigned, in either anatomical or pathological studies. We allude to the *alterations in form* which occur in the process of decay, as we trace an organized body through the several stages of putrefaction to its final dissolution.

Some investigations on this interesting and important class of subjects have been undertaken by the writer.†

Several instances having come under his notice in which structures submitted to microscopic examination appeared to have undergone considerable alteration, by reason of a more or less advanced condition of decomposition, he was led to institute a series of observations for the purpose of discovering the order (if any) of the morphic changes which take place in the passage of organized bodies through the several stages of putrefaction to their final dissolution and decay, until they return, "ashes to ashes," and "dust to dust." As he believes that the researches he has already made warrant him in stating that a certain order of morphic changes is brought about, he has introduced the term Histolysis, to designate the morphic changes of putrefying tissues, the use of a single word being convenient for the purposes of description and reference.

Not only will the study of histolysis be found interesting in itself, as a portion of scientific inquiry, presenting, as it does, several beautiful and unexpected phenomena; but, moreover, the knowledge thereby acquired admits of several valuable and practical applications. Thus, it affords most favourable opportunities for the study of the intimate structure of complicated normal textures, which are thus, as it were, unfolded to our view by a process of natural dissection, in which the least possible violence is done to the most delicate parts. In the memoir referred to the writer details some of the observations which he has already made;

* In the next number of this Journal, Quarterly Reports on Medicine, Surgery, Midwifery, and Medical Jurisprudence will be commenced, in place of the abstracts at present given. The Annals of Micrology and Physiology will be continued half-yearly. Every third or fourth number a Report on Materia Medica and on General Therapeutics will be given; and, as occasion may require, every year, or every eighteen months, a Chemical Retrospect will be published. The Therapeutical Record will be continued as at present, and a section will be introduced for miscellaneous medical topics, for which a place may not easily be found under the above-mentioned headings.

† Researches on the Primary Stages of Histogenesis and Histolysis, by Robert D. Lyons, M.R.I.A. &c. (Proceedings of the Royal Irish Academy, vol. v. part 3, 1853.)

they relate to cutaneous structures and fat, some varieties of human and animal blood, and muscular fibre.

1. *Integument.*—This specimen had undergone change for four months. The epithelium of the cuticle was entirely destroyed, its place being supplied by a soft, pulpy mass, which presented an amorpho-granular structure under the microscope.

2. *Subcutaneous Structures after* 6½ *Months.*—They had the appearances of what is known as cadaveric fat; under the microscope, D. 900, there was seen an abundance of minute but well-defined granules, with fine, dark, well-marked border, and clear, transparent centre; they were arranged in masses, isolated, and in groups; small granular corpuscles of different sizes, and a few oil-globules; very large oval, pale, semi-opaque cells or vesicles; they had no nuclei; their borders were clear and well-defined, but most of them presented fissures, some as many as seven or eight, which extended inwards for about a sixth of the smaller axis; a very remarkable linear or moniliform arrangement of granules presented itself in certain parts of the field, sets or rows of granules being arranged longitudinally, parallel, or convergent, and apparently in connexion with very faint subjacent striations in the same direction. The writer regards this as an instance of granular disintegration of a band of fibre;—lastly, numerous stellate crystals, apparently of the fatty acids, lay scattered over the field, some also being included in cells, and appearing to be the stage of *lysis* of their previously granular contents, preparatory to the final rupture and dissolution of the including membrane.

3. *Blood.*—He has met with specimens of blood in which changes took place with great rapidity; in one specimen, after twenty-six hours, he found the hæmatine had escaped from numerous corpuscles, and had assumed the shape of granular masses, heaps, and crystals (hæmatoid crystals of Virchow).

4. *Histolysis in Fluid Blood.*—Blood of duck after two years (kept in a bottle). There was presented an assemblage of forms widely different from those of the natural blood; none of the characteristic elliptical corpuscles of this animal's blood was to be found. There existed abundance of granules, granular corpuscles, and spherical vesicles of moderate size. There were also numerous prisms and needles of hæmatine, and large irregular hæmatine masses. He has since examined a specimen of human blood five years old; it was fluid, and the appearance even similar to those now given.

5. *Blood of Salmon.*—Of same date as last, and presenting nearly the same appearances; the granular bodies were, however, much larger; there existed also very large cells, including vesicles, and masses of granular base. Cells, including stellate crystals, were also visible. He is disposed to think that these forms resulted from the greater quantity of oil in the blood of fish, which, with the albuminous element, gave abundant means for these formations.

6. *Histolytic Changes in Muscle.*—The specimens examined had undergone the process of putrefaction for a considerable time; yet, by the aid of the microscope, the nature of the structures admitted of demonstration in a clear and positive manner, though, by the unaided sight no amount of careful study would have sufficed for their recognition and identification. Under a power of 900 D., the following appearances were presented:—A semi-fluid granular mass, tinged of a light brown colour, in which granules, granular corpuscles, and spherules, were visible; here and there lay larger and smaller masses of elementary fibres, many of which retained, either in whole or in part, their characteristic striæ, but presented internally more or less change. There were also various stellate and acicular crystals, some within cells, others free on the field.

These results of the histolysis of muscle are incomplete, yet the author thinks they give indications of the mode in which the tissue breaks up.*

* At p. 455 of the present number will be found a quotation embodying the general results of the inquiry.

In connexion with this subject, we may notice one or two observations of other authors, made evidently without any definite object in view, and with which the writer was quite unacquainted at the date of publication of his researches. Virchow* gives some account of an examination made by him of an amputated leg and foot which had lain for a considerable time in maceration; the parts were converted into a soft white substance, having but little smell, which had all the characters of adipocire, even its fusibility. Gibbes (quoted by Virchow) appears to have made some observations on macerating bodies.† Quain, also, has engaged in a similar inquiry.

DEGENERATIONS.

Fatty Degenerations.—This important class of subjects has been very fully considered elsewhere,‡ and we shall now merely refer to our former pages. There appears good reason to believe that the process of fatty degeneration is the mode by which the uterus *post partum* is restored to its normal dimensions. Professor Retzius has already investigated this subject; and more recently, a memoir has been devoted to it by Dr. Heschl.§ This author states that the proper substance of the uterus undergoes so complete a transformation into molecular fat, that not one single fibre of the organ existing previous to child-birth remains behind. This transformation he has not observed to commence before the fourth or sixth days, and not later than the eighth. In the single muscular fibres, the process begins in many points at once; the outlines become pale, and there appear yellow granules, which, when the ends of the fibre-cells are thin, lead to their early dissolution.

ABNORMAL CONDITIONS OF BLOOD.

Leukæmia Lienalis (*Leucocythemia*, Bennett).—Dr. J. P. Uhle‖ has published the results of his investigation of a remarkable case of this affection of the blood. The patient had from early life been the subject of various attacks of disease. Signs of enlargement of the spleen were observed for the first time four months before death; the tumour increased in size until finally it reached the mesial line, and extended from the ninth rib to the crest of the ilium; the liver was but slightly increased in dimensions. Blood withdrawn by cupping presented a dark brown-red colour, which, however, became clearer on exposure to the air, and formed a clear reddish-brown coagulum. Under the microscope, the colourless corpuscles were found to be remarkably increased in quantity, and on being counted, appeared to equal in number the coloured corpuscles on the field. The latter presented nothing abnormal; the colourless corpuscles, however, differed much in size, some being enormously large, equal to double their usual dimensions; all of them presented regular round borders, and generally a pale granular surface. On the addition of acetic acid, two to three, seldom four, large nuclei were brought into view, presenting the greatest variety in their manner of arrangement. When the blood was treated with water, the colourless cells became much swollen, the nucleus, however, undergoing no change.

The blood was again examined two months subsequently, and ten days before death. After rest for twenty-five hours it presented a perfectly clear serum, occupying one-fourth of the entire mass; beneath lay a small soft coagulum, covered with a clear red thin layer, transparent at its borders, under which there was a somewhat thicker dark-red coagulum. Under the microscope, the quantity of the colourless cells appeared to be about $\frac{1}{3}$ to $\frac{1}{5}$ of the coloured. The size of the colourless cells was more uniform than on the former examination, their diameter varying between 0·009 and 0·013^{mm}. After death, the blood showed many differ-

* Verhandlungen der Phys. Med. Gesellschaft in Würzburg, Band iii. Heft 3, 1852.

† Philosophical Transactions, 1794.

‡ British and Foreign Medico-Chirurgical Review, April and July, 1853.

§ Researches on the Condition of the Uterus after Delivery, by Dr. R. Heschl, Vienna, translated from the German. Dublin, 1853.

‖ Virchow's Archiv, Band v. Heft 3, p. 276.

ences in regard to the colourless cells, in portions taken from different vessels. Blood taken from the left side of the heart (which, however, appears to have been mixed with some from the jugular vein) presented appearances very similar to those found in that last drawn during life. The coloured and colourless cells were in the proportion of 5 : 3 to 3 : 2 ; the colourless showed no remarkable differences in size, their average diameter being 0·011^{mm}. In the blood of the splenic vein, the colourless corpuscles appeared to be as numerous as the coloured, if not more so; most of them were finely granular; in many the cell-membrane was difficult to be seen; and without the addition of any reagent, a nucleus, usually single, only rarely bi- or tri-partite, could be distinguished. A few pale spindle-shaped bodies, with oblong nuclei, were occasionally met with; also a molecular mass, apparently a coagulation-product, with fine punctiform granulations, which surrounded these bodies. No blood-corpuscle-holding cells were observed. In the blood of the vena portæ, the colourless cells were from $\frac{1}{3}$ to $\frac{1}{2}$ the quantity of the coloured. In both the last-named vessels, their average diameter was 0·009 to 0·011^{mm}. In the clear reddish-grey upper layer of the coagulum from the basilar artery, there were to be seen only colourless cells embedded in fibrine. In the blood from the jugular vein, the colourless cells were of extraordinary size, measuring 0·013 to 0·015^{mm}; they were in general darkly granular, showing a nucleus only on the addition of water or acetic acid. The thyroid gland, as well as the lymphatic glands generally, presented only their usual elements. The author regards this case as a very good example of pure *Leukæmia Lienalis*, as there was no process of purulent formation either in the external or internal organs, no loss of blood, no alteration in the lymphatic glands, and no intermittent fever. To determine definitely the relation of this blood-change to hypertrophy of the spleen, he suggests the necessity of a careful investigation of the blood in cases of slight hypertrophy of the organ ; and also the estimation of the size of the spleen in those cases in which a slight increase in the quantity of the colourless corpuscles has been already observed (pregnancy, puerperal states, &c.)

Lymphatic Glandular Hypertrophy, without Leukæmia.—In a note to the foregoing communication, Virchow states that he has met with a case of most enormous general lymphatic glandular hypertrophy, embracing the axillary, jugular, and other glands, in which no observable alteration of the blood existed.

Leukæmia and Pyæmia.—Dr. Griesinger* has investigated the relative proportion of the colourless corpuscles of the blood in cases of leukæmia, in different parts of the vascular system.†

An important case of leukæmia has been very ably reported by Leudet,‡ who gives also a succinct general resumé of the researches hitherto prosecuted by other observers. Holland§ has observed leucocythemia in two cases in which the thyroid gland was enlarged, while the spleen and lymphatic glands generally preserved their normal condition. In the second case, the increase in number of the white corpuscles was well-marked in the venous blood (that of the pulmonary artery), though it was not observable in the arterial (that of the aorta).

In connexion with this subject, we may consider the researches of Moleschott‖ on the results with regard to the variations in quantity of the corpuscles of the blood, produced by removal of the liver and spleen. His experiments were performed on frogs, and they have led him to some interesting deductions. Having arranged a suitable apparatus, he was able to estimate the quantity of carbonic acid produced by frogs before and after exsection of the liver; the mean of his observations gives only half the standard quantity of carbonic acid for frogs after removal of the liver. With reference to the corpuscular elements of the

* Virchow's Archiv, Band v. Heft 3, p. 391. † See Chronicle, No. 22, July, p. 269.
‡ Gazette Médicale de Paris, No. 24, 1853.
§ Quarterly Journal of Microscopic Science, No. 3, 1853.
‖ Müller's Archives, Heft 1, 1853. See also our last number, p. 252.

blood, in conjunction with Donders, he had on a former occasion determined the relations of the colourless to the red corpuscles in the heart's blood of the frog as 1 to 8. The mean of nineteen observations of the blood after removal of the liver, gave the numbers of the colourless to the red corpuscles as 1 to 2,24. In some cases life was maintained for many days after the excision of the liver, till the thirteenth day in one instance. Removal of the spleen seemed to produce but little inconvenience to the animal: this author says, that after this operation he scarcely lost a single frog. The results with regard to the numerical proportions of the corpuscles are curious. The proportion of the colourless to the coloured after removal of the spleen appears to have been lessened. The mean of twenty-two observations gives for the number of the white to the red corpuscles in the blood of the heart, 1 to 9,06. When both liver and spleen were removed, then the influence of the removal of the liver predominated; thus, on the fifth day after the operation, the colourless corpuscles of the heart's blood were to the coloured as 1 to 1,35; on the sixth day as 1 to 2,69; mean 1 to 2,02.

The author believes that by the removal of the liver, and consequent increase in the relative proportions of the white corpuscles of the blood, we are enabled to trace the development of these bodies into the coloured, with much greater facility than under ordinary conditions. Some figures accompany the paper, illustrative of his views. He believes to have followed the changes in form of the colourless cells, with division and disappearance of their nuclei, to the formation of the elliptical vesicle, which he considers it erroneous to regard as nucleated. The body described as a nucleus he conceives to result only from coagulation of the contents of the vesicle, and to be entirely absent in fresh and perfect blood-cells in the frog.

M. Chaumont* has conducted some experiments on the action of chloroform on the blood-corpuscles. He finds that a gradual solution of the cell-wall of the red corpuscles is effected, and suggests the following explanation of the re-action. The effect of chloroform is like that of acetic acid, but is produced more slowly. Ether has no similar effect. Regarding the composition of these substances, he conceives that the solvent effect is manifested in the direct ratio of the electro-negative nature of the re-agent. Thus acetic acid dissolves the cell-walls speedily; chloroform dissolves them, but more slowly; ether does not dissolve them at all. The action is thus directly as the electro-negative natures of the re-agents for albuminous, and the opposite for the case of fatty substances: contrary to the opinion of some observers, he finds that the colour of blood is much heightened by chloroform. Venous blood assuming a brilliant arterial hue, in some cases it even returns the bright colour to blood-stains on cloth, &c.

MISCELLANEOUS LESIONS.

Pathology of Bronchio-pulmonary Mucous Membrane.—Dr. Black† has continued his researches on this subject. He recognises three forms of acute inflammation of the bronchio-pulmonary membrane. 1. Simple acute epithelial bronchitis. 2. Bronchitis involving the submucous tissue. 3. Cellulitis, or inflammation of the epithelium of the pulmonary cells. In the first form, the secretion, examined microscopically, consists of well-formed mucus-corpuscles (young unflattened epithelial cells?), mingled with epithelial or basement patches, floating in a viscid fluid menstruum. The epithelial patches are produced by a blighting of their cells, caused by the suspension of the natural fluid transudations from the blood. Sometimes the patches are formed entirely of epithelial cells having attained an almost perfect development, but which yet cohere by their edges. Occasionally, the action of the cilia can be seen on the surface of mucus-corpuscles (?) which have just been expelled.

* Proceedings of Edinburgh Physiological Society: Edinburgh Monthly Journal, April, 1853.

† Edinburgh Monthly Journal of Medicine, March, April, May, and June, 1853.

When in the second form, exudation takes place into the submucous tissue, it coagulates by virtue of its contained fibrin, and a nutritive matrix is thus formed similar to the basement-membrane in the healthy condition and action of that structure; germinal centres are produced by molecular aggregation of the fibrin, which subsequently assume the shape and endowments of cells. Exudation masses, cells, and molecules of fibrin, epithelial patches, and casts of the tubes, are present *invariably* (lec^{m} auct^{m}) in the exudation during its early stage; they are therefore diagnostic of the existence of inflammatory exudation. The conversion into pus-cells, the author supposes to be due to the action of oxygen in the structures of the plastic corpuscles, by which they undergo an *adipocerous degeneration*. Hence the origin of the fat which invariably forms a chemical constituent of pus; and hence the colour of this latter fluid as a consequence of the saponification of such fat by the alkaline present in the exudation.

In *pulmonary cellulitis*, a term used to designate inflammation confined to the epithelium of the pulmonary cells (air-cells), when the pathological condition is fully established, the sputa present the desquamated epithelial cells, well-developed mucus-corpuscles, and a few points and masses of coagulated exudation. Variations will be observed in advanced forms of the disease, the cellular elements being more or less abortive. When a constitutional peculiarity in particular diathesis, or another disease is associated with cellulitis, the exudation from the pulmonary membrane frequently contains evidence of the presence of certain products dependent on the diseased condition of the system. Thus in the uric-acid diathesis the author has observed urate of ammonia, and uric acid in the sputa of cellulitis; in the oxalic-acid diathesis he has seen distinct crystals of oxalate of lime, and occasionally cystine; while in jaundice, cholesterine and the colouring matter of the bile (biliphin) were found. He believes that such morbid products are eliminated by the epithelial cells of the membrane; that they therefore appear as accidental constituents of the mucus-cells, that they likewise escape in the exudation from the denuded surfaces of the basement-membrane, and that they are never present in the exudation-cells. That the bronchio-pulmonary membrane can thus act the part of an excretory organ, but that this peculiarity of action is dependent rather on the particular character of the blood than on any selective power inherent in the epithelial cells. That before such eliminative action takes place, an undue accumulation of morbid products occurs in the blood; that the action of the different organs intended for the elimination of such morbid products is for the time deficient; and that on its becoming more vigorous, the sputa regain their simply morbid character.

Inner Callus, its composition and significance.—Dr. Ulrich Hilty* has investigated the formation of callus within the medullary canal, in fractured bones. His experiments were conducted by inserting pegs of ivory and silver wire into the tibia in cats and rabbits. In all the cases he found a deposit of inner callus, distinguishable from the surrounding bone by its white colour. On microscopic examination it showed numerous irregular vascular canals, in continuity with those of the old bone; in the bony mass itself there was to be seen a turbid hyaline substance, with round and elongated bone corpuscles, having long and tortuous radiating canaliculi.

Inflammation of Joints.—Dr. F. Führer† has prosecuted some investigations into the different forms of inflammations of the joints. Some interesting results are given with reference to the changes produced in the structures of the diseased joints. He believes that the softening and removal of the cartilage is brought about principally by a proliferous growth of cartilage-cells. In hypertrophy of the cartilage there takes place, under certain conditions, a superficial fibrillation of the

* Henle und Pfeuffer, Zeitschrift, Band iii. Heft 2.
† Virchow's Archiv, Band v. Heft 2, p. 151.

basic structure, and some lamellæ of the fibrinous cartilage may assume a membranous appearance. The bony eminences thrown out round joints, in *Arthritis chronica sicca*, are not entirely owing to pressure and excavation of the joint cavity, or to expansion of the head of the bone, but are mostly new deposits, consisting of bone and cartilage. Fatty degeneration of the muscles and nerves in the neighbourhood of the diseased joints was observed; also a fatty degeneration of the bone-corpuscles. In two cases there was found a fibrous thickening of the nerve-sheaths; and a fibrous degeneration of the muscular fibre in one instance.

Dermoid Cysts. Plastic Heterotopia.—Lebert* has investigated this subject in a series of valuable memoirs laid before the Societé de Biologie. M. Lebert acknowledges his obligations to the essay of Meckel (1815), on the abnormal development of hairs and teeth; to the work of Voigtel, and to the essay of Stahlberg (1842). According to the researches of these authors, confirmed by his own, the right ovary seems to have a peculiar predisposition to the formation of these cysts. He concludes, from the study of the structure of these dermoid cysts, that they are the result, in all cases, of a tegumentary heterotopia, and not of any modification of the products of conception. Proceeding from without inwards, we perceive first a more or less vascular fibro-cellular envelope, on the internal surface of which there exists either a fibrous investment or an osseous layer, which is nothing but a calcification of the fibrous laminæ. If, however, the internal investment have not undergone a fatty or calcareous degeneration, there may be observed an epidermis, most distinctly characteristic, and having the greatest analogy with the surface of the skin; it is formed by irregular or polygonal scales, of a fortieth or fiftieth of a millimetre in diameter, and furnished with a small nucleus. In some instances the cells, though filled with fat, are recognisable by their pavement arrangement. Under this layer Lebert has recognised a well-organized derma, with an areolar disposition of its fibres, and in some parts furnished with papillæ. Sebaceous glands also exist; he has been able to recognise their lobules and excretory ducts in some instances. Multitudes of small pores and infundibula also occur. Kohlrunsch has even met with sudoriparous glands. Hairs are found either free or implanted: of the former some still retain their bulbs; frequently these hairs appear implanted when they are really only adherent; sometimes their implantation is complete through the entire wall. The included fatty mass has been long recognised as a sebaceous fat. The fall of the hairs allows them to become mixed up with the fat, and thus are formed the balls and masses of hair and fat so often met with in these cysts. Teeth are also met with, in many cases in large numbers: in one instance more than 300 were found. Molars, canines, and incisives, may be met with, either separately or variously combined; there does not appear to be any reason for admitting the statement of Meckel, that the normal arrangement is followed. The roots offer no greater variety than is found to prevail at the different phases of evolution in normal teeth; and the same may be said with regard to the dental cavities, which, in point of structure, were found to present the same histological elements, in the enamel, ivory, and cementum, as the natural teeth. The pathological history of these cysts, the various morbid alterations they undergo, the openings which they effect into the cavities and tubes in whose neighbourhood they are found, are all considered at length in the concluding portions of these elaborate memoirs. Rejecting the doctrines of fœtal inclusion and ovarian pregnancy, M. Lebert concludes that the origin of these tumours is due to a force which he calls *plastic heterotopia, by which simple or compound tissues, and even more complex organs, may be formed in all parts of the body, when in the normal state no such structures can be recognised.*

Lyons has met with a case of dermoid cyst in the testis (right); the quantity of fat and waxy matter and hair was very considerable. On being opened the cyst gave exit to a thick fluid, of the consistence of butter-milk, but of a creamy, yellow

* Gazette Médicale, Nos. 46, 51, 52, 1852; Nos. 9, 12, 15, 1853.

colour. When cold it became of the consistence of marrow, which, in general appearance, it very much resembled. The testis was unaffected, and lay behind the tumour.

TUMOURS.

Gelatinous Cystoid Growth.—Professor Virchow* has given the conclusion of the pathological history of a compound gelatinous cystoid growth, which exhibited most extraordinary powers of reproduction, having been extirpated by Professor Textor not less than seven times, after which it appeared for the last time, and proved fatal. The subject of recurrent tumours, and the important surgical questions connected therewith, are of such high moment, that we do not feel it necessary to apologize for the length of the extracts we purpose making from Professor Virchow's paper.

In this extraordinary case the skill and courage of the surgeon were only equalled by the noble fortitude of the patient. In the first instance, the disease presented itself as a tumour on the shoulder-blade, which was removed by sawing off a considerable portion of this bone below the spine. The growth soon re-appeared, and was again extirpated; subsequent to which it grew again, and a third operation was performed. It is unnecessary to pursue the history of the case through each period of re-appearance of the growth and operation for its removal. The seventh and last operation was performed in February, 1851, every precaution being used to ensure the complete removal of the tumour, and all suspicious structures in its neighbourhood. In the winter of 1851-52 the patient came again to the hospital, but the growth had now reached such dimensions that, for its complete extirpation, it would have been necessary to remove the shoulder-joint and scapula. A puncture was made in one portion of the growth, which gave exit to a large quantity of a reddish-brown and yellowish gelatinous fluid. On the shoulder-blade there were two dense elastic tumours, each of the size of a fist. The region of the left shoulder was remarkably swollen and discoloured, and a dense globular body, of the size of a (Borsdorf) apple, could be felt between the acromion and the clavicle. The left arm was swollen and œdematous to some distance below the elbow, and in some places gave signs of fluctuation. The tumours increased considerably in size; discharge continued from the wound; the skin on the back became gangrenous; insatiable thirst, sweating, and diarrhœa set in, and the patient died in February, 1852.

The original primary tumour consisted of two large sacs, with irregular prolongations attached to each side of the amputated part of the shoulder-blade, and communicating through it. The outer sac, of about two inches in its greatest diameter, was attached to the bone by a somewhat narrow base; it possessed a thick, strong, white fibrous coat, intimately united to the surrounding parts, (muscles, &c.) The inner sac, of about the same size as the outer, had also a narrow base, and a strong fibrous covering, likewise united to the soft parts. The inner surface of the first sac was covered with a thick, soft, transparent cartilaginous layer, in some places smooth and shining, in other parts rough, velvety, and beset with small elevations. There were also numerous tuft-like formations, of the most various sizes; and lastly, large white fibrous masses, incomplete partitions, and cords. Small bony particles were found in portions of the wall. At the base of the tumour, the bone itself was in parts rough and uncovered, without any investing membrane, and presenting gelatino-cartilaginous masses, similar to those on the wall of the sac. On the posterior part of the tumour, the wall was very thick and dense, and on section exhibited a remarkable alveolar structure, containing several cavities, with numerous tufts and projections springing from their walls. Many of these cavities were entirely shut and isolated; some communicated with each other by fine passages, others through large openings in their walls; and from this Virchow concludes *that there can be no doubt that the large sac-like cavity resulted from the confluence of numerous smaller cavities, produced by the breaking-down of their*

* Virchow's Archiv, Band iv. p. 10; Band v. Heft ii. p. 216.

walls. From a minute examination of the bone and periosteum in and about the base of the tumour, he is of opinion *that it originated in the development of numerous small foci as well in the bone as in the periosteum.*

Taking into consideration all the pathological data of this case, Virchow believes that the process of development of this class of growths is identical with that already observed by him in *ovarian colloid.* Numerous small alveoli are first formed, consisting of a dense fibrous capsule, and solid gelatinous contents; these alveoli enlarge by increase of their contents, the centre becomes softened, melts into a *mucous* mass, and a cyst is formed. When many such cysts exist, their walls become thickened, break at last in one or more places, and the cysts thus become confluent. New cysts spring up around, and the old ones constantly enlarge, and thus a compound cystoid tumour is formed, with one single or many large sacs, which, however, may have a single appearance.

The first relapsing tumour was seated entirely in the soft parts, with which it was most intimately united, and showed no trace of bone; its wall was thick, white, and covered with a transparent gelatino-cartilaginous layer. The relapse of February, 1850, was distinguished from the above chiefly by the existence of numerous small cysts in the soft parts, some isolated, some grouped and confluent.

The necropsy showed most extensive gangrenous destruction of the posterior and lateral parts of the left half of the thorax. The scapula was but little affected above the line of the spine; the portion that remained of the infra-spinous space was very uneven, nodulated, and in some parts deeply excavated, and much thinned. At the root of the spine, the bone was destroyed by a cystoid formation, such as those already described, which sprang from it. From the borders of the scapula there extended on all sides numerous cystoid growths—some very large, some entirely isolated, some pressed together and confluent; a large group of them filled the situation of the old cicatrix, and extended backwards to the spinal column; another chain of tumours, five to six inches long, two and a half inches broad, extended through the axilla into the neck; a third group was traced through the axilla to the upper arm. About an inch below the head of the humerus there was found an exostosis, which formed the base of a large cystoid tumour, whose inner wall was directly in connexion with the periosteum. No such formations were to be found in the ribs or vertebræ.

Virchow examined the growth for the first time on the occasion of the third operation. Some ounces of fluid obtained by puncture presented a reddish-brown colour, and it was interspersed with numerous small gelatinous, transparent bodies, so that the entire had the appearance of a red wine soup, mixed with sago. The single gelatinous bodies consisted of a clear hyaline basic substance, in which were deposited cells, in part single, in part aggregated together; many of them pale, grey, granular, and nucleated; others clearer, with finer and larger included fat molecules, besides a nucleus; in short, observes Virchow, nothing essential was wanted in the resemblance to young cartilage. The fluid in which these bodies swam consisted of a clear substance, in which similar cells floated; some of them being shrunken and altered in various ways. On the addition of acetic acid to this fluid, a thread-like, fibrous, striated precipitate was produced, insoluble in excess of the reagent. Nitric acid produced a precipitate, soluble in excess. Alcohol produced a coagulam, soluble in water. Virchow considers this substance to be identical with the gelatinous mass he had before observed in the umbilical cord; and in a tumour in the horse, which he denominates mucin. Scherer confirmed its identity with the substance found by him in the fluid contents of a large cyst in the neck, which he calls *fluid mucin* (Schleimstoff). The tumour itself consisted of a substance which in structure resembled cartilage. Under the microscope it presented an abundant, completely hyaline, intercellular substance, and large thick-walled transparent spaces, with endogenous elements. On its inner surface, the mass had passed into a thick pulp, having all the character of *fluid mucin* (Scherer). Externally, the intercellular substance was fibrous; the spaces had disappeared, and there remained a thick, fibrous, areolar tissue. *The hyaline inter-*

cellular substance gave no chondrium on boiling, but comported itself as a protein compound. Virchow observes that this is the first instance in which a substance has been found in the human body *similar in morphological characters to cartilage, but totally different in chemical composition.*

Thus within each sac there was a gelatinous substance, morphologically identical with cartilage, and a *mucous* fluid mixed with fragments of this substance. Followed in a direction from without inwards, the mass was observed to become gradually somewhat softer; a lightly granular turbid appearance prevailed, which was rendered darker by acetic acid; while the cells became irregular, withered, ragged, and split up. In other places, the intermediate substance showed a fibrous, irregular network, or a simply striated appearance; at last the cells and intercellular substance became broken down into the *mucous* fluid. At the time these observations were made, Virchow had not yet prosecuted his researches into the nature of the histological elements of areolar tissue. He had recognised the accordance between the intercellular substance of cartilage and the areolar tissue substance; but he did not know, what he believes he has since established—namely, that the areolar (connecting) tissue possesses permanent elements having a cellular signification.* He now considers the point of departure of the disease to be in the areolar-tissue corpuscles; and regards the whole cystoid formation as the result of a process of endogenous growth from this starting-point. The connecting-tissue corpuscles are the proper foci of the process, which next determines the development of a quantity of cartilaginous elements. The cartilaginous formation is thus the derivative of the same elements of the connecting tissue, which till recently were known under the name of nucleated-fibres. The genesis of the cystoid thus takes place in the following way, according to Virchow's theory. The corpuscles of connecting tissue, scarcely still recognisable as cells, and in which at first long single nuclei are observable, become enlarged, and show themselves again as manifest cells in the interspaces of the intercellular substance, being in some instances filled with fatty molecules, in others with pigment-granules. While the intercellular substance becomes softer, the cells enlarge, become broader, their nuclei begin to grow, and in some points form long rounded masses, with numerous nuclei. This growth of nuclei is followed by endogenous cell-formations, and thus is commenced a manifest alveolar type of formation by the grouping together of endogenous nuclei and cells in the interspaces of the compressed areolar tissue. The greatest difficulty attends the question of the formation of the hyaline intercellular cartilaginous substance. By most especial care, Virchow states, that he was once enabled to find an element which he had already described and figured,† in his essay on endogenous cell-formations in cancer. These elements are large round or oval bodies, formed externally of a hyaline capsule, analogous to the intercellular substance; they have an internal cavity with endogenous cells. A figure is given of one of these bodies (t. ii. fig. 4), with two included spaces, within each of which, but separated by an interval from the edge of the capsule, is seen a shrunken cell, with a nucleus and some fat granules. This appears also to indicate the development of the later intercellular substance in the interior of pre-existing cells, in the same manner as in the germinal spaces in cancer, cancroid, and cartilage, as already supposed by Virchow. He has likewise observed that the gradual change of the fibrous basic substance of the tissue takes place so slowly, that a partial inclusion of the former in the latter often occurs. He thus thinks that he has been able to trace the history of the development of this peculiar tumour through all its most important phases without interruption, showing the continuity of the growth with pre-existing normal elements, even to the highest stage of cystoid degeneration. He considers that it is most important that we should not refer the stage of exudation and of free cell-formation to an interstitial blastema; and that only endogenous proliferation, growth of constituent parts from a parenchymatous exudation received into tissue elements, and also from contained blastema, should be accepted. He believes that these principles will be found applicable to a large number of analogous new formations—in

* See Annals of Micrology, Part I., in our April number.

† Archiv, Band iii. p. 213, Taf. ii. figs. 2—4.

particular to cancerous, sarcomatous, and tuberculous growths, and that these structures, how heterologous soever they may appear, are to be referred to proliferous growth of normal tissue elements, in particular to that of the areolar-tissue corpuscles. He enters at much length into the claims of this tissue to be ranked with any known forms of disease; and concludes it to be a particular variety of enchondroma. The entire of this valuable memoir deserves special study.

*Cancer Eburné.**—The affection of the breast so-called by Alibert, is but little known to the generality of practitioners. M. Lesauvage has recently observed several instances of it, and has published a memoir with the results of his researches.

This waxy degeneration is in no way allied to cancer; it is extremely indolent, and causes no suffering for a long period. Ledran had already suspected that the peculiar transformation of the areolar tissue of the breast and axilla was continued into that of the organs within the chest. This opinion was confirmed in two of M. Lesauvage's cases, in which he was able to follow the extension of the disease. The areolar tissue between the tumour and the pectoral walls tore readily into dry laminæ, of a dull white colour. The areolar tissue under the pleura, also, was indurated and white; in one case, two to five millimetres thick. The serous membranes themselves had undergone no change. It is much to be regretted that no details of a microscopic investigation of this disease (if any such were made) accompany this paper.

Chloroma.—A remarkable case of this peculiar form of disease has been observed by Dr. A. King.† The tumour—which presented itself in a young girl, previously robust, healthy, and free from hereditary disease—appeared first in the temporal fossæ, and then caused the protrusion of both eyes; small tumours of various dimensions presented themselves on the crown of the head, and on the mastoid processes. The appearances post mortem were as follow:—On withdrawing the scalp, the crown of the head was found nodulated all over with flattened swellings, of a yellowish-green colour; there were some seven or eight patches, of from two to three inches in their longest diameter, and from half an inch to two inches in their shortest. On dividing the temporal aponeurosis, a diffuse swelling of exactly similar appearance was found to girt the temporal fossæ on each side. No trace of temporal muscles could be detected with the naked eye, but their position was occupied from origin to insertion by the peculiar green substance. The contents of the orbits were converted into the same green substance, with the exception of the eye and its muscles and nerves. The bones were more or less affected. A careful search was made for similar tumours over all the bones of the body, but only two were found, placed symmetrically on the external lateral aspects of the inferior maxilla; they had precisely the appearance of those already described, and the same relation existed between the green mass and the substance of the bone. On the inside of the dura mater there were two flattened masses, placed symmetrically on each side of the falx, projecting inwards, and indenting the substance of the brain over the upper and back part of the middle lobes; one of these masses was half an inch in thickness, and felt firm under the knife. The cut surfaces had everywhere the same yellowish-green colour; the outer surface of the dura mater was quite healthy. On a careful inspection of the parts removed it was ascertained that the diseased masses presented the same characters in all situations, except in so far as variations were produced by intermixture with the surrounding fibrous, osseous, or muscular tissues. The prevalent colour was a uniform greenish-yellow, the masses were in general perfectly homogeneous, like size or coagulated albumen, and no bloodvessels or other traces of a complex organized structure could be discovered, either with the naked eye or with a lens. In some situations it was found that the periosteum was converted into the yellow-green substance, the bone

* Gazette Médicale de Paris, No. 6, 1853. † Edinburgh Monthly Journal, April, 1853.

underneath, however, remaining smooth and natural. When the tumours were thickest, spicula of bone were developed, and in all situations the connexion between the spicula of bone and the diseased soft parts was most intimate; and it was only after several months maceration that they could be torn from each other. The microscopic examination of the green substance did not afford very satisfactory evidence of its nature. The histological characters agree pretty much with those given by Vogel. The substance of the tumours, when altogether free from admixture with surrounding structure, presented a mass of irregular granules, some round, others elongated; these were mingled with imperfect fibres or fine filaments; in many situations there was a disposition of the granular mass to a larger fibrous arrangement. The larger fibres seem to communicate with each other, or to subdivide and reunite, but had none of the characters of bloodvessels, appear to be quite solid, and split up into smaller fibrillæ. From these fibres, probably elements of a fibrous tumour in a state of formation, there were to be distinguished in some parts of the tumour those of fully formed areolar tissue. No microscopic appearance was observed capable of explaining the source of the colour. Dr. R. D. Thomson reports that he could detect no trace of bile, "the only source from which green colouring matter could emanate in the human system, so far as we know at present." Lebert and Vogel, as is well known, ascribe the colour of this class of tumours to some new proximate principle different from bile, to which the former assigns the name of *Xanthose*. Dr. King thinks it possible that the cyto-blastema consisted simply of extravasated blood, or of blood and lymph. During the progress of the case, remarkable changes in volume had been observed; thus, two very large exudations at one time covered the mastoid processes, but only one of them could be found after death.

Non-cancerous, so-called, Cancroid Formations.—It is not our purpose to go at large into this most important subject on the present occasion. Some of the chief topics connected herewith have been discussed in recent numbers of this journal,* and we need therefore only refer our readers to these sources. While so much is being now done to separate growths hitherto confounded together under the common vague term of "malignant," to which no precise or definite notion whatever is attached, we regret extremely that any observers of authority should, by the retention and use of names most unworthy of the present *positive* efforts in this department of science, still perpetuate doubt and uncertainty. We allude at present more particularly to the work of Mr. Paget, and in expressing a difference of opinion with that distinguished pathologist, we do so with great diffidence indeed, and with entire respect for his scientific scruples. But we are strongly of opinion that the use of such words as "epithelial cancer" is calculated to retard the progress of accurate inquiry and scientific diagnosis. We believe that provisional classification and division, even though further remark should prove them unfounded, lead in every case to accurate individual study.

Epithelioma.—We will refer our readers to a review of Hannover's treatise on this subject,† merely remarking that further observation only leads us more strongly to the conclusion, that we are quite warranted in accepting this term to designate a special class of growths now rescued from the indefinite regions of "*cancroid.*" Even Mr. Paget's own description of the disease is, we conceive, quite sufficient to condemn the use of the word *cancer* as he employs it, unless, indeed, he would entirely ignore its claims to any *special* significance: a view the justice of which, perhaps, at a more advanced period of pathological anatomy, we may not ourselves be prepared to deny. He says,‡ "The essential anatomical character of the epithelial cancer is, that it is chiefly composed of cells, which bear a general

* Review on Cancer, British and Foreign Medico-Chirurgical Review, Jan. 1853.
† Hannover on Epthelioma, ibid., &c.
‡ Paget's Lectures on Surgical Pathology, vol. ii, p. 413.

resemblance to those of such tesselated or scaly epithelium as lines the interior of the lips and mouth, and that part of these cells are inserted or infiltrated in the interstices of the proper structures of the skin or other affected part." This description agrees almost verbatim with that of Hannover, and with the experience of Lebert and others, and we may add with our own.

Cancroid or Epithelial Cancer.—Under this head a memoir has been published by M. Maisonneuve, but we shall only allude to it, as an abstract has already appeared in our pages. His conclusions are very similar to those of Hannover.

Polypoid Epithelioma.—Of this form of epithelioma, the writer has had an opportunity of examining two specimens taken from the meatus auditorius externus by Mr. Wilde. One was intensely hard and cartilaginous on section, but besides the presence of a small quantity of fibrous matter, it contained nothing but epithelial scales. The second was soft, in some parts broken down, and with epithelial scales contained an abundance of pusiform bodies.

Cancerous Stroma.—Rokitansky* has investigated the development of cancerous stroma: his memoir is accompanied by sections, illustrating his views.

Papillated Cancer (Zottenkrebs).—We shall at present merely allude to the researches of Gerlach and Rokitansky on this subject: the latter† has published some results of his observations on the histological characters of this form of growth, which must be considered at length at another time.

Cancer.—The elaborate work of Lebert‡ has undergone review so recently, that we shall not here enter on the question of diagnosis of cancer. M. Broca's work has also been considered in the same number.

Colloid Cancer.—The same indefatigable author, M. Lebert, has investigated the subject of colloid cancer in a separate memoir.§ As the result of an analysis by Wurtz, the following proportions are given:—Carbon, 48,09; hydrogen, 7,47; nitrogen, 7,00; oxygen, 37,44. In the small proportion of nitrogen it resembles *chitin*, which has as yet been found only in insects, crustacea, and arachnida. The cells measured $0{,}015$—$0{,}02$—$0{,}01^{mm}$; the nuclei, $0{,}006$—$0{,}008^{mm}$; and the nucleoli, $0{,}002$—$0{,}0025^{mm}$. Some of the cases detailed are interesting, as showing the co-existence of other pathological lesions.

In case 2 there was colloid of the pylorus, with tubercles in the lungs. The cells were not very characteristic. In case 3, there was colloid of one mammæ, with scirrhus of the other. In the fourth case colloid existed in the sigmoid flexure, the peritoneal coat, the colon, and mesenteric glands. In this specimen there existed large mother cells, having a diameter of $\frac{1}{20}$ — $\frac{1}{10}^{mm}$, and even in some cases $\frac{1}{5}^{mm}$. There was a second class of cells more numerous, having a diameter of $0{,}04^{mm}$—$0{,}05^{mm}$. There were some characteristic cancer-cells in the mesenteric glands. In case 6, colloid and encephaloid were combined in an orbital tumour. Case 11 was an example of colloid of the uterus and pelvis, with encephaloid of the vagina and bladder. The following represents the order of frequency in which parts are affected with colloid, according to this author's experience. Stomach (right side), intestinal tube, especially the points of transit from the small to the large, and from the latter to the rectum, the rectum itself, the bones, the glands, including the mammæ. This affection, he considers, must be grouped amongst the cancerous diseases. It has usually a long course, and is

* Sitzungsbericht der math. nat. Classe d. k. Akad. Wien, Band viii. † Ibid.

‡ See British and Foreign Medico-Chirurgical Review, Jan. 1853.

§ Virchow's Archives, Band iv. 1852.

long localized. Rokitansky* also has investigated the nature and structure of colloid growths; his views must be studied at length in his memoir.

Cancerous Phlebitis.—Meyer† of Zürich records some observations on this subject, in which he gives a *resumé* of the cases already recorded. Hasse has given a case of cancer of the liver, in which the branches of the vena portæ and the hepatic veins, as far as the vena cava, were filled with cancerous matter. In another case of cancer of the glands of the neck, cancer was found in the subclavian vein, and could be traced thence through the vena cava superior to the right auricle. In a third, the cancerous mass was found in the pulmonary veins, and thence passed to the left ventricle. Rokitansky states, that in cases of cancer of the uterus he has found cancerous matter in the internal spermatic vein, the hypogastric vein, the internal iliac and crural veins. The great question to be solved here evidently is, whether the cancerous matter directly obtained entrance into the veins, or whether the mass within the veins may not have arisen from a cancerous transformation of a coagulum, the result of a phlebitis. This latter view, in the opinion of Meyer, contains nothing extraordinary, as we see that intra-vascular exudation is capable of assuming different forms of organization, being changed to pus, to areolar-tissue plates, and fibrous bands; why therefore, he asks, may it not, under suitable conditions, undergo cancerous organization?

From a critical examination of the data furnished by a remarkable case given by Virchow, he concludes that in like manner it, too, must be considered as an example of cancerous phlebitis. The author himself details a case of cancer of the pylorus, with co-existent deposits in the liver, in which he was able to trace cancerous matter through the vena portæ and its branches. The greater part of the inner surface of the venous membrane was smooth, but in many places it appeared rough, and the mucus adhered to it. Microscopic examination proved the presence of cancer elements in the contents of the veins.

While we must admit that there is no good reason for denying the possibility of cancerous development in the blood *within* vessels, we will only remark that these researches of Meyer *in no way prove* that the cancer found in the vessels was thus produced. It may possibly have entered their walls by some morbid opening.

A very excellent *resumé* of the state of science in regard to the diagnosis of cancer by the study of its histological elements, has been published by Dr. F. Donaldson. He will be found, on the whole, a supporter of the views of the most advanced microscopists of the day, believing, within rational limits, in the specific and diagnostic characters of the nuclei and cells of cancer.

A paper by Dr. Murchison‡ contains the particulars of an important case of cancer of the uterus and adjacent organs, in which there was also a lesion of the brain, much resembling to the naked eye some forms of cerebral softening, but which, on careful microscopic examination, was ascertained to be cancer. As in another case published by Dr. Redfern,§ there was no fibrous structure present; from which it appears, that this element is in no way to be regarded as an essential constituent of true cancer.

Tubercle.—As with the subject of cancer, so also with tubercle. We refer to the analytic reviews of the researches of Virchow, Shroeder van der Kolk, Ancell, &c., which have appeared in late numbers of this journal.‖

Tuberculosis of the Vagina.—Virchow¶ records an instance of this affection, which he states to have been unknown to him before. The right kidney, ureter, and the bladder, presented tubercular patches, some grey and solid, others white

* Sitzungsbericht der math. nat. Classe d. k. Akad. Wien, Band ix.

† Henle und Pfeuffer, Zeitschrift, N.F., Band iii. Heft 2.

‡ Edinburgh Physiological Society, May, 1852, Aug. 1852.

§ American Journal of the Medical Sciences, Jan. 1853.

‖ See No. 21 (Jan. 1853, p. 183), No. 23 (July, 1853, p. 197), and the present number, p. 443.

¶ Virchow's Archiv, Band v. Heft iii. p. 404.

and ulcerated; the urethra also was beset with numerous small, single, grey tubercles. In the vagina were found similar appearances; in many places the tubercles were grouped in masses on a reddened base; the single ones resembled fine grey pearls. The microscopic appearances were similar to those of tubercle, presenting the same soft cellular nucleated masses, which this author considers to indicate young tubercle. The uterus, rectum, and left kidney, contained only some cysts.

Scabies Crustosa (*Spedalskhed*) *Sc. Norwegica Boeckii.*—The labours of Danielssen and Boeck have made this form of tubercular lepra sufficiently well known. In a case remarkable for the thickness of the yellowish brain-crusts, which were as hard as horn, they found by the aid of the microscope millions of dead acari in the tubercles, on the surface as well as in the softened ulcerated substance. Three authors were not clear whether or not this animalcule was to be ranked with the sarcoptes scabiei. Fuchs of Göttingen* has met with a case in a patient 42 years of age, who stated that he had suffered from the disease since his youth. Under the microscope, the mass showed quantities of epithelial scales, overlying each other; between these, as in Boeck's case, there were innumerable itch mites, of all sizes and sexes, with their young, ova, and excrement. Many of these acari were alive, and moved with activity; they resembled completely the usual sarcoptes. This case, remarks the author, establishes that the ***scabies crustosa Boeckii*** is not peculiar to Norway; and he is further of opinion, that the animalcule is the same variety of acarus in the sarcoptes scabiei.

Complete Stoppage of the Œsophagus by Aphthous Masses.—Virchow† gives the particulars of the case of a child seven weeks old, in which the œsophagus was completely blocked up by a solid cylinder of aphthous product. On transverse section, there could not be found any trace of a central canal; the mass reached as far as the cardiac orifice. Under the microscope it showed the well-known numerous threads and spaces; with remains of ingesta, milk, epithelial formations, &c. In the lungs, also, some masses were found.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

The Determination of Urea. By Professor Liebig.

The new method proposed by Liebig has the advantage of simultaneously determining the amount of chlorine and of urea. It is as follows—100 grammes‡ of mercury, carefully purified from bismuth and lead, are dissolved in pure nitric acid; the salt is evaporated to a syrup-like consistence, and then enough water is added to reach accurately to 1400 cubic centimetres. Every 100 cubic centimetres contains 7·140 grammes of mercury. If this solution be added to a solution of pure urea, a ***snow-white*** precipitate falls, which is a compound of urea and oxide of mercury ($U+4HgO$.) When the urea is all precipitated, and when, consequently, nitrate of mercury is in the solution, a *yellow* precipitate, hydrated oxide, is thrown down by the addition of a little carbonate of soda to a drop of the urea. In order to determine the exact moment when the yellow precipitate occurs, the solution of the nitrate of mercury is of course added drop by drop from a burette, and from time to time a drop of the urine is taken and tested with the soda. One cubic centimetre of the solution corresponds to 10 milligrammes of urea, and from the quantity used, the amount of urea is calculated.

* Henle und Pfeuffer, Zeitschrift, N.F., Band iii. Heft ii.

† Verhandlungen der Phys. Med. Gesellschaft z. Wurzburgh, Band iii. Heft iii, 1852.

‡ We have retained the French weights, as their conversion into the inconvenient English weights is difficult, and as these delicate manipulations will not permit fractions to be disregarded.

Although nitrate of mercury will thus precipitate urea, the bichloride will not do so, and on this fact is founded the determination of the chlorine. If the solution of urea be not pure, but mixed with chloride of sodium, no precipitate at first occurs with the nitrate of mercury, because sublimate is formed; if the nitrate continue to be added, then at last, the chloride being exhausted, the mercury combines with urea, as in the pure solution. Therefore the quantity of solution used before the white precipitate appears, shows the amount of chlorine which must have combined with the mercury first of all. A cubic centimetre of the mercurial solution corresponds to 10 milligrammes of chloride of sodium.

In testing urine the phosphoric and sulphuric acids are first precipitated with barytic water; and after filtration, the urine is weakly acidified with nitric acid.

Although both the chlorine and the urea *may* be determined with the *same* quantity of urine, it is advisable to use two portions, one for the chlorine, and one for the urea. The quantity of nitrate used for the chlorine alone in the first specimen, can be deducted from that used for the urea and chlorine together in the second, and the remainder gives, of course, the quantity which has been used for the urea.—*Annalen d. Chem. und Pharm.*, Band lxxxv. 1853.

On the Effect of Coffee. By Dr. Zobel.

In a long and interesting article Dr. Zobel discusses the effects of coffee as well as, incidentally, other dietetics. He denies that the use of coffee (and of tea) is to be estimated by the quantity of nitrogen it contains, and shows by calculation how comparatively small a quantity of nitrogen could by this means enter the system. He also denies the accuracy of Rochleder's opinions, that coffee gives rise to the formation of creatine, or if it does so, he questions whether this may not result from its action on the nervous system, and not by immediate transformation of its own substance. With respect to the influence of coffee on the health, he refers to the opinions of a few enthusiasts, such as Jury and Thierry, who have supposed it to be most prejudical to life. He then inquires what are the chemical changes which occur in the caffein when introduced into the blood. The first change he states to be as follows:

$$\text{1 equiv. caffein } C_{16}N_4H_{10}O \text{ makes } \begin{cases} \text{1 equiv. hydrocyanic acid } C_2NH. \\ \text{1 equiv. of a body of the composition } C_{14}N_3H_9O. \end{cases}$$

The quantity of hydrocyanic acid is very large, and quite enough to appear at first sight a justification of those who have asserted the danger of coffee. But if the examination be continued, this apprehension is dissipated; another equivalent of caffein acting on the substance formed by the separation of the prussic acid, gives rise to four equivalents of ammonia, the great antidote of the acid, and a third equivalent of caffein and water gives rise to one equivalent of quinine, two of oil of turpentine, and three of urea, while twenty-seven of oxygen remain. The formula is thus given:—

3 equiv. caffein=3($C_{16}N_4H_{10}O_4$) are	$C_{48}N_{12}H_{30}O_{12}$
23 equiv. water . . . . are	$H_{23}O_{23}$
	$C_{48}N_{12}H_{53}O_{35}$

Thence are formed—

1 equiv. hydrocyanic acid . . .	$C_2N_1H_1$
4 equiv. ammonia	N_4H_{12}
1 equiv. quinine	$C_{20}N_1H_{12}O_2$
2 equiv. oil of turpentine . .	$C_{20}H_{16}$
3 equiv. urea	$C_6N_6H_{12}O_6$
27 equiv. oxygen	O_{27}
	$C_{48}N_{12}H_{53}O_{35}$

By the influence of the ammonia and turpentine, the sedative effects of the hydrocyanic acid are neutralized, and are replaced by a slight stimulant effect on the peripheric nervous system. The ammonia would produce its stimulant effect, and soon pass off, were it not fixed in some measure by the strongest neurine tonic known, the quinine. To these effects of caffein, it is necessary to add those of the tannic acid and volatile oils which the roasted coffee nuts contain, when coffee itself is drank. Dr. Zobel thus confirms the statement of Lehmann, that the amount of urea is increased by the use of caffein. A few pages at the end of the paper are devoted to a consideration of the mode of origin of caffein in the plant, which we omit.—*Prag. Viertel. Jahrsch.*, 1853. Band ii. pp. 105—136.

The Effect of Tea. By Dr. Böcker.

Dr. Böcker details at great length several series of experiments made on himself. In the first he determined the exact amount of food and of water (each of known composition) taken into the system; the exact amount of exercise; the exact amount of urine and its constituents, and of fæces, and of expired carbonic acid. The amount of perspiration was calculated. These experiments were continued for seven days.

In the second series the experiment was conducted exactly in the same manner, only cold tea was substituted for water. These experiments were also continued for seven days. The following table shows the mean result of the experiments on the weight of the body, the fæces, urine, and perspiration:

In 24 hours.	1st Series. 1260 grammes of water being taken.*			2nd Series. 1260 grammes of tea being taken.	
A. Exercise	84·14	minutes	...	87	minutes.
B. Excretions,—					
1. Loss of weight	539	grammes	...	203	grammes.
2. Quantity of fæces	178·30	„	...	96	„
3. Calculated perspiration	1349·90	„	...	1335·7	„
4. Urine	2621·143	„	...	2550·000	„
Water	2543·519	„	...	2474·016	„
Solids	77·624	„	...	75·984	„
Urea	35·194	„	...	34·221	„
Uric acid	0·356	„	...	0·231	„
Ammonia	0·421	„	...	0·660	„
Muriate of ammonia	1·250	„	...	1·959	„
Oxalic acid	0·092	„	...	0·088	„
Potash	4·466	„	...	5·274	„
Sulphuric acid	2·841	„	...	2·815	„
Sulphate of potash	6·187	„	...	6·132	„
Chloride of potassium	1·774	„	...	3·113	„
Chlorine	11·475	„	...	10·687	„
Chloride of sodium	16·156	„	...	13·043	„
Phosphoric acid	2·923	„	...	2·926	„
Phosphate of soda	5·483	„	...	5·488	„
„ lime	0·724	„	...	0·772	„
„ magnesia	0·756	„	...	0·717	„
Salts by incineration	28·633	„	...	27·229	„
Volatile salts and extractives	13·309	„	...	14·304	„
C. Food—total weight	3610·50	„	...	3617·00	„
Water	2938·84	„	...	2957·87	„
Solids	671·66	„	...	659·13	„

* We have retained the French weights.

When the tea was taken, it appears that not only was more nitrogen introduced by it, but that daily three grammes of dried meat was also taken in excess of that consumed during the series of experiments with water; and yet, in spite of this greater quantity, the average amount of fæces, and the amount of urine, was diminished by the use of the tea. It would then seem that (contrary to Lehmann's statement) the quantity of urea (judging only from these experiments) was not augmented, but, on the contrary, both the *urea and uric acid were diminished.* The author confirms these results by another series of experiments, in which the ordinary amount of food was taken: the quantity of urea was much greater than with the use of tea.

We need not quote the average table of the carbonic acid, but state merely the result—viz., "that tea, in the quantity mentioned, exerts no marked influence on the quantity of carbonic acid excreted, or on the frequency of the respiration, or on the beats of the pulse."

The author demands whether the diminution of excretions observed from the use of tea, might not be accidental—i. e., attributable, not to the tea, but to the unexplained variations in the metamorphosis of tissue. He answers this in the negative, because he has found by previous experiments that in his own case these variations occur at stated times of the year, and that during the period of his experiments he has no doubt the amount of excretion would, under the usual conditions, have remained stationary.—*Archiv des Ver., für gem. Arbeit.,* Band i. Heft 2, p. 213.

On the Constituents of the Flesh-fluid of Frogs. By Dr. Grobé.

Professor Moleschott has affirmed that both urea and oxalic acid exist in the fluid which can be expressed from the muscles of frogs. To test the accuracy of this statement, Dr. Grobé has undertaken an elaborate investigation in the laboratory at Giessen, which has led to the conclusions—

1. That neither urea nor oxalic acid exist in this fluid.
2. That the crystals considered to be these substances were creatin, creatinin, and sulphate of potash.
3. That the same organic and inorganic substances are found in the flesh of frogs as of other animals.—*Schmidt Jahrb.,* Band lxxviii. No. 6.

On the Action of Water. By Dr. Falck.

The author has endeavoured to ascertain to what extent the imbibition of pure water, at a temperature of 60° Fahr., increased the excretion of the water and the solids of the urine. The following table (in which we have retained the continental weights) shows the result at a glance:

—	At the commencement of each period of 12 hours he took—			In each 12 hours he excreted—					
	Bread in grammes.	Pudding in grammes.	Water in grammes.	Urine in grammes.	Solids of urine in grammes.	Water of urine in grammes.	Specific gravity.	Solids per cent.	Water per cent.
1	500	164	...	332	19·568	312·432	1·030	5·894	94·106
2	500	161	1000	489	27·184	461·817	1·027	5·559	94·441
3	500	165	2000	1310	27·243	1283·757	1·008	2·078	97·922
4	500	165	4000	3911	33·478	3877·522	1·003	0·856	99·144

If the experiments 2, 3, 4, are compared, it will be seen that as the quantity of water drank was augmented in geometric progression with the factor 2, so the quantity of water passed in the urine augmented in geometric progression with the factor 3: or in other words, if the quantity of water drank was doubled, the quantity of water passed was trebled. The geometric progression is visible also in the specific gravities, and (inversely) in the per centage of the solids; so that, in fact, the calculation based upon the geometric progression is almost identical with the results actually determined by experiment.

These experiments show that the quantity of solids carried out of the body does not increase with the quantity of fluid drank. One pint of water washes as much organic detritus out of the body as two or four pints; temperature and other circumstances being of course constant. [This result appears completely opposed to the experiments of Becquerel, on which much stress has been laid by the advocates of the water cure.—Ed.]—*Vierordt's Archiv*, 1853, Heft 1, p. 150.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the condition of the Grey Substance of the Brain after excessive Mental Exertion. By Dr. Albers.

In allusion to a statement made in the 'Psychological Journal' by Dr. F. Winslow, in an article entitled "The Overworked Mind," that the grey substance undergoes softening as a consequence of excessive mental exertion—Albers states that he has dissected the brains of several persons, who have for many years undergone great mental labour, and that in all of these he has found the cerebral substance unusually firm, the grey substance as well as the convolutions being remarkably developed. In several of these instances a settled melancholia had taken possession of the mind during the later period of life. He believes, therefore, that to produce a softened condition some additional influence beyond mere over-exertion is required. Softening of the cortical substance is a frequent consequence of apoplexy of the convolutions, which gives rise to numerous small depositions of blood, especially at the convex portions of the brain, being accompanied also by an atheromatous degeneration of the small arteries. In this latter condition the quantity of fat is not only accumulated in the arteries, but also in the cerebral substance itself. This degeneration is oftenest seen in gouty subjects, in whom it certainly is not attributable to excess of mental exertion. Several such cases, too, have been met with in rustic labourers.—*Froriep's Tagsberichte*, No. 696.

On the Secretion of Sugar by the Liver, and the Modification of this by Disease. By M. Vernois.

M. Vernois, with the object of verifying and extending M. Bernard's researches, has submitted 173 livers to examination; and the following are the general results he has arrived at:—1. He believes, with Bernard, that sugar constantly exists in the human liver, independently of alimentation. He found it in a case in which no food had been taken for fifteen days. 2. *Age* appears to exert a certain amount of influence. The minimum amount was found from birth to two years of age: the maximum from twenty to fifty years of age. Sugar is found in the liver of the fœtus (as early as the fourth month), but in less quantities than in that of the adult. There may be sugar found in the liver of the fœtus and not in that of the mother, and *vice versâ*. 3. *Sex* exerts no appreciable effect. 4. The *influence of disease* upon the secretions is indubitable. (1.) The quantity of sugar is in an inverse ratio to the *duration* of the disease. (2.) The *nature* of the disease which has caused death influences the secretion. In some diseases, and especially in epidemic cholera, sugar is more easily found in the liver than in the normal state:

while in the majority of diseases sugar is seldomer detected, as in sclerema, cerebral affections, tubercular disease, &c. (3.) The ***anatomical conditions of the liver*** exert an influence, the quantity being lessened in proportion as the secretory structure is destroyed. The putrefactive process, however, does not prevent its detection. Gübler says none is found in the livers of syphilitic infants. 5. The opaline aspect of a decoction of the liver is usually proportionate to the amount of sugar, as determined by the potash and Trommer's tests.—***Archiv Gén.***, 1853, i. p. 657.

On Intermittent Pneumonia. By Dr. Constant.

Dr. Constant, practising in one of the marshy districts of the department of the Lot, draws attention to the signs which distinguish what he terms intermittent pneumonia, as when they are overlooked the disease proves rapidly fatal.

1. The initial shivering is usually more intense and prolonged than in ordinary pneumonia. 2. The pleuritic pain is felt early and always in front of the chest, although the pulmonary congestion is almost always localized posteriorly. It is much more amenable to blisters than to leeches. 3. Violent cephalalgia is one of the earliest symptoms, being either frontal or sincipital, and it is often accompanied by severe lumbar pain, which observes the same stages of increase and decrease as itself. 4. The shivering is followed by intense heat, which after several hours gives place to abundant sweating. 5. The pulse, during the paroxysm, in place of being full, strong, and vibrating, as in ordinary pneumonia, is rapid, soft, undulant, and compressible. 6. There is never any purulent expectoration, these pneumonias never proceeding beyond the second stage—i. e., red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation. 7. Auscultation and percussion are of the highest value, often revealing the disease when unsuspected. A distinctive feature is the rapid passage from the first to the second stage of the disease, so that 8 or 12 hours after auscultation had revealed only a slight circumscribed *râle*, a whole side will be found hepatized. Under the influence of large doses of quinine this rapidly disappears, giving way to returning subcrepitant *râle* during the remission of the fever, but returning again during the paroxysm if this have not been cut short. 8. The crepitant *râle* of the first stage is almost always moist, the parchment-crackling *râle* only having been heard for a short period two or three times in more than 60 cases. It invades large surfaces rapidly, being heard posteriorly, sometimes laterally, but never in front. 9. This form of pneumonia especially affects the posterior part of the lower lobes. 10. It especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. 11. It attacks all ages indiscriminately, except early infancy. 12. The blood which flows from a vein is often below the normal temperature, very black, and deficient in plasticity. After rest, its surface acquires a bluish colour, especially if the patient is taking quinine. The clot is slow in formation, and soft. The buffy coat is absent, or very thin, and inclines to a bluish colour. This condition of the blood, conjoined with the soft pulse and rapid hepatization, constitutes the chief distinctive sign of the affection.

In this district, during winter, purely inflammatory pneumonia is met with; but in proportion to the high temperature and the production of malarial emanations, this inflammatory element is replaced by the paludal one. There are indeed three forms met with:—1. Simple pneumonia; 2. Spring inflammatory pneumonia, complicated with the intermittent paroxysm; 3. Summer and autumn intermittent pneumonia. The first requires bleeding and antimony; the second, antiphlogistic treatment with quinine, given either simultaneously or subsequently; and the third, quinine in combination with external revulsives. These forms may still undergo further admixture, accordingly as the inflammatory or paludal element prevails, requiring appropriate modification in the treatment.—***Bull. de Thérap.***, xliii. pp. 481 & 491.

SURGERY.

On the Reduction of Luxations of the Extremities, when complicated with Fracture. By M. Richet.

M. Richet relates, in great detail, the case of a man, æt. 68, in whom a dislocation of the upper end of the humerus was complicated with fracture of the anatomical neck of the bone. Four days after the occurrence of the accident, he was placed under the influence of chloroform, and the reduction of the dislocation was easily effected by pressing backwards the head of the bone, without any traction being resorted to. The fracture was afterwards adjusted and consolidated; and when the patient was seen a year after, he had recovered the complete use of his limb.

M. Richet reviews the opinions of the classic writers, who agree in pronouncing the impossibility of reducing a dislocation of the humerus or femur, until after the fracture complicating it has become united. He shows the great power chloroform confers upon us in these cases, by the complete relaxation of the muscular resistance it produces, and the care with which the head of the bone may, by due manipulation, be forced back into its socket. He thinks the passive obstruction offered by the fibrous tissues of the parts has been exaggerated and ill-understood. In numerous autopsies he has made after recent dislocations, produced accidentally or experimentally, he has always found the aperture in the capsule broad and irregular, and in no condition to offer an obstacle to reduction. He does not deny that such obstacles may occasionally be offered by the fibrous structures, independently of the aperture of the capsule; but he maintains, from clinical and experimental observation, that such obstacles are much more easily overcome by pressing the head backwards than by the usual practice of traction of the limb, which, indeed, only aggravates them. By the aid of chloroform, he believes a dislocation of the humerus into the axilla may thus always be reduced by pressing the head directly backwards. In several experiments that he has made, in which the head of the femur has been dislocated, and the bone then sawn through just below it, so as to simulate dislocation complicated by fracture, the reduction has also been easily effected by direct pressure.—*Bulletin de Thérap.*, tom. xlv., pp. 18, 104.

On Traumatic Effusions of Serosity. By M. Morel Lavallée.

The author here draws attention to contusions, in which the subcutaneous effusion has consisted, not of blood, but serosity; and as the subject is quite unnoticed by systematic writers (except incidentally by Velpeau, in his 'Closed Cavities'), he gives a brief history of it, derived from the 12 cases he has collected.

The *causes*, in the great majority of cases, have been the oblique pressure of a wheel in motion, brought in contact with the part, and by its rapid motion causing extensive detachment of the skin from the adjacent aponeuroses, without inducing any breach in its surface. The pressure of a wheel of a carriage should always raise the presumption of a production of this lesion. As yet the occurrence has only been met with in men; whether from their being more exposed to external violence, or from the greater quantity of adipose tissue under the skin in women rendering its detachment less easy. The *nature* of the lesion seems to consist in the rupture of minute or capillary vessels, the bruised extremities of which allow only the thinnest portion of the blood to escape. It resembles the serous discharge that issues from a wound that no longer bleeds, or which is imbibed by the first dressing after amputations. The *effused fluid* is usually abundant, amounting to more than a *litre*, being generally limpid, and of a light citron colour. It may, however, assume a reddish or even a blackish colour; and this has led to its being mistaken for blood prevented from coagulating by the contact of living tissues. It undergoes no coagulation or any perceptible change under the influence of time.

Its permanent fluidity quite distinguishes it from blood, as it never coagulates except when mixed with blood, simultaneously effused. The examination of its composition by Lebert and Robin leads to its being considered as analogous to the serosity of the blood, containing some fatty globules, and some almost colourless blood-corpuscles. On standing, about $\frac{1}{20}$th of its quantity is deposited, consisting of colouring-matter in the form of minute granules.

Symptoms.—There is nothing constant in regard to pain. The skin usually preserves its normal colour, though occasionally it is ecchymosed. The collection may assume the form of a tumour, but usually it remains flattened and undefined. The fluid is spread out in a large cavity, which only becomes gradually filled, and generally, never indeed, becoming quite full. The commencement of the collection is thus often very obscure, and its amount is much greater than external appearances would lead one to suppose. Owing to the partial fulness of the sac, a tremulous movement, visible to the eye, is produced on the motion of the parts, and sometimes a strong current of air suffices to induce its undulation. To produce true fluctuation, the skin has to be stretched, or the fluid forced into one part of the sac. In most cases, a more or less distinct ridge surrounds the base of the tumour.

Diagnosis.—It has often been mistaken for an effusion of blood, especially when the fluid has been of a dark or red colour; and when it has been accompanied with pain and fever, it has given rise to the belief of suppuration.

Treatment.—All topical applications have failed; and opening the tumour is the only efficacious procedure. Still, experience thus far shows, that success is proportionate to the delay with which an aperture is made. In general, M. Morel prefers puncturing the tumour by means of a trocar to subcutaneous incision; though in some cases the latter may be preferable. Compression is to be afterwards employed to induce adhesion of the walls of the sac, and flying blisters will be found of utility. If the fluid is reproduced, the subcutaneous incision may be resorted to, a little charpie being also employed to keep the aperture open. When simple puncture and well-regulated compression do not succeed, and the lesion assumes a chronic form, we need not hesitate to employ iodine injections. A remarkable example of the success of this practice is given.—*Archives Générales*, 1853, tom. i. p. 691.

Case of Hydatids of the Breast. By M. MALGAIGNE.

AN example of this exceedingly rare affection came under M. Malgaigne's care, at the St. Louis. It occurred in a woman æt. 42, who had had two children (the youngest being 10 years of age), but had never suckled. She was in good health, and not wasted: and had never received a blow upon, or suffered from pain in, her breasts.

About six years since, she first perceived a tumour of the size of a pea at the inner and lower part of the left breast, and it slowly increased in size, until during the last year, when it has remained stationary. On admission, an oblong, roundish tumour, the size of a pigeon's egg, presented itself, which was moveable over the subjacent tissues, but adherent in part to the skin. The skin was of its normal colour and thickness. The tumour offered a remarkable resistance, and was not at all tender. No fluctuation could be felt, and the axillary glands were unaffected.

When M. Malgaigne made an incision across the tumour, a jet of transparent citron-coloured fluid issued, and the tumour at once subsided. On pressing at the sides of the incision, a largish hydatid cavity issued forth. It had been opened by the incision, but still contained some of the fluid. Its walls were whitish and semi-transparent; but no vesicle was found in its interior. The serous sac, which had contained the hydatid, adhering to the surrounding tissues, required entire extirpation. The wound healed readily.—*Rev. Med.-Chir.*, tom. xiv. p. 55.

Contribution to Ophthalmoscopia. By Dr. T. Czermak.

Dr. Czermak calls again the attention of the profession to the advantages gained in the examination of the eye by an ophthalmoscope described by the author in 1851 ('Prager Vierteljahrschrift,' vol. iv.), under the name of "the Orthoscope." The leading idea in the construction of this apparatus was "to prevent, as much as possible, the refraction and deviation of the rays of light by the anterior surface of the cornea." This intention he realized by placing the eye under water, the refractive power of which is nearly equal to that of the aqueous humour and cornea. The water is kept before the eye by means of a little case with a transparent anterior wall. Through this the eye becomes extremely far-sighted (*weitsichtig*); the foci of the objects are more or less thrown behind the retina, according to their distance; large circles of diffraction (*zerstreuungs kreise*) are formed on the retina; the anterior focus becomes removed from the eye; it is only in their passage through the crystalline lens that the rays reflected from within undergo a considerable deviation from their original direction. In this way the conditions by which the inspection of the retina is ordinarily prevented, are so far changed, that by means of a good light (concentrated day as well as candle light) the whole interior of the eye may be easily looked at. Czermak remarks, that already in the last century this fact was well known ('Le Cat., traité des sens,' Amsterdam, 1744, p. 174), but was scarcely introduced into practice. As an important advantage of the Orthoscope, Czermak considers also the circumstance, that the reflected little image of the candle-flame on the cornea, which frequently is so troublesome in the examination with the instruments hitherto used, is observed only in a slight degree on the anterior wall of the apparatus. Professor Arlt, who has derived much benefit from the Orthoscope, in his "Klinik," makes use of a small case of gutta percha (with a glass wall in front), which easily fits to the face, by which the running out of the water is perfectly prevented.—*Prager Vierteljahrschrift, f. pract., Heilkunde*, 1853, ii. 137 ss.

MIDWIFERY, &c.

Anteflexion of the Uterus as a normal condition prior to Pregnancy. By M. Boulard.

A few years since, a very animated discussion took place at the Académie de Médecine, upon the subject of engorgement of the uterus. M. Velpeau was one of those who maintained that anteflexions of the uterus are often mistaken for engorgements, while other speakers declared anteflexion itself to be a very rare occurrence. M. Boulard, engaged in searching for examples of the deviation for Velpeau, was at once struck by the frequency of its occurrence in young subjects, while in older ones he scarcely ever met with a case, without, from the examination of the other organs, ascertaining that the subject of it had never been pregnant. After in this way collecting a great number of uteri from the dead-house, M. Boulard resolved to study the point during the development of the organ, and found that the uterus is almost always anteflexed in the fœtus. He has continued to pursue the investigation from that period; and the present paper is founded upon the examination of 27 adult female subjects who had never borne children, 19 young girls from two to thirteen, and 57 full-timed fœtuses. In 98 of these anteflexion has been found, and to such a point is the body bent upon the neck, that it is not possible to prevent its regaining the same position immediately after the attempt at replacing it has been made. Since these observations have been made, the author has sought every opportunity of verifying them in the living subject; but he has only had ten opportunities of examining the position of the uterus in the virgin, and in all of these the anteflexion existed.

M. Boulard accounts for this disposition having been overlooked by anatomists, by the fact that the bodies of women who have borne one or more children are those which are usually brought for dissection; and the organ is examined only after it has undergone this physiological change, or under the influence of the relaxed state of the tissues after death. When anteflexion has been noted, it has been considered as an abnormal and diseased condition, and regarded as a cause of suffering which was due really to some morbid condition which had become accidentally associated with it.—*Rev. Méd. Chir.*, xiii. 341.

On the Effects of Menstruation on the Milk of Nurses. By MM. Becquerel & Vernois.

Upon this effect, which the occurrence of menstruation exerts in women who are suckling, there is discrepancy of opinion among authors, the majority, however, with the public at large, believing in its deteriorating influence. So great is the difficulty in obtaining true statements upon this point, that among the great number of hired nurses in Paris, the authors have only been able to examine the condition of the milk in three women while actually menstruating. In these the density of the fluid was found slightly diminished, as was the proportion of sugar, and the proportion of water was sensibly so. The solid parts were notably increased, especially the caseum. The authors cannot believe that such changes in composition can induce any mischief beyond some temporary derangement in the digestive organs, and even this might be prevented by causing the child to suck less, and letting it drink a little sugared water, to replace the sugar and water lost during menstruation.

In the discussion that followed reading the paper, M. Roger observed, that while attached to the Office for Nurses, he had paid considerable attention to this point, and that he had arrived at the following conclusions:—If the menses reappear easily, without pain or derangement of the nurse's health, while her milk is under 12 or 15 months old, and the quantity of blood lost is normal and moderate, the quantity of milk does not become diminished, or its qualities altered, and the child does not suffer from its use. If, however, the menses are too abundant or too frequent, the milk may diminish in quantity or disappear. The same effect is also produced, though more slowly, in some days or weeks, when the menses are prolonged for a week, so that the loss is considerable. The milk will much more certainly dry up if the menses reappear at an advanced period of lactation—this being then the signal of the imperfection and approaching termination of the secretion.

When the milk becomes thus diminished, it rarely exhibits the physical characters of poor milk; but by its density, whiteness, and the excess in number and size of its globules, it more approaches in character and richness cow's milk. When the menstrual epochs reappear with difficulty, and are attended with pain, indigestion, diarrhœa, &c., or are preceded or followed by leucorrhœa, the child may suffer symptoms due to indigestion induced by the altered characters of the milk—the alteration of the milk chiefly consisting in increase in the number and size of the globules. These influences are, however, only temporary, and the milk soon recovers its normal character. The ailments which the child hence suffers are only temporary, and have been greatly exaggerated.—*L'Union Médicale*, No. 70.

Reposition of the Prolapsed Funis. By Dr. Fincke.

The author relates five cases in which the plan he adopts has been attended with success. Passing the entire hand into the vagina, and catching the middle of the prolapsed coil upon the points of the fingers, he carries it as high up into the cavity of the uterus as possible, and leaves the funis supported between the posterior wall of the uterus and the parts of the child. If, however, he withdrew his hand as he passed it in, he always found the funis follow it; but by doing so in another manner, he has found it continues where he placed it. After detaching the funis, the hand, placed at the upper and posterior part of the uterus, is held horizontally, and withdrawn while describing about the fourth part of a circle forwards, and then bringing it straight downwards. If the funis follows it, it will have twice to form a right angle; but in his cases it has hitherto not done so, and the children have been born alive.—*Monatsbericht für Geburtskünde,* Band i. 437.

Stricture of the Fundus Uteri as a cause of dangerous Hæmorrhage.
By Dr. Diez.

Under this new title the author alludes to a description of hæmorrhage that most experienced practitioners are familiar with as arising from partial contraction of the uterus. After the separation of the placenta, and while globular contraction of the uterus can be felt above the pubes, alarming hæmorrhage comes on. As there is good contraction of the uterus, and pains, often of considerable severity, occur, this is at first inexplicable. On passing the hand through the widely-opened os, we enter a tolerably spacious cavity filled with coagula, whose walls are firm and unyielding, and remain quite passive to the pressure of the hand. Pushing on farther, we perceive a more or less narrow, often cartilage-like stricture, separating the fundus from the body of the uterus. It is exactly the same as is observed in strong incarceration of the placenta, only, as a general rule, the contracted part is inclined to one side (usually the right), and takes on a more oval form.

The means calculated to check hæmorrhage under other circumstances are here useless or mischievous. Increasing the pains by means of ergot, cold, external pressure, friction, &c., does injury by increasing the abnormal contraction, and by favouring the flow of blood from the non-contracted part. Of a more useful character are such means as tend to arrest hæmorrhage without exciting pain, as compression of the aorta (taking care, however, not to compress the fundus uteri), acids, alum, cinnamon, ipecacuanha, warm astringent injections, &c. The most efficacious means, however, is the attacking the pathological condition upon which the hæmorrhage depends by the employment of belladonna, in conjunction with, or in quick succession with, ipecacuanha, together with compression of the aorta. When, on account of pressing danger, the operation of these means cannot be waited for, the painful and often difficult procedure of dilating the strictured part must be resorted to, allowing the hand to remain within the fundus until the organ contracts in a normal manner.—*Monatsbericht für Geburtskünde,* Band ii. p. 1.

THERAPEUTICAL RECORD.

Aneurism.—In a case of false aneurism at the bend of the arm following bleeding, M. Velpeau (L'Union Méd., Août 25) employed the method of Pravaz, and injected 8 drops of perchloride of iron into the sac; the blood did not appear to be coagulated. Twenty days afterwards, 8 drops more were injected, without producing coagulation, and six days subsequently the brachial artery was tied.

In a case of traumatic false aneurism of the subclavian artery, M. Bonnet (Archiv. Gén., Août), being unable to ligature, and unwilling to use galvano-puncture, applied over the tumour the chloride of zinc, and produced an eschar; after removing the superficial layer of which he re-applied the caustic, and so proceeded for five weeks. He presumed that the chloride of zinc would penetrate into the sac, and cause coagulation of the blood. About the fortieth day the pulsation and the murmurs had completely disappeared.

Asthma, Spasmodic.—M. Trousseau (Gaz. des Hôp., No. 93) observes, that although the *modus operandi* is difficult of explanation, he has relieved many patients during the paroxysms of asthma, by the old plan of burning in the room paper impregnated with nitre.

Cardialgia.—See Creosote.

Chloroform, Poisoning by.—In a case of a strong man, whose testicle was extirpated, Ricord (L'Union Méd., 1853, No. 34) administered chloroform; narcosis was induced in half a minute; the operation was performed. Some little time after the chloroform had been removed from the mouth, the pulse and respiration suddenly ceased, and the face became pale. Ricord threw himself on the patient, to whose mouth he applied his own mouth, blew strongly into his lungs, then compressed the thorax to expel the air, and then again blew into the lungs. After two or three of these artificial respirations, the pulse returned. In this case, Ricord believes that there was syncope, coming on as it will after hæmorrhages, or mental excitement, and that this was cured by the *immediate* use of the artificial respiration.

Cholera, Asiatic.—Mr. Macpherson (Assoc. Med. Jour., August) has employed cold affusion in India, with good results.

Collodion.—Two varieties of collodion are recommended by Clarus (Schmidt's Jahrb. No. 8, p. 27), the first for closing wounds, and the second especially for cutaneous eruptions, in which collodion is useful. (a) *Collodium terebinthinatum;* a scruple of turpentine dissolved in ℥j of collodion, forms a very tough and adhesive fluid. (b) *Collodium ricinatum;* a scruple of castor oil and ℥j of collodion forms a thin and soft solution well adapted for the skin.

Creosote.—M. Arendt (Froriep's Tagesberichte, Nos. 691 & 627) states, that the great advantage he had derived from the use of creosote in asthma and bronchitis, an account of which he published in 1848, induced him to employ it in various other affections, especially of mucous membranes. In *chronic varicose ophthalmia* he found from 1 to 3 drops of creosote to 1 ounce of water a valuable collyrium, dropped into the eye several times daily. *Cardialgia,* and especially the idiopathic form in women, was speedily amenable to creosote, 3 drops in sugared water relieving the severest pain, a repetition in two or three hours being rarely required. *Leucorrhœa,* whether vaginal or uterine, even when very obstinate, often yielded in a few days to a lotion of 2 drops to the ounce, thrown in two or three times a day. So also three or four injections usually sufficed for the cure of *gleet.* In *menorrhagia* in non-pregnant women, and in some cases of hæmorrhage prior to delivery, due to *placenta prævia,* it has been found very useful. Indeed, it is a valuable *hæmostatic agent,* whenever the bleeding proceeds from small vessels, and especially those of mucous membranes. In some of these cases a more concentrated mixture is required, as 10 to 20 drops to the ounce.

Delirium Tremens.—Dr. Prat (Ann. Ther. and Sch. Jahrb., 1853, No. 8) has administered by the mouth, *chloroform* in considerable doses; in a case related, after three doses sleep came on.

Epilepsy.—Dr. Bucknill (Lancet, August 13) relates two cases of epilepsy in lunatics treated by tracheotomy. In the first case, after the operation (thirteen months before date of report) the fits were greatly mitigated in number and severity; in the second case reported, (two months after operation) there was equal benefit from the operation.

Dr. Brown-Sequard (Philadelphia Med. Examiner, April, 1853) states that cauterization of the larynx in epilepsy may be substituted for tracheotomy. He has, however, only treated epileptic animals in this way, but he states that several American physicians have pursued the plan with benefit on the human subject. He applies a solution of nitrate of silver (60 grains to ℥j aquæ) to the larynx every day, for five or six weeks.

Fever, Continued.—Dr. Gee and Mr. Eddowes (Lancet, July) have administered quinine in continued fever (apparently maculated typhus), in doses of from 3 to 10 grains, every two or three hours, in powder. The average duration of the disease, if the treatment was commenced within the first seven days, was 4·4 days; if during the second week, 5·8 days. The effect on the pulse was usually to cause a gradual and steady reduction in point of frequency.

Dr. Todd (Med. Times & Gaz., August) has treated 18 cases of maculated typhus with repeated doses of brandy (½ to 1 oz. every hour or half-hour, day and night). In addition, ammonia and chloric æther, and strong beef-tea, were administered. One case died; in the other cases, the benefit from the treatment was very marked.

Fever, Intermittent.—Two years since, M. Bartella brought forward his plan for the more economical treatment of ague, by administering equal parts of sulphate of quinine and tartaric acid. He now (Bull. de Thérap., tom. xlv. p. 49) re-enforces his former statements by new facts, having treated altogether 208 cases in this way, 196 of these being simple intermittents, and 12 pernicious fevers.

Dr. Bastille, too, who has been pursuing the same experiments, states as his conclusions (Gaz. des Hôp., No. 87)—1. That sulphate of quinine given with equal parts of tartaric acid is more active than the simple sulphate; 2. As a general rule, half the quantity of quinine so combined suffices; but in some descriptions of fever, as in the pernicious ones of Italy, larger doses than this are required.

Furunculus.—M. Nélaton (Gaz. des Hôp., No. 96) observes, that the development of furuncles may be always arrested by keeping the part covered with a linen compress which has been dipped in concentrated alcohol. This must be accurately applied to the part, and care taken to keep it constantly moist, so that evaporation may be continually taking place from its surface.

Mr. Flint (Assoc. Med. Journ., July) recommended the application to boils and carbuncles of poultices and emollient remedies for twenty-four hours, and then the application of strips of lead-plaster to the boil and to the surrounding skin, so as to produce a considerable degree of pressure; the plaster is to be changed daily, or every three or four days, according to the amount of discharge.

Gleet.—See Creosote.

Insanity.—Dr. Oliver (Med. Times & Gaz., July 16) recommends in mania and dementia large doses of opium and morphia. He refers to patients who, at the time of this communication, were taking with benefit from 16 to 20 grains of solid opium daily, and narrates one interesting case at length, to show the value of the treatment.

Leucorrhœa.—Menorrhagia.—See Creosote.

Neuralgia.—Crasio (Schmidt's Jahrb., No. 7, p. 27) recommends the endermic application of atropine; 1 grain dissolved in alcohol is mixed with ʒj of lard, and applied to a blistered surface. The belladonna soon produces its physiological effects, and sometimes these are intense. In all cases, however, the neuralgia disappears, or is much lessened.

Ophthalmia: Granular Lids.—M. Hirson, who so strongly praises tannin as the most valuable astringent application in granular lids and other affections of the eye, recommends the following formula as ensuring the equable diffusion and application of this substance, which, when not used in an impalpable state, may induce irritation:—Tannin 5 parts, distilled water 20; dissolve, and add 10 of gum Arabic, and strain.—See Creosote.

Otalgia.—M. Delioux (Bull. de Thér., tom. xliv. p. 529) recommends in otalgia, and also in cases of tinnitus aurium, arising from excessive sensibility of the nerve, that the vapour of ether should be conducted into *the meatus by means of a small tube;* at the same time the hand is kept over the ear for five or six minutes. Absorption rapidly takes place, and a cure is sometimes at once produced. If necessary, the process may be repeated several times a day.

Pertussis.—Dr. Fleetwood Churchill (Monthly Journal, August), following the advice of Dr. Simpson, has used chloroform inhalations, not carried to narcotism in several cases. In the cases of very young children, it is difficult to cause the inhalations to be performed at the right time and in the proper manner; but in older children (of twelve or fourteen), Dr. Churchill directs the inhalation to be used at the moment when tickling in the larynx is felt; the chloroform removes the spasm, and the cough is for the time prevented. By persevering with the chloroform in this way, the threatenings of attacks become less frequent, and at last cease. Dr. Churchill does not use a handkerchief or an inhaler, but the patient is taught to drop twenty or thirty drops into the palm of the hand, and to inhale the vapour mixed with a good deal of atmospheric air.

Dr. Watson (Assoc. Med. Journ., August) repeats the statement lately made as to the great utility of topical applications to the larynx in hooping-cough.* He uses the sponge and whalebone employed by Dr. Green, with a solution of nitrate of silver, of *variable* strength (from grs. xv to ℈ij to water ℥j), according to the stage of the disease; the weaker solutions being employed in the early and inflammatory stages. The application is used every second day, or more frequently if the hoops are violent. If the application causes violent vomiting, frequent small doses of heavy magnesia, with a small quantity of trisnitrate of bismuth, are recommended. Dr. Watson has used the hyposulphate of soda and silver† with good effect; and promises a further communication on this subject.

Phthisis.—M. Trousseau (L'Union Méd., Août 25) has revived a method of treatment proposed by Dioscorides—viz., *arsenical inhalations.* He mixes one part of arsenite of potash with twenty of water, and moistens with part of the liquid a piece of filtering paper. When this is dry it is rolled into the form of a cigarette, and smoked, once or twice a-day, for one or two weeks. The vapour appears to cause a good deal of irritation, and often after four or six whiffs the cigarette must be laid aside for a few minutes. M. Trousseau states that this plan diminishes the bronchial catarrh and the expectoration, but has no effect on the deposit of tubercles.

Porrigo.—See *Tinea.*

Rheumatism.—Dr. Owen Rees (Lancet, June 11) refers to his recommendation of *lemon-juice,* and states that although he considers it as "an antidote to the true rheumatic diathesis," he has found it useless in gonorrhœal and syphilitic rheumatism, and in the rheumatic pains in Bright's disease, or in those connected with non-gonorrhœal purulent discharges.

Spina Bifida.—M. Chassaignac (Bull. de Thérap., xlv. 63) strongly protests against the prevalent practice of abandoning cases of spina bifida as hopeless; for although many of these are accompanied by such organic changes as to render a cure impossible, others are placed in more favourable conditions, and call for attempts for their relief. In proof of this he relates an interesting case, in a child five months old, in which a radical cure was effected by iodine injections.

Strabismus.—Mr. Spencer Wells (Med. Times and Gaz., August 27) speaks highly of the effect of prismatic spectacles. On the sound eye the use of the prism produces an image on a point which does not correspond with that of the other eye; double vision is thus produced, which, if the prism be not strong, is corrected by increased action of an abductor or adductor muscle, as the case may be, which brings the image on to the corresponding point. Advantage is taken of this in strabismus, and contraction of the relaxed muscle is caused. In some cases the prismatic glasses were found very useful after operation, when by themselves they had been inefficient.

Tænia.—Dr. Christison (Monthly Journal, July) employs with great success in tapeworm, the etherial oleo-resinous extract of the male-shield fern (Lastræa Felix-mas), in doses of 20 to 24 grains.

* See No. 23: Therapeutical Record—Pertussis. † See No. 22, p. 567.

Tinea favosa.—In a case of favus, Dr. Jenner (Med. Times and Gaz., August) has used with great effect a wash of *sulphurous acid*. The acid is made by passing a stream of the gas through water to saturation; and of this strong solution one part is added to three of water. Rags wet with the solution are applied over the head, and covered with oil-silk. The parasitic plant is destroyed, and the crusts rapidly separate, leaving a healthy or somewhat reddened skin, to which simple dressing may be applied.

Tumour, erectile, of the Orbit.—Dr. Brainard (Lancet, Aug. 20) relates a case of orbital tumour, unaffected by ligature of the carotid, partially improved by puncture with hot needles, but finally cured by the injection into the tumour of ʒj of lactate of iron (8 grs. to ℥j aquæ).

Ulcers.—Mr. Spencer Wells (Med. Times and Gaz., July 23) has treated ulcers by galvanism with great success. He uses either two plates (zinc and silver joined together by a wire) or Pulvermacher's chain. If the former be used, the silver plate is placed over the ulcer, the zinc plates on the moistened skin at some little distance. Granulations form on the ulcer very rapidly, so that a great difference is often noted even in the short interval between two dressings. The plates (or chain, if used) are kept constantly applied.

Urine, Incontinence of.—In cases of involuntary micturition at night, Dr. Deiters (Pr. Ver. Ztg., 1853, p. 16) praises extremely the effects of cubebs, given in tolerably large doses twice a day, for three to eight weeks. The same remedy is useful in nocturnal seminal emissions.

Uterus, Prolapsus of.—In cases of prolapsus, and in vesico-vaginal fistulæ, Mr. Marshall (Med. Times & Gaz., July 2) has obtained excellent results from cauterization with a wire heated by galvanism. In prolapsus, a series of eschars are made by the same means in the mucous membrane of the vagina; and after cicatrization, the passage is considerably contracted.

Worms.—MM. Beauclair and Viginer (Gaz. Méd., No. 30), in the course of a paper having for its object to demonstrate that the production of worms must be regarded as a diathesis, and that in treating those suffering from the disease we must aim at the correction of the vitiation of the humours, testify to the great benefit they have derived from the administration of cod-liver oil. They recommend the following formula:—5 drachms of the oil are to be mixed with 6 of powdered sugar, 15 grains of bicarbonate of soda, 6 drops of essence of mint, and 1 drop of the essence of bitter almonds. This is to be given fasting, divided into two doses. In the case of adults, the entire quantity is to be given, substituting carbonate of potass for the soda. They at the same time recommend tepid alkaline baths for half or three-quarters of an hour; and when the functions of the skin are slow in re-establishing themselves, warm-air baths, or baths giving off ammoniacal fumes, are useful. Good diet, and all hygienic means calculated to fortify the cutaneous, respiratory, and digestive functions, are also indicated.

POSTSCRIPT.

We have received from Dr. M'William a statement in answer to an article in the last number of the 'Edinburgh Review,' entitled "Quarantine, Small-Pox, and Yellow Fever." We regret extremely that we are unable to publish Dr. M'William's answer at present; but we hope to do so in a future number. To those who have followed the progress of the discussion on yellow fever in our pages, it will be no surprise to hear that Dr. M'William has had no difficulty in showing, that of all the inaccurate statements which have disgraced the yellow-fever controversy, this article in the 'Edinburgh Review' is nearly the worst. We must of course admit that the reviewer intended to state what he believed to be truth; but if so, we can only wonder at the violence of the partizanship which has so completely blinded and confused his judgment. We cannot avoid expressing our great regret that the editor of so influential a work as the 'Edinburgh Review' should have published this article without putting it into the hands of some competent medical man, who might have laid before him the overwhelming array of facts which are disguised, distorted, or ignored in the mischievous pages of the 'Review.'

BOOKS RECEIVED FOR REVIEW.

The Retrospect of Medicine. Edited by W. Braithwaite, Lecturer on Obstetric Medicine at the Leeds School of Medicine. Vol. xxvii. January—June, 1853.

The Decline of Life in Health and Disease, being an attempt to estimate the causes of longevity. By Barnard Van Oven, M.D. London, 1853. 8vo, pp. 300.

Die Erkenntniss der Lungen-krankheiten, vermittelst der Perkussion und Auscultation. Von Dr. H. Locher. Zurich, 1853. 8vo, pp. 312.

Notes on Pericarditis, Endocarditis, and Organic Disease of the Heart and Aorta, as observed chiefly in the Jamsetjee Jejeebhoy Hospital at Bombay. By C. Morehead, M.D., Professor of Medicine, Grant Medical College.

Vestiges of the Natural History of Creation. Tenth edition. Illustrated by numerous engravings on wood. London, 1853. 8vo. pp. 325, with an appendix.

Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D. Cantab., and C. B. Radcliffe, M.D. Lond. L R.C.P. Vol. xvii. January—June, 1853.

The Gulstonian Lectures. On the Acute Specific Diseases. By W. Jenner, M.D. Lond. F.R.C.P., Professor of Pathological Anatomy, University College, &c. London, 1853. Pamphlet, pp. 55.

The Pathology of the Bronchio-pulmonary Mucous Membrane. By C. Black, M.D. Edinburgh, 1853. Part I. 8vo, pp. 99.

Lehrbuch der Pathologischen Anatomie und Diagnostik. Von Dr. Carl E. Boeck. Leipsig, 1852. Zweiter Band.

Reports by Neil Arnott, Esq. M.D., and Thomas Page, Esq., C.E., on an Enquiry ordered by the Secretary of State, relative to the prevalence of Disease at Croydon, and to the Plan of Sewerage. 1853. (Parliamentary Paper.)

Report on the Cholera in Jamaica, and on the General Sanitary Condition and Wants of the Island. By Gavin Milroy, M.D. 1853. (Parliamentary Paper.)

Second Annual Report of the Wilts County Asylum, for the year 1852 (with an Appendix).

Report a M. le Prefet de Police sur la question de savoir si M. le Dr. Auzias Turenne, peut être autorisé à appliquer ou à expérimenter la Syphilisation, l'Infirmerie de la prison Saint-Lazare. Par MM. les Docteurs Melier, Ricord, Denis, Conneau, et Marchal (de Calvi).

Verslag omtrent de Ziekten van het Hart en de Groote Vaten, &c. Den Dr. F. J. I. Schmidt. Rotterdam, 1852.

Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands, from the Battle of Roliça in 1808, to that of Waterloo in 1815. Revised to 1853. By G. J. Guthrie, F.R.S. Fifth edition, 1853.

Beitrage zur Kenntniss der Urinabsonderung, bei Gesunden (Bestimmung der Phosphorsäure und der freien Säure im Urine). Inaugural Abhandlung. Von August Winter.

Habit, Physiologically Considered; (a Lecture delivered May 9th, 1853, at the Bristol Literary and Philosophical Institution.) By J. Addington Symonds, M.D. (From the Psychological Journal for July, 1853.) London, 1853.

Lehrbuch de Zoochemie. Von H. W. Heintz. Berlin, 1853.

Ueber Rheumatismus und Gicht. Von Dr. G. E. Wiss. Berlin, 1853.

Sketch of the Operation, and of some of the most important results of Quarantine in British Ports, since the beginning of the present century. By Gavin Milroy, M.D., &c. London, 1853.

On the prevalent Treatment of Disease. By Frederick C. Skey, F.R.S. London, 1853.

On Dysentery. By Arthur Dupuy, M.R.C.S.L. (Inaugural Dissertation.) Edinburgh, 1853.

Recherches sur la Folie Paralytique, et les diverses Paralysies Générales. Par Jules Favret, M.D. Paris, 1853.

Der Harnstoff als maass des Stoffwechsels. Von Th. L. W. Bischoff. Giessen, 1853.

A Manual of Materia Medica and Therapeutics. By J. Forbes Royle, M.D., F.R.S., Professor of Materia Medica and Therapeutics in King's College, London. London, 1853. Second edition.

Valedictory Address, delivered to the Members of the Royal Medical Society of Edinburgh. By W. M. Dobie, M.D., Senior President of the Society. Edinburgh, 1853.

An Account of the System of Clinical Instruction and Examination followed in the Grant Medical College at Bombay. By C. Morehead, M.D. (Reprinted from the Seventh Report of the Grant Medical College.)

An Essay on Organic or Life Force. By J. H. Waters, A.B. Philadelphia, 1851.

Burton and its Bitter Beer. By J. Stevenson Bushnan, M.D., Fellow of the Royal College of Physicians at Edinburgh. London, 1853.

Report of the Twenty-second Meeting of the British Association for the Advancement of Science, held at Belfast, in September, 1852. London, 1853.

INDEX TO VOL. XII.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

END OF VOL. XII.

Savill & Edwards, Printers, 4, Chandos-street, Covent-garden.