II.—*A Course of Lectures on the Physiology and Pathology of the Central Nervous System.* By C. E. BROWN-SÉQUARD, M.D., F.R.S., &c., Philadelphia. Collins, 1860.

THIS course of lectures, delivered by Dr. Brown-Séguard before the Royal College of Surgeons of England in 1858, attracted an unusual amount of attention, and earned for the distinguished lecturer a wider fame and a more lasting reputation than he could otherwise have hoped for. The reason of this we had occasion to point out in the last number of the *Glasgow Medical Journal*, in the course of a review of a work by the same author, "On Paralysis of the Inferior Extremities," by calling attention to the manner in which Dr. Brown-Séguard had given the results of his observations through such a variety of channels. But no sooner had he gathered together his forces, and concentrated his efforts upon the course of lectures above alluded to, before so critical an audience, than his reputation as a physiologist of the highest eminence was at once established; whereas a full knowledge of his labours had been previously confined to the comparatively few who had watched his progress for many years.

We explained already in our last number how it was that we had so long left this work unnoticed; and even now we cannot but think that the time for a full review of the book on its merits has almost passed away. We should regret, however, that this idea should prevail to the extent of altogether excluding the subject from our pages, inasmuch as we cannot but believe that in this work future generations of physiologists will find some of those footprints which have, since the beginning of the century, marked the brilliant, almost triumphal progress of the Science of Life. True it is that the time has gone by for anything like elaborate review of so elaborate a work, familiar as it must now be to many of our readers; but we shall hope that in briefly noticing what science especially owes to Dr. Brown-Séguard, we may at least supply what would otherwise have been a gap or omission in the pages of our Review. We have more than once heard it said that most of the doctrines of our author were mere *rechaufés* of those of Magendie. Let us attempt to show that, so far from this being the case, originality, not only of research, but of discovery and demonstration, is everywhere to be seen in his writings.

"These lectures," says Dr. Brown-Séguard, "contain the result of the work of almost all my life, since I began to study medicine." For twenty years he has laboured to elucidate points which had eluded the grasp of many master minds, and in more than one instance his results have been a brilliant suc-

cess. It would be absurd to deny the probability, nay even the certainty, that his conclusions have in some instances been erroneous, however fairly they may apparently be deducible from experimental facts or other premisses; but surely no impartial critic would detract from his merits on grounds such as these. The whole history of the rise and progress of the physiology of the nervous system is one great example of this: here and there facts sparkling brightly amid the conglomerate of false doctrines, slipshod theories, and shaky facts. Here we find one observer, who, on the strength of one great discovery, carries into the public belief along with this many crude ideas and imperfectly digested theories, which are, when their time comes, demolished by subsequent observers. But should the author be blamed for this, more than the public who accept his conclusions without due evidence? Certainly not; the physiological world being satisfied, further investigation is thereby discouraged. Let all new theories be thoroughly investigated. Searchingly, but fairly, pry into all corners; shake it up, turn it outside in; and if you discover a flaw, by all means put your finger upon it, proudly if you will—although we would advise humility in these matters—and point out the mote which has obscured the vision, mental or physical, of your brother observer. This do fearlessly; it is your duty, putting altogether aside the possibility of the beam being in your own eye. But from those who presume to detract from the merits of a painstaking and laborious student of nature, because, forsooth, they have some dim recollection that Magendie showed them the same thing thirty-five years before, we are entitled to demand satisfactory proof of their assertion, or consign it for ever to oblivion. We shall, for the benefit of such critics, point out shortly some facts, a knowledge of which we owe exclusively to Brown-Séquard: facts and theories which Magendie “never dreamed of in his philosophy.” If there is one thing more than another which tends to establish the great benefits which science owes to Dr. Brown-Séquard, it is the singular unanimity (if one may use the expression, speaking comparatively) with which his doctrines have been received. Points, for the most part of minor importance, have indeed been here and there selected for unfavourable criticism; but, as a whole, these lectures have had a reception which could only have followed as a consequence of the logical, almost mathematical precision, with which the author lays his proofs before us. We doubt much whether a professional jury in such cases ever gives the literary culprit the benefit of a doubt; and, therefore, we think that the verdict in Dr. Brown-Séquard’s case speaks all the more strongly in his favour. Far from shunning criticism, he boldly courts it, and is ready to express his thanks to any who may show him

where he has erred; while he frankly admits, with characteristic modesty, that he must have come to erroneous conclusions on several points.

We think we shall best perform our task by giving an abstract of the views of our author, resting mainly on those points where he lays claim to originality. We do not despair of convincing even our sceptical friends that Dr. Brown-Séquad has discovered and demonstrated facts, in regard to which no one can dispute with him the palm; and that he has even presumed so far as to attempt to show, ay, and has succeeded in showing, that Magendie himself is prone to error, and has been a little too hasty in some of his conclusions.

The first lecture opens with a statement as to the best mode of conducting such investigations. The importance of combining pathological observations with experiments on animals is thus alluded to:—

“To try to solve the very important but complicated questions which are so numerous in both the physiological and the pathological history of the parts of these nerve-fibres which are in the nervous centres, it is necessary to make use of all the means that science may furnish, and particularly of the two best, which are—experimentation upon living animals, and observation of pathological cases. When employed together, these means of scientific research have allowed errors to be made; but, of course, the chance of committing errors is by far greater when either of them is employed alone. The danger of making use of one of these means exclusively is very strikingly illustrated by the many errors, concerning the cerebellum, committed by experimental physiologists, who mistook the effects of certain circumstances of their experiments for the results of injuries or of the absence of the cerebellum. Had they taken the trouble of comparing the phenomena they saw with those observed by medical men in cases of disease of the cerebellum, they would not have introduced in science a number of hypotheses which impede its progress. It is by so doing that experimentalists have thrown discredit on the means of scientific inquiry of which they have made so much use and abuse, and have given ground to many critics to blame their means of research, while the fault was in the men who employed those means, and not in the means themselves.

“If erroneous views had been arrived at by the exclusive use of one of the means of scientific research we have named, we find that, on the contrary, many great advances in the medical sciences are due to the combined use of vivisections and clinical observation. Perhaps I may be allowed to say that the lectures I am about delivering will afford some proof of the advantage of a comparison of pathological cases observed in man, with the result of experiments upon living animals.”

The great discovery of Sir Charles Bell is, as one might expect, one of the earliest to attract the attention of our author. He directs attention to the fact, as illustrative of the opinion expressed in the above extract, that had Sir Charles had recourse to experiments on living animals, he would probably have at once succeeded in proving the correctness of his theory in regard to the roots of the spinal nerves. He certainly proved that there was a different set of conductors for sensation and motion;

but here proof stopped short and theory took its place, until Magendie demonstrated clearly that the anterior roots contain the motor, and the posterior roots the sensory conductors. The experiments on living animals, by means of which he succeeded in proving the correctness of the celebrated theory of Sir Charles Bell, are at the present day so well understood, even by the physiological tyro, that we may here pass them over, pausing only to note one point which puzzled Magendie for some time, as it forms the ground-work of a most ingenious theory by Dr. Brown-Séquard. In the course of his experiments, Magendie found that irritation of the anterior roots evidently caused pain, although in a less degree than that of the posterior. Again, he found that irritation of the posterior roots sometimes caused local movements. Here, then, were two facts which apparently invalidated the theory of Sir Charles Bell. Magendie, however, pushed his investigations further, and found that in dividing the anterior roots irritation of the distal end alone caused pain; while, if the posterior root of any of the spinal nerves was divided, irritation of the anterior root of the same nerve no longer caused pain. He thus concluded, mainly from these data, that there is what he called a "recurring sensibility," which manifests itself on irritation of the anterior roots in the following way:—The irritation, starting from the anterior roots, passes to the trunk of the nerve, and by this channel to the periphery of the body, whence it returns, reaching the spinal cord by means of the ganglionic roots. "We must say," says Dr. Brown-Séquard, "that the name of *recurrent* sensibility is a very bad one, because sensibility is a vital property which cannot *move* from a place towards any other, and therefore cannot be *recurring*. It is the cause, whatever it may be, of the painful sensation which is recurring, and not sensibility."

After discussing the question as to what is the channel of the nervous irritation generated when the anterior roots are excited, in the course of which he states that it has not been absolutely determined how far the current extends towards the periphery of the body before returning in its course to the spinal cord and the sensorium, he puts the following query—*What is the cause of the pain produced by the irritation of the anterior roots of nerves?* It is in answer to this, that he gives his own theory on the subject; and this again is so well put and so replete with interest, that we make no apology for quoting the passage at length, especially as it would be next to impossible to place the matter in a clear light by a mere abstract:—

"According to Carus, the nervous loops, which he thought existed in the muscles, give an easy explanation of the facts discovered by Magendie. Unfortunately for this explanation (which Mr. Flourens has again proposed quite

recently), the existence of nervous loops in muscles is now disproved. Indeed, even in the skin there is good ground to doubt the presence of many loops. Some experiments which we have made render it probable that the pain caused by the irritation of the anterior roots is exactly of the same nature as that of cramps, and that both the pain of cramps and the pain which we will call *recurrent*, to avoid circumlocutions, depend upon a peculiar kind of irritation of the sensitive nerves of muscles. The theory we will propose is also applicable to many pathological and physiological phenomena, which have puzzled for a long while both practitioners and physiologists, and which seem, indeed, to be very plain and natural now that we have the key to their explanation.

"Professor Matteucci, nearly twenty years ago, found that when a nerve going to a muscle is put upon another muscle, a contraction takes place in the first one when the second contracts. In this case the nerve receives an *excitation* at the time the muscle, upon which it lies, contracts. The cause of the excitation of the nerve, according to Professor Matteucci, is a galvanic discharge which accompanies the muscular contraction. Dubois-Reymond explains otherwise the excitation of the nerve. He thinks it is due to a diminution of the galvanic current of the muscle when it contracts, and that the changes occurring in consequence in the nerve irritate it. But whatever be the right explanation, it seems certain that it is *some change in the galvanic state* of the muscle which causes the excitation of the nerve. We will, therefore, call it a galvanic excitation. Now I have tried to prove, in 1849, that the sensitive nerve-fibres which are in muscles, receive that peculiar galvanic excitation which acts upon the motor nerves in the case of the preceding experiment, so that there is a galvanic cause of sensation in muscular contractions. I have also tried to show elsewhere that our faculty of guiding our movements depends upon the sensations that we have of the state of our muscles, from the galvanic excitation which accompanies our muscular contractions. I cannot dwell at length here upon this explanation, which is only slightly connected with my subject; but I will try, at least, to show that several experiments and pathological cases agree perfectly with this theory.

"If we fix a thread to the tendon of a muscle of a frog, and attach to this thread a weight capable of entirely preventing the contraction of the muscle, which is fixed by its other extremity, we find that every time the muscle *tends* to contract, there is an excitation of the nerve lying upon it, and a contraction of the muscle to which this nerve is distributed. Hence it is not necessary for a muscle to contract in order to produce in nerves in contact with it a galvanic excitation. I repeat that it is sufficient that they *tend* to contract. Now, I have found that the greater is the resistance to the contraction of a muscle, the greater is the galvanic excitation that it gives to nerves in contact with its tissue. On the contrary, if there is no resistance at all, as already shown by Professor Matteucci, after the section of the tendon, then the galvanic excitation of nerves in contact with the contracting muscle no longer exists.

"If we compare these results with the following pathological facts, we find that the phenomena are much alike in the two series of facts that we compare, and, therefore, that they seem to depend on the same causes. I suppose a case of painful contracture of the anterior muscles of the thigh; the pain is increased very much every time the contracted muscles are elongated, *i.e.*, when the resistance to the contraction is augmented: on the contrary it diminishes when the resistance to the contraction is rendered less than it was, and, at last, it *disappears entirely, or almost entirely, when the resistance is completely, or almost completely destroyed*, after tenotomy. Surgeons, till our researches, had not been able to explain this apparently strange cessation of pain; now it seems quite simple to understand that such should be the case.

"In cases of *fissura in ano*, it is very well known that the pain due to the spasm of the sphincter is increased when there is a resistance to the contrac-

tion, and that the greater the elongation of the muscular fibres, the greater also the resistance to their contraction and the degree of pain. At last, when the muscular fibres can contract freely, and almost without resistance, the pain disappears, as is the case after the operation of Boyer (the section of the sphincter). Of course the pain depending upon the fissure persists, but that due to the muscular spasm disappears. In this case, also, surgeons could not explain the cessation of pain. We find here, as in the preceding case, that the excitation of the nerves due to the muscular contraction augments, decreases, and disappears in the same circumstances, concerning the degree or the absence of resistance which produce analogous phenomena in the experiments above mentioned.

"In cases of neuralgia, when the sensibility of nerves in muscles is increased, there is pain produced or increased every time the muscles contract.

"The contractions of the uterus, which are the more painful the more there is resistance opposed to them, cause also pain in the same way as the spasm of the anus or the contraction of the muscles of the thigh. The relation between the degree of contraction of the uterus and that of pain is so evident, that the word 'pains' is employed for that of 'contraction.'

"Every muscular contraction seems to generate a galvanic excitation of the sensitive nerves in the neighbourhood of the muscular fibres; and the degree of excitation being in proportion to the degree of energy of the contraction, we have in this way an excellent means of judging of the state of the muscles. When the contraction acquires the degree which it has in cramps, then it causes pain, which is also in proportion to the energy of the spasm. If we succeed when we have a cramp in making it cease by elongating the contracted muscle, we find that the pain often increases at first, and disappears only when the contraction has ceased. I regret not to be able to bring forward all the reasons which have led me to admit the views I have just proposed, but I must keep within my programme.

"If now we examine what takes place in the apparently paradoxical experiment of Magendie, we find that nothing is more easily explained. When the anterior roots of a spinal nerve are excited, a cramp is produced in the muscles in which the nerve-fibres of the roots are distributed, and the pain which belongs to a *cramp* is generated. I think this pain is due, as I have said for cramps, to a galvanic excitation of the sensitive nerve-fibres exciting in the muscles which contract; but whether this theory be true or not, is in a measure indifferent as regards the general cause of pain in the experiment of Magendie. In fact there is then the same cause which exists in a cramp; and this cannot fail to be so inasmuch as a real cramp is generated by the irritation of the anterior roots. So then we can conclude, 1st, that the *recurrent* sensation is only in appearance recurrent; 2d, that the anterior roots of the spinal nerves cause pain when they are irritated, because they produce a cramp; 3rd, that, consequently, *there is no sensibility of any kind in the anterior roots, and that it is because they are motor, and not because they are sensitive, that they cause pain when they are irritated.* Therefore, the objection which has been urged against the views of Sir Charles Bell, and which was found upon the fact that the anterior roots cause pain when irritated, is unfounded."

This ingenious theory affords a good example of the painstaking manner in which Dr. Brown-Séquard works out his physiological problems; and we think it may fairly be conceded that, whether or not his conclusions are strictly right, his theory is at all events preferable to that of Magendie. In continuing his review of the objections which have been, or might be taken to the theory of Sir Charles Bell, he notices the views of J. W. Arnold, who held, as the readers of the *British and Foreign*

Medico-Chirurgical Review will remember, that the anterior roots of the nerves contain the conductors to the sensorium of the so-called "muscular sense." With reference to this, our author appears, to some extent at least, to endorse the views of Arnold. At all events, he gives the result of an experiment which certainly goes further to prove his theory than any that Arnold himself ever performed. Having divided *all* the posterior roots of the spinal nerves in frogs, he found that the voluntary movements appeared to be nearly as perfect as if no operation had been performed; and that if the skin of the head is pinched on one side, the posterior limb on the same side tries to repel the cause of the pain as well as if no injury whatever had been made. He has also obtained precisely the same results in frogs which had been rendered blind. This is certainly a very striking experiment, and strongly confirmatory of Arnold's views. Dr. Brown-Séguard does not, however, go so far as to admit that all the conductors of muscular sensibility pass by the anterior roots, and gives some reasons deduced from experiment to corroborate this opinion. The first lecture closes by showing that, although Sir Charles Bell's great theory has received the most abundant proof from the labours of subsequent observers, that great physiologist was entirely mistaken in his ideas as to the particular parts of the spinal cord which serve to conduct the sensory and motor filaments.

Dr. Brown-Séguard devotes the chief part of his second Lecture to the detail of certain experiments which go to prove that sensitive impressions are transmitted along the spinal cord, and chiefly by means of its central part, *i.e.* the grey matter. The opinion first entertained by Bell with reference to this, was that sensitive impressions were transmitted along the spinal cord exclusively by means of the posterior columns, with which alone he imagined the posterior roots to be connected. In 1842 the French physiologist Longet adopted this theory, and seems indeed to have placed it before the world in so attractive a light that both in France and in England it was almost universally admitted. In England, indeed, it was all but unanimously recognized as part of the great theory of Sir Charles Bell, although Bell himself had, in 1835, publicly repudiated this doctrine in a paper which he then read before the Royal Society. Dr. Brown-Séguard comments with some severity on the carelessness of men of real scientific attainments in thus rashly accepting, without due criticism and inquiry, an erroneous doctrine. He then reviews at some length the views of Longet, shows how he contradicts himself, and how shallow and insufficient his conclusions are. Through this controversy we need not, however, follow him, although he confirms the views of Dr. R. B. Todd and Mr. Lockhart Clarke with con-

summate ability and much original observation. Although he demolishes the theory of M. Longet with complete success, that of course had already been done by the gentlemen named above, and especially by Dr. R. B. Todd, whose writings on the subject are very generally known and appreciated. Some of the causes of error in experimenting on the spinal cord, are very clearly explained. Then he details some experiments which show most clearly that a part may be a conductor of the sensitive impressions without being endowed with sensibility; which fact being entirely overlooked by Longet, was one of the principal, if not the main cause of his falling so grievously into error. He then gives experiments which show—1st, That a transverse section of the posterior columns of the spinal cord, instead of causing anæsthesia, is followed by hyperæsthesia; 2nd, That a transverse section of the whole spinal cord, except the posterior columns, is followed by a complete anæsthesia. These experiments alone, even when taken by themselves, appear to be sufficient for the final dismissal of the false theory of Longet. A number of anatomical and experimental facts are now given, showing the relative share which the grey matter and the various columns of white matter bear in the transmission of the sensitive impressions; these all tending to show that the grey matter is the main, though by no means the sole channel, by means of which the sensitive impressions travel upwards towards the brain.

The third Lecture is one of great interest. The subject of which it treats is that which, probably more than any other, will continue to be associated with the name of Brown-Séquard. Here we have a new and most striking fact in physiology discovered, and, by the most laborious and praiseworthy zeal, *proved* by one man to the satisfaction we may say of the whole scientific world. The facts demonstrated are—that the conductors of sensation decussate in the spinal cord; that the decussation is complete in mammals, but not so much in birds and reptiles; and that the decussation takes place at a short distance from the points of insertion of the posterior roots. Until this discovery was published, in 1849, it had been universally admitted that the well known experiments of Galen conclusively showed that there was no such decussation of sensitive conductors in the cord. This, however, is easily shown to be an erroneous idea, inasmuch as Galen, in detailing his experiments, never says a single word concerning sensibility. Haller, in like manner, in minutely describing the effects of a section of a lateral half of the cord, speaks of a paralysis of voluntary movements only, and not of sensibility. Indeed, the idea of one kind of paralysis occurring, as the result of an injury, without

the other, never seems to have been entertained by the earlier observers; and, indeed, so far as we know, no one has even attempted to dispute, either for himself or any previous observer, the question of priority with Dr. Brown-Séquard—a species of detraction which appears to be rather common than otherwise in these days. Let us now see how he proves his position.

After disposing of the pseudo-Galen theory, he proceeds to detail the various experiments by which, step by step, he has proved his theory to be correct; and some of which experiments, we may add, we had the satisfaction of seeing the learned lecturer perform with admirable skill and signal success. We shall allow him to speak for himself, as the experiments are so concisely stated as not to admit of abbreviation:—

“1st, The spinal cord of a mammal is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the grey matter on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section, while it seems to be lost, or extremely diminished, in the posterior limb on the opposite side. There seems to be, therefore, *hyperæsthesia* behind and *on the side* of a transversal section of a complete lateral half of the spinal cord; while, on the contrary, there seems to be *anæsthesia* behind the section, and *on the opposite side*.

“This experiment is one of the two made by Galen; but he seems not to have looked at all at the condition of sensibility, and he simply states that there is a paralysis on the side of the section, and no paralysis on the opposite side.

“Sir Astley Cooper, under the suggestion of Dr. Yelloly, has made a similar experiment, except that the section was higher; the state of sensibility is not mentioned, and, as regards movements, there was paralysis on the side operated upon.

“Schœps, Van Deen, and Stilling have observed that sensibility is not lost in the limb or limbs behind, and on the side of the section of a lateral half of the spinal cord; but they have not remarked the most important fact, that on the opposite side there is anæsthesia. They also do not mention this curious result of this experiment, the existence of hyperæsthesia on the side of the injury.

“Fodéra was very near discovering that there is a decussation of the sensitive fibres in the spinal cord. He says he has found in some cases that a section of one of the posterior columns caused a diminution of sensibility in the opposite side of the body; but he states that in other cases he has seen the reverse. He also has sometimes remarked that the section of one of the posterior columns causes hyperæsthesia in the same side, and that a section of these two columns produces hyperæsthesia in the two sides, but he declares also that he has seen the reverse.

“Two explanations for some of the results of a section of a lateral half of the spinal cord may be proposed as regards sensibility. Either it may be imagined, as it has been by several German physiologists, that the grey matter has the power of transmitting sensitive impressions in such a manner that one lateral half of this substance is sufficient for the two sides of the body, or that the conductors of the sensitive impressions decussate in the spinal cord, so that those which come from the left side of the body pass into the right side of the spinal cord, and *vice versa*. The hypothesis of the Germans may explain the fact that sensibility persists on the side of the section, but it is proved to be absolutely

inadmissible by the fact that there is anæsthesia on the opposite side. We will see that the other experiments we have to mention are also in opposition to the view of the Germaus (Stilling, Schiff, and others). On the contrary, all the facts concur to prove the existence of a decussation.

"2d, If, after having made a first section of a lateral half of the spinal cord in the dorsal region, on the right side for instance, and after having ascertained that the *right* posterior limb is hyperæsthetic, or at last extremely sensitive, we divide the *left* lateral half of the spinal cord in the cervical region, we find then that the *right* posterior limb loses entirely, or almost entirely, its sensibility. This experiment shows clearly that the sensitive impressions coming from the *right* posterior limb, after the first section, passed across the spinal cord from the right into the left side, along which they were transmitted to the encephalon.

"3d, To obtain a very striking result from the experiment which consists in only one section of a lateral half of the spinal cord, it is better to make it after the posterior columns have been divided. We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the posterior columns have been divided, we find that the hyperæsthesia seems to increase on the side of the second operation, while, on the opposite side, not only the hyperæsthesia but sensibility entirely disappears.

"4th, There is another mode of proving that the conductors of the sensitive impressions decussate in the spinal cord. In several points of view this mode of demonstration is superior to the preceding. It consists of a longitudinal section of the spinal cord, an experiment already made by Galen, but the results of which, as regards sensibility, have been overlooked by him.

"The spinal cord is laid bare in the whole lumbar region, and a careful division of the entire extent of the part of the organ giving origin to the nerves of the posterior limbs, is made so as to separate the two lateral halves of the organ, one from the other. If this experiment could be executed perfectly well, nothing would be divided in the cord except the commissures, which unite the right side with the left side of the cord, and all the longitudinal elements of this nervous centre would be left uninjured; but it is impossible not to cut more or less on either side. However, when the operation has succeeded well—*i.e.*, when the two separated halves have been very little injured, a striking result is obtained. The voluntary movements still exist in the posterior limbs (though diminished on account of the injury to the muscles of the lumbar region), but *sensibility is entirely lost* in them. To those who know that injuries to the spinal cord, which cause a diminution of sensibility, always produce a greater diminution of voluntary movements, this fact will not be explained by the supposition that some injury has then been made to the two halves of the cord, and that it is in consequence of this supposed injury that the loss of sensibility is due. At least it will, I think, be easily admitted that if the two lateral halves of the cord had been injured enough to produce a complete and a lasting anæsthesia, there would be a notable degree of paralysis of voluntary movements. We repeat that such is not the case: the animal has the use of his two limbs; he moves about pretty freely, as Galen had already said. The loss of sensibility, therefore, must depend upon the section of the commissures of the spinal cord, or, in other words, on elements of this organ which cross each other in the median line, or rather the median plane.

"If now we compare the results of this experiment with those of a transversal section of a lateral half of the spinal cord, we find that they agree perfectly in showing that the conductors of the sensitive impressions decussate in this organ. It is useless to stop to show that the longitudinal separation of the lumbar enlargement of the spinal cord would not produce anæsthesia, if the German physiologists were right in admitting that the grey matter has the power of transmission in every direction. It would be useless also to insist upon the disagreement between the results of a longitudinal section of the spinal

cord, and the views of the physiologists who admit that the posterior columns are composed of sensitive fibres coming from the posterior roots, and going up to the encephalon. These columns are left almost entire and uninjured, and, nevertheless, sensibility is lost.

"5th, Another experiment, which a combination of two of the preceding gives a still better proof of the decussation of the conductors of sensitive impressions in the spinal cord. A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves. I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs. Of course, if the loss of sensibility in the two anterior limbs depended upon an injury to the two sides of the cord, and not upon a section of the decussating conductors of sensitive impressions, there would be a loss of sensibility, or at least a diminution of it in the posterior limbs. The admission of a decussation explains the two facts—loss of sensibility in one set of limbs, and conservation in the other set. If we divide transversely in the same animal the right lateral half of the spinal cord, we find then that the posterior limb on the same side becomes more evidently hyperæsthetic than before, and that the left posterior limb loses its sensibility. The transmission for this last limb, therefore, took place by the right half of the cord, while that for the right posterior limb continues to take place by the left half of the cord.

"I think that the facts I have mentioned sufficiently show that there is a decussation of the conductors of sensitive impressions in the spinal cord."

He now discusses several minor points bearing directly on this subject; such as whether or not the decussation is complete? This he assumes to be the case, in man at least; and this is confirmed in a remarkable manner by some pathological cases which are detailed in a subsequent chapter. There are here, however, so many sources of error that it is a matter of great difficulty to decide this point by means of experiments alone. It would appear that in animals lower in the scale than the mammalia, the decussation is not complete. This, again, has evidently led some able experimenters into error. M. Chauveau, of Lyons, concluded that there was no decussation; but he has evidently arrived at this conclusion from the fact of his having confined his experiments to pigeons; whereas had he operated, like Dr. Brown-Séquard, on rabbits and guinea-pigs, he would probably have come to conclusions identical with those of the latter. Dr. Brown-Séquard, in noticing M. Chauveau's experiments, points out that a certain amount of decussation evidently takes place even in pigeons, although this does not occur so near the point of insertion of the posterior roots in the spinal cord, as in the higher grades of the animal kingdom.

In Lecture IV. the author attempts to show that we may learn from experiments on animals, not only facts which enable us to judge as to the transmission of painful impressions, but also impressions of touch, cold, warmth, &c. Most of the elements which are employed as conductors of the purely tactile impressions appear, according to experiments detailed by our author,

to pass by the same parts of the spinal cord as those which transmit the impressions which give pain. He then proceeds to show that the disposition of the conductors of the various sensitive impressions in the spinal cord is such, that very deep alteration of this organ may not entirely destroy sensibility. It is now shown that, although the grey matter plays such an important part in the transmission of sensitive impressions, its functions as a conductor are by no means confined to this. On the contrary, it would seem that the motor fibres, or those which convey the orders of the will to muscles, are also found here in considerable abundance; so that his experiments go so far to confirm the theory of Todd, that all the spinal nerves terminate in the grey substance of the spinal cord, and that the white columns, instead of being continuations of the nerves through the cord to the brain, are commissural fibres connecting the segments of the cerebro-spinal centres with one another. The opinions of Dr. Todd on this point are clearly laid down in his splendid essay in the "Cyclopedia of Anatomy and Physiology." The lecturer here shows that the anterior columns of the spinal cord in the upper part of the cervical region have but a slight participation in voluntary movements, and the lateral columns, with the surrounding grey matter in that part of the cord, are almost the only channels between the will and muscles. The whole subject of the transmission of the orders of the will to the muscles through the spinal cord has been found by the author to be much more difficult to determine with precision than questions connected with the transmission of sensitive impressions. The principal results at which he arrives are that in the dorsal region all the various parts of the spinal cord, except the posterior columns, are employed in the conveyance of the orders of the will to muscles—that in this region the *anterior* columns and the grey matter are the most important; and of the grey matter that in the anterior half of the cord, in the conveyance of the orders of the will; but near the medulla oblongata he finds the *lateral* columns and the grey matter between them the most important. In some animals there are reasons to believe a decussation of the volitional fibres takes place to some extent in the spinal cord; but pathological facts do not bear this out as occurring in man, in whom the crossing of the anterior pyramids in the medulla oblongata is the only decussation that takes place. The lecture concludes by showing that the results of experiments afford no support to the assertion of Bellingeri and Valentin that the motor fibres that go to the extensor muscles pass through the posterior columns, while those that go to the flexor muscles go through the anterior columns.

Lectures V., VI., and VII. consist, in greater part, of detailed pathological cases, bearing upon the various points already

noticed. The cases have to a considerable extent come under the immediate observation of the author; but by far the larger share consist of cases gleaned from all sources, which of course renders the whole much more valuable as the groundwork of pathological data. We would fain delay here for some time, and study the cases with care; but we have so much before us that we must hurry on, remarking only that the cases will well repay a careful perusal. The majority of them have a bearing more or less direct on the question of the decussation of sensitive impressions in the cord; nor can any one deny that they afford the most remarkable confirmation of Dr. Brown-Séguard's doctrine.

Lecture VIII. reviews the pathological cases cited in the three immediately preceding. Here the author proceeds to deduce certain conclusions relative to the diagnosis of alterations of various portions of the spinal cord. After briefly indicating the principal symptoms of injuries or alterations of the spinal cord, he proceeds to dispute the views of Bellingeri and Valentin, in reference to the pretended motor functions of the posterior columns, and adduces some cases adverse to the theory. These go to confirm the objections raised in Lecture IV. to the views of these eminent physiologists. He then discusses at considerable length, and with occasional pathological illustrations, the relation which alterations in the posterior columns bear to paralysis of motion. He shows that, while a complete section of these parts is unattended by paralysis, an apparent paralysis is observed when a great part of the length of the posterior columns is altered. This he explains by showing that, if the alteration in these columns is limited in extent, reflex actions can still take place; whereas if, for instance, it extends throughout the whole length of the lumbar enlargement, it effectually puts a stop to reflex actions of the lower limbs. Thus the actions of walking, standing, &c. cannot be satisfactorily performed in the absence of the reflex functions; and if the alteration is of still greater extent, the voluntary movements are more completely suspended; but our author only admits this as an apparent paralysis. It would appear that paralysis of voluntary motion is associated not only with alterations of the anterior and lateral columns, but exists also in cases where the grey matter is affected. The following are his conclusions with reference to anæsthesia:—

“1st, Anæsthesia, in cases of disease of the spinal cord, is a symptom indicating that the grey matter is altered. This conclusion seems to hold good also with respect to the loss of each of the various kinds of sensibility.

“2nd, Alterations, limited to either of the white columns of the spinal cord, do not cause anæsthesia of any kind.

“3rd, Anæsthesia, limited to one-half of the body in cases of disease of the spinal cord, is a symptom indicating that the grey matter in the opposite half of the cord is altered, except in cases of which we will now say a few words.

"We have said already that anæsthesia alone—i.e., without a paralysis of voluntary movements, is not a symptom of disease of the spinal cord, or at least of ordinary diseases of the organ, as we have acknowledged that there are two kinds of alterations that might produce a much more marked anæsthesia than a paralysis of voluntary motion, and even perhaps diminish voluntary movements almost only on account of the anæsthesia produced. One of these alterations would be an injury or a morbid transformation all along the posterior horns in which the posterior roots pass before going to the other parts of the spinal cord. In such a case there would be anæsthesia on the side where the alteration exists, if it is limited to one side of the cord, and the loss of sensibility would be localized to those parts which receive their sensitive nerve-fibres from the regions altered, so that, below and above, sensibility might exist. We do not know of a single case of this kind; some, however, may have been observed.

"The other cause of anæsthesia, without paralysis of voluntary movements, consists in an injury to the decussating conductors of sensitive impressions in the spinal cord. Suppose, for instance, a sword introduced from below upwards, or in the opposite direction, in the spinal cord, and dividing this organ in its two lateral halves, and producing as little injury as possible to its anterior columns. The decussating fibres being then divided, sensibility would be lost in all the parts of the body from which these fibres come. It is hardly possible to conceive the production of such an injury, which is made with so much difficulty even in experimenting. But sometimes disease causes almost this kind of injury; for instance, a softening beginning in the very place where the decussation exists, or a slow increase of the amount of water in the central canal of the spinal cord, may produce a greater diminution of sensibility than of voluntary movements. This is what occurs not rarely in the beginning of softening of the spinal cord, and in cases of spina bifida with hydrorachis. It would be very interesting to look for this symptom in cases of *diplomylia*. Our learned friend M. Depaul has seen sensibility lost and voluntary movements partly preserved, in a case of division of the spinal cord (*diplomylia*). This is a fact well worthy of attention, as it realizes in man what is shown in the most important of the experiments which I have shown in the preceding lectures.

"We do not need to say that anæsthesia may be due to alterations of the posterior roots, and to the nerve-fibres of the various sensitive nerves in any part of their length. We do not need also to remind our hearers that anæsthesia may be due to diseases of the encephalon, to poisoning by lead, arsenic, belladonna, &c. But there is a kind of anæsthesia which is much less known, and about which we must say a few words. This peculiar anæsthesia depends upon a morbid reflex action; it may arise in diseases of all the viscera, and also from irritations upon any sensitive nerve in any part of its course. If time allows we will, in another lecture, give many illustrations of this kind of anæsthesia; at present we will merely relate a curious case as an example of this affection."

We have also some interesting observations in the course of this lecture in reference to the particular lesions which cause a loss of particular sensations, such as touch, tickling, pain, change of temperature, and the sensation of muscular contraction. It will be remembered that our author maintains that there are separate conductors for these various impressions—an assertion which his cases go some way to prove; but we think that this part of the subject will certainly bear a little more investigation, especially as to the anatomical lesions which cause the depravity of sensation. The symptoms which characterize alterations limited to certain parts of the various columns of the spinal cord and of

its grey matter are grouped together in a manner extremely convenient for reference.

In Lecture IX. we have a change of scene—*exit* the Spinal Cord; *enter* the Great Sympathetic. Our author's labours, even when diverted into a new channel, appear to flow on unchecked. We have here again constant proof of the painstaking, anxious, truth-seeking nature which characterizes all Dr. Brown-Séquard's efforts. He shows considerable originality of idea in the method which he has adopted in endeavouring to throw light on the functions of this comparatively neglected portion of the nervous system; and many of his results are both new, and perhaps as interesting to the student of physiology, as any portion of his work. We shall give a short outline of this chapter. The lecturer begins by studying the effects of a section of the sympathetic nerve in the cervical region. In 1851 and 1852 Bernard announced that he had found that, on a section of the cervical sympathetic nerve, a considerable afflux of blood took place in those parts of the head to which the sympathetic goes. When this discovery was given out, Dr. Brown-Séquard at once concluded that the cause of this—which Bernard had not attempted to point out—was a paralysis of the bloodvessels after the section of the sympathetic; and he thought that, if he was right in this, galvanization of the nerve would produce the reverse of the effects of the section. On performing the experiment he found, as he had expected, that this was indeed the case, and that the bloodvessels at once contracted, this being attended by a corresponding diminution of temperature. This fact was published by Dr. Brown-Séquard a few months after the discovery of Bernard was announced; but the same thing was nevertheless given out as a novelty at a subsequent period both by Bernard himself and Dr. Augustus Waller. Our author certainly makes out a clear claim to priority in this point; but at the same time we must acquit the other eminent observers of any suspicion of plagiarism, inasmuch as it was in the highest degree improbable that either of them ever saw the periodical in which he published his discovery, as we do not think it at all likely that the *Philadelphia Medical Examiner* has a very large circulation on this side of the Atlantic. It is stated, and we think correctly, that the knowledge of the effects of paralysis and irritation of the sympathetic nerve opens a new and most important field in physiology, pathology, and therapeutics. The greater afflux of arterial blood is itself, through the increased chemical changes of nutrition and secretion, a cause of attraction of arterial blood, as an increase in the normal *suction-power* of the capillaries is thereby induced. Another cause mentioned is that the augmentation of temperature—a manifest result of hyperæmia—is a direct cause of increased

chemical action; this again being, as above stated, a cause of attraction of blood. Any cause which tends to produce the same effects, acts in a similar manner; thus the hanging up of an animal by its hind legs produces a congestion of the head by gravitation, and very nearly all the effects of this section.

A most interesting series of experiments—by Dr. Augustus Waller, Professor J. Budge, and the lecturer—show that the sympathetic nerve originates chiefly from the cerebro-spinal axis. Thus, it has been found that a section of a lateral half of the spinal cord in the dorsal region produces, in the lower limb of the same side, almost all the effects of a section of the sympathetic nerve in the neck. Some very curious facts are now stated in reference to a reflex function of the sympathetic nerve, through the branches which convey motor power to the bloodvessels. Thus the lecturer has found, in the course of some experiments performed by himself and Dr. Tholozan, that if one hand be dipped into water at a very low temperature, the bloodvessels of the other contract. Subsequent experiments by other observers corroborate this view. The persistent contraction or spasm of bloodvessels, which is the cause of the coldness of the extremities in epileptics and in certain cases of paralysis, is stated to be due to an irritation of the cerebro-spinal axis. In this manner, also, the cold stage in intermittent fever, in cholera, or after the introduction of a catheter, is accounted for. In some cases this spasm is extreme. Thus in the posterior limb of a dog, in which the bloodvessels are in this condition, the circulation is so much impeded, that the cutting of the skin hardly gives a drop of blood. The author seems to have long clung to the idea that this discovery of the reflex influence of the sympathetic system would eventually afford a clue to the solution of all the phenomena, physiological and pathological, which have a reference to the influence of the nervous system on nutrition and secretion. He now, however, frankly admits that subsequent researches, and especially those of Bernard, point to a different conclusion. We need only mention, without going at length into the discussion, that the conclusion arrived at is that there are two kinds of influences of the nervous system upon nutrition and secretion. By one we can see, on exciting the sympathetic nerve, a manifest contraction of the bloodvessels, with a corresponding diminution of secretion and nutrition; by the other—the discovery of which we owe mainly to Bernard—the bloodvessels dilate in consequence of a greater attraction for arterial blood developed in the tissues. “Which of these two modes of action,” says the lecturer, “is the most frequent? and which is the most powerful in producing the normal and the morbid phenomena of nutrition and secretion? These are questions very difficult to be solved.” I

is interesting here to state that these conclusions, now well established by experimental facts, are very nearly those which our English physiologists—Todd, Paget, and Carpenter—arrived at on reasoning which was perhaps purely theoretical.

In Lecture X. the consideration of the subject of the influence of the nervous system on nutrition and secretion is continued; and the lecturer remarks on the importance of a knowledge of this influence in the treatment and in the explanation of the production of many diseases. This chapter is also full of points of great interest. We will only mention some of the most important. Three kinds of reflex action are described—contraction, secretion, and modification of nutrition. Several cases are given to prove that in some cases stoppage of the heart's action is due to a sudden excitation of the abdominal sympathetic nerve, as in drinking cold water when overheated, or in the case of a blow on the abdomen, or injury to an abdominal viscus without hæmorrhage. The theoretical *modus operandi* in these cases, and some other ingenious applications of the theory, are given in the following extract.—

“The excitation goes up to the spinal cord chiefly along the great splanchnic nerve, and ascends the spinal cord until the place of origin of the par vagum, and through this pair of nerves it comes to the heart. This is proved by the fact that a section of either the par vagum, or the spinal cord, or the splanchnic nerves, allows any kind of irritation to be made on the abdominal sympathetic without a stopping taking place in the heart. In some animals the influence of the irritation of the sympathetic in the abdomen is much more marked than in others; it is so probably in man. I have seen a gentleman suddenly drop down pulseless, in the most complete syncope, from a pain in the abdomen. The same gentleman is easily attacked by syncope from any kind of pain. One day while I was trying to bleed him with the assistance of my learned friend Professor Natalis Guillot, he had, as soon as pricked by the lancet, a complete stopping of the heart's movements, and we thought for two minutes that he would die. I took him by his feet, which I put on my shoulders, and then rising, I held him, the head hanging down, and he gradually recovered.

“It is by the reflex influence due to the sudden irritation of the branches of the par vagum in the lungs that chloroform has killed in the very rare cases in which the heart's action has been stopped before the respiration. In dogs, in which we can cause death in this way rather easier than in other animals, I have found that this mode of death never exists after the section of the par vagum. On the other hand I have ascertained, in the same kind of animals, that the state of the heart is just the same as when death has been produced by the irritation (by galvanism) of the medulla oblongata and par vagum, or by the extirpation of the so-called *vital knot*. Besides, another proof that it is in this way that chloroform kills in the cases which I try to elucidate is, that in some dogs on which the heart's action has been suddenly stopped by the inhalation of a very large quantity of chloroform, I have been able to restore life by merely exciting the heart to contract by mechanical excitation (pressure on the chest).

“I must point out, *apropos* of the stopping of the heart's action by a reflex mechanism, that one of the means employed to restore life in asphyxiated children—which consists in the alternative dipping of the body in warm and cold water—is a most dangerous one. No doubt that it is a powerful means of pro-

ducing reflex actions as long as any reflex power remains in the cerebro-spinal axis, but in this very thing lies the danger. I have seen puppies asphyxiated, and having no more respiration, while the heart was still beating fifteen or twenty times in a minute, killed at once on being dipped into cold water, the heart stopping by a reflex action. I do not intend to say that such a means ought not to be employed; I wish only to point out the chance of a sudden arrest of the heart's action, so that practitioners may be on the watch respecting this accident.

"An extensive burn may also produce a stopping of the heart's movements, but it frequently produces other effects which are much more interesting, and prove the great power of the nervous system on nutrition. In an important paper of Mr. Long of Liverpool, it is stated that death was caused, in many cases of extensive burns, by an inflammation of the various viscera. The three following conclusions have been arrived at by Mr. Long:—1st, That in almost every burn, indeed in every burn, lesions of one or more of the viscera contained in the three great cavities exist, being according to their frequency as follows—abdomen, chest, head. 2nd, That the lesions of the different tissues contained in the abdomen are in the following order—mucous membranes, serous membranes, parenchymatous tissues; in the chest it is quite the reverse, namely, parenchymatous tissues, serous tissues, and lastly mucous; in the head—membranes, brain. 3rd, That the seat of internal inflammation corresponds sufficiently often with the external position of the burn, but that in a precisely equal number of instances no such correspondence can be traced. Mr. Curling, in a paper on the Influence of Burns on the Bowels, relates ten cases of ulceration of the duodenum as a consequence of this powerful irritation of the skin. Lastly, in a very remarkable paper, Mr. J. E. Erichsen gives the following as the result of observations of many cases of burns:—

The cerebral organs were diseased in 33 out of 37 cases.

The thoracic viscera " " in 30 out of 40 "

The abdominal viscera " " in 31 out of 42 "

"I have given these numbers to show the frequency of this reflex influence of burns. When I come to the deductions to be drawn from the facts I have mentioned in this lecture for the treatment of disease, I will show the importance of the knowledge of this influence of burns, and I will show also what should be done against this frequently deathly influence, according to the view that it acts by a reflection from the nervous centres upon themselves, or upon the thoracic or the abdominal viscera.

"When we come to the demonstration that the phenomena which we have mentioned in this lecture are really to be attributed to a reflex action, we will show what parts of the alterations in the various viscera, after extensive burns, belong to a reflex influence, and what parts are due to other causes. But we will at once relate cases which show that several of the inflammations of internal organs after burns may be due to a reflex action, in showing that inflammations in various parts of the body may be caused by an irritation of the nerves of the skin or of other sensitive nerves."

Lecture XI. treats of the etiology, nature, and treatment of epilepsy, with a few remarks on several other affections of the nervous centres. The main point of interest is, however, the detail of the experiments by which the lecturer has succeeded in producing an artificial epilepsy in animals. In a few weeks after inflicting certain injuries in the dorsal or lumbar regions of the spinal cord in guinea-pigs, he found that fits appeared

spontaneously, at least once every two or three days, generally several times a day. But the most curious and apparently inexplicable fact is, that he is able to produce a fit when he chooses by simply pinching a portion of the integument where it covers the angle of the lower jaw, and from thence to the eye, the ear, and downwards nearly to the shoulder. It is the skin alone which has this power, as the three nerve trunks which send filaments to this part of the skin can be irritated without causing any convulsion. The pinching must also take place on the same side as that on which the spinal cord has been injured. From these facts he concludes:—

“1st, That the spinal cord in animals may be the *cause* (I do not say the *seat*) of an epileptic affection.

“2nd, That there is a mysterious relation between certain parts of the spinal cord and remote parts of the skin of the face and neck.

“3rd, That epileptiform convulsions may be the constant consequence of slight irritations upon certain nerves.

“4th, That the trunk of a nerve may not have the power of producing convulsions, whilst its cutaneous ramifications possess this power.

“5th, That even when an epileptiform affection has its primitive *cause* in the nervous centres, some cutaneous filaments of nerves, not directly connected with the injured parts of these centres, have a power of producing convulsions, which other nerves, even directly connected with them, have not.”

These facts are evidently of the highest interest in establishing the connection between epilepsy and a diseased condition of the spinal cord. Pathological cases in man, however, bearing on this point have as yet not been observed with sufficient frequency to warrant absolute conclusions with reference to all the facts observed. There can be no doubt, however, but that epilepsy is frequently the result of a peripheric irritation, whether in the skin or the mucous membrane; so that we have epilepsy induced, just as in the case of the guinea-pigs, by a reflex action. We would willingly prolong our examination of this interesting subject, had we not already been so far carried away by the extreme interest of the subject as to push this review considerably beyond the limit which we originally had fixed. We would only add that this chapter, besides being eminently interesting to the physiologist, contains many points of high practical importance which are treated in a truly practical spirit; so that the physician may here learn much that will be of great use to him in cases of doubt and perplexity.

Lecture XII., which with an appendix concludes the work, treats of still more abstruse points connected with the medulla oblongata, and the pons varolii. The general conclusions of the writer are then epitomized as follows:—

“1st. Excitations of the anterior roots of the spinal nerves may be a cause of

pain, because these roots, being motor, produce a *cramp*. The pain due to this cramp is what has been erroneously called *recurring sensibility*. Cramps, and several other kinds of painful spasms (of the uterus during parturition, of the sphincter ani in certain cases, &c.), are painful on account of a galvanic irritation of sensitive nerves accompanying muscular contractions. (Lecture I.)

"2nd. Our movements seem to be guided by the peculiar sensations we derive from the galvanic irritation of certain sensitive nerves of muscles, while they contract. (Lecture I.)

"3rd. The power of transmitting sensitive impressions exists in many parts which are not able to give pain or any other sensation when they are excited by our usual means of irritation; so it is with the grey matter of the spinal cord, and with many parts of nerves, which, however, are conductors of sensitive impressions. (Lecture II.)

"4th. Hyperæsthesia is a constant result of certain injuries upon, or alterations of, the posterior parts of the cerebro-spinal axis, from the tubercula quadrigemina down to the lower end of the spinal cord. (Lectures II., IV., and V.)

"5th. The transmission of sensitive impressions in the spinal cord takes place chiefly through the grey matter, and partly through the anterior columns; but, before reaching the grey matter, the impressions, in a certain measure, pass through the posterior columns. (Lectures II., IV., and V.)

"6th. The conductors of sensitive impressions from the trunk and limbs decussate in the spinal cord, and not in the encephalon, as was universally admitted. (Lectures III. and VII.)

"7th. Although the spinal cord is greatly altered or injured, sensibility, more or less diminished, may persist everywhere, on account of a peculiar arrangement of the conductors of sensitive impressions. (Lectures IV. and VI.)

"8th. The various kinds of sensitive impressions seem to be transmitted by quite distinct conductors, in the nerves and in the nervous centres, and the place of passage of some of these conductors, in the spinal cord, seems not to be the same as that of the others, but none of them go up to the sensorium along the posterior columns. (Lecture VII.)

"9th. In the upper part of the cervical region of the spinal cord, near the medulla oblongata, most of the conductors of the orders of the will to muscles are in the lateral columns, and in the grey matter between these and the anterior columns. (Lectures IV. and VIII.)

"10th. The voluntary motor conductors decussate at the lower part of the oblong medulla, and not all along the median line of the base of the encephalon. (Lectures VII. and XII.)

"11th. The posterior columns of the spinal cord have a great share in reflex movements, and this is the principal cause of the peculiar kind of paralysis so often observed in cases of alteration of these columns. (Lecture VIII.)

"12th. The effects of excitation of the vaso-motor nerves consist essentially in a contraction of bloodvessels, which is followed by a diminution of the quantity of blood, in the temperature, and in the activity of nutrition. The effects of interruption of continuity of the vaso-motor nerves (*i. e.*, their paralysis) consist essentially in a paralytic dilatation of bloodvessels, which is followed by a greater afflux of blood, an increase of temperature, and a greater activity of nutrition. (Lecture IX.)

"13th. As a great many vaso-motor nerve-fibres go up to the brain and to the cerebellum along the spinal cord, the medulla oblongata and the pons Varolii, the diseases or injuries of the various parts of the cerebro-spinal axis, besides symptoms concerning sensibility and movement, present symptoms depending upon irritation or paralysis of vaso-motor nerves: contraction or relaxation of bloodvessels, diminution or augmentation in the quantity of blood, increase or diminution of temperature, alterations of nutrition, of secretions, &c. (Lectures IX. and XII.)

"14th. Besides the kind of influence of the nervous system upon nutrition, absorption, and secretion, through the vaso-motor nerves, there is another kind, which seems to consist in changes in the elements of the tissues—changes producing various modifications in the quantity of blood attracted, and in the interchange of materials between the blood and the tissues. (Lectures IX. and X.)

"15th. The absence of the influence of the nervous system on any part of the body is hardly a cause of other alterations of nutrition than atrophy, while the irritation of the nervous system is a most powerful direct or reflex cause of a great many morbid changes in nutrition, secretion, &c. (Lecture X.)

"16th. The sympathetic, normal, and morbid changes of nutrition, secretion, &c., are reflex phenomena, the study of which shows how many diseases are produced by a reflex action, and how a rational mode of treatment might be arrived at. (Lectures X. and XI.)

"17th. The loss of consciousness in simple vertigo or in complete attacks of epilepsy does not depend upon a disease of the brain, but upon a contraction of the bloodvessels of the cerebral lobes—contraction due to some irritation of the vaso-motor nerves of these vessels, either by some direct cause irritating them in the base of the encephalon or the spinal cord, or by a reflex influence.

"18th. Much more frequently than has been imagined, all the following affections may be produced by a peculiar kind of irritation starting from almost any centripetal part of the nervous system; epilepsy, the various forms of insanity, chorea, catalepsy, hysteria, tetanus, hydrophobia, &c. (Lectures X. and XI.)

"19th. The medulla oblongata is neither the only nor an essential nervous centre for the respiratory movements. (Lecture XII.)

"20th. There are a great many nerve-fibres and nerve-cells in the medulla oblongata, the pons Varolii, and the other parts of the base of the encephalon, which are not employed in the transmission of sensitive impressions or of the orders of the will to muscles, and are endowed with the singular property of producing, after even a slight irritation, a *persistent* spasm in certain muscles, and especially in the neck. Rotatory convulsions very often depend chiefly upon the production of such spasms, and of changes in the bloodvessels of certain parts of the encephalon. (Lecture XII.)

"21st. The irritation of the auditory nerve may cause rotatory or simple clonic convulsions. (Lecture XII.)

"22nd. The conductors of the orders of the will to muscles, of the sensitive impressions and of the nervous influences to bloodvessels, decussating at different places in the cerebro-spinal axis, various symptoms are to be observed, depending upon either the irritation or the paralysis of these three kinds of conductors, according to the part of a lateral half of the cerebro-spinal axis where an alteration exists. (Lecture XII.)

In closing this book we cannot do so without expressing a regret that we leave much that is of deep interest and great practical importance entirely unnoticed. Indeed such a work as this can be but imperfectly treated of within the limits prescribed by an ordinary review. We can only assure our readers that a close study of it will give the careful reader unmixed satisfaction. He will find many points, in which his ideas have hitherto, it may be, been in chaotic confusion, cleared up in a manner which the earnest student can alone appreciate; and we think that he will agree with us in ascribing to the talented author the very

highest praise for the admirable manner in which he has executed his arduous task. We look for more yet from the fertile pen of Brown-Séguard. May he long retain that vigour of manhood and intellect which has earned for him, at a comparatively early age, such a deservedly brilliant reputation.

III.—*The Principles and Practice of Obstetrics.* By GUNNING S. BEDFORD, A.M., M.D., Professor of Obstetrics, &c., in the University of New York, Pp. 731. New York: Samuel S. and William Wood. 1861.

THE works of American obstetricians have perhaps, as a general rule, attracted more attention on this side of the Atlantic than any class of professional literature emanating from the New World, and it was, therefore, with considerable interest that we opened the imposing volume which Dr. Bedford has given to the world. We must confess that our first impression was one of unmitigated disappointment, inasmuch as we somewhat hastily concluded that a book could not in any respect be a good one which was disfigured with such execrable wood-cuts. The illustration upon which our eye first fell is that at page 609, which, to say the least of it, is so hopelessly bad that we could only account for it by thinking that Dr. Bedford had secured the services of some extremely incompetent amateur in the art. Certain it is that a deal of good box-wood has been spoiled in the undertaking.

First impressions, however, are certainly not always confirmed by subsequent observation; and we confess that a further examination of the book afforded us a somewhat striking illustration of this profound truism. On turning over the leaves we found that there were, at least, some illustrations that we might look at with some satisfaction. Four coloured lithographs, exceedingly well copied from Dr. Montgomery's work "*On the Signs and Symptoms of Pregnancy*," show the appearance of the nipple at the various stages of utero-gestation, and indicate further that, in one direction at least, some trouble has been taken in getting up the illustrations, no unimportant accessory in a systematic work on midwifery. Having thus begun, like children, by looking at the pictures, we must now turn to the subject-matter of the work itself, and attempt to show in what it differs from the numerous works already published. Should we see faults we shall point them out as frankly and fairly as we shall its merits, for of one thing we are persuaded, that, good or bad, it has cost its author many an hour of thought and toil, which would other-