PART II.

CRITICAL ANALYSIS.

Art. I.—Anatomie et Physiologie du Systeme Nerveux de l'Homme et des Animaux Vertebres. Par F. A. Longet, Laureat de l'Institut de France, &c. &c.

Anatomy and Physiology of the Nervous System of Man and the Vertebrated Animals, with pathological observations on the nervous system, and experiments on the higher classes of animals. By F. A. Longet, M.D., Laureate of the Institute of France, Professor of Anatomy and Physiology, &c. &c. 2 vols. 8vo, with numerous Plates. Paris, 1842. Vol. i. pp. 942. Vol. ii. pp. 968.

Since the brilliant discoveries of Bellingeri, Bell, and Magendie were published, much has been done by other inquirers to elucidate various points which were necessarily left by them more or less imperfect. These discoveries, however, still form the ground-work of all that has since appeared, and several of the seemingly anomalous results at which their authors arrived have been since explained by more minute investigation into the anatomy of the anastomosing branches or origins of various filaments. It will be remembered, however, that these original experimentalists did not quite agree relative to the function performed by those nerves which originate from the posterior column of the spinal chord; for, while Bell maintained that they were simply nerves of sensation, Bellingeri attempted to demonstrate that they both regulated sensation and so much of motion as to prove antagonists to those nerves which originated from the anterior columns of the chord. We shall presently see how far subsequent research corroborates or disproves either of these theories. It is to Brachet especially that we are indebted for the more consistent views of the functions of the ganglionic system of nerves—views which have been more or less completely adopted by all late writers on the subject.
M. Longet’s work contains one of the best accounts we have seen of the present state of our knowledge of the anatomy, physiology, and pathology of the nervous system; and no one appears to be better qualified for the task, as he has proved himself one of the most successful late investigators into this difficult branch of physiological science. M. Longet’s work is divided into two great sections, the first treating of the general anatomy and physiology, the latter of the special anatomy and physiology of the nervous system.

The development of the nervous system forms the subject of the first chapter. Three views have been at different times maintained, relative to the development of the nervous system. 1. That the central parts were first developed, and from them originated the nerves; 2. That the nerves were first developed, and that by their subsequent junction and union in one mass they formed the central parts of the nervous system; and 3. That all parts of the nervous system were developed together. Malpighi and Meckel, from observations made on the chick in ovo, endeavoured to show that the first nervous chord which became visible was that which represented the spinal marrow. Rolando, however, thought it was the medulla oblongata. Ackerman, however, denied this, and described the great sympathetic nerve, but especially the cardiac plexus, as the first portion of the nervous system which became visible. Serres held that the spinal chord and brain were developed after both nerves and that distinct ganglia could be traced in various organs; and referred, in proof of this, to acephalous foetuses, whose nerves are fully developed though the brain be wanting, and to the circumstance more than once remarked, that if the eye be not formed, there is either no optic nerve, or only a trace. Desmoulins admits with M. Serres that the nerves do not originate from the brain or spinal chord, but he also denies that the one class of organs exists before the other. As to the arguments drawn from the occurrence of acephalous foetuses, he agrees with Morgagni, Brunner, and Lallemand, that these cases do not prove an original non-development, but that these central parts have been subsequently destroyed in consequence of serous effusion, or other disease. The same arguments apply to the want of an optic nerve, where the eye is not developed. According to the time which has elapsed since the organ was destroyed, the optic nerve is found either rudimentary, as in Rudolphi’s case, or completely wanting, as in Klinkosch’s case, where, at the same time, the brain was only partially developed. Baer and Burdoch seem to agree that the view which harmonizes best with all recorded observations, is that which considers the development of central and peripheral parts to go on simultaneously, and which holds that the one part does not originate from the other.
M. Longet appears to adopt the views of Serres, in so far at least as this, that "the peripheral system of nerves seems to be developed independent of all primitive relation with the central nervous system," and then he adds, as corroborative of his views, that "the first is already very apparent, when the second is scarcely visible."

All anatomists agree, with the exception of Gall and Spurzheim, that the white cerebral matter is found before the gray; and Gall's assertions on this point have been shown to be incorrect by every late anatomical investigator.

M. Longet draws the following conclusions from the above statement:

1. No known relation exists primitively in the fetus between the cerebro-spinal axis and the nerves. 2. The peripheral nervous system is already very apparent when the central nervous system is scarcely visible. 3. In monsters, we find the first formed in the absence of the second. 4. The development of the nervous system seems to go on from the circumference to the centre. 5. None of the primitive points of the nervous system ought to be regarded as centres of irradiation. 6. The nisus formativus organizes each of the organs in its proper place; there is succession in their development; but the one cannot be the efflorescence of the other. 7. It appears that the gray substance does not exist before the white substance; the reverse appears to be the fact. 8. The physiological laws which regulate nutrition are opposed to that view which considers the gray matter of the nervous system as the nutrient organ of the white; one solid substance cannot nourish another solid. 9. Before assuming the forms which are proper to it, the cerebro-spinal axis of man presents, in its successive evolutions, nearly the different forms of the same organ in the vertebrated animals of different classes."—Vol. i. p. 22-23.

M. Longet, in thus adopting the conclusions of M. Serres, with some inconsiderable modifications, appears to have gone further than the facts warrant. We agree with him that none of the points of the nervous system ought to be regarded as the centres from which all the other parts originate; but neither is it requisite to consider that the great central masses are developed from the nerves, as M. Serres and he endeavour to prove is the case. In the germinal vesicle of all animals, parts which become the spinal chord appear as soon as traces of a blood-vessel, or of nerves around it; in fact, the development of these three structures seems to occur so simultaneously that it is quite vain to attempt to prove that one part appears before the other. M. Serres lays great stress on the circumstance of the distinctness of the nerves as compared with the softness, fluidity, and apparent structureless condition of the brain and spinal marrow in the germ. But this
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is not at all to be wondered at. The brain and spinal cord are of no use to the fetus until it is born or is hatched, or, more strictly speaking, till quickening takes place; but its ganglionic system of nerves, which are the first to be discovered in a state of distinct development, are required from the moment when the blood-vessels begin to circulate a fluid. These ganglionic nerves, and especially, as Ackermann remarked, the cardiac plexus, are the first nerves which become distinctly visible; but even at this stage the traces of the spinal cord are quite distinct.

Were the views of M. Serres correct, that nerves are primarily developed in the organs, and afterwards extend to form the central parts, when the eye was wanting, we ought not only to find no optic nerve, but also no tubercula quadrigemina. In all the recorded cases, however, these organs were present, though the optic nerve existed simply as a band of condensed cellular tissue, or had even entirely disappeared. The reference to the optic nerve, however, is a most unfortunate one for M. Serres's theory, because no traces of the eye or optic nerve can be discovered long after the brain and spinal chord are visible; and when the eye does become visible, the optic filament appears at the same moment, all the way from the brain to the back of the eye. It never appears in the eye before it is in connection with the brain; it is never seen disjoined from or pushing an extremity towards the brain. That the optic nerve, then, should be wanting, or be in a simple rudimentary state when the eye is wanting, is nothing more than what we might expect. If anything destroy the organ while yet it is imperfectly developed, the optic nerve, which is developed with that organ, is arrested in its growth; and that nerve, which requires a microscope to discover when the arrestment of development probably took place, could never be expected to be discovered after the animal was born. Recorded pathological cases, in which vision of an eye has been destroyed, have fully proved that from that moment the optic nerve begins to shrivel, and becomes finally reduced to a cartilaginous chord, or to a fibrous, filamentous, or ligamentous band. Even in these cases, Andral states that "the optic thalami are very seldom altered;" and Magendie states that the atrophy rarely extends beyond the commissure, which could scarcely be the case if they owed their origin to, and depended on, the optic nerves.

The evidence derived from the consideration of acephalous monsters proves nothing more than that the nerves of organic life are independent of those of animal life, and the formation of both independent of the great nervous centres. Knowing, as we now do, that the ganglionic system of nerves regulates the involuntary acts and presides over the vascular system, and, of course, over secretion and nutrition,—knowing that the nerves of every organ are

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formed at the same moment as that organ itself—we can easily understand, that although, from some arrestment of development, the brain and spinal chord should cease to grow, or be destroyed, at a very early period of foetal life, the other organs of the body, and the nerves of animal life of these organs, should be fully formed under the presiding action of the ganglionic system of nerves. But, supposing all this were the case, it leaves the question exactly where it was; or, if good for anything, it rather proves that the whole parts of the nervous system are dependent on the ganglionic system, which, in this case, ought to be the first developed. None of the phenomena of growth, however, seem to favour the views of M. Serres, that the nervous system is developed from its circumference to its centre. With regard to some organs of the foetus, we know the very reverse is the case. Long after the brain and spinal chord are distinctly visible, the limbs begin to bud from the body; and, just in proportion to the growth of these limbs, are the nerves from the spinal chord seen to become more and more enlarged, and extend further and further into the limbs. Here, then, is a distinct case of the nerves being developed, as it were, from the spinal chord; but, as we explained above, we do not hold this to prove anything relative to the dependence of the development of the nerves from the chord, because, whether there had been a chord or none, the nutrient vessels, in continuing the growth of the body, would have furnished each organ with nerves, directed, as they ever are, by the ganglionic system; but these nerves of sensation and of voluntary motion are of no more use to the foetus till its birth than are the brain and spinal chord. In fact, the brain, spinal chord, and nerves of animal life, are intended solely for extra-uterine life, so that they may be more or less completely arrested in their development, without the growth of the foetus being materially interfered with. Those organs only in which the arrest of development takes place will be affected. Thus, if the arrest of development takes place in the brain, no cranium will be found, because there is no brain to protect. If it be in the spinal chord, no vertebral laminae will cover the site of the defective organ. But this never will prove that these central organs are developed posterior to the nerves, unless we admit with Ackermann, that the ganglionic system of nerves are the first to make their appearance. Our own researches on this very point have led us to conclude that all parts of the nervous system appear so very nearly at the same time, that we have as yet failed to detect which was first developed. On this only we were able to satisfy ourselves, that the nerves in the neighbourhood of the blood-vessels, those which afterwards become the ganglionic or sympathetic system, were those which seemed first to become most perfectly organized.
M. Longet's second chapter is devoted to the consideration of the distinction of the organs of sensation from those of motion in the nervous system. This is a subject on which we have in former volumes entered somewhat largely, so that there appears less necessity for dilating on it here. M. Longet, however, has himself repeated many of the original experiments of Bell and Bellingeri, and, from using greater precautions than appears to have been done by the latter, and also by employing electricity or galvanism as the irritating agent, he satisfied himself that the views of Sir Charles Bell were those which were most correct, viz. that the posterior roots of the spinal nerves were alone concerned in sensation, and the anterior roots in those of motion. M. Longet saw nothing in his experiments which could in the least authenticate the views of Bellingeri, as to the nerves from the posterior column of the spinal chord being the antagonists of those from the anterior columns, nor did he ever meet with any symptom which indicated that the nerves from the posterior columns of the chord possessed the property of exciting muscular action. He, therefore, fully confirms the views of Sir Charles Bell, that every nerve originating from the posterior columns of the spinal chord, or from the prolongations of these chords into the brain, (restiform bodies,) are devoted to the conduction of sensations alone, and that all these have one character in common, viz. a ganglionic enlargement shortly after their origin from the nervous centres.

The author divides the nerves of sensation into two classes,—those of general and those of special sensation. The first includes the ganglionic portion of the trifacial, the glossopharyngeal, and the pneumo-gastric, together with the posterior roots of the thirty-one spinal nerves. The olfactory, optic, and auditory nerves form the latter class. M. Longet is far from believing that experiments on the brains of the lower animals will ever enable us to arrive at any conclusions relative to the central seat of motion or of sensation in man. Birds may have their cerebral lobes entirely removed, yet walk, fly, feel, and even live for months, as Flourens proved. The brain in them does not, therefore, appear to be the seat of motion or of sensation. Even in the rabbit the cerebral lobes and the corpora striata may be removed, and the animal retains the power of standing and walking, and, if it be pinched, it manifests pain by crying. Removal of the cerebellum only causes the movements to be executed disorderly, and the sensations, though diminished, are far from being entirely destroyed. In man, however, the smallest injury of the cerebral lobes will often cause general loss of both sensation and motion. We have, therefore, much to learn as to the minute anatomical peculiarities of the brain of these animals and of man before we can be expected
either to account for the different effects of injuries of these parts, or in what manner their different parts are connected with the nerves of sensation or of voluntary motion.

The mode of action of sensitive and motor nerves forms the subject of the third chapter. That sensation may be perceived, or voluntary motion produced, it is necessary that the nerves be connected with the brain, either immediately or by means of the spinal marrow. When the nerves are divided, irritation of the free extremities of the first causes no appreciable effect, but irritation of those of the latter excites for a certain time muscular action. The nerves of sensation act only by conveying impressions to the brain, those of motion by transmitting the moving principle from the brain.

In experimenting on motor nerves it is constantly remarked that, after a nerve is cut across, no irritation of that part of the nerve which is still connected with the spinal marrow will excite contractions in muscles provided with nerves below the point injured. The primitive fibres of a plexus never inosculate with one another as do blood-vessels. Van Deen, Muller, and Kronenberg have fully proved that each of the primitive fibres remains isolated, communicates no nervous force to the others with which it associates, and simply contributes with them to form the enlarged nervous trunk which proceeds from the plexus. Thus dissection reveals that the three branches of spinal nerves which go to the thigh of the frog are distributed to different muscles. They form a plexus before being distributed to the thigh; but if the first branch be cut across, between the plexus and the spinal chord, all those parts supplied by nerves from it are deprived of the power of voluntary motion, which they would not have had the primitive nerves in the plexus communicated with each other. Panizza, therefore, in asserting that there is inosculaion of the primitive fibres at a plexus, and that injury of one of the branches of the nerve between the spinal chord and plexus does not arrest the power of volition in the branches of the nerve so cut, seems to have allowed himself to be deceived from not attending to the special distribution of each nerve; for Muller, Van Deen, and Longet, on repeating his own experiment, constantly observed the fact as above stated. M. Longet cites several analogous experiments he performed, with the view of ascertaining whether the plexus acted as it were the part of a new nervous centre, or allowed that free interchange of nervous force, that, though several branches going to that plexus were cut, the others which remained entire could convey the power of volition to all parts of the limb. He invariably found they could not. M. Longet very properly remarks, that simple observation might have led to the same conclusion,—that no inosculaion can occur among the pri-
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mitive fibres,—because we find the same nervous trunk supplying antagonist muscles. Thus, the upper and lower rectus of the eye are supplied by branches of the same nerve, and if any inoscula-
tion occurred at any part, the force of volition conveyed along the
nerve would as often excite the wrong as the right muscle. The
harmony and combination of movements, then, must originate in
the brain itself, and be simply conducted by the several primitive
nervous chords to those parts which we wish to move.

M. Longet next proceeds to point out the causes which excite
or destroy the irritability of the motor nerves, the duration of this
property when their connection with the nervous centres is cut
off, and their influence on muscular irritability.

When various agents are applied to the cut free extremities of
motor nerves, contractions are excited in the muscles to which
they are distributed. Pinching and pricking, the application of
galvanism, of potash, soda, ammonia, opium, chloride of barium,
tartrate of antimony, chlorine, alcohol, &c. to the cut extremities
have, in the hands of various experimenters, been seen to produce
distinct muscular contractions. Heat and cold even produced
contractions. M. Longet found, that applying the flame of a
candle to the extremity of the nerve produced very violent con-
tractions; but ice did not excite them so much. M. Longet also
found that the local application of heat or of cold for a short time
destroyed the irritability of the nerve at that place where these
agents were applied. The same was produced by an aqueous so-
lution of opium, of belladonna, &c.

It was reserved for M. Longet to determine exactly the influ-
ence of the nerves on muscular irritability. Legallois found that
irritation of the sciatic nerve, after the spinal chord was destroyed,
still excited muscular contractions. Muller, Sticker, Steinruck,
Schön, and Günther, made more accurate experiments. They
cut the sciatic nerves of various animals, and found that, after the
lapse of many weeks and months, the excitation of the lower ex-
tremity of the cut nerve failed to excite muscular contractions.
These experiments, however, neither proved the dependence of
muscular irritability on the integrity of the nerves, nor the length
of time when excitation of the cut extremity of the nerve ceased
to excite muscular contractions. M. Longet proceeded different-
ly. Instead of waiting for weeks or months before he tried whe-
ther irritation of its cut extremity would excite muscular contrac-
tions, he irritated it day by day, both by galvanism and mechani-
cal agents, and constantly found that "its excitability was wholly
lost after the fourth day." To ascertain this fact beyond a doubt,
not only was the experiment frequently repeated, but some of the
muscles to which branches of the cut nerve were distributed were
exposed, and irritation of even these branches, by means of galvanic
or mechanical agents, failed to excite the slightest muscular action. To render these results more striking the nerves on the opposite extremity were laid bare, when their slightest irritation produced the usual muscular contractions. These experiments thus demonstrate, says M. Longet, that we cannot admit that a principle analogous to what emanates from the cerebro-spinal axis is generated in the nervous chords; but, on the other hand, that it is necessary they remain attached to the spinal chord in order to remain excitable; and that their excitability is wholly destroyed within four complete days after their communication with the cerebro-spinal axis is cut off.

Seeing this was the case, it seemed of some consequence to ascertain how far the muscular irritability depended on nervous influence. Haller and his followers endeavoured to prove that the muscular contractility was independent of nervous influence, by tearing the heart from the body, or cutting a piece of muscle from the living animal. But in both of these cases the nerves were entire, and on being irritated could excite the muscular irritability. M. Longet, however, after he found that the excitation of the nerves which had been cut across ceased to convey the excitation to the muscles, found that the muscles themselves contracted freely on the slightest direct irritation, even for twelve weeks after the nerve lost its excitability.

When a nerve of sensation is cut across, irritation of its free extremity gives no pain, no sensation; but if the extremity still in communication with the cerebro-spinal axis be irritated, pain is instantly experienced. If the trunk of a sound nerve of sensation be injured, the pain is not only referred to the point injured, but also to the extreme branches of the nerve, as is experienced when the elbow is struck over the ulnar nerve. It is well known also that a man whose limb is amputated, refers the chief pain to the fingers or toes of the limb which is removed, and not to the cut extremity of the nerve on the face of the stump; and Muller has recorded many cases in which, even after the lapse of years, uneasy feelings, consequent on some irritation of the stump, have been referred to one part or other of the lost limb. Surgeons also are perfectly aware that section of a nerve frequently fails to remove neuralgia, because, if the seat of irritation be in the trunk of the nerve nearer the nervous centre than where it is cut across, the pain will still be referred to the same point, just as happened with the nerve on the face of the stump.

Having found that the nerves of motion lose all power of exciting muscles to contract four days after they are cut across, it became an object of some importance to ascertain how far the nerves of sensation influence muscular irritability. When M. Longet cut across a mixed nerve, as the sciatic, in a dog, voluntary mo-
tion and sensation were immediately extinguished in the limb. At the end of four days no irritation of the extremities of the nerve or of its branches caused muscular contractions, but at the end of fifteen days direct irritation of the muscle seemed to cause it to contract as actively as ever. It could still be excited to contract at the end of a month, but by seven weeks the contractions or irritation were scarcely perceptible. The muscular fibre was by this time much discoloured, and evidently degenerating; and, shortly after, the strongest direct stimulation could not excite the slightest contraction. The following experiment was also performed. The infra-orbital, the buccal, the anastomosing branches before the ear of the auriculotemporal with the middle branch of the seventh pair of nerves were cut across, and portions removed to prevent reunion. All the filaments of the trifacial of one side were thus cut across which go to the muscles of the nostril and upper lip. Six weeks after the operation the muscles on that side were found pale and colourless, yet still irritable, but to a very inferior degree to those of the sound side of the face. The hairs had also fallen from the upper lip, which had a doughy feel, evidently due to defective nutrition. From this experiment M. Longet concludes that muscular irritability is to a certain extent dependent on the nerves of sensation, seeing that when the nerves of motion were alone cut across the muscular irritability remained tolerably entire for at least three months; whilst in this experiment, in which the nerves of sensation were alone cut across, the muscular irritability was notably diminished after a period of only six weeks. In reviewing his opinion, however, he modifies it so far as to allow that the whole may be accounted for by the organic changes which take place in the muscle, for, in all the instances, the muscular irritability was manifested so long as the muscle retained its proper physical properties, and only ceased when it became pale and degenerated.

The microscopic structure of nerves next engages M. Longet's attention, and after an examination of the varied statements of observers on this point, arrives at the conclusion that nerves, both of sensation and of motion, consist of cylindrical tubes filled with a matter which, in the recent nerve, is fluid, but speedily becomes granular. Modern research has not succeeded in proving the nature of this fluid, which has been variously supposed by microscopists to be "oleiform or mucilaginous." The nerves of sensation appear to consist of more delicate tubes than those of motion, as the least handling throws them into varicose dilatations. In their natural state, however, they are quite cylindrical. The nerves of the ganglionic system do not seem to differ in structure from those of the ordinary nerves, excepting always the gray ganglionic matter. The latest researches appear to show that the
primitive fibres of the white matter of the brain and spinal chord are also composed of tubes. The gray matter of the brain, spinal chord, and ganglia, has been recognised to be composed of a granular substance, or of minute more or less globular-shaped bodies of various sizes. From this statement it would appear that the observations of Leuenhoeck and Malpighi, the fathers of the microscopic anatomy, however much they have been abused as having been made with imperfect instruments, come nearer the truth than modern microscopists will allow. Leuenhoeck described nerves as composed of parallel cylindrical tubes, on which he occasionally observed bulgings; and the brain as composed of straight parallel fibres or tubes, which had a particular distribution in reference to the surface of that organ. Malpighi also described nerves as composed of parallel cylindrical tubes, which he recognised to be filled with a fluid which he thought was of a serous nature; and modern microscopists, with all the aids of modern discovery and improved instruments, have not advanced one step farther. This fact is highly important; and shows that every one is not fitted for making such minute investigations. It requires the practice of a life to enable one to examine correctly. It requires one to begin with the simplest form of instruments and with low powers, and accustom himself to investigations with these, before he uses the more compound instruments and the higher powers. He requires also to be able to divest himself of all theories, otherwise he will never see minute objects but through that theory.

Modern research has done much towards ascertaining the mode in which nerves terminate. Rudolphi appears to have been the first who described them as terminating in a loop, which turned back on itself; and this fact has now been satisfactorily proved by the researches of Prevost and Dumas, Valentin, Emmert, Brechet, Burdach, Mandl, &c. In the case of the motor nerves, the last ramifications run parallel to each other, and generally also parallel with the muscular fibre, and, dividing into their primitive fibres, send these across the fibres of the muscle, when they either return to join the nervous branch they have left, or anastomose with a neighbouring one. The terminal loops of the nerve thus cross every muscular fibre. The nerves of sensation appear to terminate much in the same way, only that the loops are in general variously contorted before they return to reform a nervous twig by their reunion. In general, also, the primitive fibril does not return by the same branch from which it was given off, but joins an adjoining one. The mode in which the primitive fibres terminate in the brain has not yet been satisfactorily ascertained. Valentin asserts, they terminate in loops; but later observers, as Burdach and Longet, with the assistance of M. Mandl, have not been able to detect such a structure. Valentine must assuredly be
wrong in his statement, that the primitive fibres of the nerves which join the spinal chord are continued onwards to the brain, inasmuch as the nerves of the axillary plexus alone would form a chord thicker than the spinal chord itself at any point from which its various branches were given off. What then becomes of all the nerves given off below this point? This is one of the points on which microscopic anatomists have not as yet thrown much light. Purely anatomical considerations would lead us to infer that the greater portion of the fibres originate from the spinal chord itself, and do not pass up through it to the brain. That there is an intimate connection between the brain and the nerves no one can deny, but when we see that the spinal chord is not materially diminished in size after giving off the large nerves, which, on each side of the body, form the axillary plexus,—nay, when we find that the spinal chord above the origins of these nerves is of the same diameter as it is below the origins of these nerves, but is sensibly enlarged where they are given off, we are almost infallibly led to the conclusion, that many, if not most of the spinal nerves originate in the chord itself, and are not continued onwards to the brain.

Before leaving the structure of the nerves, there is one point to which we think sufficient attention has not been paid, and that is, whether the nerves of sensation do not, at their terminal loops, unite with the nerves of motion, just in the same way as do the veins with the arteries. No inquirer seems to have taken up this interesting point; but there seems nothing improbable in the idea, that such is really the case. We know that the agency of volition, whatever that be, is transmitted in one direction only, viz. from the nervous centres to the terminal loop of the nerve; and we also know that sensations are transmitted solely in the opposite direction, viz. from the terminal loops towards the nervous centres. Seeing that this is the case, we are unable to discover the use of the ultimate fibrils of the nerve bending on themselves, and returning to the brain or spinal chord, unless it were that the return chord conveys sensation from the organ to the brain or chord, the exit chord conveys volition from the brain or spinal chord to the organ. The extreme difficulty, or rather impossibility, of tracing by the knife the minute subdivisions of the nerves, has hitherto prevented us from satisfying ourselves completely on this point, but we have already seen sufficient to show that it is most probably the mode in which these two sets of filaments are connected in every case. Our researches were confined chiefly to the nerves of the face, because there the nerves of motion are easily distinguished from those of sensation, and may be traced separately, almost to their final distribution. After tracing a minute branch of the fifth and of the seventh pairs of nerves to a par-
ticular muscle, and carrying the dissection as minutely as the un-
assisted eye could guide, we continued it by means of fine needles
under the microscope, and on more than one occasion satisfied
ourselves that the terminal loops of the nerves communicated by
their one extremity with the sensatory portion of the fifth pair,
and by their other with the motor portion of the seventh pair.

If our observations are correct on this subject, we ought to find
that those nerves which preside over special sensations, as the
olfactory, optic, and auditory nerves, do not terminate in loops,
or bend back on themselves. Accordingly, we find that, excepting
Valentin and Breschet, who assert that all nerves terminate in loops,
neither Treviranus, Gottsche, Müller, Mandl, nor other recent
observers, have been able to detect the termination by returning
loops in these nerves of special sensation. We may add, that
we have also failed to detect the presence of returning loops in
these nerves. Reason, therefore, as well as our own observa-
tion, so far as it has yet gone, would seem to render it pro-
bable that the nerves of common sensation unite with those of mo-
tion in the same way as arteries do with veins; that is to say, that
each ultimate filament of a motor nerve, originating from the an-
terior column of the spinal chord, becomes a nerve of sensation
at its looped extremity, and, as such, returns to become united with
the posterior column of the chord. Nerves of sensation, we
know, are present in all parts of the body, but the microscope has
hitherto only detected in muscles nervous loops disposed in one
way, and the above explanation seems to be the only way of ac-
counting for the fact of these organs being possessed of sensation
as well as motion.

M. Longet, in his next chapter, details the chemical composi-
tion of the nervous substance, and, after quoting the observations
of chemists on this point, regards the results of M. Fremy as
those most entitled to credit. He found in the brain a white
matter, which he terms cerebric acid, cholesterine, a particular
fatty acid which he names the oleophosphoric, traces of oleine
and margarine, the usual fatty acids, albumen, and sulphur. M.
Fremy also found that considerable difference existed between the
composition of the cortical and of the white matter of the brain.
The fatty substances above named were also found in the white
substance; only traces of them existed in the cortical or gray mat-
ter. In fact, he states, that when the fatty matters were all re-
moved from the white matter of the brain, the residue presented
the same chemical characters as the gray matter.

So little attention has been paid to the chemical analysis of the
cerebellum, spinal marrow, ganglia, or nerves, that M. Longet
states they would all require to be repeated before any con-
cclusions could be drawn as to their similarity or dissimilarity in
composition with the brain.
The consideration of the nervous agency forms the subject of the sixth chapter. In order to explain the phenomena of physical life in man and the animals, most writers admit the presence of an imponderable agent, which has been variously designated, nervous principle, agent, or fluid, nervous force, active principle of the nerves, &c. Baron Cuvier thought it very likely that nerves acted by means of some imponderable fluid; but physiologists have been in vain attempting to discover the nature of this fluid. Some consider it as identical with the electric fluid; others that it is only analogous to it, and may, like the magnetic fluid, be but a simple modification thereof; while a third class consider it to be an agent sui generis. The first two opinions, which, in fact, are but modifications of one another, are based on the following conclusions:

1. Electricity can supply the nervous agency, in producing contraction of the muscles.
2. Wilson Philip caused food to be digested in the stomach of an animal whose pneumogastric nerves were cut, by passing a current of electricity through them.
3. Dutrochet has seen many muscular filaments produced in an emulsion of the white of eggs, by passing a current of electricity through it.
4. By placing needles in the nerves of man and of animals, Beclard, Beraudi, and Prevost have seen them become magnetic.
5. David, by means of nervous electricity, produced a deviation of the magnet in the multiplicator of Schweiger.
6. By means of the galvanometer of Peltier, M. Donné discovered in the living body electrical currents flowing from the skin to the mucous membranes, from the liver to the stomach, &c.
7. M. Lembert and Jobert having exposed the spinal column in living animals, saw small light threads attracted towards it.
8. Vassali-Eandi discovered free electricity in the blood, urine, and bile of different vertebrated animals, which, when concentrated by conductors, excited contractions in the muscles of the frog's thigh.
9. Nervous and electrical transmissions are equally rapid.
10. Electricity and nervous agency may be excited by heat, chemical action, frictions, percussions, &c.
11. Certain phenomena are equally produced by both, as elevation of temperature, expansion, decomposition of some substances, and the formation of others.
12. Electricity, when applied to the body, causes effects which seem exclusively regulated by nervous agency, as convulsions and muscular contractions.
13. Lastly, The identity of the two seems to be put beyond the power of dispute, by what occurs in the electrical fishes. According to some observers, the electrical shocks given by these animals are so dependent on innervation, that section or ligature of the nerves which go to the electrical apparatus, or removal of the posterior lobe of the brain, destroyed the electrical properties of the organs.

M. Longet then states that, even though the whole of the
above conclusions were proved, they would not demonstrate the identity of the nervous agency with electricity, but only show they were analogous. He then, at great length, proceeds to examine each argument separately, and adduces under each head all the experiments, pro and con, which have been made by different experimenters. The result of this inquiry, in almost every case, is to show that the facts may be very differently explained, and that simple mechanical or chemical irritations are quite as efficacious in exciting the nervous agency as electricity itself. He shows, from the experiments of others, that all parts of the body give out electricity; but he has never succeeded in causing the slightest magnetic power in a needle, by inserting it either into a nerve, or into the brain or chord themselves. As to the electrical apparatus in fishes, he shows that M. Matteucci proved that the electricity in the torpedo is given off in one direction only, and that tying the nerve sufficed to prevent the animal giving shocks, which it could not have done had the agent been truly electrical. He therefore infers that the electricity in those fishes which generate it, is most probably formed in the organ itself. He then shows that M. Person ascertained, by direct experiments, that nerves were inferior conductors of electricity to metals; that they were no better conductors than muscular fibre or other moist membrane; that destruction of the pulp of the nerve, though it completely destroyed the conducting power of the nervous agency, did not in the slightest interfere with the conduction of electricity; and that an electric current passes freely from the nerve to the muscle or adjoining parts, taking always the direct line between the positive and negative wires.

The conclusions which M. Longet very justly draws from his minute investigation of all that has yet been written on the subject is,

"1. There does not exist at present any direct and certain proof in favour of the hypothesis of electrical currents in nerves.

"2. Electricity and the nervous force are not identical.

"3. In the present state of our knowledge it would be rashness to affirm that they are totally different; and that they do not offer the slightest analogy.

"4. The neurilema, which transmits even feeble electrical currents, cannot transmit the nervous agency.

"5. Electricity is probably only a simple excitant of the persistent nervous force, and its action is probably the same as those of mechanical or chemical irritants."—Vol. i. p. 143.

For farther information on this interesting subject, we beg to refer to a paper "On the Nature of the Nervous Agency," in the present Number, which takes very decided views of the matter.

Such, then, is a short abstract of the first part of M. Longet's
work. The remainder is devoted to a very full detail of the anatomy and physiology of each individual portion of the nervous system. The anatomical details necessarily occupy by far the greater portion of this part of the work; but, under almost every head, after giving the special anatomy of the organ in question, the comparative anatomy of the same structure in the lower classes is also described, then the functions of the organ, and pathological facts. In this manner is described, not only the brain as a mass, but each individual part of it, as the tuber annulare, the pineal gland, the corpora striata, the corpus callosum, the optic thalami, &c. For the minute details of the anatomy and comparative anatomy of these individual parts of the nervous system, we must refer to M. Longet's work itself, merely noticing any new facts brought forward by the learned author, or any new light thrown by him on the physiology of the organs in question.

In describing the dura mater, M. Longet states that he has been able to authenticate the researches of Cruveilhier as to the existence of distinct nerves in its structure, a fact denied by many eminent anatomists. These nerves come exclusively from the trigeminal nerve; 1. from the large branch before its fusion with the Gasserian ganglion; 2. from the ganglion itself; and 3. from the ophthalmic nerve. The filaments which appear to come from the sympathetic really proceed from the ophthalmic branch.

The author then demonstrates, by a reference to several experiments of Magendie's, most of which he has himself repeated, that there exists a fluid in the spinal canal of every living vertebrated animal. This fluid is always met with in man, and is found in the cellular tissue, between the pia mater and arachnoid, and communicates directly with the fluid of the fourth ventricle. Many facts are mentioned to show the directness of this communication. The probable uses of this fluid appears to be to regulate the amount of pressure on the centres of the nervous system. Both Magendie and M. Longet found that, during inspirations, this fluid descended from the ventricles to the spinal chord, but, during expiration, again ascended to the ventricles. This observation is accounted for on the supposition that, during expiration, the blood stagnates in the venous sinuses of the spinal canal, and by its pressure forces the fluid of the chord back upon the brain; but, during inspiration, the venous blood flows freely, or is drawn towards the heart, and thus leaves the fluid to descend from the brain to the chord.

Many interesting points are touched on in considering the spinal chord of the inferior animals. M. Longet justly limits the name spinal chord to that portion of the nervous matter which is contained within the spinal canal, as, if the bulb were also to be reckoned as belonging to it, most of the cerebral nerves must also be considered as spinal, seeing they originate from it or from its
prolongations. It has been usual to attribute the want of a tail in man to the ascent of the spinal chord at a certain period of utero-gestation. The fact is undoubted; but M. Longet shows, from a reference to the comparative anatomy of the chord, that the conclusion is not legitimate. Birds, which of all animals have the shortest tails, have the spinal chord prolonged even to the coccygeal vertebre; whereas, in many monkeys with prehensile tails, and in the kangaroo, whose length of tail is undoubted, the spinal chord stops at the lumbar region, and does not even occupy the sacrum. In many fishes, again, notably in the Lophius piscatorius, the spinal chord ends at the third vertebra; whereas, in many monkeys with prehensile tails, and in the kangaroo, whose length of tail is undoubted, the spinal chord stops at the lumbar region, and does not even occupy the sacrum. In many fishes, again, notably in the Lophius piscatorius, the spinal chord ends at the third vertebra, the nerves alone being continued to the end of that fish's long tail. In many of the lower animals, in birds especially, M. Serres showed that enlargements occur on the spinal chord, either on the point from which the nerves go to the anterior or to the posterior extremities. If the animal were one which chiefly made use of its anterior extremities, as the eagle, swallow, &c., M. Serres showed that the dilatation on the spinal chord at that point exceeded that from which the nerves to the posterior extremities originated. But if the animal were one which used chiefly its posterior extremities, as many of the wading birds, the cassowary, ostrich, &c., the posterior dilatation greatly exceeded the anterior in size. M. Desmoulins and M. Longet have shown that there are exceptions to this rule, but the general fact remains uncontradicted. Something of the same nature is observed in man. The spinal chord, where it gives off the first cervical nerves, is somewhat contracted; but where it gives off the large branches which go to form the brachial plexus, the dimensions of the chord are notably increased.

It is to M. Longet that physiology is chiefly indebted for having ascertained, beyond the possibility of dispute, the functions of the anterior and posterior columns of the spinal chord, and has thus definitively settled the question as to whether the views of Sir Charles Bell or of Bellingeri are the correct ones. The numerous experiments, which M. Longet performed for the establishment of this much disputed subject, were all repeated several times before a commission of the Institute of France, and received their entire approval. Sir Charles Bell, it may be recollected, attributed the regulation of the voluntary motions to those nerves which originate from the anterior column of the spinal chord, while the sensations of the body were directed by those nerves which had their origin in the posterior column of that organ. Bellingeri, on the other hand, while he maintained that sensation was alone communicated through the nerves which originated from the posterior column, attributed to these nerves a motive power also, by virtue of which they regulated the movements of the muscles concerned in extension, elevation, and abduction. To the nerves which were
connected with the anterior columns, he attributed the regulation of those muscles only which were concerned in motions of flexion and adduction. According to his view, the anterior column of the chord formed the antagonist of the posterior chord, and it was only to those nerves which originated from the gray matter of the posterior chord that he attributed the power of conducting sensation.

The physiologists who have taken up this important subject since the papers of Sir Charles Bell and Bellingeri were published, have been far from being at one either as to the correctness of Sir Charles' views or of those of Bellingeri. Magendie, in his first experiments, published in 1823, espoused the views of Sir Charles Bell; but in his work on the Nervous System, published in 1839, admitted that the anterior column of the chord possessed "a very apparent sensibility." Schoeps, in 1823, and Rolando, in 1828, attributed the regulation of both sensation and motion equally to anterior and posterior chord; and Calmeil, in 1828, while he adopted the same views, was inclined, with Bellingeri, to attribute the conductors of sensation to the gray matter of the chord. Backer, in 1830, published a few very unsatisfactory experiments, from which he deduced that Sir Charles Bell's views were incorrect. Seubert, in 1833, tried in vain to deduce any conclusions from the results of numerous experiments he performed; and Müller, in 1840, in his work on the Physiology of the Nervous System, after detailing the results of numerous experiments, declares, that "the hypothesis which regards the anterior columns of the spinal chord as motors, and the posterior as sensitive, rests on no satisfactory facts, either experimental or pathological."

Such was the unsatisfactory state of the question when M. Longet, in 1840, began that brilliant course of experiments by which he to the full proved the correctness of the views of the British physiologist; and when we find among the names of those who witnessed his experiments, and were satisfied with their correctness, those of Flourens, Blainville, Breschet, Lallemand, Marshall Hall, Cruveilhier, Bouillaud, Blandin, Gerdy, &c., we may rest assured that their results were as conclusive and satisfactory as the nature of the subject would allow.

M. Longet's experiments were made on adult dogs, as he found that in them the size of the different columns of the chord enabled him to distinguish between them and isolate them far more satisfactorily than in young animals, and also that the irritation of one column by galvanism or mechanical agency had less tendency to be propagated by any reflex movement to the other parts of the chord. When he exposed the chord in the lumbar region, and after carefully opening its dura matral covering, cut it across,
he found that no irritation of the free extremity of the posterior columns of the chord could excite muscular contractions; but when the extremities of the same columns, still in connection with the brain, were touched or irritated by galvanism, the animal experienced the most excruciating pain, as expressed by its cries. When the free extremities of the anterior columns were irritated, the posterior extremities, which were previously hanging paralysed, were thrown into strong convulsive contractions; but irritation of the extremities of the anterior columns still in connection with the brain, failed to excite any sensation, or any convulsive movement. These facts prove, adds M. Longet, that sensation is conveyed only from the nerves to the brain, and voluntary motion from the brain to the nerves. The same results followed when the spinal chord was exposed in the neck. When the chord was cut across, voluntary motion and sensation of the whole body were destroyed. When the free extremities of the anterior columns of the chord were irritated by galvanism, convulsions of the whole muscles of the body were occasioned, but no effect followed irritation of the free extremities of the posterior columns. Irritation of the extremities of the anterior columns, still in connection with the brain, failed to excite any sensation; that of the posterior columns produced excruciating pain. The anterior columns, then, of the spinal chord, and the nerves proceeding from them, preside over voluntary motion. The posterior columns of the chord, and the nerves in connection with them, preside over sensation.

As to the function of the lateral columns of the spinal chord, M. Longet has not been able to arrive at any determinate conclusions. That the nerves which originate from this portion of the chord go to those muscles which are concerned in the respiratory function, is undoubted; and section of the chord above their origin arrests the respiratory motions. He has never yet succeeded in cutting across the lateral columns of the chord without injuring the adjoining columns, so that no certain conclusions could be drawn from his experiments. He states, however, a very strong negative fact, which would tend to establish the views of Sir Charles Bell, viz. that when he cut the anterior columns of the spinal chord at the root of the neck, the respiratory motions continued as before, though the motion of all other parts supplied by nerves below the point of section was arrested. Even the neutral result of his experiments thus goes to prove the correctness of so much of Sir Charles Bell’s views, as that the lateral columns of the spinal chord and the nerves proceeding from them preside, to a certain extent, over the motions of respiration. M. Longet, however, demonstrates that Sir Charles Bell allowed his theory to lead him into some assertions which will not bear examination. Thus Sir Charles stated that the pneumo-gastric and
glosso-pharyngeal nerve originated from these lateral columns of the chord, and were consequently respiratory nerves, possessed neither of the powers of sensation nor of voluntary motion. It is now, however, fully ascertained that these very nerves originate from the sides of the restiform bodies, in the same groove from which all the nerves of sensation originate, and the distribution of these nerves to the mucous membrane of the larynx, trachea, bronchii, oesophagus, stomach, tongue, pharynx, &c., proves them to be so far, at least, sentient nerves. M. Longet, however, satisfactorily ascertained, that neither the lateral columns nor the nerves originating from them are possessed of sensation.

A little further on, when considering the physiology of the medulla oblongata, the author endeavours to prove, chiefly from the experiments of Flourens, that that organ specially presides over the respiratory function. But after a rather severe criticism of Sir Charles Bell's order of respiratory nerves, he is forced to admit that "the functions of the columns formerly described as the intermediate or lateral columns of the spinal chord, influence respiration." And he then adds, that the structure of these columns, and the bodies which are their continuations in the medulla oblongata (the olivary bodies) is such as to render them "fitted to represent a centre of innervation in the spinal chord;" for while the pyramidal and restiform bodies are exclusively composed of white fibres, a considerable quantity of yellowish-gray matter, abundantly supplied with arteries, is found in the centre of these parts. If we exclude, then, the pneumo-gastric and glossopharyngeal nerves, which ought never to have been included among the respiratory nerves, the original division of Sir Charles Bell will remain as a physiological truth.

Our limits will not permit us to enter into all the physiological points of interest discussed at length in M. Longet's work. After a full examination of all the experiments and arguments as to whether the spinal marrow influences the movements of the heart and of the circulation, the author states that no unanswerable arguments have yet been brought forward to prove that the spinal chord exerts no influence on the movements of the heart in the adult; while, on the other hand, numerous facts, founded on experiment and the phenomena of disease, have established the necessary connection of the spinal chord with the maintenance of the circulation. He in like manner shows that lesions of the chord affect the integrity of nutrition and secretion, and also, to a certain extent, of calorification.

M. Longet next enters at considerable length into the inquiry, whether the spinal chord is to be considered simply as an organ of conduction, like the nerves, or whether it forms a true centre of nervous energy. From a consideration of all its phenomena, he
shows that it must be considered as a nervous centre, and that, in some cases of acephalous foetuses, its nervous centrality is remarkable. It is to its property as a nervous centre he attributes the existence of that reflex function first described by Prochaska, demonstrated by Sir Charles Bell, and more lately taken up by Marshall Hall, by virtue of which, sensations propagated to the chord are reflected on the motor nerves. M. Longet is far, however, from agreeing with Marshall Hall's "pure hypothesis" of the existence of special excito-motory nerves, but is inclined, in so far as the phenomena connected with the spinal chord are concerned, to adopt the explanations of them given by Müller.

The pathological facts by which M. Longet establishes the fact of the anterior columns of the chord presiding over voluntary motion, and the posterior columns over sensation, are of extreme interest, and well deserve careful perusal. Cases are related where the anterior columns were more or less completely diseased, and in them all the parts supplied by nerves below the morbid structure were deprived of the power of motion, while sensation remained unimpaired. Where the lesion was confined to one side, that same side of the body was alone affected. Other cases are related where the posterior columns were alone found diseased, and in them sensation was destroyed, but the power of motion remained unimpaired. One very curious case is related in which only an obscure impression or sensation was conveyed to the brain, and in this case it was found that several white filaments had escaped the diseased action which involved the other parts of the posterior columns.

As to the functions of the tuber annulare, M. Longet states that it is not only a conducting organ, but also a centre of innervation. Irritation of the posterior part of the tuber causes acute pain, a fact easily explained by its anatomical relations, seeing that the restiform bodies, which are continuations of the posterior columns of the chord, send fibres to this part. Injury of the anterior part of the tuber causes no pain, but convulsive motions of the whole limbs and face. The tuber annulare is therefore one of the highest points of the brain, which, on being irritated, gives rise to muscular contractions; for neither the optic thalami, the corpora striata, the cerebrum, or cerebellum, exert any direct influence on muscular contractions. It is a remarkable fact, as showing the importance of this part of the nervous system, that in rabbits and young dogs, the hemispheres of the brain, the corpora striata, the optic thalami, the tubercula quadrigemina, and even the cerebellum, may be removed, and yet the animals live for upwards of an hour, their circulation and respiration going on regularly, and their common sensations so entire as to cause the animals to cry with pain if the tail is strongly pinched. If one of the hairs of its moustache be pulled, or ammonia be held to
its nose, it makes an effort with its fore-feet to remove the source of irritation. If the tuber be removed, every function is abolished with the exception of respiration and circulation, which continue for a little while.

M. Longet confirms the results of Magendie's experiments as to injury of the peduncles of the cerebellum causing a rotatory motion of the animal. He shows, however, from a reference to his own and M. Lafargue's experiments, and also to a few pathological cases, that when one crus is injured the rotatory movement is in the opposite direction of that indicated by Magendie. Thus, if the right crus be cut across, the animal turns round from the right to the left, as, in consequence of the crossing of the fibres, the opposite side of the body is affected with the paralysis. The same was witnessed when the peduncles of the cerebrum were cut across. The animal ran round in a circle, having the injured side of the brain on the outer side of the circle, or, in other words, with the semi-paralysed side of the body towards the centre of the circle.

From a careful examination of fourteen cases of disease of the tuber annulare in man, cases which he records, M. Longet draws the conclusion that epileptic fits generally precede the paralysis of the trunk and limbs; that the patients always die of asphyxia; and that, when the lesion is situated on one side only, the opposite side of the body is affected, thus showing the decussation of the fibres.

The tubercula quadrigemina are shown to be continuous with the optic thalami anteriorly and with the olivary bodies behind, and that on their integrity depends to a great extent that of the sense of vision. The true nature of these bodies has been long misunderstood. Even in the lower animals they have been variously described as the cerebral lobes, optic thalami, tubercula majora, tubercula olivaria, &c. By a reference to the comparative anatomy of these organs, and to their development in the fetus, M. Longet shows that they are continuous with the optic nerves, as had been demonstrated previously by Carus and Tiedemann. In all M. Longet's experiments removal of the tubercula quadrigemina caused total blindness in animals and birds; results which were perfectly in accordance with those of M. Flourens, but at variance with those of Magendie published in 1836. M. Magendie has since, however, admitted the influence of these bodies on the sense of vision. M. Longet found that the sense of vision remained after removal of the cerebral lobes. When this was done to some mammalia and birds, and the animals placed in a dark apartment, whenever a light was brought near the eyes, the iris contracted, often the eyelids winked; and the animal followed the motion of the light with its eyes. Irritation of the surface of the tubercula did not give rise to any expressions of pain in M. Lon-
get's experiments, but when the interior was injured, the animal expressed by its cries the most acute suffering. Mechanical irritation of the surface, or even of the gray matter, never caused convulsive muscular contractions: in order to produce this effect it was necessary to continue the incision to the medullary fibres. A number of interesting cases are detailed in which, during life, a greater or lesser amount of blindness was observed, and after death the tubercula were found more or less diseased; convulsive, epileptic, or paralytic symptoms often attended along with the blindness, facts sufficiently accounted for by other morbid degenerations in other parts of the brain.

In noticing the physiological action of the optic thalami, M. Longet remarks, that they have far less influence on vision than the tubercula quadrigemina. As to their power of regulating the movements of the upper extremities, as was endeavoured to be proved by Foville and Serres, neither his own experiments, nor the numerous pathological cases of lesion of these lobes, recorded by Andral, seem to countenance such an idea. Andral relates the history of 75 cases in which the lesion was limited either to the corpora striata or to the optic thalami. In 40 of these the paralysis affected both the upper and lower extremities of one side, and of this number the lesion was limited in 21 cases to the corpora striata, and in the remaining 19 to the optic thalami. In 23 of the 75 cases the paralysis was limited to one of the upper extremities; in 11 of which the lesion was met with in the corpora striata, and in 10 in the optic lobes. In 12 of the 75 cases the paralysis was limited to the lower extremity; the lesion being found in 10 cases in the corpora striata, and in 2 in the optic lobes. From these facts M. Andral drew the conclusion, that, in the present state of our knowledge, we cannot assign in the brain distinct seats of movement to the upper and lower extremities, and M. Longet quite agrees with him. It may be added, that all experiments demonstrate that irritations, both of the corpora striata and optic thalami, neither excite sensations nor muscular contractions.

We must refer to the work itself for many interesting anatomical points connected with the pineal gland, septum lucidum, fornix, corpora albicantia, tuber cinereum, pituitary gland, ventricles, and cerebral lobes. Almost nothing is known as yet relative to the physiology of these parts, nor does M. Longet appear to have succeeded in throwing any additional light on this obscure subject. This only seems to be pretty generally agreed on, that the cerebral lobes preside over the intellectual and perceptive faculties and instincts. In idiots it is generally the cerebral lobes alone which are atrophied; all the other parts of the brain are in a natural condition, and have a normal development.
This fact, therefore, tends to prove that the higher faculties of the soul are intimately connected with the integrity of the cerebral lobes. Many anatomists, and among them Bichat, maintained that if the volume of the two hemispheres of the brain differed, there existed considerable intellectual inferiority, and such a defect or imparity of reasoning power as to prevent the individual from arriving at fixed conclusions relative to things. It is a remarkable fact, that Bichat's own brain proved the fallacy of this assumption,—for the one cerebral lobe was, after his death, found smaller than the other. M. Longet, however, narrates three instructive cases in which the intellectual faculties were perfect, and yet in which the cerebral lobes were found of very different dimensions. In the first case the right hemisphere of the brain was a half smaller than the left; in the second, the left hemisphere was at least a third less than the right; and in the third, the whole right hemisphere of the brain had disappeared, and its place been supplied by a watery fluid. M. Longet narrates the history of many cases of injury of the cerebral lobes, in which loss of substance took place, and yet the intellects remained entire. After a full review of all these cases, he arrives at the following conclusions:

"In man the hemispheres of the brain may be partially disorganized, or even undergo a notable loss of substance, without being followed by any appreciable affection of the intellectual powers.

"The same negative result is observed when the disorganization or loss of substance takes place at the expense of the anterior or posterior lobes of the cerebral hemispheres.

"The exercise of the sensorial function continues unimpaired notwithstanding such grave lesions.

"On the other hand, the locomotive functions are more or less severely affected, and epileptic fits frequently ensue."—Vol. I. p. 679.

The learned author next endeavours, by a reference to pathological facts, to experiments on living animals, and to the comparative anatomy of the brain in the lower animals, to ascertain whether there is any truth in the phrenological doctrine of the localization of the intellectual and moral faculties in particular parts of the brain. After a very full examination of the question in all its bearings, he shows that, however specious many of the reasonings of the phrenologists may be, they are quite at variance with recorded pathological facts, with the results of direct experiment, and with the comparative anatomy of the brain and skulls of the lower animals.

In mentioning the weight of the cerebellum, M. Longet takes occasion to refer to the interesting researches of M. Leuret as to the influence of castration on the development of this organ. M.
Leuret, instead of trusting to his eye-sight alone to determine the relative bulk of the cerebellum, weighed both it and the brain of a number of stallions, mares, and castrated horses or geldings. Contrary to the assertions of Gall and all phrenologists, M. Leuret found that the size of the stallion's cerebellum was less than that of the mare's, and both of these less than that of the gelding's. Thus the average weight of the cerebellum in the mare and stallion was only 61 grammes, while in the gelding it amounted to 70 grammes. The cerebrum of the stallion, however, generally weighed more than that of the gelding, being on an average 433 grammes in the stallion, but only 419 grammes in the gelding. M. Flourens, on the other hand, found that removal of the cerebellum of a cock did not prevent that animal manifesting the usual sexual desires; and eight months after removal of the cerebellum the testicles were found of enormous size.

In a subsequent part of his work, M. Longet enters more fully into the examination of the phrenological doctrine of the influence of the cerebellum on the generative system, and proves that it is at variance with recorded pathological facts and with the comparative anatomy of that organ. M. Longet not only relates a very instructive case, in which a girl given to masturbation had no cerebellum, but he shows that where erection of the penis is observed during disease of the cerebellum, it is always dependent on some pressure or morbid alteration of the origin of the spinal chord, and that erection is a more common symptom in many injuries of the spinal chord than it is in disease or injury of the cerebellum. It is besides to be remembered, that Calmeil, Magendie, and Flourens found that removal of the cerebellum did not destroy the sexual desires in animals, but, on the other hand, they found that removal of the cerebral lobes produced this effect.

In all M. Longet's experiments, as in those of Flourens, Magendie, Bouillaud, Calmeil, and Hertwig, the cerebellum appeared insensible to ordinary irritations, nor did injury of its substance cause any convulsive muscular action. Even its removal did not extinguish the power of motion, but only produced a want of harmony in the movements of the animal. In man, however, injury or disease of the lobes of the cerebellum has been found to be attended in many cases with loss of motion of the opposite side of the body from that of the lobe affected, and M. Longet relates several such cases. M. Andral, however, who has collected no fewer than 93 cases of disease of the cerebellum, agrees that the want of harmony in the movements was the chief physiological fact which could be deduced from them. Pathology thus bears out experimental physiology in proving the cerebellum to be the organ which presides over the regulation or harmony of the
The author has collected a number of cases of diseased cerebellum in man, for the purpose of ascertaining whether the intellects are generally affected, or to what extent. The general result has been, that, even in cases where tumours or abscesses have compressed or materially destroyed the cerebellum, the intellects have been very little affected, and the sensations scarcely at all.

The movements of the brain next engage the author's attention; and as the subject is one which has in all ages been the object of inquiry to the physician, it is treated at considerable length. It is well known that when the soft fontanelles on an infant's head are examined, the brain is seen to be the subject of an alternate elevation and depression. The same is observed in the adult, if the bones of the cranium be so injured as to expose the brain or its membranes. The causes of this movement have given rise to much disputation, for while some maintained that it was due to the influence of respiration, others have attributed it to the arterial pulsations, while a third class have asserted that these two kinds of movement subsisted simultaneously. These, as well as the various mechanisms by which the movements were supposed to take place, are discussed at great length by M. Longet, and the conclusions which he arrives at, while they differ somewhat from those of most late writers on the nervous system, seem to be alone those to which reason and experiment lead. He shows, by a reference to incontrovertible experiments, that so long as the skull is entire and solid—so long as no atmospheric pressure is exerted on the soft surface of the brain, no distinct movement of elevation or depression is observed. The moment, however, atmospheric pressure is allowed to exert its force on the surface, distinct motions of elevation and depression are observed synchronous with the respiratory acts. In addition to this movement, however, distinct pulsatory movements are observed synchronous with the arterial beats. For the able review of the different theories on this subject we must refer to M. Longet's work; but his conclusions are as follows:

1. The brain does not move in the adult so long as the skull is unbroken. It augments its mass during expiration; it diminishes its mass during inspiration, but its volume never varies.
2. It moves in infants so long as the sutures of the cranium are not united. It moves also when the parietes of the cranium of the adult have been destroyed for a greater or lesser extent by pathological causes, or by operations.
3. In all these cases the movement is due to the alternate turgescence and depletion of the vessels of the brain, and not to a locomotion of the organ. Locomotion of the brain is impossible.
4. These movements are of two kinds. It is easy to satisfy
ourselves in infants, that the one corresponds with the contrac-
tions of the heart, the other with the respiratory movements. 
These last are the most marked.

5. The turgescence or elevation of the brain corresponds with 
the expiration. It is produced by the stagnation of the venous 
blood in the encephalic veins, and by the greater afflux of arterial 
blood. The depression of the brain corresponds with the inspi-
ration. It is produced by the afflux of the encephalic venous 
blood towards the thoracic organs, and by the concomitant lower-
ing of the arterial circulation.—Vol. i. p. 790.

The influence of the circulation on the functions of the ence-
phalon are next noticed. After referring to several cases in which 
death has followed the application of a ligature to even one of the 
carotid arteries, M. Longet shows that such is not the common 
result, but that, in very numerous instances, phenomena are pro-
duced which can alone be attributed to the derangement of the 
cerebral circulation. These are dimness or loss of vision in the 
eye of the side operated on; hemiplegia of the opposite side of 
the body; and very frequently notable impairment of the intel-
lectual faculties. Syncope or convulsive fits of coughing have also 
been noticed the moment the ligature was tied.

In animals, as might be expected, such serious symptoms are 
not produced by ligature of one carotid artery. Even both ca-
rotid arteries have been tied in dogs without producing notable 
disorder in the cerebral functions. Ligature of the vertebral ar-
teries in the lower animals gives rise to symptoms more nearly 
resembling those observed in man, when one or both carotids have 
been tied. In Sir Astley Cooper’s experiments, when one verte-
bral artery was tied, dyspnoea was produced; and when the ligature 
was tightened on the second, the difficulty of breathing was much 
increased, and the anterior extremities became paralytic. The 
animal died on the seventh day after the operation. Sir Charles 
Bell found that ligature of the vertebral arteries of the rabbit, or 
even their compression, produced instant death; and in Flourens’ 
experiments, though he could tie the two carotids and one verte-
bral with only the effect of producing coma and paralysis, yet in-
stant death followed the ligature of the other vertebral artery. In 
dogs, however, in whom pretty free inosculations occur between 
the vertebral and carotid arteries, Sir Astley Cooper found that 
both sets of vessels might be tied, and yet the animal, after a 
while, recover. Thus in one dog, in which he tied the same day 
the two carotids and two vertebral arteries, coma, stupor, hemi-
plegia, and convulsive movements were produced; but within three 
days these symptoms began to abate, and the animal recovered. 
In another instance the vertebral arteries were tied eight days after 
the carotids; paralysis of the fore extremities was instantly pro-
duced, but the animal was able to run about the next day. In a third dog, however, in which the vertebral arteries were tied nine days after ligature of the carotid, instant death was the result.

The remainder of M. Longet's work is devoted to the anatomical examination of the various nerves of the body; but as mere anatomical details prove of little general interest, we must limit our further notice to some points of physiology connected with one or two of the cerebral nerves.

Finding fault with the division of cerebral nerves proposed by Sir Charles Bell, as inconsistent, in more cases than one, both with their anatomical characters and physiological uses, M. Longet divides the cerebral nerves into three orders; 1st, Nerves of special sensation, (olfactory, optic, and auditory); 2d, Nerves of general sensibility, (ganglionic portion of the trifacial, the glossopharyngeal, and pneumo-gastric); 3d, Nerves which preside at one and the same time over voluntary and respiratory motions, (the common motor nerve of the eye, the trochleator or pathetic nerve, the non-ganglionic portion of the trifacial, the external motor of the eye, the facial, the spinal, and the hypoglossal nerves.)

The experiments of M. Magendie, which M. Longet has repeated, satisfactorily prove that the nerves of special sensation transmit to the brain those sensations alone for which nature destines them. Thus the olfactory and auditory nerves may be cut or torn without causing any pain. Even the irritation of galvanism only causes flashes of light to be perceived by the optic nerves, and sensations of sound by the auditory nerves. We know absolutely nothing relative to the cause of this difference in the transmission of the sensations, for the microscope can detect no difference between the nerves of special and those of common sensation. We are, therefore, perfectly in the dark as to whether their aptitude to receive these different impressions resides in the nerves themselves, or in the parts of the brain with which they communicate. M. Longet seems inclined to think that both of these may be the case,—that they may have a different structure as well as a different origin from the other nerves of sensation.

In noticing the experiments of Magendie, who endeavoured to prove that the sense of smell is in part directed by the branches of the fifth pair of nerves, which go to the nose, M. Longet shows that that physiologist mistook common sensation for the special sense of smell. When the olfactory nerves were destroyed, the sense of smell was destroyed, but the animals still remained sensible to the impression produced by the vapours of ammonia, or of strong acetic acid. But these same agents produce the same action when applied to the conjunctiva of the eye. The action produced was, therefore, nothing more than an excitation of the common sensibility of the part which is regulated by the fifth pair.
of nerves. It is true that when the fifth pair of nerves is diseased the sense of smell is greatly impaired, because, adds M. Longet, the secretion of the nasal mucus is deranged, and on its quality depends to a great extent the perfection of the sense of smell. Many interesting pathological cases are related, illustrative of these views, and satisfactorily prove that the sense of smell depends on the integrity of the olfactory nerves, the general sensibility of the nose to that of the fifth pair.

In enumerating the functions of the fifth pair of nerves, M. Longet relates a very satisfactory experiment which he has many times performed, which demonstrates that the fifth pair presides over the general sensibility of the face and parts to which its ramifications extend. He, with great care, cut the fifth pair of nerves across within the cranium of a rabbit, and when this was done, so carefully as not to injure either the seventh pair or the brain itself, it produced exquisite pain, and was followed by instant loss of sensation of all the one side of the face, so that the eye of the animal could be cut out, its teeth pulled out, its face cut or burned, without its exhibiting the slightest sensation of pain. In several experiments in which this operation has been attempted by others, the brain, or seventh pair of nerves, have been injured, hence the discordant results at which they arrived. In all M. Longet’s experiments the movement remained unpaired so long as the lesion was confined to the fifth pair of nerves.

Section of the fifth pair of nerves leads to rapid disorganization of the eye, and consequent loss of vision. Even within twenty-four hours after the section, the cornea has commenced to become opaque; within the second day the conjunctival covering is inflamed, and is secreting puriform matter; in about eight days the cornea becomes detached from the sclerotic coat by ulceration, after which the humours become turbid, more or less opaque, are discharged, and the eye shrinks. M. Longet states, “vision is never abolished, but as a consequence of the opacity of the cornea,” but that even from the first it appears to be somewhat weakened. A very curious fact, noticed, but not explained, by Magendie, is, that when the fifth pair of nerves is cut across in the temporal fossa at the level of the semilunar ganglion, the nutrition of the eye is rapidly impaired in the way above mentioned; but if the section be made within the cranium near its origin, and before it traverses the petrous portion of the temporal bone, such an effect is very partially produced. M. Longet explains this phenomenon by attributing the disorganization, in the one case, to the section or injury of the semilunar ganglion, and the branches of the sympathetic, parts which are not hurt when the section is made within the cranium; and this opinion is supported by the fact, that when section is made of the cervical por-
In noticing the influence of the fifth pair of nerves on the sense of taste, M. Longet shows that section of the branch of the fifth pair, which goes to the tongue, destroys the tactile sensibility as well as the perception of taste over all that portion of the tongue to which the nerve is distributed. When the lingual branch of the fifth nerve was cut, the anterior portion of the tongue could be torn, cut, or burned without the animal exhibiting any sensation of pain. This nerve, however, only supplies the anterior two-thirds of the tongue, the posterior portion of that organ having tactile sensibility and its perception of taste guided by the glosso-pharyngeal nerve. The organs of taste have thus no special nerve for the manifestation of the sense of taste. This subject M. Longet examines at some length, and satisfactorily proves that the exclusive views of MM. Panizza and Magendie, the first of whom attributed the sense of taste to the glossopharyngeal alone, the other to the lingual branch of the fifth nerve alone, are not correct; but that the sense of taste and the general sensibility are at once supplied by the two nerves, the one supplying the back part of the tongue, the other the anterior and middle portions of that organ.

The author then reviews what has been at various times advanced, regarding the possibility of the fifth pair of nerves supplying the place of the special nerves of smell, of vision, and of hearing. After a very full examination of the subject, he arrives at the conclusion that it cannot, but that its integrity is required for the free development of these various senses, inasmuch as it regulates the secretions and nutrition of the organs which are fitted for the manifestation of these senses.

The non-ganglionic portion of the fifth pair of nerves is shown to have nothing to do with the respiratory function, as Sir Charles Bell maintained, nor yet to be distributed to the buccinator or orbicularis oris muscles. It is distributed to the temporal, external and internal pterygoid, mylo-hyoid and other muscles which raise, depress, and move laterally the lower jaw, and act on the soft palate. This distribution, therefore, while it disproves the assertions of Sir Charles Bell, also satisfactorily demonstrates the fallacy of Bellingeri's views of the antagonism of the nerves originating from different parts of the spinal columns, inasmuch as this one branch supplies at once both elevators and depressors of the lower jaw,—supplies muscles which are antagonists to one another.

A number of interesting pathological cases are recorded, in which the trigeminal nerve was diseased, and the symptoms which
were observed during life confirm, in almost every particular, the conclusions at which M. Longet has arrived relative to the functions of this nerve.

The glosso-pharyngeal nerve is proved to be simply a nerve of sensation, from its point of origin in the continuation of the posterior grove of the spinal chord, from its ganglionic character, and from the results of direct experiment. M. Longet, however, demonstrates that, in consequence of inosculations with the facial nerve, and pharyngeal branch of the spinal accessory nerve, it also exhibits, below its ganglionic portion, some phenomena of a nerve of motion. The glosso-pharyngeal nerve, along with the lingual branch of the fifth pair, presides over the sense of taste. M. Longet's experiments on this subject are the only ones which have satisfied us on this point. These of Dr Alcock and Dr John Reid were not free from objection. After section of this nerve on both sides, they introduced bitter substances into the mouth of the animal and found them to be instantly rejected. And how, indeed, could it be otherwise? The whole anterior three-fourths of the tongue, receiving its gustatory twigs from the trifacial nerve, transmitted the bitter taste to the sensorium quite independent of the cut nerves. M. Longet, however, after section of the nerves, applied the bitter substance to the back of the tongue,—to that part alone supplied by nerves from the glosso-pharyngeal trunk,—and found the animal quite insensible to the presence of the bitter drug. If the smallest portion, however, by accident, touched the anterior portion of the tongue, the animal instantly manifested by its motions the presence of the bitter substance, as had always occurred in Dr Alcock's and Dr John Reid's experiments. When the bitter substance was applied to the back of the tongue before the nerves were cut, the sensation of bitterness was instantly perceived.

At the same time that M. Longet thus satisfactorily demonstrates the influence of the glosso-pharyngeal nerve on the sense of taste, he shows how former experimentalists, as Magendie and Alcock, have allowed themselves to be deceived in attributing difficulty of swallowing to section of this nerve. From a reference, indeed, to M. Magendie's own experiments, and the physiological effects following the section, he renders it next to certain that that physiologist had cut across the pharyngeal branches of the spinal accessory nerves instead of the glosso-pharyngeal nerves. In all M. Longet's experiments, provided the section were limited to the glosso-pharyngeal alone, the general and special sensibility of the back part of the tongue and palate were alone affected; but if, from the animal resisting strongly, some of the pharyngeal branches of the spinal accessory nerves were injured, deglutition was instantly rendered very difficult. The slight contractions remarked in the pharyngeal muscles, when the glosso-phary-
ryngeal nerve is irritated in the throat, is shown to be dependent on the presence of a few motor filaments which are united with that nerve, and not to any supposed reflex action, as some have supposed.

It is demonstrated that the pneumo-gastric and spinal accessory nerves, which form but one trunk when they leave the skull, stand, in their origins, in the relation of posterior and anterior roots of spinal nerves, the pneumo-gastric roots being, from their origin and functions, nerves of sensation, the spinal accessory nerves of motion. M. Longet, both by a copious reference to the experiments of others and to his own researches, establishes the double function of most of the branches of this compound nerve. He at the same time shows that, while some branches are chiefly composed of sensitive filaments, others are more especially formed of motor filaments. The many interesting points connected with the functions of this pair of nerves, such as their influence over the motions of the larynx, on the voice, on respiration, on the arterialization and fluidity of the blood, on the heart, oesophagus, stomach, &c., are reviewed with great ability. He shows how some physiologists have allowed themselves to be deceived as to the section of the recurrent nerves not completely abolishing the powers of voice. In adult animals it invariably abolishes all power of voice; but in young animals, from the peculiar conformation of the larynx, a sharp cry may still be elicited, from the rapid passage of the air over the passive chordae vocales. Section of the recurrent nerves, which M. Longet shows supplies movement to the constrictors as well as the dilators of the glottis, contrary to the opinion of Magendie, does not necessarily cause speedy death by suffocation in adult dogs, but it does so in the young. This is accounted for by the peculiarities of the larynx in all young animals, which is then margined as it were by membranes, and scarcely supported by the still soft cartilages. When the recurrents, therefore, are cut, the whole parts are paralysed, the glottis is closed by the rapid rush of air to the lungs, and asphyxia is the consequence. In the adult animal, however, the parts are sufficiently rigid to resist, to a certain extent, the atmospheric pressure when the chest is dilated; in fact, M. Longet has kept dogs alive for five weeks after section of the recurrent nerves, and, when killed after this time, their lungs were found quite permeable, and exempt from all traces of engorgement.

The pneumo-gastric nerves, besides regulating the general sensibility of the trachea, bronchia, and lungs, are shown to influence the respiratory phenomena, in so far as the contraction and dilatation of the pulmonary tissue is concerned, and the proper arterialization and fluidity of the blood. Inasmuch as the respiratory efforts continue after section of these nerves, M. Longet attributes
to the influence of the medulla oblongata the sensation of the necessity for respiration,—a sensation which is at once abolished by injury of that organ. The opinion of Professor Mayer of Bonn, that the fibrinous clots found in the heart and blood-vessels after death were attributable to the loss of influence of the pneumo-gastric nerves, has been often disputed. M. Longet having ascertained that dogs in whom these nerves were cut lived a certain time, and constantly presented these fibrinous clots in their heart and blood-vessels after death, cut these nerves in a number of dogs, and killed them at various periods after the section, and instantly examined them. After twenty-four hours, there was no pulmonary engorgement, and the blood was very fluid. After thirty-six hours, black, very soft clots, resembling black currant jelly, were found in the auricles and ventricles of the heart, and in the trunks of the pulmonary artery and aorta. Pulmonary engorgement had also begun in certain points. On the third, but especially on the fourth day, the pulmonary engorgement was very marked, and solid, yellowish coloured clots were found filling the auricles and ventricles, and insinuating themselves between the columnae carnea; they also extended to the pulmonary arteries and veins, even to their ramifications. These experiments were performed on thirty dogs. After section of the pneumo-gastric nerves, no dog lived longer than five days; many died on the second and third days. Rabbits never lived beyond thirty-six hours after section of these nerves.

As to the effect of section of the pneumo-gastric nerves on the pulsations of the heart, M. Longet remarks, that, after the section, the pulsations were invariably increased in frequency. In place of 60 or 70 per minute, they averaged 150. The pulsations, however, are irregular and incomplete; but, as the blood which reaches the heart is somewhat imperfectly arterialized, he is not satisfied that this frequency and irregularity of the pulsations ought to be attributed to want of nervous power alone. He is, however, inclined to believe that it is by means of the cardiac branches of the pneumo-gastric nerves that mental emotions affect the pulsations of the heart.

The contradictory results at which physiologists arrived relative to the influence of the pneumo-gastric nerve on the stomach, induced M. Longet to perform a number of experiments for the purpose of ascertaining whether irritation of that nerve excited muscular contractions of that organ. He found that, when the stomach was empty, no contractions were excited on irritating the pneumo-gastric nerve in the neck, by mechanical or electrical agents; but that, when the stomach was filled with food, such irritation not only excited contractions, but occasionally produced the hour-glass contraction to such an extent that it seemed as if
strangulated at its middle by means of a cord. This contraction caused the more fluid aliments to pass out by the pyloric orifice. These results sufficiently account for the varied statements of different physiologists as to the influence of this nerve over the muscular action of the stomach; for while some hold that its irritation produces no sensible effect on this organ, others assert it always does. M. Longet concludes from his experiments, that section of the pneumo-gastric nerve does not prevent the sensations of hunger and thirst being felt, nor yet food from being digested. He simply found that the gastric juice was secreted in lesser quantity, and that the digestion was rendered extremely slow. As the experiments of Spallanzani and Beaumont have proved that simple rubbing the internal surface of the stomach causes a secretion of gastric fluid, and as section of the pneumo-gastric nerves prevents the usual rubbing action of the coats of the stomach against the food, M. Longet accounts in this way for the diminished quantity of fluid secreted, without supposing that this secretion is otherwise under the influence of these nerves. This diminished supply of gastric fluid, and the total absence of all the natural movements of the stomach on the food, may, he thinks, to a great extent, account for the very slow, yet, after all, complete digestion of the food in the stomach of the animal whose pneumo-gastrics have been divided. Section of these nerves did not prevent poisons being absorbed from the stomach, only they took a few minutes longer to produce their usual toxicological effect than in animals whose pneumo-gastrics were whole.

A number of curious facts are recorded relative to the organic changes produced on the lungs by section of the pneumo-gastric nerve of one side only. Dogs on whom this operation was performed all survived. Emphysema of the corresponding lung was almost invariably remarked in all those which were killed the third day after the section. On the seventh day the emphysema was less, but many points of the lung were engorged with blood. By the end of the second week the lung could still be inflated; but the engorgement was general, and rendered it less permeable to the air than the lung of the opposite side. At the end of six weeks only a few lobules here and there could be inflated; the rest was impermeable both to air and to the circulation of blood. The animals all got lean after section of one nerve. In one, which was observed for three and a half months after the operation, the voice was hoarse, the breathing was as if asthmatic under exertion, and a kind of whistling proceeded from the larynx; and vomiting occurred daily for the first month, but gradually disappeared afterwards.

The motor nerves which proceed from the brain are most minutely described. These are the third, fourth, and sixth pairs of
nerves which go to the muscles of the eye; the facial, or *portio dura* of the seventh pair, which supplies with motor power all the muscles of the face; and the ninth pair, or hypo-glossal nerve, which is distributed to the muscles of the tongue. It is shown, from a reference to direct experiment, and to the effect of disease, that these are all solely nerves of motion.

The remainder of the work is devoted to the great sympathetic or ganglionic system of nerves; but it appears to be unnecessary to enter into M. Longet's views on this subject, seeing that they closely agree with those of M. Brachet, whose work was so fully noticed in our 45th and 46th volumes. Instead, therefore, of detailing his experiments and remarks on the views of other physiologists, we shall content ourselves with giving his general conclusions.

"1. The great sympathetic undoubtedly exhibits a certain amount of sensibility to our ordinary means of irritation.

"2. It is easy to demonstrate experimentally the motive action of the great sympathetic.

"3. The ganglionic substance, which forms part of this nerve, but especially the gray matter of the spinal marrow, must be regarded as the sources of the activity of this nerve.

"4. Facts are far from confirming the opinion which regards each sympathetic ganglion as a small centre which acts independently of all cerebro-spinal influence. If we cannot deny to the ganglionic swellings an active co-operation as centres of innervation, we are, at least, forced to allow that their own action is insufficient for the proper maintenance of the functions of the great sympathetic.

"5. Every sudden impression on the central organs of the cerebro-spinal system reacts powerfully on the sympathetic system; and reciprocally acute impressions emanating from the organs to which the sympathetics are distributed, cause an intense reaction on the cerebro-spinal nervous system.

"6. The existence of a reflex power in the sympathetic system, analogous to that of the spinal chord, is a hypothesis which experiment disproves.

"7. If we are not generally sensible of impressions made on the organs to which the sympathetics are distributed,—if the will has no command over their movements, we must refer the cause of these peculiarities not to the ganglia themselves, as do Johnston and Reil, but to the circumstance, that these impressions are quenched in the spinal chord, and that the original fibres of the great sympathetic do not extend, like those of the ordinary nerves, to the source of the voluntary influence.

"8. Seeing that the parts which act involuntarily, as the heart and intestinal canal, preserve, for some time after separation from the body, the type of their peristaltic and rhythmical movements, it follows that these are independent of the cerebro-spinal influence; and seeing that these movements continue even after the re-
merval of the ganglia and ganglionic plexuses, they cannot be considered as under the guidance of that system. A similar phenomenon may depend on this, that, in the terminal ramifications of the sympathetic system, the nervous principle may dissipate itself in successive and periodic discharges.

" 9. If we irritate a cerebro-spinal nerve of motion by chemical, mechanical, or galvanic agencies, we excite contractions, which instantly follow the application of the irritant, and as instantaneously disappear. When the same irritants are applied to a branch of the sympathetic system, a few seconds always elapse before the motor reaction is observed; and it does not reach its maximum of intensity till after the irritating cause is removed. Thus, in the one case, contraction commences and ceases with the irritation; in the other, it only commences after the irritation, and endures for a longer period than it.

" 10. The accessory nerve of Willis, or spinal accessory, may be regarded as establishing a transition between the cerebral nerves and the great sympathetics; for, on the one hand, it animates muscles of animal life, (muscles of the larynx, &c.), and muscles of organic life, (the contractile tissues of the bronchii, oesophagus, stomach, and heart); and, on the other hand, it presents a mode of origin similar to the sympathetics. If the sympathetic originates, in fact, from the whole length of the spinal chord, by means of an infinity of roots, the spinal accessory arises from no inconsiderable portion of the same axis.

" 11. Since the works of Muller and Marshall Hall on the reflex properties of the brain and spinal marrow, we are not at liberty to explain many of the sympathetic reactions, as we were wont to do, by means of the ganglionic nervous system.

" 12. It seems probable, in proceeding by way of exclusion and analogy, that arterialization of the blood, and the secretion of bronchial mucus, are chiefly under the guidance of the sympathetic system.

" 13. Irritation of the cervical ganglia or of the cardiac sympathetic nerves renews the contractions of the heart after they have ceased.

" 14. The results of experiment and of the anatomy of the organ prevent us from admitting, with M. Brachet, that the cardiac ganglion is the source whence the heart derives its motive principle.

" 15. The sympathetic system has nothing to do with the contractions of the oesophagus; it only influences its mucous secretion.

" 16. The contractions of the stomach are not under the influence of the sympathetics, as Muller asserts; our own experiments demonstrate that they are entirely under that of the eighth pair of nerves. The secretion of the gastric juice has appeared to be distinctly influenced by the sympathetics; nevertheless, it is impossible to deny some direct influence from the eighth pair of nerves on this important secretion.

" 17. Chemical irritants and galvanism applied to the sympathetic...
tic system excite contractions in the small intestines after they have naturally ceased; we cannot, therefore, conclude, as M. Brachet has done, that these nerves have nothing to do with the motion of these tubes, but are simply confined to the direction of the intestinal absorption, exhalation, and secretion.

"18. The bladder of a living animal, exposed to the air, comports itself absolutely as the intestine,—that is to say, it becomes the seat of a vermicular motion, which, when extinct, may be revived by irritation of the sympathetic nerves. I am inclined to believe that, as in the intestinal tube, the sympathetic has two functions to perform: 1. to guide the involuntary contractions of the bladder; and, 2. to direct the mucous secretion; while the cerebro-spinal nerves which go to the bladder are simply for the purpose of making known the desire to micturate, and producing voluntary contraction of the neck of that organ.

"19. After irritation of the inferior lumbar and superior sacral portions of the sympathetic, very distinct contractions are observed, according to Valentin, in the vesiculae seminales of the guinea-pig. These contractions, which are really peristaltic, begin at the posterior extremity of the vesicule, and are so strong when the animal is at the rutting season, that the contained fluid escapes by the urethral orifice.

"20. The uterus of a gravid female, when exposed to the air, executes peristaltic movements quite similar to those of the intestines, and this organ can discharge its contents after death. When the uterine movements have ceased they may be renewed by irritating the inferior lumbar and superior sacral portions of the sympathetics. When we consider that the uterus receives alone filaments from the sympathetics, that it contracts in the same manner as all other muscles under the influence of the ganglionic system, that the will has no power over its movements, we may well be surprised that M. Brachet has asserted that its movements, during parturition, are under the sole influence of the cerebro-spinal system.

"21. Very recently Muller and Remak have described a special class of fibres, which they term gray or organized fibres, as occurring in the cerebro-spinal and sympathetic nerves, independent of those of motion and of sensation, and these they suppose preside over the organic acts of secretion and nutrition. It is not irrational to conclude that they also come from the cerebro-spinal axis, and conduct the principle of their action both into this central axis, into the ganglia of the sympathetics, and into the ganglia of the cerebro-spinal system. This mode of viewing the subject would explain, on the one hand, the incontestable influence of the cerebro-spinal axis on secretion and nutrition, and, on the other, (by the medium of the ganglia multiplying the nervous force,) the persistence of organic acts in parts deprived of motion and sensation.

"22. If, from the facts and experiments which we have recorded, we are forced to attribute distinctive characters to the sympathetic system, we yet cannot but acknowledge that it possesses
numerous analogies with the cerebro-spinal system. It is not correct to endeavour to prove them to have absolutely distinct functions from the cerebro-spinal system, by asserting that they preside over the nutritive and secretory functions, seeing that this latter system, as we have proved, also exercises the same function.

"23. The sympathetic system is a chain or reunion of cerebro-spinal nerves, destined for the use of the vegetative organs, which forms a nervous system fitted to maintain the conflict between animal and vegetative life. The numerous links of this chain are indicated by so many ganglia, which, besides, have their analogies in the cerebro-spinal nerves.

"24. The difference of properties which each exhibits is probably owing to an organic cause; in fact, we regard the cerebro-spinal system as continuous with the white matter of the spinal chord,—the sympathetics as especially connected with the gray matter of the same chord." — Vol. II. p. 629 to 634.

This able work is terminated by giving a short sketch of the ganglionic system in the Invertebrata. From the experiments and dissections of Newport, Muller, Grant, Geoffroy St Hilaire, &c., and the results of his own researches, M. Longet demonstrates that the ganglionic system of nerves in the invertebrated animals is not the analogue of the sympathetic system of the higher classes. On the contrary, their nervous axis, with the nerves issuing from it, represent at one and the same time the cerebro-spinal axis and nerves, as well as the sympathetic system, and even in many of these animals the sympathetic system is quite distinct from the principal chain of nervous ganglia. He also satisfactorily demonstrates by experiments that the cephalic ganglia of these inferior classes, but especially of the Articulata, in so far as their direction of sensation and movement are concerned, perform the same functions as the brain in the higher classes, and ought to be considered as its analogue.

Art. II.—On Dysmenorrhœa and other Uterine Affections in connection with derangement of the Assimilating Functions. By Edward Rigby, M. D., &c. 8vo, Pp. 138.

In laying before our readers Dr Rigby's work on dysmenorrhœa, it is our intention merely to give a concise review of the subject treated. We adopt this arrangement not only with the desire to do justice to the author, and at the same time to render our criticism intelligible to those who may not yet have perused the book for themselves, but likewise with the view of doing justice to readers. We think it due, however, to Dr Rigby to premise, that although he has advanced some new views on uterine pathology, he does not leave us in ignorance of the premise from