BIBLIOGRAPHIC NOTICES.

On Latent Life, and on Vital Crystallization.

CONSIDERATIONS SUGGESTED BY SOME PASSAGES IN THE WRITINGS
OF CARUS AND OF TREVIRANUS. BY DR. GRAVES.

CARUS, so well known by his works on anatomy and physiology, has made some observations on latent life,* that appear deserving of attention, and have suggested to me the following reflections :—

An accurate observation of physical phenomena has led to the universal adoption of the hypothesis that the most active principles which operate on the material world, may exist in a dormant state denominated latent. Thus we speak, for instance, of latent heat ; Carus is of opinion that life may be placed in a similar condition, and certainly in the seeds of plants it continues in a quiescent state that almost justifies the analogy. Here the germ is already impregnated, and the matter forming the seed contains, encased within its envelopes, a something which exhibits however no evidence of its existence, until that matter is exposed to the effects produced by certain physical agents, heat, moisture, air, &c. ; then, indeed, the quiescent principle immediately asserts its empire, and every subsequent change, which the matter forming the seed is henceforward destined to go, is evidently dependent on vital influence. That the power of germination may be retained for a great length of time, is proved by numerous facts, many of which are mentioned by Decandolle, in his *Physiologie Végétale*. Thus it is more than sixty years since a bag of seeds of the sensitive plant was brought to the *Jardin de Paris*, and it is found that they still grow when sown. Pliny mentions that wheat 100 years old has germinated ; and Horne had an opportunity of trying the experiment with wheat 140 years old, which grew very well ;

* Müller's Archiv. Heft. vi. 1834.

Gerardin took the seeds of French beans, preserved in their pods in Tournefort's *Hortus Siccus*, and consequently 100 years old at least, and found that their power of germination was unimpaired. These facts are sufficient to warrant the conclusion, that the period during which the seeds of plants are capable of preserving the power of germination, might be indefinitely prolonged, provided that the physical influences to which they are exposed be favourable, and we are consequently led to reflect on a singular and marked difference between latent and manifest life; the former may exist on, I had almost said for ever; the latter, once commenced, has its appointed end and termination. Thus the curious fact has been observed of a bulbous root taken from the hand of an Egyptian mummy, having germinated when placed in the soil; how happened it that this bulb remained for several thousand years in contact with the fingers of death, without its own vital principle being either extinguished or called into active operation? What power at once preserved that principle, and held it in abeyance? And yet so it was, and age after age passed away without summoning into action that wondrous spell, which can thus convert this long-enduring tenant of the tomb, into the *lily of the field*, the scriptural emblem of beauty, and the honoured type of the glories of vegetable life, beside the purity and brightness of whose hues, even the raiment of Solomon appeared dull and faded. If the latent principle may thus co-exist, combined with matter during so many centuries, there can be no reason why this association of that which *is*, and that which *is to be*, should not continue for ever, provided the physical medium in which this organized mass happened to be placed, was calculated like the cool, dry, dark air of the Egyptian Sarcophagus to preserve its mysterious inhabitant unawakened, uninjured! In this point of view we may almost award to life the attribute of eternity. How wonderful the arrangement of matter which can thus hold, incarcerated in its organized recesses, a principle that waits but the genial influence of heat and moisture to burst its prison. How fleeting the existence of life once actually commenced, compared with the durability of life thus dormant, but ever ready to begin its mortal race? But here the question arises, can the ova of animals be preserved like the seeds of plants; can their latent vitality resist the lapse of ages; can they, too, lay claim to so wonderful a power? There is reason to believe this not impossible, even with regard to the ova of vertebrated animals, for it is recorded that under peculiar circumstances hen-eggs have been kept for weeks, months, or even years, before being hatched. Nature has, indeed, intended that in the

ova of most animals, the epoch of impregnation should precede but by a brief period that of incubation whether performed in the body, or out of the body of the parent; and consequently the apparatus of protection, the matter containing and united to the latent principle of life, is not calculated to endure, like that of seeds, unchanged; on the contrary the physical influences to which its material arrangement is usually exposed, are of a nature fitted either at once to destroy, or at once to force it into manifest life; but were it otherwise, it is more than probable that the latent vitality of the animal germ is not inferior in longevity to that of plants. The most remarkable instances of life preserved in a latent state, are exhibited by certain infusory animalcules, as the *Macrobotus Hufelandi*, which Schulze preserved for many years dried into the form of an atom of dust and surrounded with a little dry sand; this desiccated little being, so long inanimate to all appearance, immediately revived when placed in a drop of water. Leeuwenhoek's experiments on the revival of dried *wheel animals* are equally curious; but it is needless to multiply examples of latent life in infusory animals, when obvious instances may be found in other classes. Thus what more remarkable instance of long suspended duration, or of latent life, than that recorded by the celebrated Dr. MacBride of Dublin, who having one day opened a chest in his museum, for the purpose of *washing in warm water* some snailshells it contained, was called away for a time, and on his return found the animals creeping about on the sides of the vessel; these shells had certainly been in the collection nearly forty years! Many stories, some probably unfounded, have been from time to time related, concerning toads imbedded in wood or in rocks. Doctor Edwards relates several experiments performed in France, in which these animals were imprisoned in plaster of Paris, and were, after many years, still alive when the plaster was broken. At the last meeting of the British Association, Mr. Sturge of Birmingham, read a highly interesting paper on the recent discovery of a toad, in sand-stone rock, in Park Gardens, Coventry. The sandstone was tolerably porous, but quite free from damp from which the animal could derive nutriment, or any fissure by which it could draw air for respiration. In fact it must, as was remarked, have been hermetically sealed up in a state of torpor, or of *latent vitality*. How long the animal had been so situated it is impossible to determine, but certainly it had been there for ages, and there can be little doubt that its latent vitality might have continued for ages longer. Humboldt relates that in some of the immense tracts of country exposed to the annual inundations of the great American rivers, the swamps and marshes are teeming with

life for a long time after the waters of the river have retired. As the dry season continues, however, many animals desert the more arid plains, while others, among the rest various species of reptiles, remain in the desiccated and hardened mud, awaiting in a state of torpor another overflowing of the river ; when this takes place, they revive, and the moistened mud seems to give birth to animals. It is probable that the observation of facts similar to this in the neighbourhood of the Nile, or of some Asiatic river, originated the generally received opinion of the ancients, that slime and mud are capable of giving birth to broods of noxious reptiles, by an act of equivocal generation. The question of equivocal generation presents, as is well known, many difficulties incapable of being solved, without a knowledge of the important part performed by latent vitality ; for without admitting that the ova of vast numbers of infusory animalcules, and the germs of many of the cryptogamic class of vegetables, are capable of preserving their vitality for an indefinite period when dried, we could not explain the almost universal appearance of these organized beings in every situation favourable to their development, the moment that animal or vegetable matter, mixed with water, undergoes a certain degree of decomposition. To conclude, the hybernation of certain of the mammalia, and the apparently lifeless state of torpor produced by cold, or by incomplete asphyxia, and, in fine, the almost suspended animation observed in the trances of hysterical females, are all, more or less, connected with the subject of latent vitality.

I have dwelt upon the most obvious and interesting considerations connected with the subject of latent vitality, without following *Carus* in his ingenious, but speculative and fanciful, notions concerning the latent vitality of certain diseases, and concerning the more extensive signification he attaches to the idea of life ; a term he would apply to many of those powers which act on matter, and gives rise to various modifications of its form and properties, such as crystallization, chemical attraction, &c. Without at all assenting to the analogy *Carus* seeks to establish between the causes of living motions, and those producing crystallization, I may remark that a wide and interesting field of inquiry has been lately opened in the discovery that the internal skeletons of many of the *Poriphera* consist of numerous spiculæ, in some species siliceous, in others calcareous. Now Professor Grant has shewn in his *Outlines of Comparative Anatomy*, that these spiculæ have in each animal a distinct form serving to characterize them. He asserts that the form of these crystals, whether they consist of silex or of carbonate of lime, is always constant in the same species, and he gives delineations

of ten different forms of siliceous spiculæ, each belonging to a separate species; some of these spiculæ have all the appearance of mere crystals, and their formation can be readily referred to the operation of the causes which preside over the arrangement and juxta-position of the particles of matter in siliceous crystals that occur in rocks, and in unorganized matter. But there are other forms of curved siliceous spiculæ represented by Doctor Grant, and to which no analogies, I believe, exist among siliceous crystals, whose formation has resulted from the common laws of crystallization connected with vitality. This is in truth a very curious and very interesting subject, for if it should appear that living bodies contain a group of siliceous or calcareous crystals, arranged in forms not occurring in crystals formed beyond the influence of vitality; if this be the case, I say, shall we not be justified in concluding that whatever be the nature of the forces which preside over crystallization, these forces may be generated *as functions of living bodies*, and they may, when generated by the living organs, undergo modifications, such as do not occur in unorganized matter. In fact we may almost anticipate the division of crystals into two groups, one formed by and obeying the laws that preside over inanimate matter, the other resulting from influences originating in, and generated by the living principle, and in which, perhaps, there may be found crystals oftentimes similar to those of the former class, sometimes different. Here is a new point of contact between the kingdoms of animated and dead matter, a new set of facts indicating an analogy between the forces which preside over both; whether *electricity* in its multiplied modes of action, be not the chief agent, is a subject of curious, but probably, unproductive investigation. This matter of vital crystallization becomes of importance, not merely on account of its abstract philosophical interest, but on account of the richness and variety of the products eliminated in the animal laboratory; in human bones and teeth we have the phosphate of lime, as Breschet has shewn, not merely amorphous, but crystallized. As yet, however, no investigations have been made to ascertain whether the crystallized principles that enter into the structures of the higher animals assume forms peculiar to themselves. I may take this opportunity of remarking, that Mr. Coldstream, in his excellent article on the Acalephæ, published in the Cyclopædia of Anatomy and Physiology, although he refers to Professor Ehrenberg's recent discoveries relative to the supposed eyes of the *medusa aurita*, has omitted mentioning the actual discovery by that naturalist of crystals composed of the carbonate of lime, and which lie enclosed in a little vesicle or bag connected with each organ of vision. However problematical the hypothesis of

Ehrenberg in considering the minute red points as eyes, there can be no doubt as to the existence of calcareous crystals, which, according to him, possess a great variety of forms, some tabular, some circular, and some pyramidal. The formation of crystals under the influence of vegetable life, presents another fertile field of investigation.

The best account of the researches hitherto made on this subject, is found in *Treviranus*, *Physiologie der Gewächse*, published in 1835, and *Raspail*, *Nouveau Systeme de Chemie Organique*, 1833. Crystals occur very abundantly in the substance of many living vegetables, sometimes in the form of spiculæ within the cells and adhering to their parietes, and sometimes in the intercellular spaces; in the latter situation they are deposited only on plants belonging to the tribe of orchideæ, such as cypripedium, insigne, and neottia discolor. The crystals consist usually of phosphate of lime, as in the genera narcissus, hyacinthus, orchis, ornithogalum, phytolacca, &c., or of sulphate of lime, as in many of the *Leguminosæ*. Oxalate of lime crystals occur in the bulbs of the iris florentina, in the leaves of rhubarb, and in the cactus Peruvianus. In some plants these crystals are so abundant in the tissues of the leaves, that they must there discharge an important office, analagous to that performed by the siliceous spiculæ of sponges. Raspail remarks that we cannot obtain oxalate of lime in crystals by the processes usually employed in our laboratories, while the same substance is frequently found crystallized in plants, whose living tissues seem thus capable of exerting an influence we cannot imitate, and which he is inclined to attribute to an electric agency, a view supported by the experiments of Becquerel, who succeeded in producing, by the application of electro dynamic forces, the crystallization of substances which could not be procured in the crystalline form by any other method.

To conclude, this subject appears to deserve more attention than has hitherto been bestowed on it, particularly with reference to the forms which siliceous and saline substances assume in the tissues of plants and animals, as compared with the forms of the same substances when they occur as unorganized matter. If, for instance, a substance capable of assuming so many distinct secondary forms of crystallization, as carbonate of lime, always occurs in a certain animal tissue in the same form, we must then attribute to that tissue powers capable of modifying the crystallization of that substance, and must acknowledge a new link connecting the phenomena of life with some of the more subtile agencies that influence matter.

Having been led in the course of the foregoing observations

to speak of the evident connexion existing between life and electricity, it may not be misplaced to remark that the experiments of Pouillet on the development of electricity, when fluids are converted into vapour, or when they exhale gases, render it not unreasonable to suppose that the function of respiration must be closely connected with the production of electrical changes in the blood, an hypothesis apparently supported by the instantaneous uneasiness felt the moment that breathing is interrupted. Were our means of investigation adequate to the inquiry, it would, perhaps, be found that the symptoms of asphyxia are, in part, at least, the result of a derangement in the electrical operations performed by the lungs.

WEBER on the Pulse; on the Dilatation of the Heart; and on the Use of the Cochlea in the Organ of Hearing.

ANALYSIS AND OBSERVATIONS BY DR. GRAVES.

PROFESSOR WEBER's well known learning and deserved celebrity must insure his opinions much respect on all physiological subjects, and must render an analysis of them acceptable to the English reader, particularly when, as in the present instance, at variance with those generally received in this country. In this analysis of *his* chapter on the pulse, it will be seen, that I, myself, too, have been led to adopt views different from those promulgated by Elliotson and Arnott, a circumstance which at first startled me not a little; for finding myself in opposition to such great authorities, I could not but feel distrustful of the truth of my own conclusions. As, however, my views have been approved of by some friends, on whose judgment I place great reliance, and as the subject is one evidently open for discussion, I have thought it right to append my conclusions to those of Professor Weber.

In the first chapter he relates a number of observations proving what is indeed now generally admitted by physiologists, that the pulse in arteries near the heart, precedes by a short but perceptible interval, the arterial pulsations in the remote and smaller vessels; thus although no difference can be detected between the beats of the axillary, and the external maxillary arteries, yet a very notable interval, about the seventh part of a second, elapses between the stroke of the axillary and that of the metatarsal artery. Weber's explanation of this phenomena is precisely the same that I have for several years given in my lectures, and as it in some respects differs from the

opinion generally entertained upon this important subject, I have thought it well to subjoin a literal translation of the whole passage :

" The pulse consists in a sudden dilatation of the arteries, (*extensio arteriarum*,) caused by the pressure of the contained blood. *This pressure is produced when a fresh quantity of blood is impelled from the heart into the arteries already full of this fluid.* The reason is obvious, for the blood previously in the arteries cannot flow onwards through their capillary and venous terminations, so rapidly as the blood impelled by the left ventricle into the aorta enters the arterial system ; the consequence is that the contained offers a certain degree of resistance to the entering blood. Now every fluid exposed to a pressure from behind, re-acts on the surrounding parts, not merely in the line of direction of that pressure, but on every side ; and, therefore, at each contraction of the ventricle, the blood the arteries contain, re-acts in consequence of the impulse pressure of the injected blood, not merely longitudinally but laterally, the arteries being fixed at both extremities."

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" Thus it is, that although the blood does not flow out of the arterial system with as great velocity as it flows into it, yet room is made in that system for the momentary excess, an excess which is gotten rid of before the next stroke of the ventricle, by means of a continuous flow of blood through the arterial extremities into the venous and other portions of the vascular system. The case would be very different were the blood contained in unyielding tubes, whose parietes would not admit of extension in any direction, for then the impulse or stroke of the heart's blood against the arterial blood would be propagated throughout the whole extent of the arterial system with the same velocity that sound travels in blood, a rate much more rapid than that of sound through air. It is evident that a propagation of pressure so rapid, could not be perceived to occupy any time in passing from the heart to the arterial extremities, and consequently were the blood contained in unyielding tubes, its onward motion simultaneous, even in the extreme vessels, with the application of the pressure derived from the impulse of the heart's blood, would be the only motion observed. But as the arteries of the human body easily admit of extension in the direction of their longitudinal, and less easily in the direction of their transverse diameter, it follows that they must yield to this pressure in such a way that the arteries nearest the heart are, in virtue of their elasticity, first dilated and lengthened by the impelled blood, upon which, however, they constantly contract, and so induce a distention of the next portion of the arterial system, a distention propagated with great rapidity onwards, until, like a wave, it reaches the extreme vessels."

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" We may now understand the extent of Bichat's error when

he confounded this wave-like motion of the arterial blood with its mere onward motion, and argued that as the arteries are always full, the pulse must depend, not upon an advancing wave, but upon a percussion or impulse communicated at the same instant to all the blood in the arterial tubes. There is a vast difference, however, between a wave-like motion in the particles of a fluid, and an onward or flowing motion; a wave depends upon an oscillation, not a progression of the fluid, and hence light bodies swimming on its surface are raised upwards by the wave; but when it has passed, they are found to be precisely in the place they occupied before its arrival. Bichat was right in attributing the pulse to an impulse communicated to the blood in the arteries; he was wrong in thinking it arose from a pressure acting simultaneously upon the whole of that blood, for we have seen that the pulse is caused by a pressure so re-acting on the distensible parietes of the arteries, that the pulse travels along like a wave."

Weber, after some other remarks, observes that the force to which waves in water owe their origin, is gravity, or in other words, the weight of the fluid, whereas the arterial wave causing the pulse, arises from the elasticity and vital contractility of the arterial tunics. The more rapidly these tunics yield, in the first instance, to the increased quantity of fluid thrown in by the left ventricle, and the more rapidly they afterwards recover their former dimensions by contracting in their contents, the more speedy will be the progress of this wave. There can, indeed, be little doubt that this wave, whose propagation depends, not only on the force of the heart, but on the vital powers of the arteries, must travel at different rates in different persons; or in other words, that an interval of time must elapse between the pulsation of the arteries near the heart, and that of the more distant arteries, greater in some persons than in others. As an examination of the arterial tunics, proves that their fibres can be stretched much more easily in the direction of the arteries longitudinal, than of its transverse diameter, it is evident that, considering the whole arterial system as one tube, into which, being already full, each systole of the heart forces a certain quantity of blood, this tube will yield most in the direction in which it is most extensible, or in other words, will make room for the superadded blood chiefly by elongating itself, partly by increasing its calibre. Hence it follows that each systole of the left ventricle causes a smaller degree of transverse dilatation in the arterial tree, than would have occurred could the additional room have been gained only by an augmentation of its transverse diameter. Indeed this dilatation, although real, is so slight that it has escaped the notice of many experimenters; all, however, are agreed as to the increase in the length of the ar-

tery which occurs at the moment of the pulse. Weber's concluding observations on this subject are worthy of notice :

“ Bichat erred in saying that the blood driven by the heart into the arteries, tends to straighten these vessels, and that the more, the more flexuous they are. We have just seen that every artery is lengthened at the moment of the pulse, and consequently those which were straight, *but fixed at both ends*, must become at that moment somewhat curved, while those that were flexuous will of course become more so. Thus, take the carotid, fixed at one extremity on the arch of the aorta, at the other in the bony *canalis caroticus*, it is obvious that when elongated by the new blood derived from the ventricular systole, this artery must become more curved, a truth confirmed by observation. This is the reason why in injected subjects the vessels distended with wax are always found to be more curved and flexuous than the vessels of an uninjected body, it being chiefly by an elongation of their longitudinal fibres that room has been made for a large quantity of wax. When the additional pressure caused by the influx of superadded blood is removed, the artery contracts both longitudinally and transversely ; but as the former contraction is much more considerable than the former, it is obviously more efficient in propelling the blood onwards towards the veins. To conclude, says Weber, ut nunc quid pulsus sit, paucis verbis comprehendam, hæc addo, pulsus est effectus oscillationis propagatæ, quæ in membranis et sanguine arteriarum plenarum locum habet, originem vero a pressione sanguinis a corde propulsi ducit.”

I have been thus minute in giving Weber's explanation of the pulse, because it appears much more complete and satisfactory than that of many physiologists : he very properly dwells on the fact, that two motions in the blood are caused by each ventricular systole, viz. a progressive motion and a wave-like motion ; the former is so obvious that it has escaped the notice of none, and Dr. Elliotson, in his last edition of *Human Physiology*, a book of great learning and research, has clearly demonstrated, “ That one great office of the contractile powers of arteries is to enable these vessels to accommodate themselves to the quantity of the contained blood, so as to impart to the arterial canal such properties of a rigid tube as enable an impulse at the mouth of the aorta to be instantly communicated throughout the canal.”—(p. 186.)

Strange to say, however, Dr. Elliotson, although he allows that the arteries lengthen and become tortuous at each stroke of the heart when the pulse is felt, yet denies the existence of any lateral dilatation in these vessels during the systole of the ventricle ; he does not attempt to explain on what principle

of statics, pressure imparted to a fluid contained in a tube extensible both in its transverse and longitudinal diameter, can possibly act so as to increase the longitudinal dimensions of that tube, while the transverse dimensions remain unaltered! Dr. Elliotson has adopted the opinion of Sir David Barry, that the pulse is only felt when arteries are more or less compressed. Now, as Dr. Elliotson supposes but one motion, an onward motion of the blood taking place simultaneously in all the arteries, which he considers in the light of rigid tubes, and as he believes that the pulse only occurs at the moment when arteries are more or less compressed, it follows from his own premises, that when the compressing force of the fingers, for instance, is applied simultaneously to the carotid, and to the metatarsal arteries, the pulse should occur simultaneously in both; but it does not do so, and therefore Dr. Elliotson's hypothesis is untenable. There is here no means of explaining away the discordance between the fact observed, and the fact which ought to be observed according to Dr. Elliotson's reasoning. As to the conclusions of Sir David Barry on this subject, they do not appear tenable. The opinion of one other author requires some observation, I mean Dr. Arnott, to whose reasoning on every thing connected with the physics of physiology so much importance is deservedly attached. Dr. Arnott treats this question with great ability, vol. i. pp. 450, 51, 52, and sequent; fifth edition of *Elements of Physics*. At page 594 he says,

“At each jet of blood thrown into the aorta, a tumefaction or wave must spread from the heart to the extremities, for it is evident that if blood be at all pushed into the arterial system, it either must dilate it, or cause an equal quantity to be expelled at the same instant from the distant extremities; now, as the passage of blood through the capillaries appears perfectly uniform, there must be an intermediate dilatation. Dr. Parry and others should not have denied this dilatation, because they had never seen it, for even if its advancing front were more considerable than it is, it passes with such velocity, that, like a cannon ball crossing before the face, it could not easily be perceived.”

Now, with all due deference to so high an authority, I must beg leave to observe that the rate at which the pulse travels is comparatively slow, for we have seen that the pulsation of the carotid precedes that of the metatarsal artery by the sixth or seventh of a second; now, counting the distance between the two points at five feet, we ascertain that the rate at which the pulse travels along the arteries does not exceed thirty or at most forty feet in a second, whereas a cannon ball has a velocity of from 1500 to 1700 feet a second! To conclude, although Dr. Arnott has evidently miscalculated the velocity of the pulse, he is

right when he concludes (p. 596,) "that the cause of the sensation then cannot be the simple forward rush without tumefaction described by Dr. Young and Dr. Parry."

Dr. Arnott very properly takes into account other causes as connected with the formation of the pulse, *e. g.* a certain locomotion in the artery, as suggested by Bichat, and which is often sensible to the finger applied; another cause on which he dwells more is thus explained—

"That a tangible shock is conveyed through a fluid without any apparent accumulation of the fluid or change of velocity, and much in the manner of sound, is proved by the facts that we may discover the working of a water-pump at very great distances, through iron pipes connected with it, and even through elastic pipes of leather, as those of a common fire-engine, from which the water is spouting, nevertheless, in a uniform strain. *The pulse in a tied artery in which there is no current or rushing wave, must be chiefly from this cause and from the locomotion of the artery.*"

That this shock has anything to do with the pulse is sufficiently refuted by their different rates of travelling, the pulse occupying a very perceptible space of time in traversing the distance between the heart and the distant arteries, whereas the shock Doctor Arnott here speaks of, spreads through a fluid with the same velocity that sound traverses the same medium. There is no getting over this objection, the full force of which appears, when we recollect that sound traverses a fluid at the rate of 4708 feet in a second, as ascertained by the experiments of Colladon in the lake of Geneva, the result of which agrees very remarkably with that given by theory. As to the assertion of Doctor Arnott, that in a tied artery there is no current or rushing wave, although there is a pulse, it appears unfounded, for at the moment an additional quantity of blood is driven by heart's systole in the pervious arteries, an additional quantity is likewise forced into the tied artery, whose parietes by yielding, increase the capacity of the artery in every direction, and give rise to the phenomenon of the arterial wave as already explained; but there is this difference in the case of the tied artery, that the superadded blood is regurgitated when the arterial tunics contract, instead of being driven forward through the capillaries. On the wave hypothesis, and on that alone, can the existence of a pulse, in a tied artery, be explained. Having adopted conclusions, somewhat at variance with those of Doctor Arnott, I may be permitted to observe, that I feel in the strongest manner the danger of differing from so high an authority, particularly on such a subject as the circulation; a sub-

ject which he has advanced so materially, by disencumbering it of the theories of Barry and of Carson, which, I regret to say, still occupy a useless space in the pages of Doctor Elliotson. Let any one read Doctor Arnott's remarks upon those two theories, and he will justly feel astonishment at their having received the assent of so many, when first promulgated. I take some credit to myself, for having demonstrated to my class the fundamental errors of Sir David Barry's theory, long before the publication of Doctor Arnott's work, and at a time when it was made an object of grave inquiry by a select committee of French physiologists.

On the Dilatation of the Heart.—Bichat revived the opinion of Pechlin, that the diastole as well as the systole of the heart, is accomplished by means of muscular exertion. In proof of this assertion, he appeals to the sensation imparted to the hand, by a still moving heart, if grasped at the moment the diastole is affected; for, says he, it will be felt to expand itself with considerable force. This assertion of Bichat has, says Weber, deceived many, and is a remarkable instance of how easily we are led into error, unless we cautiously weigh all the circumstances connected with physiological experiments. It is true that the hand which grasps a living heart is pressed, but it is not true that this pressure is caused by the heart during the diastole of its ventricles. In living animals while the ventricles are relaxed and flaccid they are larger, being filled at that moment with blood, and while they are in a state of muscular contraction, they are also of smaller dimensions, inasmuch as the blood has been thereby expelled from their cavities. But matters are quite altered when, as in the case of a pulsating excised heart, no blood is either received into or expelled from the ventricles, for then their muscles, like all muscles of a similar form, swell notably during contraction. The heart is rendered shorter, reckoning from its apex to its base by the ventricular systole, but its circumference embracing both ventricles is increased. The same thing happens in the case of other muscles: grasp the biceps on the arm, and it will be found to press, so as to tend to open the hand during its contraction. In Bichat's experiment, therefore, it is plain that the heart presses the hand, not at the moment of the ventricular diastole, but of the ventricular systole. This error, into which Bichat led Richerand and Magendie, was refuted, first by Adelon, and finally completely exposed by the experiments of Oesterreicher. (*Lehre vom Kreislaufe des Blutes*, 1826). He observes—

“We may easily convince ourselves of the fallacy of this opinion

if we place a small heavy body on the heart of a frog, so adjusting its weight and size, that while it compresses considerably that organ, it should not conceal it from our view; a small piece of lead answers this purpose best. Now in the excised heart the systole occupies a much shorter time than the diastole, and consequently if the latter were capable of producing an active expansion of the heart, the bit of lead ought to be raised perpendicularly upwards, during a much longer period than is actually the case; for its motion in this direction is sudden and quite momentary, just as when the hand grasps the heart, its pressure is felt to be extremely short."

I have thought it well to give this remark of Oesterreicher, because it is directly at variance with the assertion of Doctor Corrigan, that in the frog the impulse of the heart is during the diastole. It will be remembered that this able experimenter exhibited the beating heart of a living frog before the Medical Section of the British Association last August, and that the members then present thought it evident that the ventricle is raised and pushed forward during the diastole.—(See Report of the Medical Section of the British Association, Dublin Medical Journal, vol. viii. p. 167.)

I may remark that the result of Bichat's experiment in the hearts of mammalia, entirely depends on the way in which we grasp the heart. If our fingers surround it transversely it will appear to expand during the ventricular systole; if, on the contrary, the fingers are extended in lines from the apex to the base, the heart will be felt to shorten in this direction during the systole of the ventricles.

On the Use of the Cochlea in the Organ of Hearing.—Weber has discussed this interesting question at some length, and I have thought it useful to give a literal translation of all his remarks, inasmuch as this subject has not been treated in a satisfactory manner by any English author whose works I have read. Doctor Arnott, for instance, makes no attempt at an explanation of the difference between the functions of the semi-circular canals, and of the cochlea; his only remark is that "the separate uses of these parts are not yet perfectly known." *Professor Quain*, in his admirable *Elements of Anatomy*, a work abounding in deep physiological research, does not even allude to any difference of function between the different portions of the labyrinth. Such being the case, Weber's opinion on this important subject will, no doubt, prove most acceptable to English physiologists:

"And first it is evident that the propagation of sound to the internal ear, takes place not merely through the *meatus auditorius*

externus, but also through the bones of the cranium; by the former we receive notice of sounds from without, by the latter, we more readily hear our own voice. The vibrations produced by our own voice are indeed also heard by the route of the external ear, but they are conveyed with greater distinctness through the medium of the bones of the skull. Thus, if you stop both ears firmly with the fingers, so far is your voice from being rendered inaudible, that you hear it more distinctly and louder than before. If now we remove the finger from one ear, immediately we find that the sound of our voice appears stronger in the other. *I shall now endeavour to prove that sounds propagated by and conveyed through the bones of the head, are heard chiefly by means of the cochlea, whereas sounds coming from without by way of the meatus auditorius externus, are not so readily received by the cochlea, as by the route of the vestibule and the semicircular canals.* In truth it is at once evident, that with respect to sound depending, as it always must, on vibrations communicated to the percipient nerve, it cannot be a matter of indifference in what manner that nerve is arranged for the reception of these vibrations: it cannot be a matter of indifference whether it receives them from a solid or from a fluid. I shall however, demonstrate, that in almost all animals, these vibrations are communicated to the extremities of the acoustic nerve by the two-fold means of a vibrating solid and a vibrating fluid, *and that in man the cochlea constitutes the part of the organ of hearing which is destined to place the nervous extremities in communication with a vibrating solid, whereas the semicircular canals place them in contact with a vibrating fluid.* The utility of this provision is not perhaps intelligible in the present state of our knowledge, but we have reason to conclude such a provision to be necessary, finding it so generally adopted in so great a number of animals. It is ascertained that sounds propagated through an uniform medium, whether fluid or solid, lose but little of their force, whereas they lose much in passing from a solid to a fluid, or *vice versâ*. Experience shows that in the shafts of mines, the stroke of a hammer in a neighbouring shaft is very audible when that stroke is made on the solid rock, and it is still more distinctly heard when the ear is placed in contact with the rock, whereas the voices of the miners in that neighbouring shaft are never heard. Sounds, too, are transmitted through water with the greatest celerity and distinctness, and may be heard at great distances when the head is under water, but they become inaudible the moment the head emerges above its surface. What an obstacle to the propagation of sound even a thin stratum of a different medium presents, is rendered evident by the diminished loudness of the noises in the street, when by drawing up the window we intersect the path of the ærial propagation of sound by means of thin panes of glass.

“Experience, too, proves that solid bodies communicate their vibrations to fluids, with a facility proportioned to their extent of surface, and that solids receive vibrations from æriform media more easily when the solid is shaped in the form of a membrane. A

tense cord does not easily communicate its vibrations to the air unless it be fixed to some flat body, which being of a like nature solid, receives the vibrations of the string without difficulty or loss, and propagates them to the air through the medium of its own extensive surface. It is an observation of these phenomena which has led to the adoption of sounding boards in those musical instruments, when the vibrations causing sound proceed from strings, as in case of the violin, the piano, and the harp, whereas sounding boards are not required in the various species of wind instruments. The reason of this difference is sufficiently obvious, in the one case a solid with an extensive surface must be brought into connexion with the vibrating string in order to diffuse its vibrations more energetically through the air, whereas in wind instruments, the vibrations being derived from the air itself, no such provision is necessary."

These laws by which sound proceeds with such facility through the same, and is weakened by passing into another medium, and the laws which regulate its transmission through air, solids and fluids, all tend to confirm the conclusion, that the vibrations conveyed through the bones of the head, are chiefly heard by means of the cochlea. For if the sonorous vibrations pass, as we have proved they do, from the air in the mouth to the internal ear, through the medium of the bones of the cranium, they will act no doubt on that portion of the internal organ of hearing which is nearest, and which possesses such an arrangement of the extremities of the auditory nerve as places these extremities in contact with the bone, itself a portion of the osseous communication between the mouth and the internal ear. That part is the cochlea, for the cochlea and the nervous expansion which adheres to its walls are intimately connected by means of its osseous parietes with the other bones of the cranium. These vibrations proceeding from the air within the mouth, cannot be transmitted with similar ease to the auditory nerve where it is distributed throughout the vestibule and the semi-circular canals, for here nature has separated designedly the nervous expansion from the bony parietes.

Weber is very strong and very distinct on this important anatomical distinction between the relation the nervous expansion of the auditory nerve bears to the bone in the cochlea, as compared with what obtains in the semi-circular canals. The cochlea exhibits its bony parietes and septa in a state of intimate adhesion with the auditory nerve, whereas in the semi-circular canals nature has studiously separated the bony parietes from the contained tubular and sacculated expansion of the auditory nerve, either by means of a liquid secretion, or of a loose cellular membrane.

This manifest anatomical difference is well described by Scarpa, and is tersely but clearly given by Quain, who speaking of the cochlea says, "that the nervous filaments pierced by minute power to the scalæ, upon the entire surface of which they form a delicate expansion, *supported by the osseous and the membranous part of the septum.*"

Whereas all concur in stating that nothing like this direct contact and intimate adhesion between the osseous parietes and the enveloped nervous expansion, exists in the vestibule and the semicircular canals.

"The preceding observations," concludes Weber, "render it sufficiently apparent, that the membranaceous vestibule and membranaceous semicircular canals, differ in structure from the osseous vestibule and the osseous semicircular canals, in such a manner, that vibrations travel through the bones of the cranium with more facility to the cochlea and round sac, than they do to the membranaceous portions of the internal ear.

"The next question is, whether the sonorous vibrations that are derived from the external air and proceed through the meatus externus to the ear, are propagated with greater facility and strength to the nerve of the vestibule than to the nerve of the cochlea. In the first place, it is evident that the vestibule and the semicircular canals directly connected with it, must receive a stronger impulse from the aerial vibrations than the cochlea, for the former have a solid communication by means of the chain of ossicula with the membrana tympani, whose vibrations are consequently imparted at once to the membrane of the fenestra ovalis, whereas no such direct communication exists between the membrana tympani, and the fenestra rotunda. The membranes of the semicircular canals and the vestibule too, seem to be more easily set in motion by the fluid which invests and surrounds them, than is the case with the lamina spiralis of the cochlea. The latter is, nevertheless, furnished with provisions calculated to enable it to receive impulses from the external air also; for the fenestra rotunda, being closed by a membrane, must impart the vibrations that occur in the cavity of the tympanum, while another opening, by forming a communication with the vestibule itself, must place the cochlea in connexion with the latter, in such a manner that the vibrations which the chain of ossicula have imparted to the fluid of the vestibule, must through that fluid be at once propagated to the cochlea

"I have next to prove the assertion which I made at the commencement, viz. that nature has so constructed the ear in man, and various other animals, as to make a provision for the reception of the sonorous vibrations in a two-fold manner, by means of the acoustic nerve, which is so disposed in the internal ear as to present, for receiving these vibrations, a double surface of contact; the one consisting of a soft, pultaceous, nervous expansion, surrounded by a fluid, the other formed of a reticulated net-work of extremely

minute, but firm, nervous, ramifications. The former receive the sonorous vibrations through the medium of a fluid, the latter of a solid. In man the cochlea is the portion of the organ where the firmer extremities of the acoustic nerve are disposed for this purpose. Fishes and amphibious animals have no cochlea, but they have an arrangement which answers the same purpose; for in osseous fishes, we find that the membranaceous labyrinth contains three white little stony bodies, of great specific gravity, very hard, and much resembling vitrified argillaceous clay; two of these are enclosed with a sac full of fluid, adjoining the vestibule, and lodged in the basilar portion of the occipital bone; these lapilli are furnished with fine nervous filaments, fastened to rough depressions and elevations on their surface; thus vibrations are imparted from the lapilli to the acoustic nerves; the third lapillus is situated in the anterior portion of the membranaceous vestibule, and receives no nervous filaments. It has, however, another mode of communicating with the acoustic nerve, for it lies against a very large branch of that nerve where it expands on the membrane of the vestibule, and thus compresses this nerve between itself and the cranium. In cartilaginous fishes and in amphibious animals, in the place of these lapilli, we find certain little bodies consisting either of concrete gelatine or of a chalky pultaceous matter, and to which both Scarpa and myself have traced ramifications of the acoustic nerve; neither is the *lamina spiralis* of the cochlea in man formed without reason of two structures, an osseous, and a cartilagino-coriaceous tissue; for as the same nervous branches pass from the osseous to the cartilaginous portion of the *lamina spiralis*, it is natural to conclude that receiving sonorous impulses from both, these impulses are communicated by means of a different mechanism in the two cases. This idea is confirmed by an examination of the calcareous fragments found in the labyrinth of Rays, and which are composed of two portions; one pellucid and resembling a tremulous jelly, the other white and chalky, seem to discharge the same function in these animals, that the cochlea does in man. These fragments are so divided, that their gelatinous and chalky portions lie in contact by means of extensive smooth surfaces. Nature has so disposed the extremities of the auditory nerves in the semicircular canals of all animals, that these extremities receive the sonorous impulses directly from a fluid. This is very plain, even in man, where the dilatations termed ampullæ, and which correspond with a similar enlargement of the nerve, are both filled with and surrounded by a fluid. In fishes it is still more evident, for in Rays, a nervous filament can be traced to each ampulla, which it enters and forms within a crescent-like septum.

“Scarpa was the first to discover the remarkable difference in consistence and texture which exists between that portion of the auditory nerve which supplies the cochlea, and that which is distributed to the vestibule and semicircular canals. The latter surrounded on all sides by a fluid, are soft and pulpy, evidently fitted to receive impulses from a fluid; the former, on the contrary, firm

and ramified, are adapted to the reception of vibrations from a solid."

Weber next proceeds to detail the circumstances connected with the proper discharge of the function of hearing, and which render this double provision necessary. On this subject I shall not copy his observations, for if the fact of this double provision be conceded, its utility can be at once proved both *a priori* and by experiments; one, Weber relates, which is striking. Take a musical sounding fork while vibrating, and place the handle between the teeth, keeping the lips closed on it, while both ears are stopped with the fingers; we now hear its tones more loudly even than when the ears were open: open now one ear, and immediately the sound heard by that ear appears to be diminished. The same thing happens when the instrument is applied to the left temple, the right ear being closed with the finger; we now hear the tones more distinctly with the right ear than with the left, although the former is at so much greater a distance from the instrument; now, here, the sonorous vibrations must be propagated through the bones of the head.

Weber is at a loss how to explain the remarkable fact that closing the ear makes the sound received from a vibrating body, through the bones of the cranium, appear louder than before. This fact, he observes, appears paradoxical, for we cannot understand why one source of sound should seem to produce a greater effect, the moment another co-operating source of sound which arrives by the route of the meatus externus is cut off by thus stopping that opening with the finger. There is in truth a great difficulty here; three explanations suggest themselves: the apparent increase of loudness in the sound heard through the bones of the head may arise from the cessation of all other sounds that usually arrive by the meatus externus, and which in the natural state of the organ serve to dilute and drown the sounds derived through the other channel, in the same way that light admitted into a room from several different windows seems to diminish the intensity of that which enters from any one window. This appears to me a more tenable explanation than either of those suggested by Weber, who thinks that the closure of the ear may act mechanically on the internal organ, so as to dispose it to propagate sounds through the bones with more readiness. How this could be effected I cannot even guess, nor do I place much reliance on his hypothesis, that the sound coming through the external meatus produces vibrations in the internal ear, which to a certain extent obliterate those that arrive through the bones.

Weber mentions a curious fact with regard to sounds. Shut one ear, and then whistle the note *d* marked with four lines $\equiv$
d, or any still baser note, and you will hear it more distinctly with the closed than with the open ear; whereas if the note be higher, we hear it more distinctly with the open ear!

Many facts prove the facility with which sounds are conveyed through the solid textures of the body to the internal ear. It is by this channel that we hear the slightest touch applied to the hair, the slightest scratching of any portion of the integuments of the head or face. Close the lips and bring the teeth into contact ever so gently, and it is scarcely possible to avoid producing a noise which is heard distinctly. This latter experiment is still more striking when the external ears are closed. Experiments and observations are still wanting to ascertain how far the same channel may not be the means of conveying vibrations imparted to even distant parts of the skeleton; from some experiments I have made, it seems extremely probable, that when we stand erect upon, or when we place a hand on a solid, its vibrations may be conveyed in each case to the internal ear. The different joints which intervene do not present any interruption of the continuous solidity necessary for this conveyance, inasmuch as the articulating surfaces are always in close contact. Our solids, therefore, may be regarded as so many natural stethoscopes interposed between the cochlea and the media in contact with all parts of our bodies. It may be well to bring this subject to a close by giving Weber's concluding sentence:—

“ Si igitur ex his, quae nunc diductius exposita sunt, verisimile est, vocem propriam hominis et mammalium ab ossibus cranii excipi, ad cochleam potissimum propagari et hac de causa facilius ab aliis sonis extra corpus excitatis et ad meatum auditorium propagatis discerni; utilitas, ex hac auris fabrica ad nos, maxime vero ad infantes et bestias redundans, non parvi aestimanda est. Multum enim haud dubie hac re proficimus, quod iam in prima infantia diverso modo id, quod a nobis efficitur, ab effectibus causarum extra nos positarum discernere discimus.”

Birth Days of celebrated Living Authors.

A CHANGE in the fashion of almanacks indicates unequivocally a change in the spirit of the times, for each generation being wont to attach to particular days the memory either of occurrences they consider peculiarly important, or of personages they revere, we can detect in their calendars the most prominent

characteristics of the “*homme moyen*” the average man of each epoch. It would form a curious subject of inquiry to compare the almanacks of the dark ages with those which followed the restoration of letters, and with those which have ushered in each succeeding year of the last half century; but we have not time or space for this comparison, and must consequently content ourselves with observing, that we hail it as a good sign of the times to find the birth days of philosophers and learned men, noted in some of the Continental calendars. Formerly those annuals exhibited no record of science, afforded no memorial of the greatest benefactors of mankind, the mighty dead who dedicated their lives to enlarging the boundaries of knowledge, and whose genius revealed the secrets, the most precious secrets of nature; and, indeed, in British almanacks, even now, not one day out of the three hundred and sixty-five is so consecrated, not one is marked as having given birth to Newton, Harvey, Watt, Jenner, or Davy! It is not our intention to enter further on this subject, and we shall therefore proceed to present our readers with a list of the birth days of living German authors, selected from amongst those most distinguished in the medical sciences, with an addition of a few who enjoy an European fame as naturalists or philosophers. The life of man is but three score and ten, and at the present moment its whole length when completed lies equally divided between this century and the last, so that the years of the man of seventy fall as it were into two portions nearly equipoised and balanced on the year 1800 as a centre. At present the literary workers born in the *last* century are the most productive, but their numbers already exhibit a notable diminution, and in a few years they must retire to make way for more buoyant and youthful energies.

Blumenbach,	Gottingen,	May 11,	1752.
Tiedemann,	Heidelberg,	August 23,	1781.
Trommsdorf,	Erfurt,	April 2,	1770.
Liebig,	Giessen,	May 14,	1803.
Rose,	Berlin,	August 6,	1796.
Mitscherlich,	———	January 17,	1794.
Humboldt,	———	September 14,	1769.
Döbereiner,	Jena,	December 13,	1780.
Olbers,	Bremen,	October 11,	1758.
Oerstedt,	Copenhagen,	August 14,	1777.
Ehrenberg,	Berlin,	April 19,	1796.
Rust,	———	—— 5,	1775.
Schweigger,	Halle,	—— 8,	1799.
Callisen,	Copenhagen,	—— 8,	1786.
Seiler,	Dresden,	—— 11,	1778.
Himly,	Gottingen,	April 30,	1762.

Gräfe,	Berlin,	March 8,	1787.
Carus,	Dresden,	June 3,	1789.
Burdach,	Königsberg,	— 12,	1796.
Kunth,	Berlin,	— 18,	1788.
Weber,	Leipzig,	— 24,	1796.
Schultz,	Berlin,	July 8,	1796.
Valentin,	Breslau,	— 8,	1810.
Nägele,	Heidelberg,	— 12,	1778.
Müller,	Berlin,	— 14,	1801.
Hufeland,	—	August 12,	1762.
D'Alton,	Halle,	July 17,	1803.
Barkow,	Breslau,	August 4,	1790.
Rathke,	Königsberg,	— 25,	1793.
Horn,	Berlin,	— 24,	1774.
Oppenheim,	Hamburg,	October 5,	1799.
Purkinje,	Breslau,	— 17,	1790.
Brandes,	Salzuffen,	— 18,	1795.
Ideler,	Berlin,	October 25,	1787.
Clarus,	Leipzig,	November 5,	1774.
Langenbeck,	Göttingen,	December 8,	1776.
Schlemm,	Berlin,	— 11,	1795.
Schönlein,	Vienna,	April 2,	1790.
Hecker,	Berlin,	January 5,	1795.
Froriep,	—	February 21,	1804.

ROBERT J. GRAVES.

An Essay on the Laryngismus Stridulus, or Croup-like Inspiration of Infants; to which are appended Illustrations of the general Principles of the Pathology of Nerves, and of the Functions and Diseases of the Par Vagus and its principal Branches. By HUGH LEY, M.D. &c. &c. London. Churchill, pp. 480.

THE importance of laryngeal disease is so forcibly impressed on us every hour, that we are disposed to rejoice at each attempt to elucidate so difficult a subject, and we receive with kindness every novel opinion, even although it may not altogether coincide with our own previously formed ideas. The time is yet within our own recollection when these diseases were but imperfectly understood: when but few attempts had been made to classify and arrange them under their respective heads, and when consequently an indecision and uncertainty of practice prevailed with respect to them that were but few degrees removed from absolute empiricism. If it is not so at present, if improvements in diagnosis and in practice have been introduced that could scarcely have been calculated on even by the most sanguine; if numbers of lives are now saved which heretofore

would most probably have been lost ; it is scarcely so much to be attributed to an increase of interest being excited towards the subject, as to the energy with which pathological science has in latter days been cultivated. Brought into operation in the investigation of almost every disease, it has been applied to those of the larynx and trachea with singular success, and it is a matter of no small pride and gratification to us that some of the practitioners on this side of the channel, if they have not led the way, have certainly not been very far behind in this particular branch of our science.

The work before us professes to treat of but one affection of the larynx ; one always formidable and too often fatal, and one which, although mentioned by a host of writers, from the father of medicine downwards, has been but imperfectly described, and its pathology almost, if not entirely overlooked. It is known to the medical public by a variety of names ; the asthma of infants of Millar ; the cerebral croup of Pretty ; the spasm of the glottis of Dr. Marsh ; the spasmodic croup of other writers ; and the laryngismus stridulus of Mason Good ; the latter appellation is that which our author prefers to retain. Indeed in no respect can indefatigable diligence and patient research be better exemplified than in the summary of opinions contained in the introduction to the work before us, wherein the names of no less than forty-one authors are mentioned as recognising the existence of this as a peculiar malady, "having in some respects a resemblance to croup, although essentially distinct, and requiring a total difference of treatment."

Our author has divided the first part of his treatise (that which refers directly to the laryngismus stridulus) into six chapters. In the first the history of the disease is discussed, the second treats of its causes, the third of its pathology, the fourth of the diagnosis, the fifth of the prognosis, and the sixth and last of its treatment. It is not, however, our intention to follow him through these different sections, being satisfied that by examining the second and third, every useful purpose will be answered. When the producing causes and the pathology of any disease are well understood, it will not be very difficult to distinguish it from all others, or to apply the required remedies, provided we possess them. Few medical practitioners of the present day require to be informed that young children are occasionally attacked by a peculiar difficulty of breathing, occurring for the most part suddenly, accompanied by a crowing sound, and oftentimes with a suspension of respiration for several seconds. That this difficulty of respiration varies in intensity from a single "crow" to a more prolonged paroxysm, threatening suffocation, and terminates when in recovery by a long deep drawn

inspiration with a peculiar stridulous noise—when in death by such convulsive struggles as might lead, and indeed have led to a belief that the cerebrum was engaged, “pallid and exhausted, the child falls lifeless upon the nurse’s arm, and is then generally said to have died in a fit.” In these cases there is no cough; no raucal sound of voice; no continued stridulous breathing, except we consider as such, an occasional mucous rattle in the windpipe, heard only while the infant sleeps: there is no fever, and on examination after death no trace of inflammation, nor indeed any deviation from the ordinary healthy appearance of the organ can be discovered. Under these circumstances, pathologists had no method of explaining the phenomena but by spasm, an irregular and involuntary contraction of the muscles of the larynx closing up the rima glottidis to a greater or less extent, and in proportion to such closure interfering with and obstructing respiration.

So far, then, we suppose all practitioners to be agreed, because these are facts of daily observation and too obvious to require proof: the next and the most difficult step is to discover the cause of this spasm. Some have supposed it to have an intimate connexion with an hydrocephalic tendency, because it has been sometimes seen in children with large heads and sluggish dispositions, and because signs of cerebral congestion have been discovered after death: but we have seen the disease prove fatal to the liveliest and apparently most healthy children, and the congestion may just as well be the consequence as the cause of the closure of the glottis. Others, again, have referred it to the general constitutional irritation that proceeds from painful dentition, and doubtless, cases have occurred in which the “crowing respiration” was relieved by successive scarifications of the gums according as each tooth became prominent underneath: but this, although teaching an important practical lesson, leaves the pathological connexion between the facts in as much obscurity as ever. According to others, there is a constitutional tendency to this disease in some children, a fact which it must be conceded has been painfully exemplified in more families than one; but this hereditary disposition to disease, although abundantly obvious, is too imperfectly understood to be discussed with any thing approaching to pathological accuracy. Lastly, improper or unwholesome food, indifferent clothing, a close and tainted atmosphere, and exposure to vicissitudes of climate, have been regarded as influential exciting causes, and change of circumstances in these respects has often produced an almost magical amendment in the condition of our little patients; but still are we at a loss to discover the immediate “modus operandi” of these pernicious influences, or why

they should be determined to the larynx in the form of an involuntary spastic contraction of its muscles.

We turn now to the explanation of these phenomena as offered by Doctor Ley. He supposes that the muscles that open or dilate the rima glottidis, and those that close it, are supplied by different nerves; the former being dependent on the recurrent nerves of the pneumo-gastric, the latter on the superior laryngeal branches derived from the same trunk; that these muscles, whilst in health and equally supplied with their own proper nervous influence, fairly antagonize each other, and that the one can no more spastically close "the chink" than the other can enlarge it; but if from any cause one order of these nerves comes to be deteriorated in energy, its opposing influence being proportionally impaired, its opposite will come into undisturbed operation, and the muscles it supplies act with an apparent increase of power. Now, as the superior laryngeal nerves are so situate as scarcely to be subjected to any unnatural pressure; and as the recurrent nerves in their very long course, are liable to be compressed, by the bronchial glands in the thorax if enlarged, by the lymphatic glands in the neck, or by any accidental tumours, it follows that the energy of the muscles supplied by them is very frequently impaired, and therefore the constrictors of the glottis are allowed to act without opposition and spastically shut up the glottis. It is true Doctor Ley does not deny the possibility of spasm in the sense in which we have generally understood the term, but lest by any accident we should misinterpret his meaning we shall quote his own words:—

"Although I have adduced strong reasons for believing that the closing, total or partial, of the glottis in this disease, is commonly the result of a paralytic affection of the recurrent nerves, yet it is quite *within the bounds of possibility* that a spasmodic affection of the closing muscles may also produce a similar constriction. To deny this, were to commit the same error as that which I have ventured to denounce in others, of advocating exclusive opinions with regard to the nature of this disease. We know that irritation of the extremities of the superior laryngeal nerves, as from an extraneous substance sticking within the chink of the glottis, and that active inflammation of the same surface, as in acute laryngitis, will occasion a similar crowing inspiration; and this probably from convulsive closing of the glottis."

"It is possible then that the laryngismus stridulus may occasionally be a convulsive malady; but the general history of the predisposing and occasional causes of the disease, the exciting causes of the paroxysms and the appearances upon dissection, all point to the conclusions that such is rarely the case, and that an

immense numerical majority of instances of this complaint, are the result of a paralytic affection of the recurrent laryngeal nerves, produced by the compression of enlarged and indurated glands in their course."

Now, the first observation that occurs to us, with reference to the above pathological explanation, is, that it rests on a physiological hypothesis which is far from having been satisfactorily proved; namely, that the muscles which close, and the muscles which open the rima, are supplied by nerves, not only of distinct and separate, but opposing influences. And this hypothesis is supported first on the authority of Le Gallois, who observed, after a division of, and consequent annihilation of nervous energy in the recurrences, that "every effort to inspire made by the animal only closed the glottis more effectually; and this on account of the pressure of the exterior column of air, which still further increased the approximation of the ligaments of the glottis, in consequence of their oblique position and of the cul de sac, which they formed on their anterior surfaces; on the contrary, expiration was easy." And, secondly, on the experiments of Majendie, "by which it has been *definitively determined* that the destruction by their division of nervous energy in the superior laryngeal nerves, causes the glottis to remain open." With respect to the definite validity of these experiments we can offer no observation, for having constantly experienced the insufficiency of experiment on the lower animals to explain disease in the human subject, we have not repeated them; but it may not be inapplicable to remark, that if the results of these experiments be true, they must depend on something else than the isolated distribution of the superior or inferior laryngeal nerves to the orders of muscles above referred to, for fibrils of each may be traced by dissection into both those that open, and those that close the rima.

It is, however, but justice to our author to state that he adduces a vast number of cases of this disease in which dissection, after death, shewed scrofulous enlarged glands pressing on the recurrent nerves; these were of various sizes, some large, others so small that their paralyzing influence might well be doubted, but all of them calculated, by their situation, to make a direct and positive pressure on the par vagum or its recurrent. He seems to think also, that similar appearances would have been observed by others, if *post mortem* examinations were conducted with more accuracy, and if the attention of practitioners had been directed to this point. "They have, perhaps, partaken of the common lot of things that are obvious and lie upon the surface; their very frequency and notoriety have prevented them from

being particularly noticed." We take this acknowledged frequency of enlarged glands at our author's hands. How can it be so constant, so general and laryngismus stridulus not by far more frequent than it is ?

The causes of this disease have been divided by Mr. Ley, into the predisposing, the exciting, and the causes of the paroxysm ; these embrace a great variety of supposed influences, many of which it would be quite superfluous to enumerate. We may, however, take a simpler view of the subject, by supposing that if the direct and immediate cause of the disease, be a tumour pressing on and paralysing the recurrent nerve, any thing and every thing that can lead to the development of such tumour, must rank either as a predisposing or exciting cause ; that which determines the sudden occurrence of a paroxysm, considering that the tumour is fixed and permanent, and always present, it is not so easy to understand. Of the first class of causes obviously the most influential, is scrofula, and to it may be referred most, if not all, of those that come within the same class or section, namely, age, constitution, climate and season, and diet. "It may doubted," says Dr. Ley, "whether this proneness does not generally resolve itself into a mere disposition to scrofula ;" a remark in which he is corroborated by Dr. Marsh, who states that "all the cases of this disease which he had witnessed, had occurred in children, either themselves exhibiting marks of the strumous diathesis, or sprung from scrofulous parents." We are firmly convinced that the majority of children affected by spasm of the glottis will answer this description, because in this climate the taint of struma is so universally prevalent ; and for the same reason we are convinced also that a vast number might be found with enlarged glands in the neighbourhood of, and compressing the recurrent nerves ; but we cannot agree that these have been satisfactorily made out to be the causes of the spasmodic disease in question. Let us look to one of the remarkable facts confessedly connected with it.

Children only are susceptible of laryngismus stridulus, adults never being thus attacked.

The reasons assigned by Dr. Ley for this exemption are, at least, extremely ingenious. He states that the glands of adults do not so frequently become enlarged, and that even when they do, the recurrences are more effectually protected by the diameter of the trachea being wider ; and, also, that the glottis of the adult is so much larger, that it is not so easily closed in consequence of want of power in the opening muscles. "Hence then it is, that, in early life a cause, which by its influence, may partially close this small chink, will occasion a sonorous inspira-

tion, but as it grows larger, although great distress in breathing may be produced by similar causes, if in an aggravated degree, the sound is very nearly obscured ; and hence also, it is, that in women, whose glottis only increases in the proportion of two-sevenths of its infantile size, the symptom, or something bearing a very close resemblance to it occasionally occurs, as has been observed in hysteria." Now, although we concede that we have never seen laryngismus stridulus, nor any thing approaching to it in the adult, we yet cannot coincide either with the facts or the reasoning that is here offered in explanation of them. Doubtless, purely scrofulous enlargements of glands do not often appear in the necks of grown persons ; but independent of them, there are other tumours occasionally that would answer the purpose quite as well. We have seen hard and unyielding and malignant tumours of the neck, that must have compressed both the par vagum and its recurrent, and at this moment there is before us the dissection of a case of large aneurism of the arch of the aorta, that seemed to have flattened the recurrent nerve, yet spasmodic difficulty of breathing formed no part of the symptoms of these cases. It may be said, however, that the increased size of the laryngeal opening will explain this fact. We think not so. When the larynx increases in size, its muscles increase with it ; their development to be perfect must be proportionate, and they are as fully capable of shutting up the rima at the age of forty as of four. This we see exemplified in many cases. The man who descends into a foul well-hole, or an uncleansed brewer's vat, suffers a closure of the rima so complete, that not a particle of air can pass, and he drops down and dies at once. We have had a patient in whom there was incipient disease of the cricoid cartilage, who died suddenly of spasm in the glottis. These, and we could adduce many other illustrations, prove that the development of the larynx in man, is not sufficient to protect him from the evil consequences of spasm.

All persons who have written on laryngeal disease, have omitted to put forward with sufficient force, the fact that it is the act of inspiration that is attended with most difficulty, and produces most distress. Every muscle that can be pressed into operation is employed to dilate the chest ; the larynx and trachea are raised as high as possible in the neck in order to meet the column of air, and facilitate its entrance ; every exertion is made to inflate the lungs, and so much energy displayed as to show that the process of inspiration, at least, is not purely passive. Thus in difficult breathing, inhalation is long drawn and attended with a stridulous sound, expiration comparatively easy, and unaccompanied by any characteristic noise ; and this is true

of all forms of laryngeal obstruction, whether produced by organic disease or by disordered function. In these terrible efforts we are satisfied that the muscles which dilate the rima are actively and powerfully engaged, and moreover, that if they were paralyzed, the struggle for existence in the majority of these cases could not be long protracted. Now, how does it come to pass that the adult, in whom this paralysis does not and has never been supposed to exist, suffers exactly the same difficulty of inspiration as the child ; and although not immediately subject to the laryngismus stridulus, is nevertheless liable to be taken off by obstruction of the larynx so sudden, so unexpected, and so violent, as to be only attributable to spasm ?

It will be seen that we have not been able to coincide with the pathological view taken of this disease by Dr. Ley ; and yet are we not able to substitute any other theory in its place. It is easier to take an hypothesis to pieces, than to build up another in its stead ; and it has really been with some doubt and hesitation, that we have put forward the foregoing remarks. There are many points connected with the pathology of the larynx still open for investigation, and amongst them there is not one more important or more difficult than that which is the immediate object of this article. Dissection throws but little light upon the subject, for the parts are found free from the marks of lesion or disease ; and there seems to be little analogy between the spasm that occurs in the adult and the child, because in the former there is always some obvious cause in the existence of some irritation or inflammation in the organ, whereas in the latter it occurs idiopathically, or at least, without our being able satisfactorily to demonstrate its origin. Under these circumstances we receive Dr. Ley's essay, as a most valuable addition to medical literature. The portion of it devoted to the history of the disease exhibits such a depth and extent of research, as leaves little for any future investigation to accomplish. The causes of the disease are detailed with all the accuracy of an experienced observer, and if we have not fully agreed with the author's pathological views, it is because we believe them to be founded on experiments that are inconclusive and may be fallacious ; and, moreover, are they not borne out by our own practical experience and observation. In the mean time Dr. Ley has a full right to the maintenance of a theory which he believes to be corroborated and established by numerous dissections, at least until one is offered more susceptible of proof ; a task to which we profess ourselves to be totally incompetent.

We have protracted this notice of the essay on laryngismus (we fear) to a most unreasonable length, but we cannot dismiss the subject without a few words on the treatment of the disease.

As might be expected, Dr. Ley rejects as empirical the various remedies that are found in the ordinary sources of information on infantile diseases; whether designed for the relief of convulsive spasm, cerebral excitement, painful dentition, or a disposition to scrofula, although he acknowledges that the latter "are neither useless nor even ambiguous remedies;" and he substitutes a line of treatment which is most conducive to the removal of that pressure on the recurrent nerve which he supposes to be the immediate cause of danger—

"In the treatment, therefore, of this complaint, it should be a primary object to ascertain if these glands are enlarged, and tracing, if possible, the producing cause of such enlargement, to adapt our remedial measures to that cause. Nor must we simply, from difficulty in detecting its existence during life, conclude that there is in reality no such pathological condition. Tumid glands may, as has been already shewn, escape detection, even when seated in the neck; whilst if situated within the thorax they are always beyond the reach of our external senses. But, as similar diseased conditions commonly produce the same or similar results, we infer from the occurrence of the latter, the existence of the former.

"But the pathological condition of these glands only establishes the liability to the attacks; the paroxysms generally require for their production, the intermediate agency of some other event, which is to be considered as their exciting cause. It is therefore a second, but far from secondary point in the treatment of the laryngismus stridulus to distinguish, and to prevent or counteract the operation of these causes of the paroxysms.

"As, moreover, the attack has been in some instances suddenly fatal by producing suffocation, the patient can never be considered as quite free from hazard, and it is necessary, therefore, so to treat each paroxysm as to shorten as much as possible its duration. These are the three leading principles of treatment, and under one or other of these indications of cure, as they are technically called, all our remedies may be arranged and considered."

We, by no means seek to inquire very minutely as to the principles on which the above indications are founded, seeing that they embrace the measures usually resorted to for the removal of a scrofulous diathesis, and involve in their details those attentions to diet, temperature, clothing, and pure air, that have by all practitioners, no matter on what theory founded, been recommended in this disease and employed with the greatest success. Into these details it is impossible for us to enter, or even to afford our readers a superficial sketch of them. It would be injurious to our author thus to mutilate the very best of his chapters, and it would be unjust to any professional reader to deprive him, even in the slightest degree, of the pleasure and

advantage he must derive from a perusal of this part of Doctor Ley's work. There is in it an attention to the smallest minutiae in the management of the infant that could only be attained by long experience and patient observation, and we venture to predict that any person carefully studying the pages now open before us, will reap an ample reward in the stores of information he may thence derive; and in the facility it will afford him in the management of many other infantile diseases besides the laryngismus stridulus.

W. H. P.

De Phaenomeno generali et fundamentali Motus vibratorii continui, in membranis cum externis tum internis animalium plurimorum et superiorum et inferiorum ordinum obviis, Commentatio Physiologica. Scripserunt Prof. Dr. JOH. EV. PURKINJE et Dr. G. VALENTIN, Wratislavienses, Wratislaviae, 1835, pp. 96, 4to.

In the seventh volume of this Journal, p. 279, was given a translation of the article in Müller's Archiv. 1834, in which the singular discovery of the existence of continued vibratory movements produced by cilia in amphibia, birds, and *mammalia*, was communicated to the world. In the following year, Drs. Purkinje and Valentin published the work now before us, containing a curtailed account of their experiments and conclusions on this interesting subject, together with an elaborate historico-critical conspectus, occupying thirty pages of what had been written on the subject of vibratory motion up to this period. In this conspectus, by the way, occurs an amusing remark on Sir Everard Home's statement in the Philosophical Transactions for 1827, that Mr. Baner discovered that the rotation of the embryo of the muscle therein described was produced by a small worm that had got into the vesicle and was feeding on the embryo. The authors declare they are at a loss to know whether he is quizzing or dreaming. "*Anglus num jocet, an somniat, nescire nos fatemur.*" As it is not my intention, however, to give a complete analysis of the work, but merely a notice that may serve as a supplement to the translation of the original communication, I shall at once proceed to select such parts as I consider best suited to that purpose.

Vibratory motion in general is defined as follows: "When any part of an animal, immersed in water or some other fluid, produces currents by which minute particles are attracted and repelled, and this process continues independently of any voluntary act or motion of the animal, proper muscular contraction,

or circulation, we say that vibratory motion is present ; and this, as shall be proved at large, depends not only most frequently, but in every instance, on the action of slender cilia." The authors proceed to observe, that when the body in which the motion resides is fixed, the current will merely proceed along its surface ; but that when it is small and suspended freely in the fluid, it will move itself also, and this motion may be simply progressive, rotatory and progressive, circumvolvent, or fluctuating, according to circumstances.

The method of observation has been already sufficiently detailed in the former notice, but there are some directions about the light to be employed, which it may be as well to add. Bright day light is stated to be the best for beginners, but when the sky is cloudy, a lamp is preferable. Direct sunshine only confuses. The observer must guard against various sources of deception, such as the flow of blood from capillaries, the employment of water containing animalcules, and, above all, the optical margin produced by the inflexion of light.

With respect to the classes of animals in which the motion occurs, that of fishes appears to form the only exception, though even here the authors are inclined to agree with Sharpey, that it may be detected in the branchiæ of the embryos of the ray and shark. In *mammalia* birds, and *amphibia*, it is universal. The same perhaps may be said of the *mollusca*. If it has been observed in the remaining classes only partially ; this may be owing, not to its absence, but to the imperfection of the instruments employed, and want of sufficient skill in the observer.

As to the parts that have been found endowed with this remarkable property, they may be referred to four classes, according as they belong to the cutaneous, the alimentary, the respiratory, or the genital system. It has been observed in those of the first class, only in the *larvæ* of some of the *amphibia*, and in many of the *invertebrata*. Under the head of the alimentary system, the authors correct an assertion they had previously made, that the longitudinal fleshy elevation in the intestine of the river muscle is alone possessed of the property, as they have since discovered it to reside in the whole of the alimentary canal in this and similar animals. The respiratory and genital systems have been sufficiently discussed in the original memoir ; and it only remains to add that the internal surface of the air-cells in birds, most distinctly exhibits the phenomenon of vibratory motion. It is to be observed that all the organs above enumerated belong to the vegetative or organic functions.

The cilia that produces this phenomenon are slender, pellucid, colourless, sub-splendent threads of equal size, with their

base resting on the surface of the membrane, and their apex free. Their length varies considerably, namely, from 0.000075 to 0.000908 of a French inch, and in almost every instance they taper from the base to the apex. Their motion is generally infundibuliform, the base of each revolving, so as to form the apex of the hollow cone; this may occasionally pass into oscillation. In a few instances the whole of each cilium appeared to wave to and fro like the tail of a *spermatozoon*; and in one case, in the branchiæ of some of the species of *unio*, the upper third of each was inflected and reflected, the two lower thirds remaining stationary. This revolving motion of the cilia must depend either on the presence of irritable or muscular tissue in their bulbs, or, more probably, on certain straight, parallel, stiff fibres, united by delicate cellular tissue, which compose the surface of the vibrating membrane, as may readily be seen by examining a portion of it with the microtomic compressor. If a portion of the trachea of an ox be macerated in warm water for half an hour, these fibres will be detached, and found floating in great quantities in the fluid. It must be remarked, however, that a similar stratum of fibres has been observed in membranes that do not vibrate.

Concussion has a considerable influence in exciting or accelerating the motion of the cilia: light has none; heat, when greater than the natural heat of the animal, disturbs or stops it altogether, according to its degree, and the length of time it is allowed to act. Moderate degrees of electricity and galvanism have no physical effect on it.

The difference of the effects of chemical agents is very remarkable. Some that act powerfully on the nervous system, such as hydrocyanic acid, belladonna, opium, &c. have no influence on the ciliary motion; while decompounds and corrosives, such as acids and alkalies, put a stop to it immediately. Blood has the remarkable property of preserving it in *mammalia* and birds for so long as three days, and in *amphibia* for four: the blood should be well shaken to prevent coagulation, but even the serum has the same property. In the *invertebrata*, on the contrary, these fluids immediately destroy the ciliary motion. Milk and white of egg have rather a conservative influence. Inflammation checks or stops it, not only in the inflamed parts, but even in the whole organ affected. During the winter sleep of hibernating animals it continues undiminished.

It is mentioned in the *addenda* to the treatise, that, by availing themselves of the conservative properties of blood, the authors were at last enabled to discover cilia in the human subject also. Their previous attempts had failed from the length of time they were obliged to suffer to elapse after death, before commencing their observations. It only remains to add

that the ciliary motion appears at a very early period of the evolution of animal life, as it has been detected in the mucous membrane of the nostrils and trachea, in the embryos of mammalia of the length of only two inches, and in the minute *larvæ* of *amphibia* while yet in the ovum.

W. WEST.

Memoirs de l'Académie Royale de Médecine. Tome cinquième, 1836.

WITHOUT possessing the interest in a practical point of view which distinguished the last Number of these admirable Transactions, the present fasciculus contains many papers of importance. We are presented, in the first place, with a long and laboured *éloge* on the celebrated Chaussier, by M. Pariset, in which the labours of this eminent pathologist and physician are fully and justly displayed. We would hold him up to the imitation of the student as one of those who have not buried their talent in the earth, but have returned it with interest at their master's call. During a period of more than sixty years, he never remitted in his exertions to forward the science of medicine, and proved himself a worthy contemporary of Fourcroy, of Vauquelin, of Corvisart, and of Frank.

We have next the announcement of the prizes proposed by the Academy for the ensuing year. These shall be mentioned in the Scientific Intelligence. But we cannot help expressing our regret at the absence of the stimulus of prizes in our Irish school. Let us hope that the evil will be soon remedied. By a little exertion, funds for such purposes could soon be raised, and of the benefit which would result we entertain not a doubt.

A notice on the Plague of Moscow, by M. Girardin, and a long memoir on Inguino-interstitial Hernia, by M. Goyrand, are the next papers. These are succeeded by one of the most interesting medical fragments we have ever perused. It is entitled, *Le Bicetre en 1792*, and gives an account of the liberation of its wretched inmates from the chains with which ignorance rather than cruelty had loaded them. It is a bright page in the history of the illustrious Pinel, which records that in the midst of the horrors of the French Revolution, he emancipated the lunatic from a grievous tyranny. Strange result, that freedom, then drove the sane to madness, but restored the lunatic to reason and sobriety. What a lesson in the moral and physical history of man.

The effects of corrosive sublimate in preserving wood, is

ably handled by M. Kerandrin. We shall not here enter into any statement of his result, as the paper has been already analyzed in our distinguished contemporary the *London Medico-Chirurgical Review*. A short memoir by our Professor Macartney, forms the next communication. It contains his views as to the treatment of wounds and injuries, and their cure without the production of inflammation. His doctrine is, that so far from being necessary to the generation of tissues, inflammation, if it be moderate, retards, and if severe, prevents the process in question.

Assuming that man and the animals placed in the higher degrees possess the faculty of the reproduction of tissues, in nearly as great a degree as the beings much lower in the scale, Professor Macartney teaches that this reproduction is favoured most by preventing inflammation, and that this prevention is best effected by surrounding the part with agreeable sensations.

Without impugning the practical conclusions to which these views lead, we may observe that in considering the analogous organic powers of the higher and lower classes of animals, the division of the tissues into red and white must be borne in mind. Dr. Graves has shown how the function of regeneration of organs seems to belong chiefly to the white parts of the red-blooded classes, which are thus seen to be analogous with the whole organization of the lower tribes. In practice, we may observe, that there are cases of constant occurrence in which stimulation becomes necessary to insure a healing process. The water dressings are doubtless of great advantage in many cases, but anatomy has as yet failed to give us certain or universal principles in the practice of either surgery or medicine. Professor Macartney admits this, in requiring that for the success of his method, the constitutions of his patients should be free from all congenital and acquired taints, such as scrofula, syphilis, &c.

Dr. Macartney's treatment may be stated to consist in the removal of every thing capable of irritating, attention to position, so as to prevent the mechanical stasis of blood, and the keeping the part bathed in water, either warm or cold, according to the feelings of the patient, or in its vapour, for which Dr. Macartney has suggested a simple apparatus.

The last paper is from the pen of the celebrated Amussat, on traumatic hæmorrhages, in which, among other important subjects, he urges the necessity of establishing schools for experimental surgery, where operations on living animals should be made the ground-work of the surgery which is afterwards to deal with the human body.

W. S.

Traite Clinique des Maladies du Cœur, &c. &c. Par J. BOUILLAUD.

THIS is a most important work, and one that will rank with the writings of Corvisart, Testa, Bertin, and Hope. We shall enter into a more detailed analysis of its contents in our next number.

The United States' Medical and Surgical Journal. No. XV.
October 1835.

ONE number of this periodical has reached us, and we hasten to notice it. It is brought out in an unassuming form, but contains a quantity of important and curious matter.

The number in question, which is that of October 1835, has eight original articles, besides reviews and scientific intelligence. The following are the titles of the original communications: On *Ozæna*, by Dr. Heron; on the Co-existence of Diseases, by Dr. Watson; on the Use of the *Datura Stramonium*, in *Sarcocele*, by Dr. Everitt; a case of Adhesion of the Placenta, by Dr. Swett; Report of Dispensary and Private Practice, by Dr. Watson; on the Use of the *Crusta Genu Equinæ* in *Epilepsy*, by Dr. Mettauer; the Pathology of *Phlegmasia Dolens*, by Dr. Jennings; and on the Management of Sore Legs, by Dr. Miller.

Of these papers, the most important are those of Dr. Watson, on the Co-existence of Diseases, and that of Dr. Mettauer. In the first of these essays, the author brings forward a great number of instances, from the experience of others, as well as his own, to show that the Hunterian dogma on this subject is really unfounded,—a dogma so commonly taught in the schools, and so completely refuted by its author.* The following are the combinations of specific diseases noticed by Dr. Watson:

Variola and rubeola; rubeola and vaccinia; vaccinia and scarlatina; scarlatina and rubeola; scarlatina and variola; variola and lepra; variola and psoriasis; variola and febris intermittens; variola and syphilis; vaccinia and syphilis; syphilis and febris intermittens.

Besides these, he mentions other groups of specific diseases referred to by authors, but of which no definite cases have been quoted; such as variola and syphilis; rubeola and gonorrhœa; rubeola and pertussis; rubeola, pertussis and variola; variola

* See Hunter on the Venereal, p. 3. The passage is an admirable example of the mystery of surgery.

and varicella; variola and parotiditis; hydrophobia and gonorrhœa; syphilis and dysentery.

"I do not wish (says the author) to maintain that an individual affected with one disease, is so susceptible to another, as he would have been prior to the commencement of the first, allowing both to be specific, and to involve, to a greater or less degree, the general system. But is this owing, as Hunter and his disciples state, to the *incompatibility* of different morbid actions? I hold that it is not; for, that such actions are not incompatible, has been (at least if we can place reliance in the words of men who are thought sufficiently able to judge) clearly and unequivocally demonstrated.

"How then are we to account for the fact, that these complications of specific disorders occur so rarely as to have allowed the suspicion that they could never occur at all? I am not prepared to answer this question definitely; but we may be allowed to suppose, that when the system is affected with one disease, either general or local, the consequence of morbid poison or of healthy inflammation, if sufficiently severe to involve the general health, the sympathies of relation are excited in a peculiar train, and are consequently not so likely to be directed into a different or opposite course, by the supervention of any cause that would, under other circumstances, give rise to another disorder. But although these complications are rare, we have nevertheless seen that they may now and then occur; and the fact of their occurrence might here lead us into several serious considerations."

We regret that our limits will not permit us to enter at greater length into the contents of this Journal, particularly as another important paper by Dr. Watson occurs. In the department for Scientific Intelligence, in our present Number, we shall give the results of Dr. Mettauer's experience of the new remedy for epilepsy; and close this notice, by congratulating the profession of New York on the appearance of their Journal, and expressing our best wishes for its ultimate success.

W. S.

New German Journal.

DIEFFENBACH, of Berlin, assisted by Fricke and Oppenheim, of Hamburg, has published the two first numbers of a new medical journal, entitled, *Zeitschrift für die gesammte Medicin mit besonderer Rücksicht auf Hospitalpraxis und ausländische Literatur*. This journal appears monthly, and under the auspices of Messieurs Perthes and Besser, booksellers in Hamburg. We have received the numbers for January and February, have read with much interest the original notices and abstracts from German and foreign works, and can strongly recommend this to such of our readers as wish to get a good German journal.