# DYNAMICS

OF

# NERVE & MUSCLE.

BY

CHARLES BLAND RADCLIFFE,

M.D., F.R.C.P.,

PHYSICIAN TO THE WESTMINSTER HOSPITAL, AND TO THE NATIONAL HOSPITAL FOR THE PARALYSED AND EPILEPTIC.

Le nom de Galvani ne périra pas; les siècles futurs profiteront de sa découverte, et, comme le dit Brandes, ils reconnaîtront "que la physiologie doit à Galvani et à Harvey ses deux bases principales."—VON HUMBOLDT.

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# PREFACE.

MORE than twenty years ago I was not a little puzzled by seeing what happened to a rabbit after death by strychnia. The animal at death was propped up against the side of a box, touching the ground only by the tips of its toes, with its legs rigidly stretched out, and with its neck and body arched backwards until the head almost pressed upon the tail, and so it remained until the putrefactive unstiffening of the muscles caused it to fall down. The contraction which had kept the body fixed in this position for some time before death had evidently not been relaxed by death, for, if it had been, the body would have then fallen as it fell eventually. The spasms before death and the rigidity after death—the true *rigor mortis*—seemed to be confounded the one with the other, and I did not know what to think until I began to wonder whether the interpretation of the spasm might be found, not on the side of life, but on the side of death—whether the key to the spasm might not be hid in *rigor mortis*—whether the spasm, instead of being the sign of a life in muscle which expressed itself in contraction, might not have to do with death rather than life, being in very deed a transitory step towards *rigor mortis*—a change brought about by something being abstracted from the muscle, not by something imparted to or awakened in it—even a physical rather than a vital phenomenon. Is it possible, I asked myself, that life may show itself, not in causing contraction, but in keeping the muscle relaxed, and that the doctrine of muscular motion may need reforming in accordance with this idea? Nor was I long undecided as to the answer which had to be returned to this question.

Shortly after this time, I published a small volume* with the object of showing that the doctrine of muscular motion did need reforming in accordance with this idea, and about which I may say what Dryden said concerning one of his own plays—"it was only a confused mass of thoughts tumbling over one another in the dark, when the fancy was yet in its first work, moving the sleeping images of things towards the light, there to be distinguished, and then either chosen or rejected by the judgment." Nor can I look back with feelings of pride at later efforts† in the same direction—not even at that, the latest of all, which took the form of lectures delivered at the Royal College of Physicians of London, in 1863, and published soon afterwards.‡ I did the best I could

* "Philosophy of Vital Motion." 8vo. London: Churchill, 1851.

† "Epileptic and other Convulsive Affections of the Nervous System: their Pathology and Treatment (incorporating the Gulstonian Lectures for 1860). 3rd edition, post 8vo. London: Churchill, 1861.

‡ "Lectures on Epilepsy, Pain, Paralysis, and certain other Disorders of the Nervous System," delivered at the Royal College of Physicians of London. Post 8vo. London: Churchill, 1864.

at the time, but I can easily see now that the best I did was not done well enough to carry conviction to a mind which did not share my own convictions. I can see that I succeeded better with the pathological part of my argument than with the physiological. I can see that the latter part was very faulty for want of work with which my leisure moments have been a good deal occupied during the last two years, and of which the results are incorporated in these pages.

Very soon after my thoughts upon this subject began to take definite form it became apparent to me that the key to the dynamics of nerve and muscle lay hid among the facts belonging to animal electricity. Fascinated by the results to which the investigations of Dr. Du Bois-Reymond had led, my attention was at first solely directed to the aspect of animal electricity which is made known by the galvanometer. At this time the muscle-current and the nerve-current, and their changes when passing from the state of rest into that of action, seemed to be the all-important facts which ought to be noticed. Afterwards I slowly began to apprehend that there was another aspect of animal electricity which is ignored in these investigations. The fact, discovered by Matteucci, that muscular contraction was accompanied by a discharge analogous to that of the torpedo, seemed to point, not to the muscle-current and nerve-current as primary and fundamental conditions of animal electricity, but to a state of *charge*, and for the evidences of this charge I began to seek, first with this electrometer and then with that, first with this condenser and then with that, and not altogether fruitlessly even at first. I made out something by an electrometer of my own contriving. I thought I had done more by means of the Potentiometer of Mr. Latimer Clark, but the end proved that I was deceiving myself, this instrument being in fact a gauge, not of tension, but of current. Only, indeed, did I begin to arrive at satisfactory results when, not long ago, I became possessed of the New Quadrant Electrometer of Sir William Thomson, together with the means for measuring the resistance of animal tissues to electrical conduction, for it was only when I began to work with these instruments that I was able for the first time to distinctly realize the facts with which I had to do, and for which I had been groping almost in the dark previously.

Working for myself with the instruments for measuring the resistance of animal tissues to electrical conduction, the fact, which I had not before sufficiently apprehended, became impressed upon me that these tissues might take rank under the head of non-conductors rather than under that of conductors. I found, for example, that the resistance of an inch of the sciatic nerve of a frog was not less than 40,000 B. A. units—as much, that is, as eight times that of the whole Atlantic cable.

Seeking for tensional phenomena of animal electricity in muscle and nerve by means of the new quadrant electrometer, I soon found that the sides and ends of the fibres were charged differently, the former positively, the latter negatively, and that these evidences of charge disappeared in great measure during action. I soon found the evidences of the charge for which I had searched before almost in vain ; but I found more than I expected. Expecting to find a single charge I found a double charge ; and what to think of this state of things I could not at all see at first. The facts would not chime in with preconceived conceptions, and the end was that the conceptions had to be modified to suit the facts.

The view I had entertained almost from the very beginning of these investigations was that in some way or other the natural electricity present in muscle during rest produced the state of rest by keeping the muscular molecules in a state of mutual repulsion, and that muscle contracted when this electricity was discharged, by virtue of its elasticity, the muscular molecules being then left free to obey the common attraction which is inherent in their physical constitution. The idea was that the muscular fibres were *charged* with *one* kind of electricity during rest, and that in this way the molecules were kept in a state of mutual repulsion. The idea was one which it was difficult to reconcile with the fact that there was a double charge of electricity in muscle. It was one which would agree well enough with the presence of a single charge, whether positive or negative ; it was one which seemed to clash altogether with the double charge, seeing that the mutual repulsion of molecules arising from the presence of either charge would be counteracted by the mutual attraction of molecules charged differently. What then?

Did the natural electricity present in the muscle produce the state of muscular relaxation and elongation in a different way? Was it possible that the great resistance of animal tissues to electrical conduction might have an all-important part to play in the process under consideration? Was it possible that the sheaths of the fibres in muscle might be so wanting in conductibility as to allow them to act as *dielectrics?* Was it possible that, the sheaths being dielectrics, a charge of one kind of electricity developed on the outsides by the reactions of the blood there circulating, or in any other way, might *induce* a charge of the other kind of electricity on the insides, and that the electrical antagonism of the sides and ends of the fibres might be accounted for by the charge induced on the insides being conducted to the ends by the contents of the sheaths? Was it possible that the fibres might be kept in the state of relaxation or elongation by the compression of the sheaths arising from the mutual attraction of the two opposite charges, disposed as in a charged Leyden jar, upon the two surfaces of the sheaths? These were the questions which in turn presented themselves to my mind, and which, as it seemed, required to be answered in the affirmative. The idea was definite and free from many objections attaching to the idea whose place it had taken. It accounted for the difficulty of detecting the charge present in the fibre, and for the fact that the fibre could keep its charge through itself uninsulated; for the two charges, disposed thus Leyden-jar-wise, upon the two opposite surfaces of the sheath of each fibre, masked each other, and at the same time imprisoned each other, just as they do in the ordinary charged Leyden jar. It was also an idea definite in this particular—that all the tensional phenomena of the muscular fibre, and all the currrent phenomena too, could be easily imitated upon a wooden model of the fibre, left bare at the two ends, and sheathed at the sides with a coating formed of two layers of tinfoil separated by an intermediate layer of thin gutta-percha sheeting, if only a charge was supplied to the outer layer of tin-foil. And in this also was the idea definite—that the elongation of the fibre, assumed to be brought about by the mutual attraction of the two opposite charges disposed Leyden-jar-wise upon the two surfaces of the sheath, was found to be reproducible on a narrow band of india-rubber covered on its two surfaces with a thin metallic coating so as to allow of its being charged as a Leyden jar is charged; for, on thus charging, this band was seen to elongate under the mutual attraction of the two charges disposed upon its surfaces, just as the sheath of the muscular fibre is supposed to do. Nay, by this apparatus everything that is supposed to happen in muscular motion can be fully illustrated, contraction as well as relaxation, for this band, which had elongated under the charge, is seen to contract when this charge is discharged.

Nor does the galvanometer tell all that has to be told of what happens when animal tissues are included in the voltaic circuit. It tells of the voltaic current; it does not tell of the charge which, under ordinary circumstances, is associated with this current. It does not tell, as does the electrometer, that the parts between the poles are charged, half positively, half negatively, the former half being on the side of the positive pole, the latter half on the side of the negative pole. And yet this is information which cannot be dispensed with; for, as will be proved in due time, the workings of voltaic electricity upon muscle are found to be resolvable into those of the charge and discharge of these very charges, and not into those of the constant current. As with the workings of animal electricity, indeed, so here, the key is to be found in the revelations made by the electrometer rather than in those made by the galvanometer.

In a word, the result at which I have arrived is that the workings of all kinds of electricity, artificial and natural, upon muscle, are resolvable into those of charge and discharge, the charge elongating the fibres, the discharge of this charge bringing about the state of contraction, while at the same time strong confirmation of the view taken of the way in which the charge and discharge operate is found in the history of *electrotonus* (a subject now gone into for the first time), for there is every reason to believe that the increased contraction which is met with in electrotonus, and which is referred to "exalted irritability," is nothing more than the return of the fibres, by virtue of their elasticity simply, from a previous state of increased elongation, which state of increased elongation, now pointed out for the first time, may be the necessary result of the sheaths of the fibres being more highly charged, and therefore more compressed, than they are naturally, by the charge imparted to them in electrotonus. All this and more will appear in its proper place and in due time; indeed, the subject is only glanced at now in order to show that there is much new matter behind, and that the electrometer does really give a new key to the interpretation of these matters.

Nor is it otherwise with the problem of sensation. On the contrary, the electrometer is still the instrument which sheds light upon the phenomena to be dealt with rather than the galvanometer, and, in short, all, or nearly all, the conclusions arrived at respecting the action of electricity, natural and artificial, upon muscle, are found to be applicable in this case also.

For the rest, I must only say broadly that the general view of the dynamics of nerve and muscle proves to be in strict accordance with this partial view, and not with the view which assumes that muscle and nerve have a special life which expresses itself in contraction or sensation, as the case may be. The action of the blood upon muscle, when it is enquired into fully, is found to be one which antagonizes muscular contraction rather than causes it. The case is not one in which excess of blood in the parts which have to do with the production of motion shows itself in excess of muscular contraction, as it would do if this latter excess were, as it is assumed to be, expressive of heightened life, but it is in every way the reverse of this.

The blood, in fact, would seem to act as does the natural electricity, producing rest rather than action—producing rest, it may be, by keeping up the natural electricity of the parts. And so likewise with the action of "nervous influence" in the production of muscular motion and sensation. The case is one in which it is far more easy to believe that muscular action and sensation are produced by the abstraction rather than by the communication of this influence—one which accords more readily with the idea that nervous influence, like the natural electricity, acts by antagonizing action in muscle and nerve—acts, it may be, through the natural electricity of the parts. The facts are all in harmony, not with the view which looks upon action in muscle and nerve as the expression of the special life with which muscle and nerve are supposed to be endowed, but with the view which is based upon the notion that there is along with the state of rest a state of charge, and that the change from the state of rest to that of action is brought about by the discharge of this charge; and with this general statement I must content myself, for the few preliminary words which are here permissible would not serve to give any clear idea of the subject as it presents itself in detail.

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# DYNAMICS OF NERVE AND MUSCLE.

## PART I.

*THE SUBJECT FROM A PHYSIOLOGICAL POINT OF VIEW.*

## CHAPTER I.

# ON SOME PRELIMINARY PARTICULARS RESPECTING ANIMAL ELECTRICITY.

A SHORT time before the close of the last century the illustrious author of "Cosmos" wrote:* "Le nom de Galvani ne périra point; les siècles futurs profiteront de sa découverte, et, comme le dit Brandes, ils reconnaîtront que la physiologie doit à Galvani et à Harvey ses deux bases principales." This is saying much, but, as I believe, it is not saying more than what is now fully borne out by the facts; and on this account I think it will not be waste of time to take a cursory glance at the history of the discovery of animal electricity before proceeding to deal with problems in which, as I hope to show before I have done, this agent supplies us with the master key.

The discovery of animal electricity dates as far

* "Expériences sur le galvanisme, et en général sur l'irritation des fibres musculaires et nerveuses." F. A. von Humboldt, Traduit par J. F. N. Jadelot. 8vo. Paris, 1799, p. 361.

back as 1786. One day in the course of this year, while amusing himself with a common electrical machine near a dish on which lay a number of frogs' legs prepared in the way in which it is usual to prepare them for purposes of cookery, Galvani, seeing that these limbs jumped whenever he drew a spark from the primary conductor, was led to think that discharges of atmospheric electricity might make themselves known by similar jumpings. Needing a more delicate electroscope than the one he then had in order to carry on some investigations in atmospheric electricity with which he was then engaged, and wishing to know whether he had found what he needed, he at once took the dish, with its contents, and went out of doors, his nephew, Camillo Galvani, who was with him at the moment, going with him. The time was a clear and calm evening in September. It did not promise success, for the sky was free from all signs of electrical disturbances: it proved to be propitious in an unexpected direction. The place was a high terrace belonging to the house at Bologna in which Galvani lived—then the Casa Panfili-Colonna, now the Casa Monti, in the Strada S. Gervasio. The house, the terrace, the railings, are still to be seen at No. 96 in the Strada S. Felice, the only change of moment being in the name of the street. Each pair of limbs was suspended by a small iron hook from the horizontal bar of the iron railings which fenced in the highest part of the terrace, the hook transfixing the portion of spine which had not been cut away. Galvani says—"Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa, septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat; en motus in rana spontanei, varii, haud infrequentes! Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur, et toties ferme quoties hujusmodi pressio adhiberetur."* How, then, were these contractions to be accounted for? They could not be due to discharges of atmospheric electricity, for the sky presented no indications of electric disturbance: they could not be due to the sparks which gave rise to them within the house, for the electric machine, which had also been left behind, was not in action: they could not be due, that is, to discharges of either of the two kinds of electricity then known. Could there be electricity in the limbs themselves, and were the contractions the consequences of the workings of this agent? Were the contractions arguments in favour of the existence of animal electricity? From this time until the day of his death, Galvani went on performing experiment after experiment, sacrificing hecatombs of frogs, always firm in his belief that these questions ought to be answered in the affirmative, and unceasingly striving to bring others to the same mind with himself. He was, however, destined to be foiled, and that, too, by a weapon which lay hid in one of his own experiments. The experiment in question was one in which a galvanoscopic frog was thrown into a state of momentary contraction by placing a conducting arc, of which one half was silver and the other half copper, between the

* "De Viribus Electricitatis in Motu Musculari Commentarius." 1791

lumbar nerves and the crural muscles.* Galvani, as was his wont, explained these contractions by supposing that the conducting arc had served to discharge animal electricity, and that the contractions were the result of the discharge. Volta, on the other hand, was of opinion that the electricity producing these contractions originated in certain reactions between the silver and copper portions of the conducting arc; and he was not shaken in this view by what he did afterwards, for, wishing to confirm it, he began a series of investigations which ended in the discovery of the voltaic pile and battery—a discovery which filled all minds with wonder, and for a long time afterwards diverted attention altogether from the consideration of the claims of animal electricity. In the meantime, however, while Volta was demonstrating the existence of that electricity which originates in the reaction of heterogeneous bodies, and which is now known as voltaic electricity, Galvani continued his search after animal electricity, and made many important discoveries as he went along. He discovered, among other things, that a galvanoscopic frog would contract without the help of a conducting arc composed of heterogeneous metals. He discovered not only that these contractions would happen when this arc was composed of a single metal, but also that an arc composed of muscle or nerve would answer the same purpose as the metallic arc. He also discovered that the limb of a galvanoscopic frog, of which the nerve had been divided high up in the

* The *galvanoscopic frog* was prepared from the hinder half of the animal, by stripping off the skin, and cutting away all the parts between the thighs and the fragment of the spine, except the principal nerves.

loins, would contract at the moment when the end of the nerve below the line of division was brought down and made to touch a part of the trunk of the same nerve. At last, indeed, he hit upon an experiment in which he seemed to have to do with an electricity other than that arising from the reaction of heterogeneous bodies—an electricity which must belong to the animal tissues themselves. He did much, but he did not do enough to win the battle in which he was engaged, for Volta still kept his position, denying the existence of animal electricity, and maintaining that the electricity which produced the contractions in the galvanoscopic frogs was always due to electricity arising in the reaction of heterogeneous bodies of one kind or other—silver and copper, metal and organic tissue, muscle and nerve, nerve in one state with nerve in another, as the case might be.*

In 1799, Humboldt took up the question at issue between Galvani and Volta, and published a work† in which he shows by many new and curious experiments that there was error on both sides—that Volta was wrong in ignoring altogether the influence of animal electricity in Galvani's experiments, and that Galvani was not less wrong in recognising nothing but this influence. He, himself, as is proved in the extract already given, was a firm believer in animal electricity; but he failed to supply reasons for this belief which can be thoroughly satisfactory to others. Still, he did something in this direction by making out—first, that the agent assumed to exist and to be animal electricity has this in common with

* "Ann de Chim.," t. xxiii., p. 276 and 301. † "Op. Cit."

electricity, that its action is permitted by conductors and prevented by non-conductors; and, secondly, that it is not to be confounded with voltaic electricity, because the action, which is permitted by conductors, is possible across a gap in the circuit which would allow the passage of frictional electricity, but which would altogether prevent that of voltaic electricity—which would, that is to say, allow electricity of high tension to pass, but not electricity of low tension. What Humboldt did, in fact, was to increase the probabilities of the existence of animal electricity not a little, and at the same time to make it appear that this electricity would prove to be of higher tension than voltaic electricity.

In 1803, Aldini, Galvani's nephew,* published an an account of certain experiments which furnish further evidence in favour of the existence of animal electricity, by showing that living animal tissues are capable of giving rise to attractions and repulsions which seem to be no other than electrical attractions and repulsions. "I held," he says, "the muscles of a prepared frog in one of my hands, moistened with salt and water, and brought a finger of the other hand, well moistened in the same way, near to the crural nerves. When the frog possessed a great deal of vitality, the crural nerves gradually approached my hand, and strong contractions took place at the moment of contact." And again:—"Being desirous to render this phenomenon more evident, I formed

* "Account of the late Improvements in Galvanism, with a series of curious and interesting experiments performed before the Commissioners of the French National Institute, and repeated in the Anatomical Theatres of London, &c." 4to. London, 1803.

the arc by applying one of my hands to the spinal marrow of a warm-blooded animal, while I held the frog in such a manner that its crural nerves were brought very near to the abdominal muscle. By this arrangement, the attraction of the nerves of the frog became very evident."

About this time, however, the discovery of the voltaic battery had given the victory to the opinions of Volta—a victory so complete that nothing more was heard about animal electricity for the next thirty years.

In 1827, Nobili* brought back the subject of animal electricity to the thoughts of physiologists by discovering an electric current in the frog. He made this discovery by means of the very sensitive galvanometer which he himself had invented a short time previously—an instrument which, as perfected by M. Du Bois-Reymond and others, by Sir William Thompson more especially, ought to be as prominent an object as the microscope in the laboratory of every physiologist. Immersing each end of the coil of the instrument in a vessel containing either simple water or brine, and completing the circuit between the two vessels with a galvanoscopic frog,—the fragment of the spine being immersed in one vessel, and the paws in the other,—he found that there was a current in the frog from the feet upwards, which current would cause a considerable permanent deflection of the needle,—to 30° or more if brine were used, to 10°, or thereabouts, if water were substituted for brine. Nobili supposed that this current was peculiar to the

* "Bibl. Univ.," 1828, t. xxxvii, p. 10.

frog, and in this he erred; but he did, nevertheless, a great thing, for, by this experiment, he furnished, perhaps, the first unequivocal proof of the real existence of animal electricity.

Twelve or thirteen years later, Matteucci published an essay* which, as M. de la Rive says,† "restored to animal electricity the place which it ought to occupy in electrical and physiological phenomena." This essay, moreover, had a great indirect influence upon the fortunes of animal electricity, for M. Du Bois-Reymond, as he himself tells us, was led to undertake the investigations which have made his name famous in this department of physiology by the inspiration arising from its perusal.

The joint labours of MM. Matteucci and Du Bois-Reymond have left no room for entertaining any doubt as to the reality of animal electricity. This will appear sufficiently in the sequel, when many of the experiments which furnish the demonstration will have to be referred to particularly. In the meantime, it may be said that Manteucci has demonstrated in the most unequivocal manner that animal electricity is capable of decomposing iodide of potassium, and of giving "signes de tension avec un condensateur délicat,"‡ as well as of producing movement in the needle of the galvanometer; and not only so, but also—a fact, the discovery of which

* "Traité des Phénomènes Electro-physiologiques des Animaux." Paris. 1844.

† "A Treatise on Electricity, in theory and practice. Translated by C. V. Walker." 8vo. Longman. 1853-1858.

‡ "Cours d'Electro-physiologie." Paris. 1858.

will always give Matteucci a place in the very foremost rank of physiological discoverers—that musclar contraction is accompanied by an electrical discharge analogous to that of the Torpedo. And as for M. Du Bois-Reymond* it may be said that he has demonstrated that there are electrical currents in nerve—in brain, spinal cord, and other great nerve-centres, in sensory, motor, and mixed nerves, in the minutest fragment as well as in masses of considerable size,—that the electrical current of muscle, which had been already discovered by Matteucci, may be traced from the entire muscle to the single primitive fasciculus,—that Nobili's "frog current," instead of being peculiar to the frog, is nothing more than the outflowing of the currents from the muscles and nerves,—that the law of the current of the muscle in the frog is the same as that of the current of the muscles in man, rabbits, guinea-pigs and mice, in pigeons and sparrows, in tortoises, lizards, adders, glow-worms, toads, tadpoles, and salamanders, in tench, in freshwater crabs, in earth-worms—in creatures belonging to every department of the animal kingdom,—that the law of the current in muscle agrees in every particular with the law of the current in nerve, and also with that of the feeble currents which are met with in tendon and other living tissues,—and that there are sundry changes in the current of muscle and nerve under certain circumstances, as during muscular contraction, during nervous action, under the influence of continuous and interrupted galvanic currents, and so on, which changes,

* "Untersuchungen über thierische Electricität." Berlin. 1849, 1853.

as I shall hope to show in the sequel, are of fundamental importance in clearing up much that would otherwise be impenetrable darkness in the physiology of muscular action and sensation.

Before the discovery of the galvanometer, the attention of those who cared to meddle in these matters was directed exclusively to the static phenomena of animal electricity. Then the only definite electrical ideas were, charge on the one hand, and discharge on the other. After the discovery of the galvanometer, the original point of view was abandoned altogether, or almost so, and the attention diverted from the static to the current phenomena of electricity. And herein, as I believe, was an unmixed misfortune. As I take it, indeed, it is necessary to go back to the standpoint occupied by Galvani and Humboldt, and to work with the electrometer rather than with the galvanometer; and this conviction has now so much gained upon me, that I am disposed to regard the New Quadrant Electrometer of Sir William Thompson—the instrument which for the first time makes it possible to arrive at an accurate knowledge of the statical aspects of animal electricity—as an instrument which is, to say the least, quite as indispensable as the galvanometer itself to those who would do the work in question. Already, indeed, as it will appear in due time, this instrument has supplied proof of the existence of a definite charge of electricity in nerve and muscle during rest, and of the discharge of this charge when this state of rest is changed for that of action, as well as of other facts without which it is impossible to arrive at any clear view of the dynamics of nerve and musle; and with this general statement I must now content myself, for it is high time to leave these preliminary matters, and proceed to the consideration of the several physiological problems which wait for solution.

## CHAPTER II.

# ON THE ELECTRICAL PHENOMENA BELONGING TO LIVING NERVE AND MUSCLE DURING THE STATE OF REST.

### I.

WHILE *at rest, living nerve and muscle supply currents to the galvanometer which gradually come to an end before the establishment of rigor mortis, and which—with certain exceptions in which this course is reversed—pass through the coil in a direction which shows that the surface made up of the sides of the fibres is positive in relation to either one of the two surfaces made up of the ends of the fibres, and that the positive surface becomes more positive, and the negative more negative, as the distance increases from the line of junction between these surfaces.*

Living muscle, while at rest, supplies a current to the galvanometer if the electrodes are brought into contact, the one with the surface made up of the sides of the fibres, the other with the surface made up of either one of the two ends of the fibres. This current, called the *muscle-current*, has a very definite history. It ceases gradually as the muscle loses its impressibility,* and comes to an end altogether before the establishment of *rigor mortis*. It passes through the coil from the surface made up of the sides of the fibres to *either* one of the two surfaces made up of the ends of the fibres, unless it be that it is much enfeebled and upon the point of ceasing altogether, in which case its direction may be reversed. It behaves in this manner whether the ends of the fibres supplying it be covered with tendon or not, with this difference only, that it is a little weakened when this covering remains.

The muscle-current, in fact, is a phenomenon of living muscle during the state of rest, which is too definite to be confounded with anything else, and too prominent to be overlooked.

Living nerve, while at rest, supplies a current to the galvanometer, in this case called the *nerve-current*, if the electrodes are applied, the one to the surface made up of the sides of the fibres, the other to either one of the two surfaces made up of the ends of the fibres, and the history of this current is in all essential respects that of the muscle-current. Like the muscle-current, the nerve-current takes its departure *pari passu* with the impressibility of the nerve. Like the muscle-current, the nerve-current, as a rule, passes through the coil from the surface made up of the sides of the fibres to *either* one of the two surfaces made up of the ends of the fibres—in a direction which

* Here and afterwards the state of nerve and muscle to which the name of irritability is commonly given, is called *impressibility*, and the excuse for this change is that a doctrine is involved in the former word, which, as will appear in the sequel, is but little in harmony with the facts of the case.

shows that the former surface is positive in its electrical relations to the latter. Like the muscle-current, the nerve-current may be occasionally reversed. Thus, the nerve-current of the brain and spinal cord of several animals may be reversed at the time when the nerves generally are on the point of ceasing to be impressible,—in the brain and spinal cord of frogs, for example, though curiously enough, there is no corresponding reversal of the muscle-current in these animals. Thus, again, the nerve-current may be reversed in nerves which have been the seat of violent and prolonged action, or which have been exposed for a time to great heat. This change does not involve destruction of the impressibility of the nerve, neither is it always final. On the contrary, the nerve in which this reversal has happened from excessive action, or exposure to heat, may be as impressible after the reversal as it was before, or nearly so, and the nerve-current may return to its original direction if the nerve be placed where it can recover the natural moisture it had lost by dessication—if it be put back, for example, among the structures from which it had been dissected out, and left there for a short time.

Besides these major currents between the surfaces made up of the sides, and either one of the surfaces made up of the ends of the fibres of nerve and muscle, each of these surfaces is also capable of supplying minor currents, of which the direction through the coil shows that the positive surface becomes more positive, and the negative surface more negative, as the distance increases from the line of junction between these surfaces. These minor currents are brought to light if the points to which the electrodes are applied are at unequal distances from the centre of the surface, but not if they are equidistant—are brought to light, that is to say, if the two points connected by the coil are of unequal electric tension, but not if they are of equal tension; for in order to account for the current being present in the first case, and absent in the second, all that is necessary is that this should be the state of things as to tension. At all events, the minor currents remain as facts, and the inference from the direction they take in the coil of the galvanometer must be that which has been mentioned, namely this, that the positive surface becomes more positive, and the negative surface more negative, as the distance increases from the line of junction between these surfaces.

## II.

*While at rest, living nerve and muscle furnish supplies of free electricity to the electrometer, which gradually disappear before the establishment of rigor mortis, and which show—with certain exceptions, in which this state of things is reversed—that the surface made up of the sides of the fibres and the surface made up of either one of the two ends of the fibres are charged differently, the former surface positively, the latter negatively, and also that the tension of these opposite charges rises as the distance increases from the line of junction between these surfaces.*

Signs of free electricity, positive as well as negative, which gradually come to an end before the establishment of rigor mortis, are now readily detected in nerve and muscle while they are alive and at rest by the New Quadrant Electrometer. Applying the electrodes of this instrument so as to bring them into contact, the one with the surface made up of the sides of the fibres of a piece of fresh muscle, and the other with the surface made up of either one of the two ends of the fibres, and examining the electrical condition of each surface separately, the ray of light is found to move upon the scale in a way which shows, not only that the electrometer has become charged, but that a different charge has been supplied by each surface. If the surface connected with the electrometer be that which is composed of the sides of the fibres, the ray moves as it moves under a positive charge ; if the surface so connected be that which is made up of either one of the two ends of the fibres, it moves as it moves under the negative charge ; and, with the exception of a reversal in direction which may happen shortly before their final cessation, these are the movements which are noticed from first to last. A positive charge is supplied by one surface, of which the tension, measured by the degree to which the ray moves on the scale, is equal to about the tenth of that of a Daniell's cell ; a negative charge of the same tension is supplied by the other surface. The case is not one in which the charge of the one surface differs from that of the other in tension merely, for if it were, the movements of the ray caused by the charge proceeding from the two surfaces would be to different degrees in the same direction, and not to the same degree in opposite directions. The case is one in which the charges of the two surfaces must differ in kind, for only upon this view are the movements of the ray in opposite directions to be accounted for. Moreover, the movements of the ray indicate differences of tension in different parts of each of these surfaces singly, which differences are indicated by different degrees of movement in the same direction for each surface, and which show, when analysed, that the positive surface is most positive, and the negative surface most negative, as the distance increases from the line of junction between these surfaces, at which line the tension is at zero.

And so likewise with the nerve. If a detached piece of fresh nerve be tested by the electrometer in the same way as that in which the muscle has just been tested, the movements of the ray are found to tell a precisely similar story of a positive charge supplied by the surface made up of the sides of the fibres, of a negative charge supplied by the surface made up of either one of the two ends of the fibres, and of differences of tension at different parts of the former surface from which it is plain that the tension diminishes in the neighbourhood of the latter surfaces. In all these particulars the agreement between the nerve and the muscle holds good, and also in the degree of tension, for this still proves to be about a tenth of that of a Daniell's cell. Indeed, the agreement only fails in the proof of the existence of differences of tension on different parts of the surface made up of the ends of the fibres, and here the failure may be manifestly due to the mere fact of the parts being too minute to admit of exact examination.

All this I have made out by experiment, and this also—that, as with the current phenomena of nerve and muscle, so with these tensional phenomena, the evidences of them gradually come to an end as the nerve and muscle lose their impressibility, and that there may be a reversal involving the change from positive to negative, or *vice versâ*, when this loss is all but complete.

## III.

*The very imperfect conductibility of muscle and nerve makes it not impossible that the sheaths of the fibres in musle and nerve may rank as non-conductors rather than as conductors—that they may act as dielectrics, in fact.*

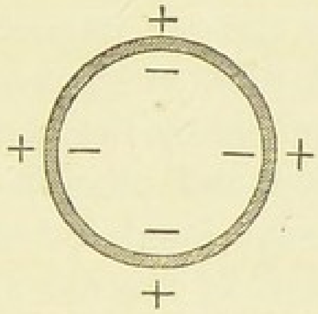
The measurements made by E. Weber, Matteucci, and Eckhard, as well as those which I have made myself, all go to show that the animal tissues generally are very imperfect conductors of electricity. E. Weber, the pioneer in this inquiry, made out broadly that certain of these tissues, nerve not excepted, conducted electricity as much as 50,000,000 times less readily than copper. Matteucci, comparing pieces as nearly as possible of the same size and shape, taken from the sciatic nerve, the brain, the spinal cord, and the adductor longus muscle of a recently killed rabbit, found that the three substances first named, nerve, brain, and spinal cord, conducted electricity at very nearly the same rate, and that the last, muscle, was the better conductor in the proportion nearly of two to one.

Professor Eckhard, of Giessen, also confirms in the main the statement of Matteucci as to the relative conductibility of nerve and muscle, and shows in addition that the resistance of tendon and cartilage is the same, or very nearly the same, as that of nerve, the mean of the means of three groups of experiments upon each substance, the muscle being put down as 1, being for the nerve, 3·3, for the tendon, 3·3, and for the cartilage, 3. And these results, so far as nerve and muscle and tendon are concerned, are borne out by some measurements which I have myself made with a Siemen's Resistance Table. In three experiments, for example, in which I measured the resistance of pieces, as nearly as possible of the same shape and size, taken from the sciatic nerve, the tendo-achillis, and the adductor longus of a rabbit, recently killed, I found the mean resistance to be, in the nerve 40,000 units—as much, that is, as eight times that of the whole Atlantic cable—in the tendon 38,000 units, and in the muscle 12,000 units. Undoubtedly many experiments have yet to be made before any accurate results can be arrived at. Undoubtedly care has not been taken in the experiments already made to eliminate effectually all the errors arising from secondary polarization and other disturbing influences. But of this there can be no doubt, that nerve and muscle, and certain other animal tissues also, are very imperfect conductors. Even now, indeed, enough is known to make it not impossible that certain parts of nerve and muscle may take rank as non-conductors rather than as conductors, and to give countenance to the notion that this want of conduction may have an important part to play in the animal economy. In point of fact, nerve and muscle are such imperfect conductors as to make it possible that the sheaths of their fibres may act as *dielectrics,* in a way which will be indicated presently. And certainly there is nothing intrinsically improbable in the assumption that these sheaths may act in this manner, for water, as is well known, may also act as a dielectric.

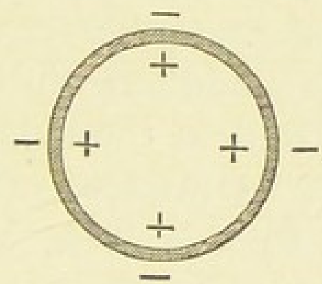
## IV.

*A charge of either kind of electricity, developed (by oxidation, or in some other way) on the outsides of the sheaths of the fibres of nerve and muscle, may, if these sheaths be dielectric, induce the opposite charge on the insides, and in this way the electric antagonism between the sides and the two ends of the fibres may be accounted for, for in order to this all that is required is to suppose that the charge on the insides of the sheaths, which, according to this view, is necessarily antagonistic to that on the outsides, is conducted to each of the two ends by the contents of the sheaths, while at the same time the gain in tension in each of the two charges as the distance increases from the line of junction between the two surfaces may have its explanation in the mutual annihilation of opposite charges along this line.*

If a charge of electricity, positive or negative, be developed on the outside of the sheaths of the fibres of nerve and muscle by the respiratory and other molecular changes which are there at work—and this surely is no unsupposable idea—and if these sheaths are capable of acting as dielectrics, a charge of the opposite kind of electricity must be *induced* on the insides of the sheaths. That must happen, in fact, by which the sheath of each fibre will be converted into a Leyden jar, with, as is the rule, the positive electricity outside, and the negative inside, thus,—



or, as is the exception, with the negative outside, and the positive inside, thus,—



And certainly this view is one which will very readily supply the explanation of all the current and tensional phenomena of nerve and muscle. It will explain why the electrical condition of the two ends of the fibres is opposed to that of the sides, for in order to this, all that is necessary is to suppose that the contents of the sheaths are comparatively good conductors, and that the charge induced within the sheath is conducted to the ends of the fibres by these contents. It will explain why the positive surface is more positive, and the negative surface more negative, as the distance increases from the line of junction between these surfaces, for the reaction of opposite charges over this line must lead to a mutual annihilation of charge, which will alter the tension in this manner. It will explain, too, the current phenomena no less than the tensional, for in order to this, all that is necessary is to change the electrometer for the galvanometer, and to suppose that the tensional differences made known by the electrometer are *kept up* by the continuance of those actions (oxygenation, and others) in which they originate. All that is supposed, indeed, may be imitated upon a wooden cylinder, covered at its sides with a coating made of two layers of tinfoil with an intermediate layer of thin gutta-percha sheeting, and left uncovered at its two ends, if the outer layer of foil be charged as a Leyden jar is charged ; and this imitation is in every way to the purpose, for the cylinder exemplifies perfectly the conditions which are supposed to exist in nerve and muscle, the cylinder being, in fact, nothing more than an artificial model of nerve and muscle according to this view, and the charging nothing less than what is supposed to happen with nerve and muscle. What, then, is it that happens when the outer layer of tinfoil on this artificial model of nerve and muscle receives a charge ? The case is plain. The outer layer of foil remains charged with the charge communicated to it, and the inner layer of foil becomes charged with the opposite kind of charge *induced* in it. That happens, in fact, by which the two uncovered ends of the cylinder will become electrically opposite to the outer covering of the sides, and for this simple reason, that the charge induced in the inner layer of foil is conducted to the ends by the wooden core within the Leyden coating. That happens, also, by which, supposing the sides externally to be positive, and the two ends negative, the sides will become more positive, and the ends more negative, as the distance increases from the line of junction between these surfaces, for the mutual reaction of opposite charges over this line must lead to annihilation of charge on each side of this line, which must be complete at this line, and which must diminish on each side as the distance from the line increases. All this is easily verified by the new quadrant electrometer, and I have often so verified it all. In this model, thus charged, indeed, everything tensional is precisely as it is in the actual nerve or muscle. And these being the tensional conditions, it follows that all the conditions for imitating all the current phenomena are present also, if only the galvanometer be substituted for the electrometer, and if the charge to the outer layer of foil be kept up. Indeed, there is no single current phenomenon, major or minor, belonging to nerve or muscle, which I have not, over and over again, got out of this artificial fibre of nerve or muscle by thus keeping up the charge, and at the same time bringing the electrodes of the galvanometer to bear in a suitable manner.

## V.

*It is not improbable that the electrical condition of nerve and muscle during rest is, not current, but static, the sheaths of the fibres at the time being so many charged Leyden jars, and the "nerve-current" and "muscle-current" no more than purely accidental phenomena.*

The view taken by Dr. Du Bois-Reymond is that the fibres of living nerve and muscle are made of an infinite number of what this physiologist calls *peri-polar molecules*—of molecules, that is to say, which are electrified negatively at the two poles turned towards the two ends of the fibres, and positively in the interpolar portion turned towards the sides of the fibres, or else the reverse. According to this view, the ends of the fibres are negative, because the negative poles of the peri-polar molecules are turned in this direction, and the sides positive, because the positive interpolar belts of the molecules are so turned, or else the ends of the fibres are positive, because the poles of the peri-polar molecules pointed towards the ends are positive, and the sides negative, because the interpolar portion of the molecules pointed in this direction are in this case negative. According to this view, the *nerve-current* and *muscle-current* are *derived* portions of infinitely stronger currents ever circulating in closed circuits around the peripolar molecules. According to this view, indeed, the primary and fundamental electrical condition of nerve and muscle during rest is, not static, but current; and this is the view which has met with general acceptance.

As it seems to me, however, the primary electrical condition of nerve and muscle during rest is, not current, but static, the condition of the sheath of each fibre at this time being in fact nothing more or less than that of a charged Leyden jar. As it seems to me, indeed, the "nerve-current" and "muscle-current" are no more than accidental phenomena, the simple result of applying the electrodes of the galvanometer to points of dissimilar electric tension. This, as I believe, is the view which arises naturally out of the premises, and harmonizes with all the evidence remaining behind; and, therefore, without further comment, I venture to adopt it provisionally as the view which has the best claim to attention.

## Chapter III.

# ON THE ELECTRICAL PHENOMENA WHICH MARK THE PASSING OF NERVE AND MUSCLE FROM THE STATE OF REST INTO THAT OF ACTION.

## I.

*THE phenomenon of secondary or induced contraction appears to show that in passing from the state of rest into that of action a discharge of electricity, analogous to that of the torpedo, is developed in and around both nerve and muscle.*

Secondary or induced contraction is a form of contraction to which attention was first called by Matteucci, and for the demonstration of which all that is wanted is a couple of rheoscopic limbs, just taken from a very lively frog, the *rheoscopic limb*, so called, being the lower half of the hind leg, stripped of its skin, and with the whole of the sciatic nerve remaining in attachment.

The simplest experiment, which is also the original one, consists—(1) in placing the two limbs, which may be distinguished as limb *a* and limb *b*, upon a plate of glass, or some other insulating material; (2) in putting the free end of the nerve of limb *b* upon the muscles of limb *a*; and (3) in pinching the nerve of limb *a*. If the frog from which the limbs were taken was not sufficiently lively, the result of pinching the nerve of limb *a* is simply to cause contraction in the muscles of this limb; if, on the other hand, the limbs belonged to a very lively frog, the contraction caused by pinching the nerve of limb *a* is, not in this limb only, but in the limb *b* as well, the latter contraction being the secondary or induced contraction. And this is also the result if, instead of placing the nerve of limb *b* upon the muscles of limb *a* directly, a piece of cotton wick, soaked in salt water, be placed between the nerve and the muscles, so as to form a bridge of considerable span between the two.

Secondary or induced contractions are also produced in the same way by placing the nerve of limb *b*, not upon the muscles, but upon the nerve of limb *a*, nerve upon nerve, not nerve upon muscle as before, only the experiment is apt to miscarry more frequently in this than in the former case. Try on his

Commenting upon the first two of these experiments, Becquerel hazarded the conjecture that the cause of the contraction in limb *b* was a discharge of electricity developed during action in and around the muscles of limb *a*; and this also is the view taken by Matteucci. Moreover, as will appear in the sequel, Matteucci gives good reason for thinking, not only that this is the true explanation of secondary or induced contraction, but also that the discharge developed during action in muscle is analogous to that of the torpedo—for thinking, in fact, that the causation of the secondary or induced contraction is identical with that of the contraction witnessed in a rheoscopic limb, of which the end of the nerve is made to touch the skin of the torpedo in the neighbourhood of the electric organ, whenever this organ is put in action. And certainly this view is as applicable to the last experiment—which brings to light the variety of induced or secondary contraction discovered by Dr. Du Bois-Reymond—as to the other two, if only it be assumed that action in nerve, as well as action in muscle, is accompanied by a similar discharge. Indeed, it is difficult to know where to turn to find another explanation, for if the idea of the discharge of electricity be excluded, what agent is there remaining which can disturb the atmosphere outside the acting muscle or nerve so as to give rise to contraction in a muscle of which the nerve is merely contiguous to the acting nerve or muscle, and which at the same time can traverse a wetted cotton wick, as it is seen to do in the second experiment, without losing its power of thus acting ?

## II.

*There are certain anatomical and physiological analogies between the electrical apparatus of the torpedo and the muscular apparatus of this or any other animal, which make it more than probable that in passing from the state of rest into that of action, a discharge of electricity is developed in and around both nerve and muscle, and that this discharge is analogous to that of the torpedo.*

The muscles and the electric organs of the torpedo agree so far in their relation to the nervous system, and in their manner of action, as to make it in the highest degree probable that the state of action in muscle and motor nerve is accompanied by a discharge analogous to that of the torpedo. Like the nerves of the muscles, the nerves of the electric organs originate in the same track of the spinal cord, and terminate in the same loop-like plexuses. Like the muscles, the electric organs are paralysed by dividing their nerves. Like the muscles, the electric organs, after they have been paralysed by dividing their nerves, may be made to act by pinching the end of the nerve below the plane of section. Like the muscles, the electric organs are thrown into a state of involuntary action by strychnia. Like the muscles, the electric organs cannot go on acting without intervals of rest. And lastly, the nerves of the electric organs, like the nerves of the muscles, respond in the same curiously alternating way to the action of the "inverse" and "direct" voltaic currents when the nerves are somewhat exhausted, if only discharge be taken as the equivalent of contraction. In a word, the physiological and anatomical analogies between the electric organs of the torpedo and the muscular apparatus of this or any other animal, may be said almost to necessitate the conclusion to which Matteucci was led in regarding them, namely this, that muscular action is accompanied by a discharge of electricity, and that this discharge is analogous to that of the torpedo.

## III.

*The almost complete disappearance of the muscle-current from the muscle, and of the nerve-current* *from the nerve, when nerve and muscle pass from the state of rest into that of action, may be looked upon as a reason for believing that there is a discharge of electricity at this time.*

Professor Du Bois-Reymond is the author of several experiments which show that the passage from rest to action in both nerve and muscle is attended by disappearance of the proper nerve-current and muscle-current.

One experiment, of which the object is to ascertain what happens to the muscle-current during contraction, is performed upon the gastrocnemius of a frog, with the whole length of the sciatic nerve remaining in connection with it. This preparation is placed, with the muscle upon the cushions of the galvanometer, and with the end of the nerve most distant from the muscle across the electrodes of an induction apparatus, not then in action. When the needle of this galvanometer has taken up the position into which it diverges under the action of the muscle-current of the relaxed muscle, the muscle is made to contract by exposing its nerve to the action of induced electricity, and not until then. What is done, that is to say, is first to get the muscle-current of the relaxed muscle, and then, by setting up contraction in this muscle, to get the muscle-current of the contracting muscle. This is what is done. What happens is this—that the needle, which stands at a considerable distance from zero under the muscle-current of the relaxed muscle, swings back to zero, or beyond it, when this muscle is made to contract. It seems as if the muscle-current acting upon the needle while the muscle is relaxed, is *reversed* during contraction. In fact, however, the current at this time is weakened only, not reversed, and that it is so is easily proved by simply shutting out the muscle-current of the contracting muscle from the galvanometer, and not re-admitting it until the needle has come to rest at zero; for on doing this the needle is found to move from zero under the muscle-current of the contracting muscle, in the same direction as that in which it moved under the muscle-current of the relaxed muscle, only not to the same distance by a great deal. Under the current of the relaxed muscle, the position taken up by the needle may be at 50° or 60° ; under that of the contracting muscle it may be at 3° or 4°, or still nearer to zero. In every case the difference is very marked, and in every case this difference is one which shows that there is a great weakening of the muscle-current during contraction.

Nor is a different conclusion to be drawn from the experiments of which the object is to find out what happens with the nerve-current when the nerve passes from the state of rest into that of action.

In one of these experiments, which exhibits the action of the tetanus caused by strychnia upon the nerve-current of the sciatic nerve of a frog, the plan pursued is to fix the animal upon a convenient frame, to expose the sciatic nerve by a suitable dissection, to lay the lower end of the nerve, (which is liberated by a cross-cut at the ham,) upon the cushions of the galvanometer, and to inject a few drops of solution of strychnia under the skin. This is what is done. Before the poison takes effect, the needle of the galvanometer is acted upon by the nerve-current of the quiescent nerve: after the poison takes effect, the needle is acted upon by the nerve-current of the acting nerve, for tetanus involves nervous action in its highest degree. And this is what happens. Before the tetanus the needle diverges under the nerve-current of the quiescent nerve, and takes up a position, say at 30°; during the tetanus, the needle, now responding to the nerve-current of the acting nerve, returns towards zero, and takes up a position very near this point, and on the same side of it. There is no reversal of the nerve-current during action in this case. It is with the nerve-current during action, as it was with the muscle-current during action, there is obvious and unmistakeable weakening, and this only.

Another experiment, from which a similar lesson is to be learnt, is also upon the sciatic nerve of a frog. The nerve, in this case altogether separated from the body, is arranged so that one end is included in the circuit of the galvanometer, and the other laid across the electrodes of an induction-apparatus, which apparatus is not put in action until the needle of the galvanometer has taken up the position into which it diverges under the nerve-current of the quiescent nerve. Before the nerve is put in action by the electricity, the needle stands, it may be, at 30°; after the nerve is thus put in action, the needle, which now of course responds to the nerve-current of the acting nerve, returns towards zero, and takes up a position a degree or two from this point on the same side. These are the simple facts. As in the former experiment, so in this, the nerve-current is much weakened during action, not reversed.

There is also another experiment to the same effect which may well find a place here—an experiment upon a rheoscopic limb from which the skin has not been stripped off, and of which the result is to show that the nerve-current is weakened coincidentally with the action set up in the periphery of the nerve by heat. In this case the method of procedure is :—(1) to place the limb in a small vessel of sufficient depth, with the nerve hanging over the edge; (2) to bring the free end of the nerve within the circuit of the galvanometer; (3) to wait until the needle is stationary at the point to which it has diverged under the action of the nerve-current of the quiescent nerve; and (4) while the needle is fully divergent under the nerve-current of the quiescent nerve, to set up a state of action in the nerve, by pouring boiling water into the vessel to a depth sufficient to cover the limb. On the addition of the hot water, the needle, which before stood at a considerable distance from zero, at once moves towards zero, and takes up a position close to this point, sometimes on one side, sometimes on the other. This is the result. During the action thus set up in the nerve, that is to say, the nerve-current is much weakened without being reversed, or much weakened and reversed, much weakened in either case. And this is the result also if the nerve be thrown into a state of action by other modes of heat, as, for example, by bringing a hot wire near the nerve.

Dr. Du Bois-Reymond does not speak of this disappearance of electricity from muscle and nerve during action, of which proof is supplied in these experiments, as a *discharge* of electricity. He speaks only of a *negative variation* of a nerve-current, or a muscle-current, as the case may be. Thinking only of a *current*, he ignores altogether the evidence in support of a *discharge* which is supplied by Matteucci. As it seems to me, however, the very proof which Matteucci requires is supplied in these experiments of Dr. Du Bois-Reymond, for if these experiments show anything plainly it is this, that electricity *disappears* in the very cases in which it is supposed to be discharged.

## IV.

*The almost complete disappearance of all tensional signs of electricity from muscle and nerve when the state of rest changes into that of action, may be looked upon as a direct proof that this change is attended by discharge of electricity.*

With Sir William Thompson's New Quadrant Electrometer, there is little or no difficulty in acquiring exact information respecting the tensional aspect of the electrical phenomena of muscle in the opposite conditions of rest and action.

The lower half of the thigh of a frog, without its skin, and with a long portion of the principal nerve remaining in attachment, is the part made use of in the experiment which most readily serves to show what are the tensional electrical phenomena of muscle during rest and during action, and which has now to be described. The electrodes of the electrometer are applied, the one to the uncut longitudinal surface of the muscular fibres, the other to the cut transverse surface ; the free end of the nerve is placed across the poles of an induction-apparatus, not then in action. In order to get the tensional phenomena of the relaxed muscle, the muscle is left at rest by not putting the induction-apparatus in action, and while at rest, it is included in the circuit of the electrometer by removing the plug which short-circuits the instrument. In order to get the tensional phenomena of the contracting muscle, the muscle—the electrometer being short-circuited—is made to contract by putting the induction-apparatus in action, and while it is contracting, it is made to act upon the electrometer by removing the short-circuiting plug. When the electrometer is short-circuited, the reflected ray of light upon the scale rests at zero ; when the muscle is included in the circuit of the instrument by removing the short-circuiting plug, this ray moves more or less from zero, to the extent of several degrees if the muscle be relaxed, to a very short distance in the same direction, or not all, if it be contracted. The tensional phenomena, which were evident enough while the muscle was at rest, are scarcely, if at all perceptible when the muscle is put in action, the ray moving to 15° or 20°, or more, in the former case, and at most to 2° or 3° in the latter. These are the facts. Charge has disappeared from the muscle during action, or, in other words, discharge has happened during contraction.

And as with the muscle, so with the nerve, there is evidence to show that the tensional signs of electricity present during rest disappear in great measure or altogether in action. Such evidence, for example, is to be found in an experiment like the one which was used to show the disappearance of nerve-current in the case in which the state of action is set up in the nerve of a rheoscopic limb by the operation of heat upon the peripheral expansion or upon the trunk of the nerve, with this difference, that (in order to get a larger nerve-surface) two rheoscopic limbs are used in place of one, and that the electrometer is substituted for the galvanometer. The electrodes of the electrometer are applied, the one to the surface made up of the sides of the fibres, the other to the surface made up of the ends of the fibres. First, the electrical condition of the nerve during rest is examined by removing the plug which short-circuits the electrometer, while the nerve is at rest. Then, the electrical condition of the nerve during the state of action set up in it by heat is tested in the same manner, the electrometer having been first discharged, and the ray on the scale so brought back to zero, by re-introducing the plug which short-circuits the instrument. This is what is done. When the nerve acting upon the electrometer is at rest, the ray on the scale moves one way or the other, to the extent of 15° or 20° from zero; when the nerve is put in action by heat, this ray moves in the same direction as that in which it moved in the first instance, but only to the extent of a degree or two, or else it does not move at all. These are the results. There is a charge present in the nerve during rest, which disappears in great measure or altogether when the nerve is thrown into a state of action by the operation of heat; in other words, there is a discharge of the electricity present during rest when nerve passes from the state of rest into that of action; and thus with nerve as with muscle, the passing from the state of rest into that of action would seem to be marked by discharge of electricity.

## V.

*The occurrence of discharge when muscle and nerve pass from the state of rest into that of action, becomes all the more probable if the state of muscle and nerve during rest be one of charge; and, vice versâ, the occurrence of discharge during action, may be looked upon as a strong additional argument in favour of the conclusion that the state of muscle and nerve during rest is really one of charge.*

The conclusion arrived at when speaking of the electrical condition of muscle and nerve during rest was to the effect that this condition was, not current, but static, each fibre at this time being, in fact, a charged Leyden jar. Each fibre during rest was looked upon as being in that very state which provided for the discharge which is supposed to happen in action—in that very state which would almost seem to necessitate discharge in action. It is indeed permissible, not only to argue in favour of the occurrence of discharge during action from the evidence in support of the existence of charge during rest, but to turn round and to look at the several proofs of the occurrence of discharge during action as so many strong additional arguments in favour of the conclusion that in muscle and nerve the electrical state during rest is really one of charge, for with so many independent proofs of charge and discharge, to argue in this manner is not to argue in a vicious circle.

## VI.

*In the electrical apparatus of the torpedo during rest there would seem to be a charge in every respect like that which is met with in muscle and nerve during rest, and the discharge of the torpedo, instead of being peculiar, may be only another form of the discharge which attends upon the action of muscle and motor nerve.*

The electrical apparatus of the torpedo is made up of from 500 to 1000 polygonal prisms, enclosed in aponeurotic cases, with the ends in contact, the one with the dorsal, and the other with the abdominal integument. Each prism consists of a very large number of horizontal laminæ, the electrical diaphragms of Pacini, separated by thin layers of fluid. Each lamina is formed of vascular nucleated texture, largely supplied with centrifugal nerve-fibres, the vessels and nucleated cellular texture, disposed in a wide meshed fibrous layer, occupying the upper surface of the lamina, the nerves being exclusively distributed on the under surface. The two ends of the prisms are in opposite electrical conditions, the end applied to the dorsal integument being positive, the other end being negative. And this also is the general construction of the electrical apparatus in other electrical fishes. There are the same polygonal prisms formed of the same transverse laminæ, separated by thin layers of fluid, the only difference being that these prisms may be disposed with their ends, the one to the head, and the other to the tail, not the one to the back and the other to the abdomen, and that the two surfaces of the laminæ, the vascular and nervous, instead of being fused together as in the torpedo, may be separated by a thin layer of fluid. This, then, being the apparatus in which the discharge of the torpedo is developed, the question is as to the method of development. Is it that the two ends of the prisms are kept in opposite electrical conditions, because the two surfaces of each one of the component laminæ are oppositely electrified? Is it that a charge of positive electricity is generated by the reaction of the blood upon the vascular surface of each lamina, and that this charge, acting through a dielectric substance, induces a charge of negative electricity on the other surface of the lamina? Is it that the prisms are in this way kept charged during rest, so as to have the discharge ready when they are called upon to act? Is it that the nerves have to do less with charging the prisms than with discharging them? These questions arise naturally out of the premises, and as naturally admit of answers in the affirmative. Indeed, after the conclusions arrived at when speaking of the electrical condition of muscle and nerve during rest and during action, it is difficult to return answers which are other than affirmative.

And if the condition of the electrical apparatus of the torpedo during rest be like that of muscle and nerve during rest—one of charge, then it may be that the discharge of the torpedo, instead of being at all peculiar, may be only another form of the discharge which attends upon the action of muscle and nerve, the two discharges differing chiefly in that the circuit of the latter is wholly within the body, while that of the former is not wholly within the body.

Nay, it may even be that the discharge of the muscle and nerve would prove to be as powerful as that of the electrical apparatus if it could be got at so as to be measured fully, for, as it is, the discharge which can be got from a muscle or nerve after removal from the body, and when the tissue must be more than half dead, is sufficient to give rise to secondary or induced contraction.

## VII.

*There is reason to believe that the passage from the state of rest to that of action in both muscle and motor nerve is attended by a discharge of electricity analogous to that of the torpedo.*

The more the evidence advanced in this chapter is weighed, the more it seems to justify the conclusion that the transition of muscle and nerve from the state of rest into that of action is marked by the discharge of the charge of electricity present during the state of rest, and that this discharge is analogous to that of the torpedo. Indeed, this and no other would seem to be the conclusion which must be deduced from all the evidence adduced hitherto, from that contained in the second chapter, as well as from that contained in this.

## CHAPTER IV.

## ON THE HISTORY OF THE SO-CALLED "INVERSE" AND "DIRECT" CURRENTS, AS INDICATING THE WAY IN WHICH MUSCULAR MOTION IS AFFECTED BY VOLTAIC ELECTRICITY.

### I.

*ON passing a voltaic current along the leg of a frog from the foot upwards to the other foot along the other leg downwards, it is found*—(1) *that the muscles contract at the moments of closing and opening the circuit, or at one of these moments singly;* (2) *that the muscles remain relaxed so long as the circuit is kept closed;* (3) *that the contractions continue for a longer time in the limb in which the current is upward or "inverse" than in the limb in which it is downward or "direct;"* and (4) *that by reversing the direction of the current the contraction may be more than once brought back in the latter limb after it has ceased, provided it have not then ceased in the former limb.*

The parts commonly used in demonstrating the action of voltaic electricity upon muscle and motor nerve are the hind limbs of frogs prepared in one of two ways. One of these ways is to remove both the limbs by a cross-cut a little above the point at which the lumbar nerves are connected with the spine, to strip off the skin, and to leave all the natural connections between the limbs undisturbed. The other is to remove the limbs at the same point, to strip off the skin in the same way, and then, after disarticulating at the symphysis pubis, to dissect away all the remaining connections between the limbs except the lumbar nerves and the intervening portion of spine. The current in each case is passed along one leg from the foot upwards to the other foot along the other leg downwards, by applying the positive pole to the first foot and the negative pole to the second; and in each case the broad results to be noticed are the same. In each case there is contraction on closing and opening the circuit, or at one or other of these moments. In each case the muscles remain relaxed while the circuit is kept closed. In each case the contraction continues longer in the limb in which the current is upward, or "inverse," than in the limb in which it is downward, or "direct." In each case, after it has ceased, contraction may more than once be made to show itself again in the limb in which the current is "direct," by reversing the direction of the current, provided only the contractions have not yet ceased in the limb in which the current is "inverse." At first it seems to be immaterial whether the limbs along which the current is passed have their nerves exposed or not; afterwards it becomes evident that the results of passing the current are not strictly the same in the two cases, and that the only way of avoiding considerable confusion is to take each case and to study it separately.

## II.

*The results of passing the "inverse" and "direct" currents along limbs which have been prepared so that the principal nerves are not exposed, differ in some respects from those which mark the passage of these currents along limbs which have been prepared so that these nerves are exposed.*

(1.) *In the case where the nerves are* NOT *exposed.*

(*a.*) *Under the "inverse" and "direct" currents indifferently, the result of closing and opening the circuit is contraction first at both these moments, afterwards at the moment of closing only.*

(*b.*) *Under the "inverse" and "direct" currents indifferently, the muscles remain relaxed so long as the circuit is kept closed.*

(*c.*) *The contractions attending the closing and opening of the circuit continue longer under the "inverse" than under the "direct" current—continue for an hour and more in the former case, and for not more than fifteen minutes in the latter.*

(*d.*) *After the contraction which attends upon the closing and opening of the circuit has come to an end under the direct current, it may be brought back by reversing the direction of the current, and this too more than once, provided it be still continuing under the inverse current.*

(2.) *In the case where the nerves are exposed.*

(*a.*) *Under both currents the result of closing and opening the circuit is contraction, first at both these moments under both currents indifferently, and afterwards at only one of these moments under each current differently, the contraction in the end being at the moment of opening the circuit, but not at that of closing*
itive *under the inverse current, and at the moment of closing, but not at that of opening, under the direct current.* negative.

(*b.*) *Under the "inverse" and "direct" currents indifferently, the muscles remain relaxed so long as the circuit is kept closed.*

(*c.*) *The contraction attending the closing and opening of the circuit continues longer under the "inverse" than under the "direct" current—continues for an hour or more in the former case, and for not more than fifteen minutes in the latter.*

(*d.*) *After the contraction which attends upon the closing and opening of the circuit has come to an end under the direct current, it may be brought back by reversing the direction of the current, and this too more than once, provided it be still continuing under the inverse current.*

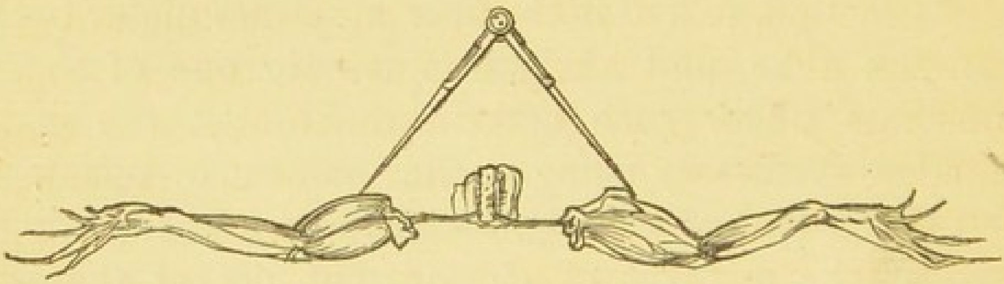
The results of passing a voltaic current up one limb and down the other in the case in which the nerves are not exposed, and in that in which they are exposed, are in the main the same. They are the same in that under both currents, the inverse and direct alike, the muscles remain relaxed so long as the circuit is kept closed. They are the same in that the contraction which attends upon the closing and opening of the circuit continues longer under the "inverse" than under the "direct" current—continues for an hour or more in the former case, and for not more than fifteen minutes in the latter. They are the same in that after the contraction which attends upon the closing and opening of the circuit has come to an end under the direct current it may be brought back by reversing the direction of the current, and this too more than once, as is seen in the so-called "voltaic alternatives," provided it be still continuing under the inverse current. It is indeed only in the result of closing and opening the circuit that a difference creeps in between the two cases. In the case where the nerves are *not* exposed, under both currents, the inverse and direct alike, the result of closing and opening the circuit is contraction, first at both these moments, afterwards at the moment of closing only; whereas, in the case where the nerves are exposed, the result of closing and opening the circuit is contraction, first at both these moments under both currents alike, and afterwards at only one of these moments under each current differently, the contraction eventually being at the moment of opening, but not at that of closing, under the inverse current, and at the moment of closing, but not at that of opening, under the direct current.

In seeking to explain the action of voltaic electricity upon muscle and motor nerve, these are the facts which have to be dealt with. The problem to be solved is perplexing enough, but not inexplicable; and, as is always the case when the true key is found, what at first seemed to be exceptional and unintelligible, becomes in the end only another proof of order.

## III.

*The different results of passing the voltaic current in the case where the nerves are not exposed, and in that where they are exposed, would seem to be accounted for by the action of the electricity being upon the muscles rather than upon the nerves in the former case, and upon the nerves rather than upon the muscles in the latter case.*

If along limbs prepared so as to have their nerves exposed a voltaic current be passed until the contraction which attends the closing and opening of the circuit is at the opening only under the inverse current, and at the closing only under the direct current, and if then a pair of compasses, or any other good conductor, be placed so as to bridge over the space between the muscular portions of the two limbs, thus,



the contraction, instead of alternating in the two limbs, as it did before, at once observes the same order, and occurs at the opening of the circuit as well as at the closing in both, or at the closing only in both. On placing the compasses in this position, that is to say, the order of contraction changes to that which is observed in the case where the current is passed along limbs which have been prepared so as not to leave their nerves exposed, and thus a point is gained from which it is possible to see why it is that the current should act differently upon limbs of which the nerves are not exposed, and upon limbs of which the nerves are exposed. For the action of the compasses can be only that of a good conductor by which the voltaic current is diverted from the nerves, which are very bad conductors, and left to act upon the muscles solely or chiefly. This is the only conclusion possible. And if so, then it follows further, that in the case in which the nerves are not exposed, the voltaic current may in the same way be diverted from the nerves, and left to act chiefly or solely upon the muscles, for it is a fact that muscles are far better conductors than nerves. In a word, only one inference would seem to be deducible from this experiment, namely this, that the alternating contraction in which the contraction is at the opening of the circuit and not at the closing under the inverse current, and at the closing and not at the opening under the direct current, is thus alternating, because in this case the currents are acting upon the exposed nerves of the limbs, rather than upon the muscles; and that the non-alternating contraction in which the contraction is, first, at the opening as well as at the closing of the circuit, and then at the closing only, under both currents alike, is thus non-alternating, because in this case the currents are acting upon the muscles rather than upon the nerves. Where the currents act directly upon the muscles the order of contraction is the same under the inverse and direct currents indifferently; where, on the other hand, they act directly upon the motor nerves, the order of contraction is in the end different under the two currents. It is, in fact, only when the current acts upon the motor nerve that there is, so far as the production of contraction is concerned, any difference between the action of the inverse and direct currents, and the inevitable inference is, that the course of the current, inverse or direct, is a matter of moment when the current acts upon the nerve, but not when it acts upon the muscles.

## IV.

*The fact of contraction being present at the closing and opening of the circuit, and absent while the constant current is passing, would seem to show that the contraction is caused, not by the constant current, but by the instantaneous extra-currents which attend upon the closing and opening of the circuit.*

The simple fact that there is no contraction while the constant current is passing, and that contraction happens only when the circuit is closed and opened, would seem to show that the contraction has to do, not with the constant current, but with the instantaneous currents of high tension to which their discoverer, Faraday, gave the name of *extra-currents*. The mere absence of contraction while the circuit remains closed is in itself a sufficient reason for thinking that the constant current can have nothing to do with the production of contraction. The mere presence of contraction at the moments of closing and opening the circuit is in itself almost a sufficient reason for believing that the contraction may be brought about by the action of the extra-currents which pass at those moments. Moreover, this power of producing contraction is precisely that power which

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these extra-currents may be expected to have. Extra-currents are, in fact, strictly analogous to induced currents, and to discharges of statical electricity—to currents and discharges, that is to say, both of which have a remarkable power of producing contraction. Extra-currents, indeed, are so closely akin to induced currents as to be by many confounded with them. Like induced currents, extra-currents are two in number, the one at the closing and the other at the opening of the circuit of the constant current. Like the two induced currents, the two extra-currents are in contrary direction and of unequal strength. In fact, extra-currents are only unlike induced currents (of the first order) in this, that their relative direction and strength is not the same. With induced currents it is the current at the *opening* of the circuit which is in the same direction as the constant current, and which is the stronger of the two. With the extra-currents, on the contrary, it is the current at the *closing* of the circuit which is the stronger of the two, and which passes in the same direction as the constant current. But this difference in relative direction and strength does not make a real distinction between the induced currents and the extra-currents as regards the power of producing contraction. It has, as will be seen presently, much to do in accounting for some of the peculiarities of contraction which have to be noticed in due time; it has nothing to do in the question at issue. Be the direction of the induced current or discharge of statical electricity what it may, inverse, direct, or other, provided it have a certain strength, contraction is the result. In other words, the direction of the current or discharge does not affect the production of contraction in these cases; what is wanted is a given strength, nothing more; and the sum of the whole matter is, that what holds good of induced currents and discharges of statical electricity may be assumed to hold good of extra-currents also. Hence, apart from the fact of contraction being absent while the constant current is passing, and present at the moments of closing and opening the circuit, there is good reason for believing that contraction is brought about by the action, not of the constant current, but of the extra-currents.

## V.

*The occurrence of contraction in the same order under the inverse and direct currents alike, that is, first at the opening as well as at the closing of the circuit, and then at the closing only, is in some degree to be accounted for by the case being one in which the muscles are acted upon directly by the currents, and by the extra-current at the closing of the circuit being more powerful than that at the opening, because muscle (so far as contraction is concerned) when acted upon directly, responds in the same way to the inverse and direct currents, and because the stronger extra-current at the closing of the circuit must continue to bring about contraction after the weaker extra-current at the opening has ceased to act in this manner.*

Contraction has been seen to happen in the same order under the inverse and direct currents alike, that is, first at the opening as well as at the closing of the circuit, and then at the closing only, in the case where these currents are passed along the prepared limbs of which the nerves are not left exposed, and also in the case where the preparation of the limbs has left the limbs exposed, but where the currents are diverted from the nerves by bridging them over with a pair of compasses or any good conductor. In other words, it has been seen, that when the muscles are acted upon *directly* by the currents, it is of no moment (so far as contraction is concerned) whether the current be inverse or direct, the contraction in this case happening in the same order with either current indifferently. And this conclusion supplies the key which is now wanted, if it be taken in connection with what has been made out respecting the action and relative strength of the two extra-currents. There is no difficulty in accounting for the occurrence of contraction in the same order under the inverse and direct currents alike, for contraction is in the same order under both currents when the muscles are acted upon directly by the currents, and the case is one in which the muscles are acted upon thus directly. Neither is there any difficulty in accounting for the order of contraction being what it is found to be, that is, first at the opening as well as at the closing of the circuit, and then at the closing only. For the case is simply this—that muscle, which at first is impressible enough to respond to the weaker as well as to the stronger of the two extra-currents, responds only to the stronger, which is at the closing of the circuit, when this impressibility has suffered a certain degree of impairment.

With muscle indifferent as it is to the direction of the currents it follows indeed that the order of contraction should be the same under the inverse and direct currents. With extra-currents differing in strength as they do, the stronger of the two being at the closing of the circuit, and with the impressibility of its muscle failing progressively as it does do, it follows also, that the order of the contraction should be what it is, that is, first, at the opening as well as at the closing of the circuit, and afterwards at the closing only.

## VI.

*The occurrence of contraction, first in the same order under both currents indifferently, that is, at the opening as well as at the closing of the circuit, and afterwards in a different order under each current, that is, at the opening of the circuit and not at the closing under the inverse current, and at the closing and not at the opening under the direct current, is in some degree accounted for by the case being one in which the motor nerves are acted upon directly by the currents, for with the nerves so acted upon, the occurrence of the contraction, first in the same and afterwards in a different order, may be explained if it be supposed that at first the nerves respond to both extra-currents, and afterwards (when the impressibility is considerably impaired) only to that extra-current which happens to pass along the nerve in the direction in which motor impulses are transmitted to the muscles.*

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In the case where the contraction happens, first, in the same order under both currents indifferently, that is, at the opening as well as at the closing of the

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circuit, and then in a different order under the two currents, that is, at the opening of the circuit and not at the closing under the inverse current, and at the closing of the circuit and not at the opening under the direct current, it has been seen already that the prepared limbs upon which the currents are acting have their nerves exposed, and that the currents are acting upon these nerves directly; and, therefore, it may be taken for granted that the explanation of the particular order of contraction at present under consideration will have to be sought in the action of the currents upon the nerves. Nor need the search be a long one. Taking for granted that the parts acted upon are the nerves, and that the extra-currents are still the causes of contraction, there is indeed no difficulty in accounting for the contraction being at first at the opening as well as at the closing of the circuit, for in order to this, all that is necessary is to suppose that the nerve is sufficiently impressible to be capable of responding fully to the extra-current of the opening as well as that of the closing of the circuit. The difficulty is not here; the difficulty is in accounting for the differences in the order of contraction which afterwards creep in. How is it that after a time the contraction is at the opening of the circuit and not at the closing under the inverse current, and at the closing and not at the opening under the direct? Is it that the nerve, now that it has lost some of its impressibility, is only capable of responding to that extra-current which happens to pass along it in the same direction as that in which motor impulses are transmitted to the muscles? That the nerve has lost some of its impressibility at this time is evident ; that a nerve which is in this case should be only capable of responding to the extra-current which happens to pass in the same direction as that in which motor impulses are transmitted to the muscles, may be conceded ; that the extra-current at the time of contraction passes in the direction in which motor impulses are transmitted to the muscles, is certain ; and, therefore, for anything that appears to the contrary, the answer to be returned to these questions may be in the affirmative. The direction of the two extra-currents, as has been seen already, is opposed to that of the two induced currents (of the first order) corresponding to them in point of time ; and this being the case, a few minutes' reflection will serve to show that the extra-current in the case in question is in the direction it ought to be, according to the hypothesis, in order to cause contraction. The extra-current agreeing with the constant current in direction is at the closing of the circuit, the induced current so agreeing is at the opening ; the extra-current disagreeing with the constant current in direction is at the opening of the circuit, the induced current so disagreeing is at the closing. The direction of the extra-current, to repeat, is the same as that of the constant current at the closing of the circuit, and the opposite at the opening. These are the simple facts, and therefore it is easy to see what the case must be with the inverse and direct currents. The *inverse* constant current is the current *up* the limb, and therefore the extra-current in this case is *up* the limb at the closing of the circuit, and *down* the limb at the opening. The case is one, that is to say, in which the extra-current is in the same direction as that in which motor impulses are transmitted to the muscles, not at the closing of the circuit, when contraction is absent, but at the opening, when contraction is present. The *direct* constant current, on the other hand, is the current *down* the limb, and therefore, the extra-current connected with it must be *down* the limb at the closing of the circuit, and *up* the limb at the opening, so that, in this case, the extra-current is in the direction in which motor impulses are transmitted to the muscles at the closing of the circuit, when contraction is present, and not at the opening, when contraction is absent. In a word, the direction of the extra-currents in connection with the inverse and direct currents is precisely what it ought to be in order to account for the contraction being at the opening of the circuit and not at the closing under the inverse current, and at the closing of the circuit and not at the opening under the direct current, if so be the nerve at this time is in that state of impaired impressibility in which it responds only to that extra-current which passes along it in the same direction as that in which motor impulses are transmitted to the muscles. And thus the whole case is quite consistent with the view that the particular order of contraction which is only noticed in the case where the nerves of the prepared limbs are directly acted upon by the currents is owing to the fact of these nerves being acted upon directly, first, when the impressibility of the nerve is unimpaired, by both extra-currents, and afterwards, when this impressibility is impaired, by that extra-current only which happens to be in the same direction as that in which motor impulses are transmitted along the nerves to the muscles.

Is it proved that direction of extracurrent has any influence upon nerve?

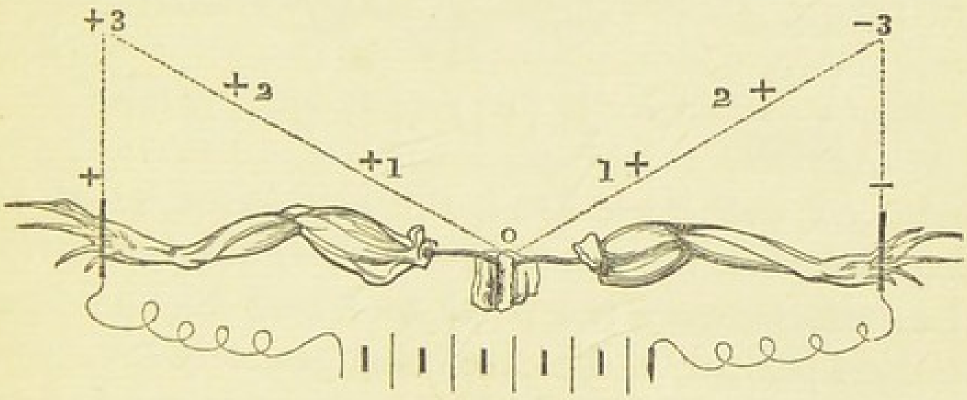
## VII.

*The fact that the contraction on closing and opening the circuit continues longer under the "inverse" than under the "direct" current, may have to do, not with the current being inverse in the one case, and direct in the other, but to there being, under ordinary circumstances, a charge of free positive electricity along with the inverse-current, and a charge of free negative electricity along with the direct current.*

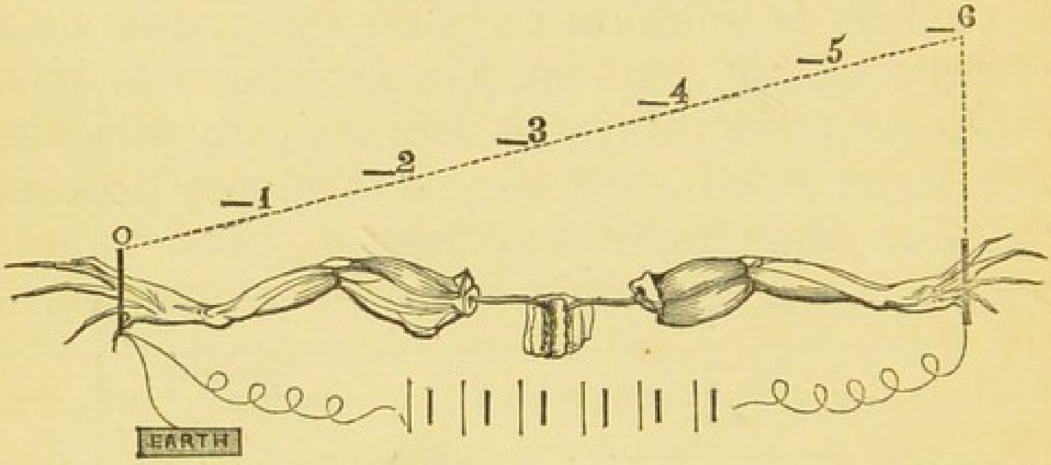
(1.) *The longer duration of the contraction when the current is* inverse, *may, under ordinary circumstances, have to do, not with the inverse current, but with the charge of positive electricity associated with the current, for it is found that by changing this positive charge for a negative charge, as may be done by putting an earth-wire to the positive pole, the duration of the contraction, becoming what it is under the direct current, shortens from an hour or more to no more than* 15′ *or* 20′.

(2.) *The shorter duration of the contraction when the current is* direct, *may, under ordinary circumstances, have to do, not with the direct current, but with the negative charge associated with the current, for it is found that by altering the negative charge for a positive, as may be done by putting an earth-wire to the negative pole the duration of the contraction, becoming what it is under the inverse current, lengthens from* 15′ *or* 20′ *to an hour or more.*

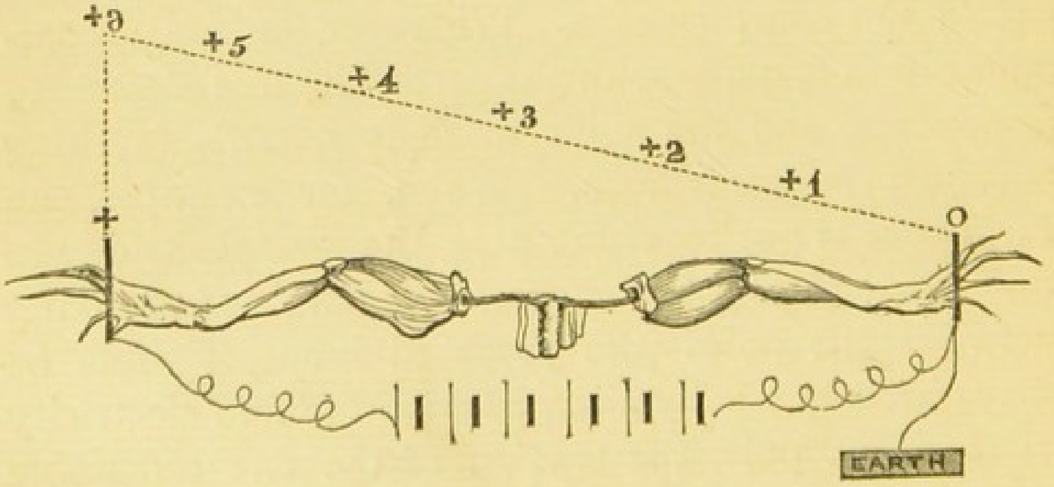
If the electrical condition of the two prepared limbs of a frog be tested by the new quadrant electrometer while a voltaic current is passing as in the ordinary experiment for showing the differences of the inverse and direct currents, that is, from the foot upwards along one limb to the other foot downwards along the other limb, the movement of the ray upon the scale shows most unequivocally that the condition is very different in the two limbs, provided only the circuit be insulated. It shows that there are different charges in the two limbs, a positive charge in the limb in which the current is inverse, a negative charge in the limb in which the current is direct, each limb receiving, in fact, from the pole contiguous to it, a charge, of which the tension falls regularly from the pole, where it is highest, to a point midway between the poles, where it is at zero, thus :—




This is the case where the circuit is insulated, but not in the contrary case. Wherever, indeed, a communication is made with the earth, the free electricity disappears, and the point there as regards anything tensional becomes zero. When, for example, an earth-wire is put to either pole, the free electricity of that particular pole runs off to earth, and the parts between the poles (the prepared limbs, if these be they) become wholly charged with the free electricity of the other pole, the tension of the one remaining charge rising greatly, becoming doubled, in fact. In the case of the prepared limbs, if the positive pole be put "to earth," the positive electricity disappears altogether from the space between the poles, and both limbs become charged negatively from the negative pole, the tension of the remaining charge falling regularly from the negative pole, where it is double what it was while the circuit was insulated, to the positive pole, where it is at zero, thus :—




If, on the other hand, the earth-wire be at the negative pole, the negative electricity disappears from the parts between the poles, and both limbs become charged positively, the case being precisely the reverse of the last, thus :—



It is with the prepared limbs, indeed, as Kohlrausch and others have shown it to be with any imperfect conductors placed in the same circumstances.

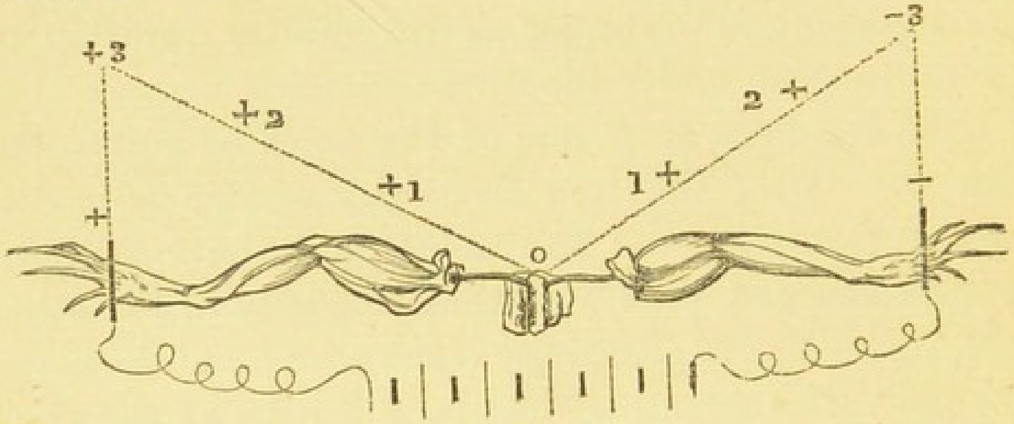
Nor are these facts beside the purpose at present in view. On the contrary, it is easy to show that the state of the two limbs as to these charges of free electricity has more to do in modifying the condition of the limbs as to contractility than the inverse and direct course of the current in these limbs—that the positive charge along with the inverse current under ordinary circumstances, and not the inverse current, is the cause of the longer duration of the contraction under the inverse current, and that the negative charge along with the direct current, and not the direct current, is the cause of the shorter duration of the contraction under the direct current.

If, as has been seen, one of the poles be connected with the earth, the free electricity of this pole disappears from between the poles, and the two limbs become charged alike with the electricity of the other pole, with positive electricity if the wire be at the negative pole, with negative electricity if it be at the positive pole. Instead of one limb being charged positively, and the other negatively, as in the ordinary experiment where the circuit is insulated, both limbs are charged similarly, positively or negatively, as the case may be. If, therefore, the differences in the duration of the contraction in the two limbs under ordinary circumstances be due, not to differences in the direction of the current in the two limbs, but to differences in the charges associated with the currents, it is to be expected that the duration of the contractions in the two limbs will be the same when the two limbs are charged similarly by connecting one or other of the poles with the earth, and so it is. Indeed, all that is necessary to show that it is so is to place the facts side by side, and leave them to tell their own story.

In the ordinary case, in which the circuit is insulated, the contractions on closing and opening of the circuit, come to an end—

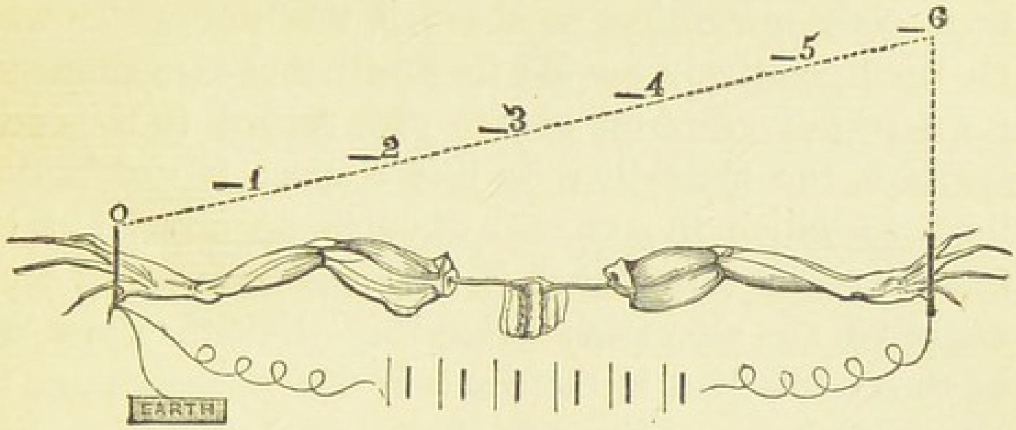
> in 15′, or thereabouts, in the limb in which the current is direct,
>
> in 60′, or thereabouts, in the limb in which the current is inverse,

and the state of the two limbs as to free electricity is this :—




The state of the two limbs as to free electricity, that is to say, is one in which the different charges of the two limbs makes it possible to believe that the difference in the duration of the contractions in the two limbs may be owing to the difference in the charges of the two limbs.

In the case in which the positive pole of the battery is "to earth," the state of the two limbs as to free electricity, and the results of closing and opening the circuit, are very different, the state as regards free electricity being this—

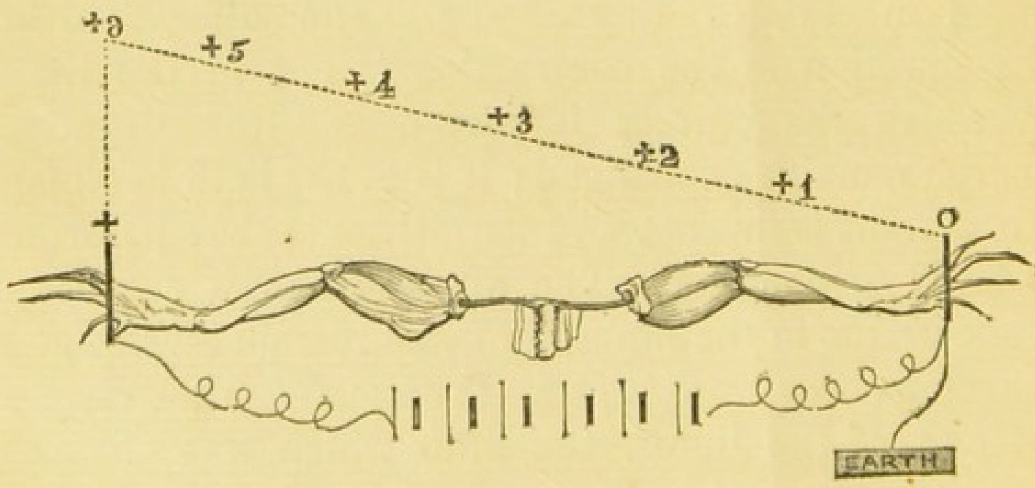



the contraction on closing and opening the circuit, coming to an end—

in 15′, or thereabouts, in the limb in which the current is inverse,
in 15′, or thereabouts, in the limb in which the current is direct.

These are the facts. The contraction comes to an end in both limbs at the same time. It does not come to an end in 15′, or thereabouts, in the limb in which the current is direct, and in 60′, or thereabouts, in the limb in which the current is inverse, as it did when the circuit was insulated, and the two limbs were charged differently. It comes to an end in both limbs in 15′, or thereabouts. The alteration in the duration of the contraction is not in the limb in which the current is direct, and the negative charge remains unchanged ; it is in the limb in which the current is inverse, and the charge is changed from positive to negative. The alteration does not affect the currents in the limbs, for these remain inverse and direct, as they were before. It only affects the charge of one of the limbs, the limb which was charged negatively remaining so charged, the limb which was charged positively having its positive charge changed for a negative. And so also with regard to the contraction, the alteration in this respect is not in the limb in which the charge remains negative, but in that in which this charge is changed from positive to negative, the alteration being this—that the duration of the contraction in the limb in which the charge is changed from positive to negative is made to become the same as that of the limb in which the charge remains negative. In a word, everything goes to show that the duration of the contraction in the case where the circuit is insulated is different in the two limbs, not because the current is inverse in the one limb and direct in the other, but because the charges associated with these currents are positive in the one limb and negative in the other.

And these conclusions are only confirmed by the results of reversing the experiment, by putting the "earth wire" to the negative pole. In this case the state of the two limbs as regards their free electricity, is that which is represented in the accompanying figure:—



In this case the contraction on closing and opening the circuit comes to an end—

in 60′, or thereabouts, in the limb in which the current is inverse ;

in 60′, or thereabouts, in the limb in which the current is direct.

In this case, as in the last, the contraction comes to an end in both limbs at the same time, and so far the results agree in the two cases, but no further. In this case the alteration in the charge of the limb is not in the limb in which it was positive, the charge in this limb remaining positive, but in the limb in which it was negative, this charge changing from negative to positive. In this case the alteration in the duration of the contraction is not in the limb in which the charge remains positive, but in that in which the charge is changed from negative to positive. It is one in which the change of charge from negative to positive carries with it another change, by which the duration of the contraction on closing and opening the circuit is made to be as long in the limb in which the charge changes from negative to positive as it is in the limb in which the charge remains positive.

It is plain, therefore, that the differences in the duration of the contractions which are noticed under the inverse and direct currents when the circuit is insulated, are to be referred, not to the inverse and direct currents, but to the two limbs being charged differently with free electricity. There is no flaw in the evidence. Let the two limbs be charged with the same kind of electricity by putting one of the poles "to earth," and the differences in question disappear, the contraction continuing in both limbs as long as it did in the one limb in which the current is inverse and the charge positive, if the charge in both limbs be made positive the contraction ceasing in both limbs as soon as it did in the one limb in which the current is direct and the charge negative, if the charge in both limbs be made negative. Let the two limbs be again charged differently, the one positively and the other negatively, by removing the earth-wire, and the differences in the duration of the contractions which are noticed in the two limbs when the circuit is insulated, at once return. Indeed, the simple fact that, without changing the direction of the current, the duration of the contractions may be lengthened or shortened at will, by merely changing the charge associated with the current, as is done by using the earth-wire in the manner which has been indicated, must be regarded as a conclusive proof that the longer or shorter duration of the contractions in the two limbs, in one of which the current is inverse, and in the other direct, is due, not to the current being inverse in the one case and direct in the other, but to the charge associated with the currents, under ordinary circumstances, being positive or negative, positive with the inverse current, negative with the direct current, the positive charge lengthening, the negative charge shortening the duration of the contractions.

## VIII.

*The fact that after the contractions attending the closing and opening of the circuit have come to an end under* *the direct current, they may more than once be restored by reversing the current, provided they have not yet come to an end under the inverse current, which restoration is known as the "voltaic alternatives," is not altogether unintelligible, if the positive charge ordinarily associated with the inverse current have the power of preserving contractibility, for it is not difficult to advance a step and see that the power which is thus preservative may be restorative also.*

Electro
Current
Galvanic

The fact that after the contractions attending the closing and opening of the circuit have come to an end under the direct current they may be more than once made to return, as they do in the so-called "voltaic alternatives," by reversing the direction of the current, provided they have not yet come to an end under the inverse current, is not altogether unintelligible, if the positive charge be as favourable to the continuance of contractibility as it would seem to be. By this reversal, the current which was direct becomes inverse, and at the same time the charge associated with the current, which was negative, becomes positive; by this reversal, that is to say, the change of charge from negative to positive in the limb in which the current was direct and is inverse, is that which has been seen to be favourable to the continuance of the contractions. That happens, in fact, which may even bring back these contractions, for if the positive charge is favourable to the continuance of the contractions, it is not difficult to go a step further, and imagine on good grounds that it may have a restorative as well as a preservative power. And further, it is easy to believe that such restoration may not be possible after the time when the contractions come to an end under the inverse current, for this time may simply mark the limit at which the restoration of these contractions can happen under the most favourable circumstances.

## IX.

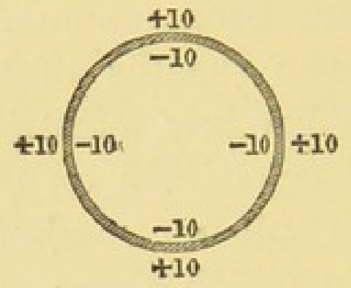
(1.) *The positive charge associated ordinarily with the inverse current may preserve and restore activity in muscle and motor nerve by favouring the continuance of the ordinary electrical condition of the nerve and muscle, if this condition be one of charge in which the sheaths of the fibres are, during rest, so many charged Leyden jars, positive on the outside, negative (by induction) on the inside; for the communication of the positive artificial charge to the outsides of these sheaths may be supposed to induce an equivalent negative charge on the insides, and so produce a state of things which is, in truth, but the exaggeration of the natural charge of the fibres.*

(2.) *The negative charge associated ordinarily with the direct current may be unfavourable to the continuance of activity in muscle and motor nerve by bringing about that state of reversal in the natural charge of nerve and muscle, which is only met with in cases where this activity is all but completely at an end; for the communication of the negative artificial charge to the outsides of the sheaths of the fibres may be supposed to induce an equivalent positive charge on the insides—a change which involves, in fact, the reversal in question, seeing that the weaker natural*

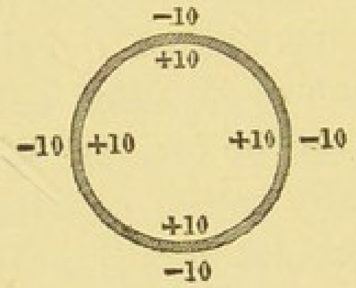
all this implies that the voltaic current flows about not within muscular fi *charge must be overruled by the stronger artificial charge.*

The natural electrical condition of living muscle and motor nerve has been assumed to be a state of charge in which the sheaths of the fibres are so many charged Leyden jars, electrified positively on their outsides and negatively on their insides, with certain exceptions, only met with in states of great vital exhaustion, in which these electrical relations of the two surfaces of the sheaths are reversed. The assumption was that the sheaths of the fibres acted as dielectrics, and that a charge developed on the outsides by oxygenation or in some other way, induced an opposite charge on the insides. What was assumed, in fact, leads naturally to the conclusion that the two artificial charges, associated ordinarily with the inverse and direct currents, may act in a very different manner upon the fibres of nerve and muscle, the positive charge keeping up and intensifying the natural charge of these fibres, the negative charge producing a state of reversal like that which is only met with when these fibres are at a very low ebb as to vitality—a state of reversal which may be spoken of as the exceptional natural charge. For if the sheaths of the fibres act as dielectrics, it is easy to see that the communication of either charge to the exterior must induce a charge of the opposite kind on the interior, and that taking, for the sake of illustration, the tension of the former to be 10 and of the latter 100, and viewing each sheath in transverse section, the state of things in the two natural charges and in the two artificial charges may be represented

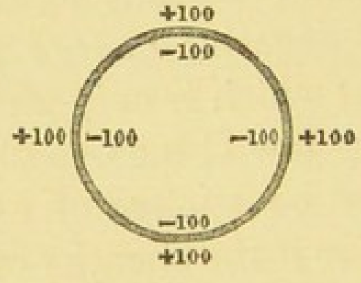
in the ordinary natural charge, thus—



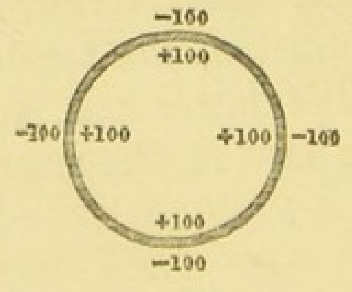

in the exceptional natural charge, thus—




in the artificial positive charge, thus—




in the artificial negative charge, thus—



What is supposed, indeed, leads naturally to the conclusion that the artificial positive charge may be favourable to the preservation and restoration of activity in nerve and muscle, by keeping up and intensifying that particular electric condition which is ordinarily found in association with an active state of these fibres, and that the artificial negative charge may have a contrary effect to this, partly because it puts an end to the ordinary natural charge of the fibres, and partly because the state of reversal in the electrical relations of these fibres produced by it is only met with naturally in a state of all but utter exhaustion. It remains to be seen how far this view will be borne out by what remains to be said—and the story is only yet half told—but even now it is not too much to say, that this view is that which may be looked upon as only a fair inference from the facts which have already come under notice.

## X.

*The continuance of the state of rest during the time the circuit remains closed may be owing to the state of electrical charge, positive or negative, then present; for if the fibres of nerve and muscle are in a state of rest while they retain their natural electrical charge it is to be expected that they will respond in the same way to the artificial electrical charge.*

The continuance of the state of rest during the time the circuit remains closed is a problem which cannot be dealt with fully until the phenomena of electrotonus have come under review. One cause, it is plain, may be the absence of extra-currents at this time. Another cause, it is possible, may be the presence of the electrical charge associated with the voltaic current, for if the fibres of nerve and muscle are in a state of rest so long as their natural electrical charge remains undischarged, it is to be expected that rest, not action, will be their state under the artificial electrical charge. Until, however, the phenomena of electrotonus have been inquired into, it is premature to speculate further upon this matter, and for the present it must suffice to say that in the end good reason will be found for believing that the electrical charge, natural or artificial, has an actual power of counteracting action in both muscle and motor nerve, of producing rest, in fact.

## XI.

*The extra-currents at the closing and opening of the voltaic circuit may throw muscle and motor nerve into a state of action at these moments by discharging the natural charge which is present in muscle and motor nerve during the state of rest.*

Extra-currents, as has been already pointed out, may be looked upon as virtually, if not actually, electrical discharges like those which may be obtained from a charged Leyden battery. Thus related, indeed, it is possible that they may cause the discharge of the electricity with which, in muscle and motor nerve alike, the sheaths of the fibres are charged during rest, for it may be supposed that the passage of the extra-currents across the charged Why fibres will issue in discharge, just as the passage of ordinary electrical discharges across a charged Leyden battery will issue in discharge. Looked at from this point of view, indeed, it is not difficult to see that muscle and motor nerve may be thrown into a state of action because the charge present in them during rest is discharged by the extra-currents. This may not be all that happens when muscle and motor nerve are thrown into a state of action by extra-currents; but this at least must happen, if the extra-currents act like electrical discharges upon fibres which during rest are so many charged Leyden jars; and, therefore, it is not too much to assume, provisionally at least, that muscle and motor nerve may be thrown into a state of action in this case because the electrical charge present in them during the state of rest has been discharged by the extra-currents.

## XII.

*The action of voltaic electricity upon muscle and motor nerve may be resolved into that of charge and discharge, the charge, whether positive or negative, producing the state of rest, the discharge bringing about the state of action, the positive charge preserving and restoring the capability of action, the negative charge having a contrary effect.*

The general drift of the evidence so far is to show that voltaic electricity acts upon muscle and motor nerve, not by polarizing them in one way when the current is inverse, and in the other way when it is direct, but by the positive charge ordinarily associated with the inverse current and the negative charge ordinarily associated with the direct current, and by the extra-currents which attend upon the closing and opening of the circuit. The positive charge preserves and restores the capability of action, the negative charge has a contrary effect, and both charges, while present, keep up the state of rest. The extra-currents, on the other hand, set up the state of action. There is no occasion to call in the polarizing powers of the current to explain what has to be explained. There is no need to fly for help to the so-called inverse and direct currents, or to the constant current in any of its aspects and workings. The case, indeed, is one which resolves itself into an action of charge and discharge upon muscle and motor nerve, the positive charge preserving and restoring the state of activity, the negative charge having a contrary effect, and both charges, while present, producing the state of rest, while contraction is in all cases connected with discharge by means of the extra-current. After all, indeed, it may be that the true action of voltaic electricity upon muscle and motor nerve may be only that which can be illustrated in an experiment in which the extra-currents and the constant current are both excluded, and in which the only possible modes of action left are those of charge and discharge. This experiment consists simply in bringing the prepared limbs of a frog to each of the poles of an open voltaic circuit in turn, care being taken so to manage the limbs as to keep the circuit always open. This is what is done. The result is contraction, not when the limbs are brought to the first pole, but when, after having been brought to this pole, they are carried to the second pole, if only the limbs are perfectly fresh and lively, and the battery sufficiently powerful—a battery, say, of six Bunsen's cells ; and this result is intelligible enough according to the premises. For the case is simply this—that the limbs do not contract when they are charged with the free electricity of the first pole, and that they do contract when, after being thus charged, they become the seat of discharge arising from the conflict of opposite charges, in consequence of being, while charged from one pole, made to receive an opposite charge from the other pole. The case is one in which, the circuit being open, extra-currents and constant current are both excluded, and in which the only modes of action left are those of charge and discharge. It is one which shows, perhaps, that the action of the open voltaic circuit upon muscle and motor nerve may not differ from that of the closed voltaic circuit ; that the action of the extra-currents and constant current may be no more essential in the one case than in the other ; that what is only essential is the charge and the discharge, the charge carrying with it the state of rest, the discharge bringing about the state of action ; while at the same time, by showing that contraction may be caused by discharges procurable from the open circuit, it is suggested that the extra-currents, which cause the contraction when the circuit is closed, may be resolvable into these very discharges.

## CHAPTER V.

# ON THE HISTORY OF ELECTROTONUS, AS INDICATING THE WAY IN WHICH MUSCULAR MOTION IS AFFECTED BY VOLTAIC ELECTRICITY.

## I.

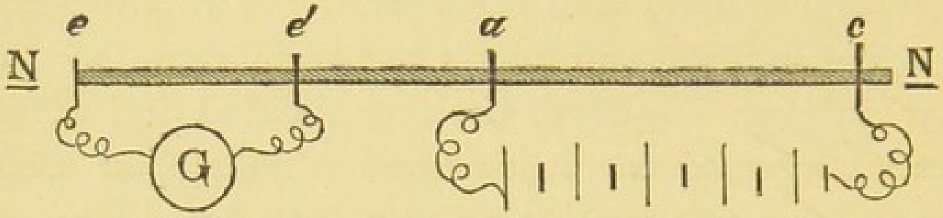
A*LL that has to be done has not yet been done in order to the full elucidation of the phenomena of electrotonus.*

The state to which the name of electrotonus is given is a change in nerve produced by the action of a voltaic current upon the nerve. It makes itself known by certain movements of the needle of the galvanometer which are believed to be due to alterations in the nerve-current, and by certain modifications of impressibility. It consists of two opposite phases, the one, called anelectrotonus, in which the nerve-current is strengthened and the impressibility of the nerve suspended, the other, called cathelectrotonus, in which the nerve-current is weakened, and the impressibility exalted. Much has been done to elucidate these phenomena. Much has been done by the discoverers—by Professor Du Bois-Reymond, who discovered the changes in the nerve-current and by Professor Eckhard, who discovered the modifications in impressibility—and much has been done by Professor Pflüger. Indeed, the complicated and careful investigations of the last-named physiologist would seem to have left but little to be done by others. In point of fact, however, much work still remains to be done which can only be done by going over the whole subject carefully, for, as will appear in the sequel, hasty conclusions have been drawn, both as regards the electrotonic movements of the needle and as regards the electrotonic modifications of impressibility.

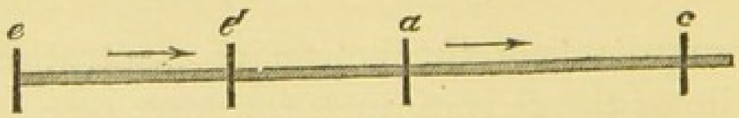
## II.

*The movements of the needle of the galvanometer in electrotonus cannot, as is commonly supposed, be due to modifications in the nerve-current consequent upon the action of the voltaic current upon the nerve, for these movements are still present when the nerve-current is excluded from the experiment by using dead nerve in place of living nerve, and even when certain imperfect conductors, as a piece of string moistened with saliva or water, are substituted for the nerve.*

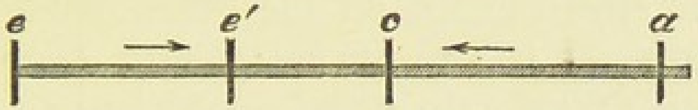
In demonstrating the action of electrotonus upon the needle of the galvanometer, a long piece of living nerve *N, N*—the whole sciatic nerve of a large frog, most commonly and most suitably—is placed with one end upon the electrodes *e, e′* of the galvanometer *G,* and with the other end across the poles, *a*, *c* (*a* for anode, *c* for cathode), of the voltaic battery, thus :—




The piece of nerve within the circuit of the galvanometer is placed so that one electrode is applied to the transverse section and the other to the uncut side—is disposed, that is to say, in a way which gives, if the nerve be fresh, a current of which the course in the nerve is from the transverse section to the uncut side ; and then, after waiting until the needle has taken up the position into which it diverges under the nerve-current, electrotonus is set up by closing the voltaic circuit—anelectrotonus on the side of the galvanometer if the anode be the pole nearest to this instrument, cathelectrotonus if the negative pole be in this position. Before the setting up of electrotonus the needle rests at the point to which it was carried by the unmodified nerve-current ; after the setting up of electrotonus the needle moves one way if the state be anelectrotonus, the other way if it be cathelectrotonus. If anelectrotonus be the state set up by the anode being the pole nearest to the galvanometer, thus—




the needle recedes further from zero, as if the nerve-current acting upon it had been strengthened by the electrotonus. If, on the other hand, cathelectrotonus be set up by the cathode being nearest to the galvanometer, thus—




the needle returns towards zero, as if the nerve-current was weakened by the electrotonus. The case indeed is supposed to be simply this—that the nerve-current is strengthened in anelectrotonus and weakened in cathelectrotonus, because its direction (as the arrows in the diagrams will show) agrees with that of the voltaic current in the former state, and disagrees in the latter. These are the facts which are accepted as constant and fundamental, and this is the explanation which is deemed sufficient to account for them.

It is, however, difficult to believe that the movements of the needle of the galvanometer in electrotonus have to do with changes in the nerve-current produced by the action of the voltaic current; and, most assuredly, this difficulty is one which does not lessen as the facts which have to be noticed are brought together.

One fact, which has the first claim to attention, is made known by simply going on with the ordinary experiment for exhibiting the action of electrotonus upon the needle of the galvanometer. On doing this it is found that the nerve-current soon disappears, but that the movements of the needle belonging to electrotonus, instead of disappearing, continue for a long time. The movements of the needle belonging to the nerve-current come to an end in about 15′, as may easily be proved by simply leaving the voltaic circuit open, and so excluding the action of electrotonus upon the nerve-current; the movements of the needle belonging to electrotonus, on the other hand, continue until the nerve is all but completely dessicated, as may be seen by simply allowing the voltaic circuit to remain closed, and so keeping up the state of electrotonus. With the movements of the needle belonging to electrotonus it is indeed the same before and after the disappearance of the movements of the needle belonging to the nerve-current. The needle moves one way in anelectrotonus and the other way in cathelectrotonus, and these ways are the same before and after the disappearance of the nerve-current. Indeed, the only difference to be noticed after this disappearance is in the starting point of these movements, this point being, not that to which the needle had diverged under the nerve-current as it was before, but that at which the needle happens to be when the galvanometer is short-circuited.

Another point to be noticed here is that movements of the needle in all respects like those belonging to electrotonus may be got when nerve is entirely dispensed with.

If, for example, a piece of common string, moistened with water or saliva, be placed as the nerve is placed in order to exhibit the action of electrotonus upon the galvanometer, the needle is found to move as it moves in anelectrotonus and cathelectrotonus, when the voltaic circuit is closed—as it moves in anelectrotonus if the positive pole be nearest to the galvanometer, as it moves in cathelectrotonus if the negative pole be in this position—the starting point of the movement being that at which the needle rests when the coil is short-circuited. And so also if a piece of cotton thread, or silk thread, or a suitably-shaped piece of gutta-percha, similarly moistened with water or saliva, be substituted for the piece of common string, the movements belonging to electrotonus being always produced, if only the voltaic conditions for producing them are provided.

In these cases the movements belonging to electrotonus are produced when the experiment for producing them is performed upon other bodies than nerve. In the case which remains to be noticed, the result is negative as regards such movement. If, for example, a piece of any kind of metal wire is dealt with in the way in which it is necessary to proceed in order to get the movements of the needle belonging to electrotonus in the other cases, the needle remains motionless. The movements are present when certain imperfect conductors are experimented on. The movements are absent when good conductors are made to take the place of these imperfect conductors. But, be the explanation what it may, the fact remains, namely this—that the movements of the needle of the galvanometer belonging to electrotonus are absent when the experiment for producing them is repeated upon a piece of metal wire; and it is simply to the fact as a fact that attention is now directed.

Until very recently, I believed that these facts had only been noticed by myself. In truth, however, shortly before my attention had been struck by them, Matteucci had discovered that the needle of the galvanometer moves as it moves in electrotonus when the experiment for producing the movement is repeated, not only on dead nerve, but also on strips taken from the substance of the brain, or from the coats of the bladder, and that it does not move in this manner if the body experimented on be a wire of amalgamated zinc, covered with cotton or linen thread, and soaked in a saturated solution of sulphate of zinc ; and thus, instead of resting upon my own observations simply, there is the highest additional testimony which can be had as to the broad inference to which it is now my sole object to direct attention, namely this, that the movements of the needle of the galvanometer belonging to electrotonus cannot be ascribed to modifications of the nerve-current produced by the action of the voltaic current, because the same movements are produced when there is no nerve-current to be thus modified, as in the case where dead nerve is experimented upon, or in that where common string moistened with water, or other imperfect conductors, are made to take the place of the nerve.

III.

*The movements of the needle of the galvanometer belonging to electrotonus cannot be due to the action of a derived current from the voltaic circuit, for with the circuits of the battery and galvanometer insulated (as they always are, or ought to be), a derived current cannot find its way from the battery into the galvanometer.*

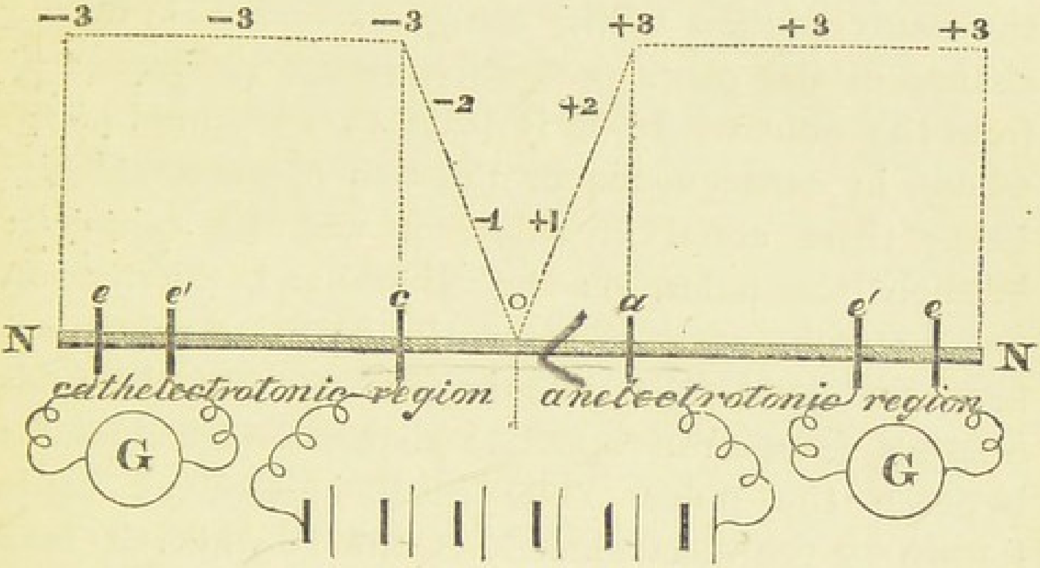
In an experiment for exhibiting the action of electrotonus upon the needle of the galvanometer, care is always taken to insulate the circuit of the battery from that of the galvanometer, so that a derived current from the former circuit cannot escape into the latter, and thus there is no room for the supposition, which might otherwise creep in, that the movements of the needle may be due to the action of a current derived from the battery. With both circuits insulated, as they ought to be, the possibility of such a derived current being the cause of the movements in question is in fact excluded, and, therefore, it may without further comment be taken for granted, that these movements cannot be ascribed to the action upon the needle of a derived current from the primary voltaic current.

## IV.

*The movements of the needle of the galvanometer belonging to electrotonus may be due to the passage through the coil of free electricity from the pole of the battery which happens to lie next the circuit of the galvanometer—of positive electricity from the positive pole in anelectrotonus, of negative electricity from the negative pole in cathelectrotonus,—because the same movements are caused by the passage of the same streams in the same direction from a friction-machine.*

If the movements of the needle of the galvanometer belonging to electrotonus are not to be ascribed to modifications of the nerve-current under the action of the battery, or to a derived voltaic current, how are they to be accounted for? Is it possible that they may be due to streams of free electricity into the coil from the nearest voltaic pole? When streams of free electricity are passed through the coil of the galvanometer from an ordinary friction-machine, the needle moves one way if the stream is positive, the other way if it be negative; and, therefore, it is quite possible that this question may have to be answered in the affirmative. And most assuredly the probabilities of this answer being the correct one do not lessen when this matter is inquired into more particularly.

With the new quadrant electrometer it is easy to see how free electricity may operate in an experiment for exhibiting the action of electrotonus upon the needle of the galvanometer. Testing by this means, signs of free positive electricity are found everywhere—not only within the circuit of the battery, but also within the circuit of the galvanometer and elsewhere—in the region of anelectrotonus; testing by this means, signs of negative electricity are found everywhere—not only within the circuit of the galvanometer, but also within the circuit of the battery and elsewhere—in the region of cathelectrotonus. There is a charge of positive electricity in the former region, of negative electricity in the latter, of which the tension outside the pole keeps at the same height as at the pole, or falls very gradually and slightly as the distance from the pole increases, while inside the pole it falls quickly from the pole, where it is at its height, to some point midway between the poles, where it is at zero. Thus, if the nerve (or quasi-nerve) *N N* be placed, as it is placed in an experiment for exhibiting at the same time the action of both phases of the electrotonic state upon the galvanometer, that is, with its middle portion across the poles, *a*, *c*, of a battery of which the tension at the poles is equal to 3, and with each of its ends upon the electrodes, *e*, *e'*, of a separate galvanometer, *G*, its state as to charge and tension is found to be this—




The case indeed is sufficiently obvious. The nerve, or quasi-nerve, being a very imperfect conductor, prevents, to a certain degree, the opposite electricities, which are continually being liberated at the poles, from running together and uniting between the poles, as they would fully do if the poles were connected by a perfectly good conductor, and thus there is an accumulation of free electricity at the poles, positive at the positive pole, negative at the negative, which free electricity may be supposed to run off by any channel which is open to it, outside the pole as well as inside it. Hence there is no difficulty in accounting for the presence of the free electricity found outside the poles towards the galvanometer, of positive electricity on the side of anelectrotonus, of negative electricity on the side of cathelectrotonus; nor is there any difficulty in accounting for the differences in tension of these charges within and without the poles. Within the poles, the tension must fall from the pole, where it is highest, to some point midway between the poles, where it is at zero, because between the poles the neutralization of the charge of each pole by the charge of the other pole will increase progressively from the pole, where it is least, to some mid-point, where, in consequence of the two opposite charges being there equal in value, it will be complete. Without the poles, on the other hand, the tension must remain what it is at the pole, or nearly so, because outside the pole there is none of the annihilating reaction of opposite charges which operates between the poles. The case, indeed, as made known by the electrometer, is one in which it may easily be supposed that the free electricity thus liberated at each pole, and thus passing outside the pole towards the galvanometer, may enter at the nearest electrode, *e'*, and so pass through the coil in a stream which may act upon the needle, because the free electricity, arrived at this point, will pass more readily through the coil than along the portion of nerve or quasi-nerve connecting the two electrodes, *e' e*, just in proportion as the resistance in the coil is less than that which is encountered in the other channel. And if so, then it is possible to account without difficulty for the movements of the needle which are met with in anelectrotonus and cathelectrotonus. For the simple fact is this—that the needle moves as it moves in anelectrotonus if a stream of positive electricity from a friction machine be passed as the stream of positive electricity from the pole is supposed to pass, and that the needle moves as it moves in cathelectrotonus if a stream of negative electricity from a friction-machine be passed as the stream of negative electricity from the pole is supposed to pass.

Nor is it an objection to this view that the movements of the needle of the galvanometer belonging to electrotonus are not affected by putting an earth-wire to the pole supplying the stream of free electricity which is supposed to act upon the needle. On doing this, the tension of the electricity liberated at this pole sinks to zero, while that of the electricity liberated at the other pole becomes doubled, and, as a matter of course, there is a corresponding change of tension without and within the pole. At first sight it may seem that the earth-wire ought to put an end to the electrotonic movement of the needle by running off the free electricity which is supposed to cause the movement; but not so, on second thoughts. The changes made known by the electrometer, as wrought by the earth-wire, are changes of tension, not changes of quantity, and there is no necessary relation between tension and quantity. There may be any change in tension without any corresponding change in quantity; and in point of fact there is no reason why, in the case in question, in spite of the change in tension, the quantity of the free electricity before and after the use of the earth-wire may be the same. In point of fact, indeed, there is no need that the electrotonic movement of the needle should be affected by the use of the earth-wire in this way, for the free electricity which is supposed to give rise to this movement must act upon the needle, if it act at all, not by the property of tension, but by that of quantity, the galvanometer being the measure, not of tension, but of quantity, which quantity may well be supposed to remain unchanged, seeing that the resistance between the poles, which determines the amount of outflow from the poles, remains unchanged.

## V.

*The fact that, within certain limits, the electrotonic movements of the needle of the galvanometer are proportionate to the degree of resistance interposed between the poles, is intelligible if these movements are due to the outflow of free electricity from one of the poles into the coil, for as the resistance goes on diminishing, more and more of the free electricity liberated at the poles passes in the channel of the constant current between the poles, the inflow ever increasing at the expense of the outflow, until at last, when the resistance is nil, the outflow is altogether lost in the inflow.*

The movements of the needle of the galvanometer belonging to electrotonus are present in cases where there is a certain degree of resistance to the passage of the constant current between the poles—present, within certain limits, in proportion to the degree of this resistance, and absent in the contrary case. Thus it is, apparently, that the divergence of the needle is proportionate to the length of the piece of nerve between the poles; thus it is, apparently, that the needle does not diverge at all when a very short piece of nerve, or a good conductor, like wire, is placed between the poles. Want of sufficient resistance, not absence of secondary polarization, may also be the reason for the absence of these electrotonic movements in the case where the experiment is performed upon amalgamated zinc wire, covered with cotton or linen thread, and soaked in saturated solution of sulphate of zinc; for in this case it is plain that the resistance will be far less than that which is met with in the cases in which use is made of nerve, or of thread moistened with saliva or common water. Instead of being at all exceptional and peculiar, indeed, the case of the covered amalgamated zinc wire, soaked in saturated solution of sulphate of zinc, may be only that of every sufficiently good conductor; and, in short, the fact of the electrotonic movements of the needle of the galvanometer being present in some cases and absent in others, may be no more than the natural consequence of a certain degree of resistance between the poles being present in some cases and absent in others, the resistance acting simply as a bar by which the free electricity which is continually being liberated at the poles is diverted from the channel of the constant current between the poles, and turned outwards into the coil of the galvanometer. The case, indeed, is precisely what it should be if the movements of the needle of the galvanometer are due to an outflow of free electricity into the coil from the nearest voltaic pole, for as the interpolar resistance increases or diminishes, it is easy to understand that the outflow must rise or fall in a way which will readily account for all that needs explanation in the behaviour of the needle as to movement.

## VI.

*The movements of the needle of the galvanometer belonging to electrotonus need not be ascribed to secon-*

*dary polarization, for instead of having to seek the explanation of anything electrotonic in secondary polarization, the explanation of everything belonging to secondary polarization may have to be sought in the direction of electrotonus.*

As has been already seen, the absence of the movements of the needle of the galvanometer belonging to electrotonus in the case where the experiment is performed on amalgamated zinc wire, covered with cotton or linen thread, and soaked in saturated solution of sulphate of zinc, may have to do, not as Matteucci supposed, with the absence of "secondary polarization," but with the absence of a sufficient amount of interpolar resistance. The movements are absent, as it would seem, simply because the connection of the two poles by a good conductor allows so much of the free electricity liberated at the two poles to run together, as not to leave an outflow from the pole into the coil of sufficient strength to act upon the needle. This is all. Instead of being in any sense peculiar, the case of the covered wire of amalgamated zinc, soaked in saturated solution of the sulphate of the same metal, is only that of any good conductor.

Nor is it necessary to call in the aid of secondary polarization to account for the presence of the movements of the needle of the galvanometer belonging to electrotonus. These movements, without doubt, point to the action of a current which is opposed in direction to the constant current—a reverse current; and, therefore, it is quite possible that they may have to do with secondary polarization, for the current belonging to this state is opposed in direction to the current of the primary polarization of the constant current. But the fact of the direction of the current in electrotonus being opposed in direction to the constant current, is no proof of its connection with secondary polarization; on the contrary, there appears to be no good reason why the reverse current of secondary polarization and the reverse current of electrotonus should not be one and the same—no good reason why, instead of having to seek the explanation of the movements of the needle of the galvanometer belonging to electrotonus in secondary polarization, the explanation of the movements of the needle referred to secondary polarization may not have to be sought in electrotonus; at all events, there seems to be no good reason for preferring the view which ascribes the electrotonic movements of the needle of the galvanometer to secondary polarization, to the view which accounts for them as the result of the outflow of free electricity into the coil from the nearest voltaic pole.

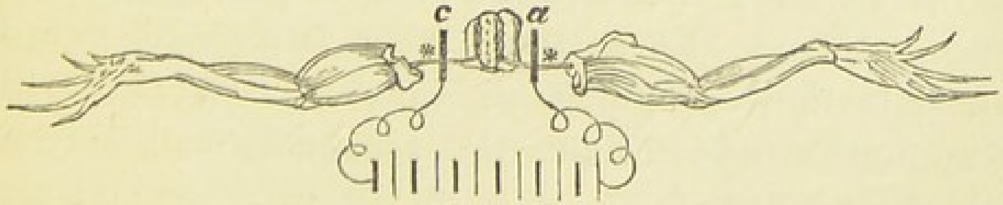
## VII.

*Instead of being suspended by anelectrotonus and exalted by cathelectrotonus, the activity or impressibility of a nerve is suspended by cathelectrotonus as well as by anelectrotonus, though not quite to the same degree.*

All is not known which there is yet to know respecting the way in which the activity or impressibility of a nerve is affected by electrotonus. The end of the investigations of MM. Eckhard, Pflüger, and others, as it is thought, is to show that this state of nerve is suspended by anelectrotonus and exalted by cathelectrotonus; but it is difficult to stay here. It is plain enough that anelectrotonus has this power of suspending; it is not so plain that cathelectrotonus differs from anelectrotonus in having this power of exalting. So far from this being true, indeed, there is reason to believe that the activity or impressibility of nerve is suspended by cathelectrotonus as well as by anelectrotonus, but not quite to the same degree; and that this is so need not long remain in doubt, though the greater part of the evidence to this effect is not at once accessible.

A reason for believing that the impressibility of nerve is exalted by cathelectrotonus is supposed to be contained in a beautiful experiment by Professor Eckhard upon the sciatic nerve of a frog with the gastrocnemius muscle remaining in connection with it. Placing this nerve across the poles of a small voltaic battery, with the cathode next the muscle, so as to allow of cathelectrotonus being set up towards the muscle at the proper time, what is done is—(1), to tetanize the muscle by placing a drop of strong salt water upon the nerve between the muscle and the cathode; (2), to put an end to this state of contraction by diluting this drop with simple water until, for want of saltness, it just ceases to have a tetanizing action upon the nerve; and (3), to set up the state of cathelectrotonus in the part of the nerve exposed to the action of the salt by closing the voltaic circuit. The tetanus has been brought to an end before cathelectrotonus is set up; the tetanus returns upon the setting up of this state. The diluted salt water acting upon the nerve is not strong enough to keep up the state of tetanus before electrotonus is established; it is strong enough afterwards. It seems as if the nerve had become more active, more impressible, by reason of the cathelectrotonus, as if this activity or impressibility had been exalted by the cathelectrotonus. Unfortunately, however, this experiment is too inconstant in its results to count for much as evidence. Not unfrequently, indeed, the tetanus is not brought back by the establishment of cathelectrotonus; now and then even the return of the tetanus is co-incident with the setting up, not of cathelectrotonus, but of anelectrotonus; and in the end the results are found to be so bewildering as to make it necessary to seek in other experiments for the clue which does not seem to be easily traceable in this.

In the last experiment the effects of cathelectrotonus upon the impressibility of the nerve were alone noticed; in the experiment which has now to be described, the effects of cathelectrotonus and anelectrotonus are exhibited side by side. In the last experiment, the sciatic nerve of a frog with the gastrocnemius remaining in attachment was made use of; in this, the parts used are the two hind limbs of a frog prepared so as to have their principal nerves exposed in the usual way. These limbs are arranged as in the figure, with the trunk of each nerve resting at its middle upon one of the poles of a voltaic




battery, of which the circuit is at first open. After this, a state of tetanus is set up in both limbs by applying a drop of salt water on each side to that part of the trunk of the nerve which lies between the pole and the muscle, and which is indicated in the figure by an asterisk, and then, while the tetanus is at its height, the voltaic circuit is closed and the state of electrotonus set up by so doing—of anelectrotonus on the side of the anode, of cathelectrotonus on the side of the cathode. The action of the salt before and after the establishment of electrotonus is the same, but not the result of this action. Before electrotonus is set up, the result of this action is tetanus in both limbs; afterwards, it is the reverse of this, both limbs at once relaxing and remaining relaxed as long as the state of electrotonus is kept up. These are the facts. The case, indeed, is one in which the impressibility of the nerve is seen to be, not suspended in anelectrotonus and exalted in cathelectrotonus, but suspended in anelectrotonus and suspended in cathelectrotonus also; and this not occasionally, but constantly.

Nor is the case otherwise when induced electricity is made to take the place of the salt in this experiment. On the contrary, the use of this new test serves only to bring out the old results in still more unmistakeable plainness.

In addition to the hind limbs of a frog, prepared so as to have the trunks of their principal nerves exposed, and the battery, what is wanted now is an induction apparatus, in which, as in Du Bois-Reymond's inductorium, the secondary coil may be slipped altogether away from the primary coil, and in which each electrode is made up of two separate wires. The trunk of each exposed nerve rests at its middle upon one of the poles of the battery, and at a point a little beyond its middle (the point to which the salt was applied in the last experiment, and which is marked in the figure by the asterisk) upon the electrodes of the same induction apparatus, this latter arrangement having been made possible by making each electrode consist of two separate wires. Before closing the voltaic circuit, and so setting up the state of electrotonus, both limbs are tetanized by putting the induction apparatus in action. Then, the voltaic circuit still remaining open, the induced currents are weakened by drawing away the secondary coil from the primary, until they are barely strong enough to keep up the state of tetanus. Then, and not until then, electrotonus is set up by closing the voltaic circuit. While the voltaic circuit remained open, both limbs continued to contract; when this circuit is closed, both limbs relax. These are the simple facts. As it was with the experiment in which salt was used to put the muscles in action, so it is in this, the setting up of the state of electrotonus is found to have the effect of putting an end to a state of tetanus, or, in other words, of suspending the impressibility of the nerve, on the side of cathelectrotonus, as well as on the side of anelectrotonus.

And this result is not at variance with what is brought to light in the second part of this experiment. Starting from the point in which the tetanizing action of weak induced currents is suspended by electrotonus, and still keeping up the state of electrotonus, the object now is to try the effect, first, of strengthening and then of weakening the induced currents, by moving the secondary coil of the induction apparatus towards the primary in the first instance, and by moving it back again in the second. This is all. The state of electrotonus remains unchanged throughout. With the secondary coil where it was at first tetanus is suspended by the electrotonus. As the secondary coil is brought nearer and nearer to the primary, and as the induced currents are made stronger and stronger by so doing, tetanus is seen to return, not in both limbs together, but first in the limb on the side of cathelectrotonus, and then in the limb on the side of anelectrotonus. As the secondary coil is removed further and further from the primary coil, and the induced currents are again made weaker and weaker by so doing, the tetanus is seen to cease, not in both limbs together, but first on the side of anelectrotonus, and then on that of cathelectrotonus. It is still the same story, that to cause tetanus stronger currents are required in anelectrotonus than in cathelectrotonus.

Using induced currents instead of salt as a means of testing the action of electrotonus upon the impressibility of the nerve, there is indeed only one conclusion to be drawn. It is plain that this state is not suspended in anelectrotonus, and exalted in cathelectrotonus. It is plain that this suspension is met with in cathelectrotonus, as well as in anelectrotonus, though not to the same degree in the former state as in the latter. There can indeed be but one meaning in the facts which have been brought under notice, and most assuredly, I know of no other facts which are in any degree contradictory to them.

The same results are obtained when the experiments are performed in the usual way, that is, upon the sciatic nerve of a frog with the gastrocnemius muscle remaining in attachment. Here also the tetanus caused by salt, and by very weak induced currents, comes to an end in cathelectrotonus as well as in anelectrotonus. A little more patience is required; nothing more. With the sciatic nerve and the gastrocnemius in attachment, one experiment is necessary to illustrate the action of cathelectrotonus, another to illustrate that of anelectrotonus; and many experiments may be necessary to eliminate certain differences arising less from electrotonus than from some accidental peculiarities in the parts experimented upon, or in the manner of conducting the experiment. In a particular experiment it is always possible to suppose that the result might have been different, if, stead of testing for the effects of one form of electrotonus, the testing had been for those of the other form. It is more than probable, also, that some unnecessary complication has been introduced into the problem under consideration, by setting up first one electrotonic state and then the other, in the experiment where use is made of the sciatic nerve and gastrocnemius muscle; for it is to be supposed that the strong tetanus set up when cathelectrotonus is made to follow immediately upon anelectrotonus, may be ascribed quite as much to the cathelectrotonus having been preceded by anelectrotonus as to the cathelectrotonus itself—to the exaltation of impressibility left by the anelectrotonus, in fact. When, indeed, the experiments are made upon a single sciatic nerve with the gastrocnemius in attachment, there are difficulties to be dealt with which do not occur in experiments upon the two hind limbs with their nerves exposed after the manner which has been indicated. With the two limbs, indeed, all these difficulties are done away with. One experiment serves for the comparison of the action of the two electrotonic states at the same moment; the two limbs are as nearly as possible in the same condition as to impressibility; and the very same battery power is used for producing both forms of electrotonus. All the conditions for making a ready comparison of the similarities and dissimilarities in the action of the two electrotonic states upon the impressibility of the nerve are provided; the experiment itself is equally simple; and, as it seems to me, there is good reason for choosing to use, as I have done, the two hind limbs of a frog, connected only by the band of nerve and piece of spine, rather than the single sciatic nerve with the gastrocnemius muscle remaining in attachment.

## VIII.

*A nerve retains its impressibility in anelectrotonus much longer than it does in cathelectrotonus, and any exaltation in this condition would seem to be in anelectrotonus rather than in cathelectrotonus.*

Taking either of the two experiments which have been commented on at the point where the tetanus caused by the salt or induced electricity is being suspended by the electrotonus, it is very interesting to watch what happens when the circuit is kept closed for some time and then opened. When the circuit is kept closed for some time, what is noticed is this. At first both limbs remain at rest; a little later, twitchings of the muscles begin, first in the limb on the side of cathelectrotonus, and then in the limb on the side of anelectrotonus. Later still, these twitchings have ceased in the limb on the side of cathelectrotonus, but in the limb on the side of anelectrotonus, instead of ceasing, they have become more frequent and more marked. They may have ceased in the former limb within 10′ or 15′; they may continue in the latter limb for a full hour, or even longer. Judging by these twitchings, indeed, it would seem that any exaltation in the impressibility of the nerve due to electrotonus is associated with anelectrotonus rather than with cathelectrotonus. And this conclusion is not set aside by what happens when the voltaic circuit is opened after it has been kept closed for some time—by what happens on the cessation of electrotonus, that is to say; for what is noticed now is momentary contraction in the limb on the side of cathelectrotonus, prolonged tetanus in the limb on the side of anelectrotonus. The contraction, that is to say, is more marked on the side of analectrotonus than in the side of cathelectrotonus; and therefore it is still fair to infer that any exaltation of impressibility in the nerve due to electrotonus is in this case associated, not with cathelectrotonus, but with anelectrotonus. It would seem, in fact, that cathelectrotonus has had the effect of lessening the impressibility of the nerve rather than of exalting it. At any rate, this is certain, that the limb on the side of cathelectrotonus ceases to contract on opening the circuit in a short time, say 15′, and that the limb on the side of anelectrotonus continues to contract for a long time—for an hour, it may be, or even longer.

## IX.

*The increased contraction detected by the myograph in cathelectrotonus, instead of showing that the impressibility of muscle is exalted in this state, may have to be accounted for by a view of muscular action which dispenses altogether with a vital property of irritability.*

Measured by the myograph, the force of the muscular contraction produced by a given "stimulus" is found to be greatly increased in cathelectrotonus. Of this there can be no doubt. But this fact of increased contraction cannot well be taken as a certain proof that a vital property of irritability has been exalted in cathelectrotonus. On the contrary, it is possible to account for this phenomenon by a view of muscular action which dispenses altogether with a vital property of irritability—to account for it on purely physical principles; and most assuredly this possibility does not lose in probability as the argument unfolds itself.

## X.

*The view taken of ordinary muscular action is this:—* (1) *that the state of relaxation is brought about by the charge of electricity present in the muscle during the state of rest, the mutual attraction of the opposite electricities disposed on the two surfaces of the sheaths of the fibres elongating the fibres by compressing the sheaths at right angles to their surface; and* (2) *that the state of contraction is caused by the discharge of the charge of electricity present during the state of rest, the discharge leaving the fibres free to return, by* *virtue of their elasticity, from the state of elongation into which they had been forced by the charge.*

The view of muscular action which I have long held is one which dispenses altogether with the help of a vital property of irritability or tonicity. It is one which looks upon living muscle as kept in a state of relaxation, or elongation, by the presence of a charge of electricity, this charge acting by setting up a state of mutual repulsion among the muscular molecules, and which explains the passing from the state of elongation into that of contraction by supposing that the discharge of the charge which had kept up the state of elongation left the fibres free, by virtue of their elasticity simply, to return from the state of elongation in which they had been kept by the charge. And this is the view which I still hold as regards contraction. As regards elongation, however, there are reasons for coming to a somewhat different conclusion. Formerly I was led to think that the muscular fibres were charged with only *one* kind of electricity; now I have reason to know that the *two* opposite kinds of electricity take part in this charge. Charged with only one kind of electricity, it was quite supposable that the state of elongation might be due to the molecules being kept in a state of mutual repulsion by the charge; charged with two opposite kinds of electricity a different explanation becomes necessary. Charged with two opposite kinds of electricity it is to be supposed that these two opposite charges may (or must) neutralize by their mutual attraction any repulsion of molecules arising from the presence of either one of the two charges singly, and it is not easy to see how this double charge can bring about the state of relaxation in the fibres. Still it is plain that in some way or other this state of elongation is connected with the presence of charge; and the question how remains. Is it, it may be asked, that these opposite charges, disposed as in a Leyden jar, the one on the outer surface of the sheaths of the fibres, and the other on the inner, attract each other at right angles to these surfaces, and that compression of the sheaths, of which elongation of the fibres is the result, is brought about by this attraction? And certainly there is nothing intrinsically impossible, or even improbable, in the idea contained in this question. The case supposed is that the sheath of each fibre is a non-conductor. It is that a charge of one kind of electricity developed on the outside of the sheath, and acting through the sheath as a dielectric, induces a charge of the opposite kind of electricity on the inside, and that the elastic sheath is compressed between those charges at right angles to these surfaces. The case supposed, indeed, is one which must lead to elongation or relaxation of the fibres, for the compression of the elastic sheaths, arising from the mutual attraction of opposite charges, thus disposed, must tell in the plane of the surfaces of the sheaths. What is supposed, indeed, may be imitated in every respect, both as regards elongation and as regards contraction, upon a narrow band of india-rubber, coated on its two surfaces within a short distance from the edge by gold leaf, or painted to the same extent with liquid Dutch metal, so as to allow of its being charged and discharged in turn like a Leyden jar, for on charging and discharging this band it is found that the charge is attended by elongation, and the discharge by contraction. The experiment is one which requires no complicated apparatus. The Leyden band, fixed by a clamp at one end and weighted properly at the other, is carried, by means of a piece of string, over a grooved wheel, the movement of which is multiplied upon a pinion carrying a long straw as an index. When the weight has been duly adjusted so as to put the band sufficiently on the stretch, and when the straw index has been adjusted so as to bring it to a zero point on the scale which records its movements, the band is charged by a few turns of a small friction-machine, and then discharged, the arrangements for charging and discharging being of the simplest. When the charge is communicated, the straw index moves steadily away from the zero-point of the scale in the direction which shows that the band is being elongated by the charge; when this charge is discharged it suddenly springs back again to its former position. Elongation is obviously the result of the charge, and shortening or contraction as obviously the result of the discharge. And the reason of all this is plain enough. The charge communicated to one surface of the band, induces the opposite charge on the other surface, and under the mutual attraction of these opposite charges, the elastic tissue is compressed at right angles to its surface—is compressed, that is to say, in a way which must lead to its elongation; for this is the direction in which it is most free to yield. And if so, then the discharge of this charge must lead to the opposite result of shortening or contraction, for by the discharge the elastic band is left free to return from the state of elongation in which it had been kept previously by the action of the charge. What is supposed to happen with the muscular fibre is precisely what is seen to happen in this experiment; and therefore there is nothing unsupposable in the view which is here taken of muscular action. On the contrary, it is scarcely too much to take this experiment as in itself a sufficient reason for removing the view of muscular action, which is here adopted provisionally, from the region of pure speculation into that of actual demonstration.

## XI.

*The increased contraction of cathelectrotonus, instead of being a sign of exalted irritability, may be simply owing to the return of the elastic fibres of the muscle from a state of elongation which is found to be greater in cathelectrotonus than that which naturally belongs to the muscle, and which may be accounted for, if the charge present in cathelectrotonus act like the natural charge, only more potently, because its tension is higher.*

In the experiment for illustrating artificially what is supposed to happen naturally in muscle, it is easily seen that the elongation of the band is, within certain limits, proportionate to the amount of the charge imparted to it, and that the shortening consequent upon the discharge of this charge is in like manner proportionate to the previous degree of elongation. That happens in this case, indeed, which suggests a physical explanation for the increased contraction noticed in cathelectrotonus. For if the charge imparted to the band act in this manner, causing elongation in proportion to its amount, the question naturally arises whether the charge of free electricity associated with cathelectrotonus, which charge is greater than that which is natural to the muscle, may not cause a greater degree of elongation of the muscle than that which is caused by the natural charge. There is, as it seems, no reason why the two charges should not act in the same manner. There is, as it seems, no reason why the negative charge associated with cathelectrotonus should not, when imparted to the outsides of the dielectric sheaths of the fibres, induce an equivalent charge of positive electricity on the insides, and that the mutual attraction of these opposite charges, so disposed, should tell upon the elastic sheaths in precisely the same way as that in which the natural charge of the fibres is supposed to tell. Indeed, if the natural charge act in the way in which it has been supposed to act, this must be the action of the artificial charge, and if this be the action of the artificial charge, then the muscular fibres must be more elongated in cathelectrotonus than in their natural state, for the simple reason that the charge associated with the electrotonic condition is more powerful than that which is natural to the muscle. All this is supposable theoretically. And that this theoretical supposition is justifiable is borne out by the results of measuring the length of the muscle in the natural and in the electrotonic condition. In making this measurement use is made of the apparatus which was used in the experiment for illustrating upon the elastic band what is supposed to happen in muscular action, with this difference only, that in this case the band is removed, and a stage affixed, upon which the muscle to be measured may be placed and secured at a convenient level. The gastrocnemius muscle, with the whole length of the sciatic nerve remaining in attachment, are the parts chosen for examination. Having fixed this muscle upon the stage by passing a pin through its upper end, a thread tied to the tendo-achillis is carried over the grooved wheel, of which the movements are multiplied by the pinion to which the index is attached, and when this is done a weight is fixed to the other end of the thread, which is just sufficient to put the muscle gently upon the stretch. After this the nerve is disposed upon the poles of a voltaic battery, so as to allow of the state of cathelectrotonus being set up on the side of the muscle when necessary, that is, with the poles so placed that the cathode is next the muscle; and then all the preparations are made, except to set the index at zero. Before setting up the state of cathelectronus by closing the voltaic circuit, the muscle is of its natural length, and the index is at zero; after setting up the state of cathelectrotonus by closing the circuit, the index moves in the direction which shows that the muscle has become somewhat elongated. The movement is not considerable, but it is unmistakeable. Such is the fact, and such being the fact, there is reason, apart from that supplied by the experiment with the elastic band described in starting, for believing that the increased contraction of the muscle in cathelectrotonus may be due, not to exalted irritability in the muscle, but simply to the fact that in cathelectrotonus elastic muscular fibres have to return from a degree of elongation which is greater than that which is natural to the muscle, which greater degree of elongation is nothing more than the necessary result of the charge associated with the electrotonic state being greater than that which is natural to the muscle.

## XII.

*Increased relaxation, as well as increased contraction, are found to be features of anelectrotonus no less than of cathelectrotonus, and so they should be, according to the premises, for the fact that the accompanying charge is positive in anelectrotonus, and negative in cathelectronus—the only essential electrical difference in these two electrotonic states—is not one which can affect the result as regards elongation and contraction.*

In anelectrotonus the accompanying charge of free electricity is, not negative, as in cathelectrotonus, but positive. In cathelectrotonus the case supposed was this, that a positive charge was induced on the interior of the sheaths of the fibres by the negative charge imparted to their exterior ; in anelectrotonus, on the other hand, what is supposed is, that a negative charge is induced on the interior of the sheaths of the fibres by the positive charge imparted to the exterior. There is this difference between the two electrotonic conditions, and none other. There is no difference which can affect the result as regards elongation and contraction in the muscular fibres, for it is evident that the attraction of each charge for the other must be the same, whether it be exercised from without the sheaths to within, or, in the opposite direction, from within to without. If, therefore, the view taken of the increased contraction and relaxation of the muscle in cathelectrotonus be the correct one, it follows that there must be increased contraction and relaxation in anelectrotonus as well as in cathelectrotonus; and that there is in fact what there should be in theory may be easily proved by a modification of the experiment which served to show the elongation of the muscle in cathelectrotonus, this modification consisting in arranging the poles of the voltaic battery so as to produce anelectrotonus instead of cathelectrotonus on the side of the muscle, that is, with the anode next the muscle, and in placing under the nerve between the anode and the muscle the two electrodes of an induction apparatus, so as to allow of the muscle being put into action when necessary. First of all, before setting up the state of anelectrotonus by closing the voltaic circuit, the muscle is made to contract by putting the induction coil in action, and note is taken of the degree of contraction as indicated by the movement of the index. In the next place, the index is brought back to zero by breaking the circuit of the induction apparatus, and so allowing the muscle to come to rest. After this, the index being still at zero, and the muscle at rest, the state of anelectrotonus is set up by closing the voltaic circuit, and then, while the state of anelectrotonus is still continuing, the muscle is again made to contract by closing the circuit of the induction apparatus. After first ascertaining the degree of elongation and contraction natural to the muscle, the object is to find out whether there is any alteration in elongation and contraction during anelectrotonus. The experiment is simple enough, and so are the results, for on watching the index which records the behaviour of the muscle during rest and action before anelectrotonus is set up and afterwards, movements are noticed which show most unmistakeably that the muscle elongates more during rest, and contracts more during action, after anelectrotonus is set up, than it did before this time. These are the simple facts. It is evident that increased elongation and increased contraction of the muscle are features of anelectrotonus no less than of cathelectrotonus. It would seem, even, that these features in these two states are undistinguishable; nay, it is certain that they are so, for on repeating the experiment which has just been described with the position of the voltaic poles altered so as to allow of the production of cathelectrotonus instead of anelectrotonus on the side of the muscle, it is found that the movements of the index in cathelectrotonus, while the muscle is at rest, and in cathelectrotonus, when the muscle is in action, agree in degree as well as in direction, with the corresponding movements noticed in anelectrotonus.

## XIII.

*The so-called suspension of irritability in electrotonus may be nothing more than the necessary result of the presence of the charge of free electricity associated with electrotonus, this charge counteracting action by keeping the muscular fibres in a state of forced elongation.*

It has been seen that a muscle may be more relaxed in electrotonus than in its natural state because the charge of free electricity associated with electrotonus is fuller than the natural charge of the muscle. It has been seen that muscle may be less prone to action in electrotonus than in its natural state, because the fuller charge of electrotonus may have more power in counteracting action than the smaller charge belonging to the muscle naturally, and, therefore, there is no necessity to suppose that the irritability is suspended in electrotonus in order to explain any lessened proneness to action in this state; for after what has been said, it seems to be far more natural to believe that the efficient cause of the change in question is to be found in the presence of the fuller charge of free electricity, this fuller charge counteracting action, like the natural charge of the muscle, only more effectually, because of its being fuller than the natural charge. After what has been said, indeed, the only conclusion would seem to be that a vital property of irritability in the muscle or nerve may be a hindrance rather than a help in the solution of the problem under consideration.

## XIV.

*The longer duration of "irritability" in anelectrotonus than in cathelectrotonus may be due to the fact that a positive charge is associated with the former state and a negative with the latter, the action of these two charges in these two cases being precisely what it was in the cases of the so-called "inverse" and "direct" currents already considered.*

It has been seen that the longer duration of "irritability" under the action of the "inverse" current had to do, not with the current being "inverse" in direction, but with the charge of free electricity associated with the current being positive; and that the shorter duration of "irritability" under the action of the "direct" current was connected, not with the "direct" current, but with the negative charge of free electricity associated with this current; and all that remains to be done now is to suppose that these charges act in the same way in the two electrotonic conditions, the positive charge along with anelectrotonus favouring the continuance of impressibility, as did the positive charge along with the "inverse" current, the negative charge along with the cathelectrotonus being unfavourable to this continuance, as was the negative charge along with the "direct" current. Indeed, between the poles the cases are the same, the part traversed by the inverse current belonging to the region of anelectrotonus, and that traversed by the direct current to the region of cathelectrotonus; and, therefore, the explanation which applies in the one case must apply in the other also. Indeed, all that remains to be assumed here, in order to make the comparison complete, is to suppose that the charges of free electricity, positive and negative, act without the poles as they did within them, the positive charge in the extra-polar region of anelectrotonus being still favourable to the continuance of impressibility, the negative charge in the extra-polar region of cathelectrotonus being still unfavourable to this continuance.

## XV.

*The history of electrotonus agrees with that of the "inverse" and "direct" currents already given, in showing that in all cases voltaic electricity acts upon muscle and motor nerve, not by the constant current, but by charge and discharge of free electricity, the charge producing, and in various ways influencing, the state of rest, the discharge bringing about the state action, while at the same time a special argument in favour of this view is to be found in the fact that the increased elongation and contraction of muscle which are met with in electrotonus, may be accounted for as the natural result of the increased charge and discharge which are present in electrotonus.*

In investigating the way in which muscle and motor nerve were affected by the "inverse" and "direct" currents, the conclusion arrived at was that the *constant* current had very little, and that the charge of free electricity associated with this current, and the *instantaneous* extra-currents, had very much to do in the matter. It was that charge went with rest, and discharge with action. It was that the extra-currents brought about action by discharging the charge present during rest. It was that the difference in the duration of impressibility under the "inverse" and "direct" currents was due, not to the current being "inverse," but to the charge associated with it being positive, not to the current being "direct," but to the charge associated with it being negative. It was that these different results were brought about in an intelligible manner by the reaction between the artificial charge, positive or negative, and the natural charge belonging to the fibres of nerve and muscle. It was, finally, that the action of voltaic electricity upon nerve and muscle agreed with that of the natural electricity of the nerve and muscle in charge going along with the state of rest, and discharge with the state of action.

And the conclusion respecting the action of voltaic electricity upon muscle and motor nerve, to which the history of electrotonus points, is substantially the same. It is that the constant current is nothing, and that the charge associated with this current everything. It is that the needle of the galvanometer moves this way or that, because a stream of free electricity, positive in one case, negative in the other, passes through the coil from the nearest voltaic pole. It is that the charge, positive and negative alike, has an actual power of producing the state of rest or of antagonizing action. It is that the increased elongation and contraction ofmuscle which are met with in electrotonus are due, not to exaltation of irritability, but to the mechanical action of the fuller charge and discharge belonging to electrotonus. In a word, the history of electrotonus agrees with that of the "inverse" and "direct" currents in showing that in all cases voltaic electricity acts upon muscle and motor nerve, not by the constant current, but by the charge and discharge of the free electricity (the extra-current being very likely a form of discharge) associated with the constant current, the charge producing, and in various ways influencing, the state of rest, the discharge bringing about the state of action, while at the same time a special argument in favour of the view that the charge produces rest and antagonizes action is to be found in the fact that the increased elongation and contraction of muscle, which are met with in electrotonus, may be easily accounted for as the natural result of the fuller charge and discharge which are present in electrotonus.

## Chapter VI.

## ON THE WAY IN WHICH SENSORY NERVES ARE AFFECTED BY VOLTAIC ELECTRICITY.

### I.

*Up to a certain point, sensory and motor nerves respond to the "inverse" and "direct" currents in the same way, both nerves acting at the closing of the circuit and at the opening also, both nerves resting while the circuit is kept closed: after a certain point, sensory and motor nerves do not respond to these currents in the same way at the closing and opening of the circuit, the sensory acting at the closing, but not at the opening, the motor at the opening and not at the closing, or* vice versâ*; but this difference, instead of showing that sensory and motor nerves are affected differently by the inverse and direct currents, is in reality an argument to the contrary, for all that it means is that each nerve is then in the same state of impaired impressibility by which it is only capable of responding to the extra-current which passes in the same direction as that in which its own natural impressions are transmitted.*

There are several experiments by Lehot, Bellingheri, and Matteucci which show how sensory nerves are affected by the "inverse" and "direct" currents, and a modification of one of these, which I have often repeated, may serve as an example of all the rest.

The two sciatic nerves of a small rabbit, insulated to the necessary extent by raising them in a loop over a piece of gutta-percha of suitable breadth and thickness, are the parts experimented upon in this instance, the "inverse" current being passed along one nerve, the "direct" current along the other, one current at a time.

On closing the circuit of the *inverse current*, and also on opening it, the animal screams and is convulsed; while the circuit is kept closed there are neither screams nor convulsions. At first the screams occur at the closing of the circuit and at the opening also; afterwards, when the nerve is somewhat spent, they occur at the closing only. At first, at the closing of the circuit and at the opening also, there are convulsions in the muscles above the part of the nerve acted upon by the current, and in the muscles below this part also; afterwards the convulsions in the muscles above the part of the nerve acted upon by the current, and in the muscles below this part are at different times, the former being at the closing of the circuit only, the latter at the opening only. Afterwards, in fact, the contractions in the muscles below the part of the nerve acted upon by the current alternate with the screams, the contractions being at the opening of the circuit and the screams at the closing. And these are the contractions to which attention is now called exclusively. Indeed, these contractions are plainly those which have the sole right to be regarded as essential, the others (those which are in the muscles above the part of the nerve acted upon by the current) being reflex phenomena, which may be regarded here as merely accidental.

On closing the circuit of the *direct current*, and also on opening it, the animal screams and is convulsed; while the circuit is kept closed there are neither screams nor convulsions. At first, the screams are at the closing of the circuit, and at the opening also; afterwards they are at the opening only. At first, the contractions in the muscles below the part of the nerve acted upon by the voltaic current (those which are in the muscles above this part, which are merely reflex phenomena, being disregarded) are at the closing of the circuit, and at the opening also; afterwards, they are at the closing only. After a certain time, that is to say, the screams and contractions alternate under the direct current, as they did under the inverse current, but in a different order, the screams being at the closing of the circuit under the inverse current, and at the opening under the direct, the contractions being at the closing of the circuit under the direct current, and at the opening under the inverse, thus :—

| | *State of Circuit.* | *Results as to :—* | |
|---|---|---|---|
| | | (1) *Contraction.* | (2) *Sensation.* |
| Direct current | At the closing | Contraction | o |
| | While closed | o | o |
| | At the opening | o | Sensation |
| Inverve current | At the closing | o | Sensation |
| | While closed | o | o |
| | At the opening | Contraction | o |

Once arrived at this point, these screams and contractions may go on alternating in this order for some time; and, in fact, the only point remaining to be noticed in the experiment is this—that the impressibility of the nerve by which either sensation or motion is practicable, is sooner lost under the direct than under the inverse current.

As regards contraction, there is nothing in the experiment which does not tally perfectly with what has gone before, and about which more than enough has been said already when speaking of the action of the inverse and direct currents in the production of motion. The facts to be dealt with, indeed, are only new in so far as sensation is concerned, and all that has to be done now is to inquire whether the conclusions drawn respecting the production of motion by these currents are applicable also to the production of sensation.

The fact that sensation agrees with motion in being present at the closing of the circuit, and at the opening also, and in being absent while the circuit is kept closed, would seem to show that sensory and motor nerves are affected in the same way by the inverse and direct currents in that sensation, like motion, has to do, not with the constant current, but with the instantaneous extra-currents; and this conclusion is also borne out by the fact that the impressibility of the nerve, in respect of both sensation and motion, is maintained longer under the inverse current than under the direct. Nor is a different conclusion to be drawn from the fact, that after a time sensation alternates with motion in the order set forth in the preceding table. On the contrary, this very alternation, when it is inquired into, is itself a conclusive reason for believing that sensory and motor nerves are affected by the inverse and direct currents in the same way, and that this way is that which was pointed out when speaking of the action of these currents in the production of alternating motion. The fact of contraction being at the opening of the circuit under the inverse current and not at the closing, and at the closing of the circuit of the direct current, and not at the opening, was explained by supposing that at this time the nerve had lost so much of its impressibility as to be only capable of responding to the extra-current which passed in the same direction as that in which motor impressions were transmitted to the muscles. In other words, it was supposed that the contraction was present in the case in which the direction of the extra-current was towards the muscles and absent in the case in which the extra-current passed towards the sensorium. What was supposed, indeed, supplies the explanation, as it would seem, of alternating sensation no less than of alternating contraction—of sensation being at the closing of the circuit under the inverse current, and not at the opening, and at the opening of the circuit under the direct current, and not at the closing. For what is the case as regards sensation? It is simply this—that sensation is present when the extra-current passes towards the sensorium, and absent when it passes towards the muscles, the extra-current which did not cause contraction causing sensation, and *vice versâ*. This is all. The case as regards the production of sensation and motion is, in fact, precisely the same, if it be supposed, as it may well be, that the impressibility of sensory nerve, no less than that of motor nerve, is at the time so far impaired as to leave each nerve only capable of responding to the extra-current which passes in the same direction as that in which its natural impressions are transmitted. And thus, these very alternating sensations, instead of showing that nerve is affected differently by the inverse and direct currents in the production of sensation and in the production of motion, in reality only supply additional reasons for believing that it is affected in one and the same way; while at the same time the fact that these alternating sensations may be explained in the same way as that which was followed in explaining the alternating contractions, may be looked upon as an additional reason for believing that these alternating contractions have been explained in the right way.

## II.

*As in motor nerves, so in sensory nerves, the state of impressibility is suspended by the establishment of electrotonus.*

Several experiments, of which the following may serve as an example, all go to show that as in motor so in sensory nerve the state of impressibility is suspended by electrotonus.

The nerve operated upon in this experiment is the sciatic of a small rabbit. The object in view is to set up electrotonus on the side of the sensorium when this part of the nerve is being acted upon by *weak* induced currents. The plan pursued is to place the poles of an induction apparatus and the poles of a voltaic battery, under different parts of a loop of the nerve, insulated as in the last experiment by means of gutta-percha, the part of the nerve lying upon the poles of the induction apparatus being nearer to the sensorium than that lying upon the poles of the battery. The induction apparatus is put in action before closing the circuit of the voltaic battery, at first with the primary coil more or less covered by the secondary, afterwards with the secondary coil removed so far from the primary as to leave the induced currents acting upon the nerve only just strong enough to give rise to sensation and motion; and then, while the nerve is being acted upon by these weak induced currents, first one phase of electrotonus is set up and then the other, by closing the voltaic circuit with the poles first in one position and then in the other. These are the several steps in the experiment.

The nerve is being acted upon by *weak* induced currents when the state of electrotonus is set up in it. The result of the action of these *weak* currents *before* the setting up of electrotonus is that the animal has all but ceased to struggle and scream as it did under the action of stronger currents of the same kind, which stronger currents are tried in the first instance. The result of the action of these *weak* induced currents *after* the setting up of electrotonus is that the animal at once becomes still and quiet. That action of weak induced currents which issues in sensation as well as motion, is suspended by electrotonus—by cathelectrotonus as well as by anelectronus, though not quite to the same extent. If the induced currents are over a certain strength, the animal may continue to scream and struggle after electrotonus is set up, but not so if they are under a certain strength. In point of fact, the action of electrotonus which may overcome that of weak induced currents may be overcome by that of stronger induced currents; and therefore it is that before electrotonus is set up in this experiment, care is taken to reduce the induced currents acting upon the nerve to the necessary degree of weakness.

## III.

*The action of voltaic electricity upon sensory nerves generally is not a little calculated to confirm all that has been said respecting the action of voltaic electricity upon motor nerve and muscle.*

What has been said of the action of the "inverse" and "direct" currents upon sensory nerves, as far as it goes, is in perfect harmony with all that has been said respecting the action of these currents upon motor nerve and muscle. It is the old story over again, or, if there be anything new, it is only what adds further proof to a part of the old story which most needed confirmation. And what has been said of the action of electrotonus upon sensory nerves, instead of contradicting, is not a little calculated to confirm what has been already said respecting the action of electrotonus upon motor nerve and muscle. In a word, all that has been said of the action of voltaic electricity upon sensory nerves, agrees with all that has been said respecting the action of voltaic electricity upon motor nerves and muscles, for what seems at first to be disagreement, in the end only proves to be a still stronger proof of agreement. And certainly nothing which would lead to a contrary conclusion has been knowingly left unsaid.

## CHAPTER VII.

## ON THE WAY IN WHICH NERVE AND MUSCLE ARE AFFECTED BY ELECTRICITY IN GENERAL.

*THE action of voltaic electricity upon nerve and muscle may be resolved into that of the charge and discharge of free electricity, the charge, the negative as well as the positive, keeping up the state of rest and impressibility, the discharge (for the extra currents are virtually discharges) bringing about the state of action.*

The evidence advanced in the three preceding chapters has shown that the action of voltaic electricity upon nerve and muscle may be resolved into that of the charge and discharge (for the extra-current is virtually a discharge) of free electricity. It has shown that a charge, the negative as well as the positive, but the negative not to the same extent as the positive, keeps up the state of rest and impressibility in nerve as well as muscle, and gives rise, in muscle, to a state of increased elongation or relaxation of the fibres. It has shown that the state of action is brought about by the extra-currents, and that every variation in this state is to be accounted for by simply taking into consideration the changes in the nerve as to impressibility, and the differences in the strength and direction of the extra-currents. Everything, indeed, went to show that voltaic electricity acted upon nerve and muscle, not by the polarization of the constant current, or by any other action of the constant current, but by the charge and discharge of free electricity associated with the voltaic circuit, the charge, the negative as well as the positive, but not to the same extent, keeping up the state of rest and impressibility, the extra-current or discharge bringing about the state of action.

## II.

*The action of franklinic electricity upon nerve and muscle may be resolved into that of the charge and discharge of free electricity, the charge, the negative as well as the positive, but not to the same extent, keeping up the state of rest and impressibility, the discharge bringing about the state of action.*

It is a fact that the living body may be charged from a friction-machine with positive or negative electricity without the production of either motion or sensation, if only care be taken to so manage the charging as to avoid a spark, and that there is neither motion nor sensation while the body remains charged. It is a fact, also, that the sudden discharging of the charge is marked by motion and sensation, one or both. Charge, that is to say, is plainly associated with the state of rest; discharge, not less plainly, with the state of action; this, and no other, is the one single interpretation which is to be put upon the facts.

Again. It would seem that as with voltaic electricity so with franklinic electricity, the positive charge is more favourable than the negative to the continuance of the state of rest and impressibility in nerve and muscle, for it is a fact that the action of muscle caused by discharge after charge will go on for a longer time when the charge is positive than when it is negative. Thus, if the prepared limbs of a frog are insulated and charged from a friction-machine, they will contract when discharged by bringing the finger near one of the toes, from fifteen to twenty times if the charge be positive, but not more than four or five times if the charge be negative. The contraction may be repeated in either case because the toe is taken away from the finger before there has been time for the charge in the limbs to be wholly discharged. The contraction may be repeated more frequently with the positive charge than with the negative, because the former charge is most potent in preserving the impressibility of the nerve and muscle. But be the explanation what it may, the fact that the limbs do contract more frequently with the positive charge than with the negative, must be taken as a reason for believing that, as with voltaic electricity, so with franklinic, nerve and muscle retain their impressibility for a longer time under a positive charge than under a negative.

With franklinic electricity, indeed, there is good reason to believe that the action upon nerve and muscle may be resolved into that of charge and discharge, the charge, the negative as well as the positive, but not to the same degree, keeping up the state of rest and impressibility, the discharge bringing about the state of action; and thus the conclusion here is identical with that which has been already drawn from the consideration of the action of voltaic electricity upon nerve and muscle.

III.

*The action of faradaic electricity upon nerve and muscle may be resolved into that of charge (for there is a charge in the secondary circuit in the interval between the two induced currents), and a discharge (for the induced currents are virtually discharges) of free electricity, the charge going along with the state of rest, the discharge or induced current bringing about the state of action.*

The action of faradaic electricity upon nerve and muscle appears at first sight to be simply that of the induced currents which are supposed to be the sum and substance of this variety of electricity. These induced currents are very conspicuous phenomena. They have a remarkable power of producing motion and sensation. They may act in this respect like extra-currents and discharges of statical electricity. They may so act because they are closely akin if not identical in nature with these extra-currents and discharges. So far all is plain enough.

But this aspect of discharge is not the only aspect in which faradaic electricity agrees with voltaic and franklinic electricity. On the contrary, there is an aspect of charge as well, which must not be disregarded, for it is a fact that the electrodes of the coil in which the induced currents are developed are in opposite conditions as regards free electricity in the interval of rest between the two induced currents—a fact which can only be explained by supposing that the coil is then charged, half positively, half negatively. The fact of charge is not so patent as that of discharge (as the fact of the induced currents may be called), but fact it is, as may be easily demonstrated by means of the new quadrant electrometer; and therefore it may be not unfair to assume that the action of faradaic electricity upon nerve and muscle agrees with that of voltaic and franklinic electricity in being resolvable into that of charge and discharge, the charge still helping to keep up the state of rest and impressibility, the discharge still bringing about the state of action. The case is plain enough as regards the action of discharge, and though not quite so plain as regards the action of charge, it is sufficiently plain to justify the inference that it may, in some degree at least, help in keeping up the state of rest and impressibility with which, and not with the state of action, it is co-incident in point of time.

## IV.

*The action of the natural electricity of nerve and muscle upon nerve and muscle may be resolved into that of the charge and discharge of free electricity, and not into that of the nerve-current and muscle-current, the charge keeping up the state of rest and impressibility, the discharge bringing about the state of action.*

The whole tenor of the evidence passed in review when speaking of the natural electricity of nerve and muscle went to show that living nerve and muscle during rest were in a state of charge, that the state of action was attended by discharge, and that the nerve-current and muscle-current were mere accidental phenomena; and therefore it is quite possible that nerve and muscle may be affected by their natural electricity, as they are affected by the different varieties of electricity which have been noticed in turn—that the charge may keep up the state of rest and impressibility, and that the discharge may bring about the state of action. Indeed, after what has been said, this is the only construction which can fairly be put upon the facts.

## V.

*The action of electricity in general, the voltaic, the franklinic, the faradaic, and that which is natural to the nerve and muscle as well, would seem to be resolvable into that of a charge and discharge of free electricity, each form of charge, the negative as well as the positive, but not to the same extent, keeping up the state of rest and impressibility, the discharge bringing about the state of action.*

The evidence upon which to frame a sound view of the action of electricity in general is not equally patent in all cases. Sometimes it lies on the surface, sometimes it is almost altogether hidden out of sight. Once upon the track, however, it is easy enough to follow on without any fault, and, in a word, the sum of the whole matter amounts to this, that the different varieties of electricity—the voltaic, the franklinic, the faradaic, and that which is natural to nerve and muscle as well—all act upon nerve and muscle, not by the constant current, but by a charge and discharge of free electricity, the charge, the negative as well as the positive, but not to the same extent, keeping up the state of rest and impressibility, the discharge bringing about the state of action.

## CHAPTER VIII.

# ON THE ACTION OF THE BLOOD IN THE PRODUCTION OF MUSCULAR MOTION.

## I.

*THE convulsion attending death by bleeding or strangling, the spasm produced by strychnia or brucia, and other facts, would seem to shew that muscular action is antagonized by arterial blood.*

The convulsion attending death by bleeding and strangling is in each case associated with marked deficiency of arterial blood, and most certainly this association is very significant.

In death by the knife at the shambles convulsion happens when the animal is at the last gasp, and when its vessels are all but completely emptied of blood. The convulsion is plainly co-incident with the loss of blood, and as plainly this coincidence is not simple accident.

In death by sudden strangling, there is at first voluntary struggling, and afterwards unconsciousness and convulsion, the convulsion becoming more and more violent as the blood loses its arterial properties, and being at its height when this loss is complete. The convulsion in this case coincides with a time in which the arteries as well as the veins are filled with black blood, and therefore it is possible that Dr. Brown-Séquard may be right in supposing that it is brought about by the carbonic acid in the blood acting as a stimulus to a vital property of irritability inherent in the muscles or the nerves. In point of fact, however, the convulsion coincides with the time when the vessels are empty of red blood, for to be full of black blood is to be empty of red blood. Hence it may be that the convulsion is due, not to the carbonic acid acting as a stimulus to a vital property of irritability, but simply to want of arterial blood, the convulsion from strangling and the convulsion from hæmorrhage in this way coming into the same category. And surely it is better to have a view which is applicable to both cases equally, than to require a different view for each case.

Want of arterial blood may also have to do with the spasms produced by strychnia or brucia, for one effect of the action of these poisons, as is proved by the investigations of Dr. Harley,* is to prevent the blood from *respiring* as it ought to do.

In one experiment in which this fact is brought to light the plan pursued is :—(1) to take two large test-tubes; (2) to fill them half of blood freshly drawn from the jugular of a calf; (3) to add a few drops of strychnia to the blood in one of them; (4) to cork them up carefully; (5) to set them aside with their mouths downwards, after first well shaking up the blood with the air corked up along with it; (6) to leave them for twenty-four hours in this position, only now and then taking them up for the purpose of repeating the shaking; and (7) to examine the air in each tube

* "Lancet," June and July, 1856.

by Bunsen's method, after it has been thus corked up with the blood for twenty-four hours. The object is to ascertain whether the respiratory activity of the blood, as shown in the composition of the air left over the blood for twenty-four hours, is affected by the poison, and if so, how. The result is that which is set forth in the accompanying table :—

| | Composition of common air. | Composition of air after having been over *simple blood* for twenty-four hours. | Composition of air after having been over *blood containing strychnia* for twenty-four hours. |
|---|---|---|---|
| Oxygen ............ | 20·96 | 11·33 | 17·82 |
| Carbonic acid ... | ·002 | 5·96 | 2·73 |
| Nitrogen ......... | 79·038 | 82·71 | 79·45 |
| | 100·000 | 100·000 | 100·000 |

The case, indeed, is sufficiently obvious. In the air which has been over the blood containing strychnia, there is more oxygen and less carbonic acid than there is in the air which has been over the simple blood ; and thus it is evident that the poison has had the effect of diminishing those respiratory reactions between the blood and the air which issue in the absorption of oxygen and the formation of carbonic acid. The strychnia, that is to say, has worked a change in the blood which may be looked upon as equivalent to loss of blood, for blood that cannot utilize oxygen is as good as lost for all vital purposes. Nay, the change thus worked must be looked upon as equivalent to a copious loss of blood, for in this particular experiment a very minute quantity of the poison has had the effect of lessening the reactions between the blood and the air to the extent of full two-thirds.

And thus there is reason to believe that the spasms produced by strychnia or brucia (for brucia acts upon the blood like strychnia, only not quite so energetically) agree with the convulsions attending death by bleeding or strangling, in being coincident with a marked deficiency of arterial blood.

A similar conclusion is also to be drawn from the vascularity of different muscles, for the fact appears to be that the muscles which are least vascular are most impressible—most ready to respond to the several external agents which are supposed to act as stimuli to muscular action, and at the same time most slow in ceasing to respond in this manner. Thus, the less vascular voluntary muscles of reptiles and fishes are more impressible than the more vascular voluntary muscles of birds and mammals. Thus, the less vascular involuntary muscles of any animal are more impressible than the more vascular voluntary muscle of the same animal. Thus, again, the voluntary or involuntary muscles of a hybernating animal during hybernation are more impressible during hybernation, when the circulation is all but at a standstill, than during summer-life, when the blood courses along the vessels in full stream and at unbated speed. There are indeed many facts, of which these are instances, which seem to show that the action of blood upon muscle, be this what it may, is favourable to rest rather than to action—that the muscle is most impressible which is least supplied with blood; and, so far as I know, there are no facts of an exceptional character which are likely to set aside this conclusion.

In a word, the impression left upon the mind after reviewing the operations of the blood in these various cases is that which was produced at the onset by the fact that death by bleeding is attended by convulsion, namely this—that muscular action in one way or another is antagonized by arterial blood.

## II.

*Arterial blood may antagonize muscular action by keeping up in the muscle and motor nerve that state of charge in the fibres which is associated, not with muscular action, but with muscular rest.*

One way in which muscular motion may be affected by the blood is obviously through the instrumentality of the natural electricity of the muscle and motor nerve. Circulating in the mesh of vessels among the fibres of muscle and motor nerve, the blood may give rise to certain molecular reactions, of which the effect may be to charge the sheaths of these fibres after the fashion of a Leyden jar, a charge of positive electricity developed on the outsides of the sheaths inducing a charge of negative electricity on the insides. The case of these sheaths, indeed, may be precisely that of each electric lamina of the electric organ of the torpedo, for here it may be that the vascular surface is positive and the non-vascular surface negative, because a positive charge is developed on the former surface by the vascular molecular reactions there at work, and that the latter surface is made negative by induction. Looked at in this way, indeed, it is easy to see how it may be that muscular contraction should be antagonized by the action of the blood. As long as the sheaths of the fibres belonging to muscle and motor nerve retain their natural charge, so long do these fibres remain in the state of rest, and thus it may be that blood, by keeping up this charge, may favour, not action, but rest—may, in fact, counteract action. Looked at from this electrical point of view, indeed, it is difficult to come to any other conclusion, for if the sheaths of the fibres during rest are so many charged Leyden jars, and if the vascular reactions on the outsides of the sheaths issue in the development of electricity in the way which has been indicated, rest, not action, must be the result of the workings of the blood upon muscle and motor nerve, or, in other words, muscular action must be antagonized by arterial blood.

## III.

*The operation of the blood in muscular action would seem to be altogether opposed to the dogma that contraction is brought about by the blood acting as a stimulus to a vital property of irritability inherent in living muscle and motor nerve.*

All that has been said upon the action of the blood in the production of muscular motion is opposed to the dogma that the blood produces contraction by acting as a stimulus to a vital property of irritability inherent in living muscle and motor nerve; and with this passing remark the present chapter may well be brought to a close, for all else that might be added in elucidation will find a more fitting place elsewhere.

## CHAPTER IX.

## ON THE ACTION OF NERVOUS INFLUENCE UPON THE MUSCLES.

*THE physiological history of convulsion would seem to show that muscular action in this case is connected, not with the presence, but with the absence of the nervous influence developed in the great nerve-centres by the action of the blood upon these centres.*

The inference to be drawn from the occurrence of convulsion in the course of fatal hæmorrhage would seem to be that muscular action in this case is in some way or other connected with wanting vital activity in the great nerve-centres related to the muscles, or, in other words, with lessened development of nervous influence in these centres. Indeed, this inference is inevitable if it be, as it must be, that the vital activity of these centres is proportionate to the activity of the arterial circulation in these centres.

And, certainly, the inference which may be drawn from the occurrence of convulsion during hæmorrhage is confirmed in the fullest manner by certain experiments of Astley Cooper, and Drs. Kussmaul and Tenner.

"I tied," says Sir Astley Cooper,* "the carotid

* "Guy's Hospital Reports," No. III. 1836.

arteries of a rabbit. Respiration was somewhat quickened, and the heart's action increased ; but no other effect was produced. In five minutes, the vertebral arteries were compressed by the thumb, the trachea being effectually excluded. Respiration stopped almost directly, convulsive struggles succeeded; the animal lost its consciousness, and appeared dead. The pressure was removed, and it recovered with a convulsive inspiration. It then lay upon its side, making violent convulsive efforts, breathing laboriously, and with its heart beating rapidly. In two hours it had recovered, but the breathing was still laborious. The vertebrals were compressed a second time ; respiration stopped ; then succeeded convulsive struggles, loss of motion, and apparent death. When let loose, its natural functions returned with a loud inspiration, and with breathing excessively laboured. In four hours, it moved about, and ate some greens. In five hours, the vertebral arteries were compressed for the third time, and with the same effect. In seven hours, it was cleaning its face with its paw. In nine hours, the vertebral arteries were compressed for the fourth time, and the result was the same, viz., suspended respiration, convulsion, and loss of consciousness. On removal of the pressure, violent and laborious respiration ensued, and afterwards the breathing became very quick. After forty-eight hours, for the fifth time, the compression was applied with the same effect."

The tale which is told by this well-known experiment appears to be that convulsion may coexist with a state of things which involves interruption in the functional activity of the great cranio-cervical nervous centres—for such interruption must necessarily be brought about by arresting the flow of blood through the cervical arteries. And this tale is also that which is told in still plainer terms in the following experiment, by Drs. Kussmaul and Tenner.*

In this experiment, the common innominate and the left subclavian arteries of a rabbit—the only two great vessels proceeding from the arch of the aorta, that is to say, for in this animal the right subclavian and both carotids usually commence in a common innominate artery, while the left subclavian springs independently from the aorta—are included in ligatures of which the knots are so arranged as to admit of being easily slipped. In the first place, the blood is suddenly shut off from the great nerve-centres of the head and neck by tying the ligatures; in the second place, a minute and a half or two minutes later, the blood is allowed to return to these nerve-centres by slipping the ligatures. Upon tying the ligatures, the animal immediately loses consciousness, and falls into a state of general and violent convulsion; upon slipping the ligatures, the convulsion, which is then raging at its height, immediately comes to an end, and soon afterwards consciousness and the voluntary power over the muscles return. Upon loosing the ligatures, the sudden passage from convulsion to muscular relaxation gives the impression of the animal having been struck down at that particular moment by a stroke of paralysis. The result, indeed, is one which appears to be only intelligible on the supposition that the convulsion is dependent upon the interruption in the

* "Untersuchungen z. Naturlehre der Menschen u. d. Thiers," von I. Moleschott, voi. ii. Frankfort, 1859.

supply of nervous influence which the muscles receive from certain great nerve-centres so long as these centres are kept in a state of functional activity by the continuance of the circulation.

In this experiment, indeed, and also in that by Astley Cooper related previously, the lesson to be learnt is that which is taught by the convulsion accompanying death by hæmorrhage, namely this, that muscular action in these cases is the consequence of the sudden arrest in the development of "nervous influence" in the great nerve-centres by suddenly cutting off the supply of blood to these centres.

## II.

*The fact that muscles which are paralyzed by cutting them off from the great nerve-centres may be made to contract with greater force than muscles which are not so paralyzed, would seem to show that contraction in this case is connected, not with the presence, but with the absence of nervous influence.*

There are several experiments, of which the three following may be taken as examples, which would seem to show that the impressibility of a muscle or motor nerve is inversely related to the supply of nervous influence proceeding from the great nerve-centres.

Of three experiments which may be selected as illustrating in a pre-eminent manner this matter, the first is by Dr. Claude Bernard, the two others by Dr. Brown-Séquard.

In the first experiment,* the spinal cord of a rabbit was divided at the root of the neck, with these results. Immediately after the operation the animal lay on its side, helpless, panting, breathing almost exclusively by its diaphragm, passing fæces continually, and deprived, as a matter of course, of all feeling and power of voluntary movement in the limbs and trunk. A little later it had recovered so far as to be able to eat with avidity a carrot which lay at hand. After the lapse of seven or eight hours, the breathings had become very slow and shallow, and a great difference was observable between the paralyzed and non-paralyzed parts, the former being cold, comparatively bloodless, and more impressible than natural, the latter, the ears especially—in consequence of the cord having been divided in the cilio-spinal region—being hot, bloodshot, and less impressible than natural. Thirty minutes after death—the intermediate stages of the experiment are of no moment—the parts which were *not* paralyzed before death had lost their impressibility, and passed here and there into the state of *rigor mortis*, but not so the parts which were paralyzed before death. Thirty minutes after death, indeed, instead of having lost their impressibility, and passed here and there into the state of *rigor mortis*, the parts which were paralyzed before death were more impressible than natural—were so impressible, in fact, that one of the hind limbs, prepared in a suitable manner, was found to be capable of behaving in every way like the so-called rheoscopic limb of a frog. In a word, the effect of cutting off the nervous influence, proceeding

* "Leçons sur la Physiologie et la Pathologie du Système Nerveaux." 8vo, Paris. Tome ii, p. 12.

from the cranial and cervical nerve-centres, is to make the paralyzed muscles of a mammal—in which, by the way, the circulation is reduced to a reptilian standard of activity—as impressible as the muscles of a reptile in their natural state.

A frog is the subject of the second experiment.* In this case, two hours after having divided the spinal cord in the middle of the dorsal region, and the principal nerve of *one* of the hind limbs high up near the spine, the impressibility of the two hind limbs is tested by means of electric shocks of a given strength. What is done is this : what happens is a change in the impressibility of both hind limbs, but chiefly in the one of which the nerve was divided close up to the spine, which is spoken of as " augmented irritability." There is " augmented irritability" in the limb, which is cut off from the brain and upper part of the spinal cord, but which retains its connexion with the lower part of the cord. There is a still higher degree of "augmented irritability" in the limb which is completely severed from the cerebro-spinal nerve-centres ; and therefore the inference would seem to be that the disposition to contraction in this case is inversely related to the supply of nervous influence to the muscles from the spinal cord, as well as from the brain, and not, as has been supposed, to increased spinal innervation upon the liberation of the cord from some " inhibiting" action on the part of the brain.

The third experiment is to measure the force of the contraction produced in one of the hind limbs of a frog by pinching the toes before and at different times after the division of the spinal cord. The plan pursued

* "Comptes Rendus," 17th May, 1847.

here is—(1) to fix the animal by its waist, with the feet hanging downwards, to a convenient support, at a height which allows a small scale, previously attached to one of the feet, to swing freely in the air ; (2) to go on placing weights in this scale, and pinching the toes until the contraction produced by the pinching is counterbalanced by the weight ; (3) to divide the spinal cord in the middle of the back ; and (4) to go on testing at different times after the operation the force of the contraction in the same way. The result of two experiments, in which the frogs may be distinguished as A and B, is as follows :—

| | | A. | B. |
|---|---|---|---|
| Grammes raised *before* the division of the cord ... | | 60 ... | 60 |
| Grammes raised *after* the division of the cord | Immediately after......... | 20 ... | 10 |
| | In 5 minutes............ | 45 ... | 30 |
| | In 15 ,, ............ | 60 ... | 40 |
| | In 25 ,, ............ | 80 ... | 60 |
| | In 60 ,, ............ | 130 ... | 100 |
| | In 120 ,, ............ | 140 ... | 120 |
| | In 4 hours ............. | 140 ... | 130 |
| | In 24 ,, ............... | 150 ... | 140 |
| | In 48 ,, ............... | 150 ... | 140 |

After remaining stationary at this point for several days, the force of the contraction then began slowly to decline, but so slowly, that at the end of a month the weight raised by it was still greater than that raised before the operation ; and it is suggested that even this slow loss of force would have been prevented if due care had been taken to prevent the muscles from wasting by exercising them with electricity.

And thus the lesson to be gathered from these three experiments would seem to be that the muscular action in these cases is, not directly, but inversely related to the supply of nervous influence supplied to the muscles from the great nerve-centres—a lesson which, by the way, may be in some degree enforced by the fact of the great nerve-centres being relatively smaller in reptiles and fishes than in birds and mammals, for if, as must needs be, the size of the nerve-centres may be taken as the standard by which to measure the amount of nervous influence supplied by them, then it follows that the more impressible muscles of fishes and reptiles will receive less nervous influence than the less impressible muscles of birds and mammals.

## III.

*The increased development of nervous influence in the great nerve-centres consequent upon an increased supply of arterial blood to these centres is not accompanied by involuntary muscular action.*

There is an experiment by MM. Kussmaul and Tenner,* which, in addition to supplying further proof that convulsion is brought about by shutting off the supply of blood to the great nerve-centres, shows also that convulsion is not brought about by diverting the flow of blood from the rest of the body to those centres, and in this way increasing greatly the development of nervous influence in these centres, and this experiment is of immediate interest for the second lesson contained in it.

The animal operated upon in this instance is a rabbit. The mode of proceeding is—(1), to cut off the supply of blood from the trunk and limbs, and so

* Op. cit.

divert the whole mass of blood in the body to the head and neck, by putting ligatures upon the subclavian arteries, and upon the aorta a little below the origin of the left subclavian ; (2) to cut off the supply of blood from the head and neck also by compressing the untied vessels between the fingers ; and (3) to allow the blood to return to the head and neck by removing the fingers. And these are the results. On tying the subclavians and the aorta, the animal is paralyzed everywhere below the neck. On compressing the untied vessels, this state of paralysis at once changes into that of general convulsion. On ceasing to compress these vessels, the paralysis returns instantly. There is paralysis, that is to say, not convulsion, under circumstances in which it may be supposed that there is increased development of nervous influence in the great nerve-centres of the head and neck, for at this time all the blood in the body is diverted to these centres ; there is convulsion, not paralysis, under circumstances in which this development must be nil, for what development of nervous influence can there be when the nerve-centres concerned are totally deprived of blood ? The case. indeed, is one which seems to supply a conclusive contradiction to the notion that "active determination of blood to the head" and convulsion go together, by showing that this state of the circulation is associated, not with convulsion, but with paralysis. In other words, the case is one which shows, as plainly as may be, that increased development of nervous influence in the great nerve-centres of the head and neck has to do, not as is commonly supposed, with a state of involuntary and excessive muscular action, but with a state which is in every way the reverse of this, that is, with paralysis.

## IV.

*Instead of being a cause of muscular action, nervous influence would seem to have an actual power of antagonizing such action.*

The drift of all the evidence up to this point would seem to be that the less muscle is supplied with nervous influence, the more it is disposed to pass into the state of action—that, instead of causing such action, nervous influence would rather seem to act by antagonizing it. The idea, it is true, is at variance with all preconceived opinion, but it is not to be got rid of. On the contrary, the more the attention is fixed upon it, the more it seems to stand out as the one logical consequence of any sound process of reasoning.

## V.

*Nervous influences may act upon muscle through the instrumentality of the natural electricity associated with it, producing rest and relaxation when present because this presence implies a state of electrical charge, causing contraction when absent because this absence* (*up to a certain point*) *is accompanied by a discharge analogous to that of the torpedo.*

The natural electricity of the nervous system is an important—perhaps the most important, certainly the most intelligible—element in the composition of "nervous influence," and therefore is quite possible that the nerves may act upon the muscles by means of their electricity ; and certainly there is nothing in the evidence advanced hitherto to make this view in any degree improbable. This evidence has gone to show that the nerves are charged with electricity during the state of rest, and that the state of action in nerves is associated with an electrical discharge analogous to that of the torpedo. This evidence has also gone to show, that muscular action is associated with the subtraction of nervous influence from the muscles rather than with the addition of such influence to the muscles. It would seem, in fact, as if nervous influence operated in the production of muscular action in precisely the same way as that in which the electricity of the nervous system has been seen to operate, and therefore it may fairly be assumed that the nerves *may* act upon the muscles by means of their electricity. Moreover, it is scarcely to be supposed that nervous influence has to act upon the muscles in a way which does not harmonize with that in which the natural electricity of the nervous system is found to act.

## VI.

*The operation of nervous influence in the production of muscular action would seem to be altogether opposed to the notion that this action is brought about by the nervous influence acting as a stimulus to a vital property of irritability, inherent in living muscle.*

The natural inference from the data contained in the context is in every way opposed to current notions respecting the operation of nervous influence in the production of muscular action. The facts, indeed, are precisely what they ought *not* to be if nervous influence brings about muscular action by acting as a stimulus to a vital property of irritability inherent in living muscle, and precisely what they ought to be if this action be counteracted by nervous influence; and with this general remark the question of the action of nervous influence in the production of muscular motion may be dismissed for the present.

## CHAPTER X.

# ON THE PHENOMENA OF RHYTHMICAL MUSCULAR ACTION AS ELUCIDATING THE ACTION OF NERVE AND MUSCLE.

## (A.) ON THE ACTION OF THE HEART.

### I.

*THE action of the heart is simplified by studying the movements of the ventricles before proceeding to deal with those of the auricles.*

For reasons which will soon become apparent, it is desirable in studying the action of the heart to deal with the movements of the ventricles before having to do with those of the auricles. Beginning, indeed, with the movements of the ventricles, and remembering the conclusions already arrived at respecting muscular motion, the way soon opens along which it is only necessary to go step by step in order to arrive in the end at a satisfactory solution of a problem which at the beginning seemed to be all but hopelessly perplexing.

### II.

*The diastole of the ventricles coincides with the time when red blood is injected into the ventricular walls* *through the coronary arteries, the systole with the time when this red blood may be supposed to have become black, so that with the muscle of the ventricle as with ordinary muscle the same rule holds good of arterial blood counteracting, not causing, the state of action.*

At the beginning of the ventricular diastole the coronary arteries fill out with the fresh red blood which is pumped into them by the ventricular systole: at the end of the ventricular diastole these vessels are emptied by the contraction of the muscular fibres among which they are embedded. The muscular walls of the ventricles, that is to say, relax when red blood is supplied to them, and contract when this red blood is converted into black, the contraction being deferred until there has been time for this transformation to take place. The case is one in which it may be supposed that the action of the blood upon the ventricles may be resolved into that of the oxygen of the blood, this latter action producing the state of relaxation while it continues, and permitting for a moment the state of contraction when it ceases. The case, in short, would seem to be essentially the same as that of the heart which can beat out of the body, for this beating, which goes on a long time in common air, and which, after it has come to a stop in common air, may be renewed by changing this air for oxygen, is at once brought to a stop by replacing the common air or oxygen with carbonic acid gas, or nitrogen, or hydrogen. The beating is plainly dependent upon the action of the oxygen for its continuance, and the facts would seem to justify the conclusion that the oxygen in some way or other produces, not contraction, but relaxation, the contraction occurring for the moment when the oxygen is used up, the action of the carbonic acid into which the oxygen is transformed being only that of the negation of oxygenation. At all events the diastole of the ventricles is concurrent with the injection of red blood into the ventricular walls through the coronary arteries, and the systole with the time when this red blood may be supposed to be converted into black, and thus there is good reason for believing that the arterial blood may act upon the ventricular muscle as it has been seen to act upon ordinary muscle, that is by counteracting, not by causing, the state of action.

## III.

*The diastole of the ventricles coincides with the time when the "rhythmic nerve-centres" may be supposed to generate fresh supplies of nervous influence under the pulse of fresh red blood then supplied to them, the systole with the time when this generation may be supposed to be interrupted in consequence of the red blood being transformed into black, and thus with the the muscle of the ventricle as with ordinary muscle there is reason to believe that the state of action is counteracted, not caused, by nervous influence.*

Mr. Paget* has connected the rhythmical movements of the heart with the action of certain nerves and nerve-centres detected within the substance of the heart by MM. Bidder and Rosenberger, using as the

* Proc. of Royal Society, 28th May, 1857.

link of connection the fact, which he himself was the first to bring to light, that in those cases in which parts of the heart can go on beating after removal from the body, these parts are only those in which these nerves and nerve-centres are met with in considerable numbers—parts which border closely upon the lines of junction between the auricles and the ventricles, and between the auricles and the great veins. When the heart of a tortoise, cut out from the body, is divided into two pieces, the one comprising the auricles and the base of the ventricle, the other consisting of the remainder of the ventricle, rhythmical movements are found to continue in the former piece, but not in the latter. When the heart of a frog is set upright in a pool of blood, and then reduced in size by snipping away bit by bit from above downwards, the auricles may be completely removed, and a certain portion of the upper edge of the ventricle also, without producing any appreciable change in the rhythmical movements ; but after this these movements become slower and slower with every fresh snip, until nearly the whole of the upper third of the ventricle has been cut away, when they cease altogether. When ligatures are tied tightly around the lines of junction between the auricles and the great veins in the heart of a tortoise, so as to crush the nerves and nerve-centres which abound thereabouts, the action of the heart is found, first to cease for a while, and afterwards to go on again in the ventricle only. When the heart of a tortoise is cut up into several pieces, some of these pieces are found to go on beating, and others not ; and when the matter is enquired into more particularly, it proves that the pieces which go on beating are those which belonged more or less closely to the line of junction between the auricles and the ventricles. In a word, the evidence contained in these and other experiments of the kind is amply sufficient to show that rhythmical movement is only manifested in those parts of the heart in which the nerves of MM. Bidder* and Rosenberger† are met with in considerable numbers, and in this way to justify the conclusion at which Mr. Paget has arrived—that this movement is connected with these nerves and nerve-centres.

Viewing these facts with the intention of ascertaining their bearing upon the interpretation of nervous action in the rhythmical movements of the heart the conclusion is not different from that already arrived at when speaking of the operation of the nervous system in ordinary muscular motion.

Mr. Paget is of opinion that the rhythm of the heart is due to "time-regulated discharges of nerve-force in certain of the ganglia in and near the substance of the heart, by which discharges the muscular walls are excited to contract;" and that these discharges are themselves due to the nuitrition of the ganglia and contractile tissues, "being, in certain periods, by nutritive changes of composition, raised, with regulated process, to a state of irritability of composition, in their decline from which they discharge nerve-forces, or change their shape in contracting." But this is not the only view which may be taken of the matter. On the contrary, it may be supposed—and the supposition arises necessarily out

* Muller's Archiv., 1852, p. 163.

† De centris motuum cordis. 8vo. Dorpat, 1850.

of the premises—that the intra-cardiac ganglia are affected by the blood in the same way as that in which the great cranial ganglia are affected in the experiments of Astley Cooper and MM. Kussmaul and Tenner (p. 135).

In the case of the heart, indeed, nature may be said to be continually repeating these experiments on a small scale, the diastole of the ventricle following the injection of blood into the coronary arteries by the ventricular systole being the counterpart of the state of general muscular relaxation which follows the experimental unstopping of the cervical vessels, the systole of the ventricles, which happens shortly after the supply of arterial blood to the coronary arteries is interrupted by the passing of the ventricles into the state of diastole, being the counterpart of the convulsive contraction of the whole muscular system which attends the stopping of the cervical vessels. Or it may be supposed—and this supposition is perhaps more applicable to the case—that the intracardiac ganglia are affected by arterial and venous blood in the same way as that in which the brain is affected—that the systole of the ventricles is the counterpart, on a small scale, of the general convulsive contraction which happens when the blood supplied to the brain is deprived of its arterial character, and that the diastole of the ventricle is the counterpart of the state of general muscular relaxation which follows the re-admission of red blood to the brain.

In a word, the simple inference from the facts would seem to be that the ventricles are in the state of diastole or relaxation as long as the supply of arterial blood to certain ganglia keeps up the development of nervous influence in these ganglia, and that the state of diastole or relaxation changes for that of systole or contraction when this development of nervous influence is interrupted in consequence of the arterial blood supplied to the ganglia being then used up ; so that, in fact, the muscle of the ventricle is affected by the "rhythmic nerve-centres," as Mr. Paget calls the ganglia concerned in regulating the rhythm of the heart, in precisely the same way as that in which ordinary muscle would seem to be affected by the action of other nerve-centres.

## IV

*The systole of the auricles may coincide with the diastole of the ventricles, because it is mainly owing to the simple falling-in of the walls of the auricles upon the blood being suddenly sucked into the ventricles at the diastole of the ventricles ; the diastole of the auricles may coincide with the systole of the ventricles, because the walls of the auricles then bulge out under the pressure of the stream of blood which is ever pouring into the auricles from the valveless openings of the great veins, and which at this moment regurgitates towards the veins in consequence of the closure and backward movement of the auriculo-ventricular valves.*

When the ventricles pass into the state of diastole, the auricles fall into that of systole, and, *vice versâ*, when the ventricles pass into the state of systole, the auricles fall into that of diastole. At first sight the movements of the auricles would seem to contradict the conclusions already drawn from a consideration of the movements of the ventricles; but this impression soon passes if the attention be fixed upon the matter for a short time. Indeed, but scant reflection suffices to show that the movements of the auricles may have to be accounted for in a very different way from that which has served to account for the movements of the ventricles—to show, in short, that the former movements are of a purely secondary character, the mere passive consequences of the latter. For what is the case? May the absence of valves at the mouths of the great veins opening into the auricles show that the systole of the auricles is partly, if not mainly, due to the falling-in of the walls of the auricles upon the blood being suddenly sucked away from the auricles into the ventricles at the ventricular diastole? This is a question which readily suggests itself to the mind, and which seems to demand an answer in the affirmative. For if the systole of the auricles had to minister to the carrying on of the circulation as the systole of the ventricles has to do, that is by contracting energetically upon the blood, there would surely be valves at the mouths of the great veins to prevent the regurgitation of blood from the auricles into the great veins at the time of the auricular systole; and so also with the diastole of the auricles, but little reflection is needed to show that this movement may be, in the main, a simple passive consequence of the systole of the ventricles, the auricles yielding to the pressure of the blood then accumulating within them. For the actual case is simply this. Not only is blood continually flowing into the auricles from the valveless openings of the veins, but at one and the same time, by the closure and backward movement of the auriculo-ventricular valves, it is prevented from flowing out of the auricles into the ventricles, and forced back towards the openings of the veins. At their diastole, that is to say, the auricles are subjected to a double distension from within which must cause them to bulge out suddenly—a distension *a fronte* as well as a distension *a tergo*—a distension, too, to which little resistance can be opposed, for the auricles are in reality little more than cisterns formed of dilated veins ; and thus there is good reason for believing that the movements of the auricles may be, in the main, passive consequences of the movements of the ventricles, the diastole being the mere bulging out of the auricular walls under the pressure of the blood accumulating within the auricles, the systole the mere falling-in of these walls on the blood being suddenly sucked from the auricles into the ventricles.

## V.

*The quickening of the movements of the heart when the medulla oblongata or pneumogastric nerves are subjected to the action of feeble electric shocks may be owing, if nervous influence produces, not the systole of the ventricles, but the diastole, to these shocks having lessened the development of nervous influence by partially paralyzing the rhythmic nerve-centres, for to lessen this development may be to quicken the action of the heart by shortening the duration of the ventricular diastole ; the arrest of the movements of the heart, when strong are used in place of weak electric* *shocks, may be owing to these shocks having paralyzed the rhythmic nerve-centres, and so left the ventricles in the relaxed state in which all living muscle remains when left to itself.*

The action of the heart in a frog or dog is immediately brought to a standstill by subjecting the medulla oblongata or pneumogastric nerve to the action of electric shocks of average strength. The brothers E. and H. Weber* were the first to call particular attention to this fact, but to a certain degree they had been anticipated by Professor Claude Bernard,† for on one occasion, while auscultating the chest of a dog whose pneumogastrics were being acted upon by a coil machine, this latter physiologist had ascertained that the sounds of the heart became inaudible whenever the machine was put in action. More recently also, Professor Lister‡ has added much to the store of knowledge on this subject by showing that the movements of the heart are sometimes arrested and sometimes quickened by these electric shocks, arrested by strong shocks, quickened by weak shocks, and that the heart remains in the state of diastole when they are arrested.

And these facts are not unintelligible.

Any shock of any kind above a certain strength transmitted anyhow to any nerve-centre will paralyze that centre; and therefore it is not surprising that strong electric shocks transmitted along the pneumogastric nerves to the rhythmic nerve-centres

* "Handwörterbuch der Physiologie : art. Muskelbewegung," vol. iii, p. 142, 1846.

† Thèse de Dr. Lefèvre. Paris, 1848.

‡ Proc. of the Royal Society, 13th Aug., 1858.

should paralyze those centres and bring the heart to a standstill in the state of diastole, for, like any other muscle, the walls of the ventricle will relax and remain relaxed if left to themselves.

Nor is it unintelligible that the effect of the weaker shock should be to quicken the action of the heart, if so be the action of these shocks be to weaken the rhythmic nerve-centres to a degree short of paralyzing them. For what is the case according to the premises? It is that the disposition to muscular contraction is inversely related to the innervation of the muscles. It is that the muscles become more prone to contract, not less prone, as the nerve-force supplied to them by their nerve-centres becomes lessened; and therefore all that is necessary to explain the quickening of the movements of the heart produced by the weak shocks is to suppose that these shocks have weakened the rhythmic nerve-centres to a degree short of paralyzing them; for to do this, according to the premises, is to leave the ventricular walls more prone to enter into action, and in this way to quicken the movements of the heart.

And this view is not contradicted by further reflection upon it.

The quick pulse of a weakly nervous person is also an empty pulse; the slow pulse of a person in vigorous health is also a full pulse. In the former case the ventricles take in a little blood and expel it quickly, in the latter they take in more blood and are slow to expel it. How, then? Is it that the ventricles in the former case take in less blood because their walls are less relaxed, and in the latter case more blood for a contrary reason? Is it that the ventricular walls are least relaxed in the case where least nervous influence is developed in the rhythmic nerve-centres by the blood supplied to these centres through the coronary arteries, because in this case the electrical charge associated with the nervous influence is then least? Is it that the electrical charge associated with the nervous influence causes elongation of the ventricular fibres in proportion to its amount, this charge acting upon these fibres precisely as did the artificial charge imparted to the band by which some of the phenomena of electrotonus were illustrated (p. 100)? This is the inference which may be fairly deduced from the premises, and which agrees well enough with the facts. Indeed, the ventricular fibres must be more elongated in the case where the pulse is full and slow than in the case where it is empty and quick, for it is to be supposed that the ventricles are emptied at each systole; and thus, the pulse being the guide, there is reason to conclude, not only that nervous influence acts upon the ventricular muscle as it has been seen to act upon ordinary muscle, antagonizing action instead of causing it, but that it keeps up, through the instrumentality of the charge of electricity associated with it, a state of relaxation which is directly proportionate to the degree of innervation.

## VI.

*This view of the action of the heart is strictly in accordance with the premises.*

What has been said is strictly in accordance with the premises. It is not necessary to explain away a single fact. It is still the same story, not of blood or nervous influence producing action by acting as a stimulus to a vital property of irritability inherent in muscle, but of blood antagonizing action, and of nervous influence antagonizing action ; or if there be any difference, it is only one which emphasises the same story, by showing that the degree of the relaxation of the ventricular walls, not the degree of contraction, may prove to be proportionate to the amount of nervous influence—and, by implication, of blood also—supplied to these walls.

## VII.

*This view of the action of the heart involves what may be looked upon as a physical explanation of this action.*

According to the view here taken of it the action of the heart resolves itself really into that of the ventricles, the movements of the auricles being in fact little more than mere passive consequences of the movements of the ventricles.

The view taken of the action of the ventricles is that the afflux of red blood through the coronary arteries to the rhythmic nerve-centres, produces a development of nervous influence in these centres of which the diastole is the result, and that the systole returns when this red blood is transformed into black blood, in consequence of this development then coming to an end for want of red blood to keep it up any longer. All muscle relaxes when left to itself, and no doubt one reason of the ventricles passing into the state of diastole or relaxation is that they are at this time left to themselves. But this is not all. On the contrary, the fact that the pulse is slow and full in persons who may be supposed to be most amply provided with nerve-power, may, as has been pointed out, be taken as a reason for believing that the effect of the special supply of nerve-power from the rhythmic nerve-centres to the muscular fibres of the ventricles during rest may be to produce a special degree of elongation or relaxation in these fibres at this time, and that herein may lie hid a reason for this rhythm, that muscle only being capable of beating which is thus specially circumstanced. It may be, in fact, that the muscle which beats rhythmically is thus kept during rest more on the stretch than ordinary muscle, and that the contraction which follows rest is primarily the effect of the liberation for the moment of the fibre from the innervation which kept it on the stretch during rest. At all events, there is reason to believe that the diastole of the ventricles is brought about by the development of nervous influence in the rhythmic nerve centres consequent upon the afflux of red blood to these centres through the coronary arteries at each systole, and that the systole returns because this development of nervous influence has come to an end in consequence of the red blood, which brought about the diastole, having become converted into black blood. Everything, in short, goes to show that there is nothing peculiar in the way in which the ventricular muscle is affected by blood and nervous influence.

The view taken of the action of the auricles, on the other hand, is that this action is little more than the mere passive consequence of the action of the ventricles. The systole of the auricles coincides with the diastole of the ventricles, because, as it would seem, it is mainly owing to the simple falling in of the walls of the auricles upon the blood being suddenly sucked into the ventricles by the ventricular diastole ; the diastole of the auricles coincides with the systole of the ventricles, because, as it would seem, the walls of the auricles then bulge out under the pressure of the stream of blood which is ever pouring into the auricles from the valveless openings of the great veins, and which at this moment is prevented from passing on into the ventricles, and made to regurgitate towards the veins, by the closure and backward movement of the auriculo-ventricular valves.

Viewed in this way, the history of the action of the heart contains nothing to contradict, and much to confirm, what has been said already respecting muscular motion generally ; and, for the rest, all that need now be said is, that the view here taken of the action of the heart derives no little support from the fact that, at one and the same time, it arises naturally out of the premises and leads as naturally to a physical explanation of this action.

## (B.)—On the action of the vessels.

### I.

*Red blood find its way through the minute vessels more readily than black blood, and thus, with the muscular coats of the vessels, as with the ventricles of the heart and ordinary muscle, there is reason to believe that the state of muscular contraction is antagonized, not caused, by the action of arterial blood.*

The history of the circulation in suffocation, as exhibited in certain experiments by the late John Reid,* of Aberdeen, and by Professor Draper,† the younger, of New York, shows very plainly that red blood finds its way through the minute vessels more readily than black blood.

In one of Reid's experiments, after first laying bare the great vessels in the sides of the neck, and adapting a hæmadynamometer to one of the carotids, a ligature is put around the windpipe of a rabbit and tied. Before tying the windpipe the exposed vessels are easily distinguished by their colour, the carotids being red and the jugulars black, from the colour of the blood within the vessels showing through the coats of the vessels: after tying the windpipe the artery becomes darker and darker in colour as the process of suffocation goes on, and at the end of two or three minutes, when the blood has become altogether venous, it is as black as the vein, while at the same time the hæmadynamometer shows that as the colour of the blood in the artery becomes darker the pulse rises in force, until at last, when the process of suffocation is at its height, this force may be as much as doubled. More and more black blood gets into the artery as the process of suffocation goes on, the artery in the end becoming fuller of black blood than it ever was of red, and at the same time the force of the pulse rises. The case, indeed, is like that which is seen to happen in the simpler experiment in which, after tying the windpipe, a small puncture is made in the carotid, for here the jet of blood is seen

* "Phys., Anat., and Path. Researches." 8vo., Edin., 1848.

† Lectures on the Phys. of the Circulation. "Amer. Med. Monthly," April, 1860.

to change rapidly in colour from red to black, and, within certain limits, to be projected to a greater distance as the process of suffocation goes on.

In one of Professor Draper's experiments, after first exposing the heart and its great vessels, a ligature is put around the windpipe of a rabbit. Before the windpipe is tied the red and black sides of the heart, and the great vessels near the heart, are seen of their natural dimensions; after tying the windpipe the blood is seen to accumulate, not in the vena cava and right side of the heart, as was expected, but in the aorta and left side of the heart, in the aorta first in order—to accumulate, that is, not in the venous system, but in the arterial, the arteries becoming larger and larger, and the veins smaller and smaller, as the process of suffocation makes headway and the blood changes from arterial to venous.

Elucidated by these experiments, and by others of the kind, the phenomena of suffocation go to show, not, as is commonly supposed, that the arterial pulse rapidly fails for want of blood, and that the venous system as rapidly becomes gorged with black blood, but that the arteries become more and more distended with black blood which cannot get on into the veins, and the arterial pulse stronger and stronger as the blood in the arteries becomes more venous in its character. In other words, the history of suffocation is one which shows that black blood finds its way less readily through the minute vessels than red blood, for it is only on this supposition that the increased fullness and force of the pulse in the arteries can be accounted for. And thus it may be taken for granted, that as with the muscle of the cardiac ventricle and ordinary muscle, so with the muscular coats of the minute vessels, the action of red blood is antagonistic to the state of contraction.

## II.

*The fact that vessels relax when cut off from the vaso-motor centres, and contract when their nerves are acted upon by electric shocks, need not show that vascular contraction is caused by nervous influence, for, as with the systole of the cardiac ventricles and ordinary muscular contraction, this contraction may be the result of the natural supply of nervous influence to the vessels being lessened or interrupted.*

More than a century ago Parfour du Petit* discovered several of the effects of dividing the sympathetic nerve in the neck, but it is to Dr. Claude Bernard† and Dr. Brown-Séquard‡ that physiologists are chiefly indebted for a full and exact knowledge of these effects, and also of those which result from the action of electric shocks upon the nerve—to the former chiefly for the knowledge of the effects of dividing the nerves, to the latter chiefly for the knowledge of the effects of electrifying them. In these matters, indeed, the names of these two great physiologists will always be associated, not only as having

* "Mém. de l'Académie des Sciences." 1727.

† "Comptes Rendus de la Soc. de Biologie," Dec., 1852; "Gaz. Méd. de Paris," 1852, p. 72; "Comptes Rendus de l'Académie des Sciences," 28th Nov., 1852; "Leçons sur la Phys. et la Path. du Système Nerveux." Paris, 8vo. Leçons 15 and 16.

‡ "Philadelphia Med. Exam.," Aug., 1852; "Exper. Researches," New York, 1853; "Lancet," 30th Oct., 1858.

discovered facts which are mutually complimentary, but as having, in more than one instance, discovered and enunciated the same fact almost simultaneously.

Removing the inferior cervical ganglion, or dividing the cervical filament of the sympathetic nerve in a rabbit, the effect is rapid and unmistakable increase in the warmth and vascularity of the corresponding side of the head and face—the temperature rising several degrees, the eye, nostril, and ear becoming bloodshot, the pulse acquiring both force and fulness—and this effect may continue with little or no change for weeks, perhaps for months.

Exposing the peripheral portion of the trunk of the divided nerve to the shocks of a coil-machine, the effect is at once to put an end to the state of increased warmth and vascularity which had resulted from division of the nerves.

And these may be taken as the results of dividing and electrifying the vaso-motor nerves anywhere, for later investigations by Dr. Claude Bernard have shown that precisely similar results are brought about when the vaso-motor nerves of the limbs are subjected to the same treatment.

From these facts it may be difficult to deduce any clear notion respecting the action of nervous influence upon the vessels. The vessels relax when cut off from the vaso-motor centres, and contract when their nerves are acted upon by electric shocks. This is obvious. But it does not follow that the action of nervous influence in this case is to cause vascular contraction. On the contrary, the contraction noticed when the vaso-motor nerves are acted upon by electric shocks, may be brought about by the natural supply of nervous influence to the vessels being lessened under these circumstances, the shocks acting upon the vaso-motor centres as they seem to do upon the rhythmic nerve-centres of the heart. And most assuredly this inference is not contradicted by the fact that the vessels relax more when cut off from the vaso-motor centres than they did previously, for this increased relaxation may be only a passive phenomenon, the vessels, relaxed because left to themselves, yielding still further under the pressure of the column of blood forced into them by the action of the heart.

III.

*Red blood may set up a state of relaxation or diastole in the minute vessels by acting upon the vaso-motor nerves as it did upon the rhythmic nerve-centres of the heart, and the cessation of this action, when the red blood is converted into black, may bring back a state of vascular contraction or systole, in the same way that the cardiac systole was brought back ; and thus the mysterious "capillary force," by which the entrance of red blood into, and the exit of black blood out of, the vessels is facilitated, may be resolved into diastolic and systolic movements in the vessels precisely analogous to those which are witnessed in the heart.*

In the fact that red blood makes its way more readily through the minute vessels than black blood, there is reason to believe that a certain degree of dilatation of the vessels is associated with the presence, and a certain degree of contraction with the absence of red blood, and that this dilatation and contraction may be brought about in the same way as that in which the diastole and systole of the cardiac ventricles were brought about, the dilatation by the red blood acting upon the vaso-motor nerve-centres as it did upon the rhythmic nerve-centres of the heart, the cessation of this action, when the red blood is converted into black, bringing back the state of vascular contraction in the same way as that in which the systole of the ventricle was brought back. It is, indeed, difficult to see what other conclusion is to be arrived at if the soundness of the premises remains unimpeached, and if so, then the power by which the entrance of red blood into, and the exit of black blood out of, the vessels is facilitated, may be resolved into diastolic and systolic movements of the vessels precisely analogous to those which are witnessed in the heart, and, by so doing, the mystery of the "capillary force" is in the main disposed of.

## (C.) ON THE PERISTALTIC MOVEMENTS OF THE ALIMENTARY VESSEL.

### I.

*The disposition to peristaltic movement is inversely related to the supply of red blood to the parts.*

M. Spiegelberg,* of Göttingen, has performed several experiments, which show that the disposition to peristaltic movement may be looked upon as inversely related to the supply of blood to the coats of the alimentary canal. In some of these the peristaltic movement in the bowels of a rabbit is seen to be increased by pressing upon the abdominal aorta so as

* Henle and Pfeuffer's "Zeitschrift," 3 Reihe, ii, 1857.

to prevent the admission of red blood to the vessels of the bowel, and diminished when the removal of the pressure allows the blood to return to these vessels. In others, the same movements are seen to be increased, though not to the same degree, when the intestinal vessels are kept full of venous blood by pressing upon the vena cava or vena porta, and diminished when, by removing the pressure, these vessels are allowed at once to get rid of their load of black blood and to receive fresh supplies of red blood. As it was before so it is here, relaxation, not contraction, is associated with the presence, and contraction, not relaxation, with the absence of red blood; and, in short, there is nothing exceptional in the manner in which blood acts upon the muscles in which peristaltic movements are manifested.

## II.

*The disposition to peristaltic movement is inversely related to the supply of nervous influence to the parts.*

The action of the blood upon peristaltic movement being what it is, it follows that the disposition to this movement must also be inversely related to the supply of nervous influence to the parts. Indeed, it is to be supposed that the blood will act upon these movements indirectly through the instrumentality of the nervous system, rather than directly upon the muscular fibres, the development of nervous influence being in direct proportion to the supply of arterial blood to these centres.

## III

*The results of exposing the special nerve-centres con-*

*cerned in the production of peristaltic movement to the action of electric shocks is in no way peculiar.*

M. Pflüger* has shown that the peristaltic movements of the alimentary canal of a dog or rabbit are suspended by submitting the spinal cord or grand sympathetic nerve to electric shocks. Mr. Lister†, going over the same ground, has shown that the movements are sometimes suspended and sometimes increased by acting upon the nerves in this way, suspended if the shocks used be over a certain strength, increased if they be under a certain strength, the results in every particular agreeing with those which he noticed in the heart when experimenting upon the medulla oblongata and pneumonogastric nerves with strong and weak electric shocks. Instead of being at all peculiar, indeed, the case is simply the repetition of that which was commented upon when speaking of the action of electric shocks upon the movements of the heart, and for which, in short, the same comments may serve for what needs to be said in explanation.

## (D.) ON THE RESPIRATORY MOVEMENTS.

### I.

*Inspiration is chiefly accomplished by reflex movements of the walls of the chest, of which the effect is to increase the capacity of the chest, these movements having their origin in impressions made upon the periphery of the afferent nerves of respiration by the*

* "Ueber das Hemmungs-Nervensystem für die peristaltischen Bewegungen der Gedärme." Berlin, 1856.

† Proc. of Royal Society, 13th Aug., 1858.

*oxygen of the air; expiration is chiefly accomplished by the falling back of the walls of the chest upon the cessation of the reflex contractions which had led to inspiration, the impressions which led to these contractions having come to an end in consequence of the oxygen which produced them being then used up in the process of respiration.*

In inspiration the air enters the air-passages because the capacity of the chest is increased by certain contractions in the walls of the chest; in expiration the air which entered the air-passages in inspiration is pressed out again because the cessation of the inspiratory contractions allows the walls of the chest to fall in and spring back again. These contractions, evidently reflex in their character, and as evidently depending upon certain impressions made by the oxygen of the air upon certain afferent nerves of respiration, continue, as it would seem, as long as the oxygen of the air is not used up in the process of respiration, and cease when this process is accomplished, this cessation involving the falling in and springing back of the walls of the chest and the consequent expiration of the air which had been received in inspiration. The contraction is reflex. The oxygen acts, not upon the motor nerve nor upon the muscle directly, but upon the muscle through the instrumentality of a reflex nerve-arc, its immediate action being upon the sensory portion of this arc; and thus the action is different from what it seems to be upon the rhythmic nerve-centres and upon vasomotor centres generally. But this difference, after all, may be more apparent than real, for it is quite conceivable that afferent nerves, which are only calculated to receive and transmit impressions, and nerve-centres, whose office it is to receive impressions and generate nerve-power, may respond very differently to the same agent, what causes action in the one causing rest in the others. At all events, the fact remains, that inspiration is chiefly caused by reflex contractions of the walls of the chest which are, in some way or other, dependent upon the impression made by the oxygen of the air upon certain afferent nerves of respiration, and that expiration is chiefly brought about by the cessation of these contractions co-incidently with the time when the oxygen of the inspired air is used up in the process of respiration.

## II

*Inspiration is partly accomplished by the oxygen of the air producing a state of relaxation or diastole in the air-passages, this oxygen acting upon the muscle of these passages as did the oxygen in the blood upon the muscle of the ventricles of the heart and ordinary vessels, and upon muscle generally: expiration is partly accomplished by the passive return of the air-passages to their former state of contraction or systole, when the oxygen is used up which led to the opposite state of relaxation or diastole, the absence of oxygen in this case permitting contraction as it did in the case of the muscle of the ventricles of the heart and of ordinary vessels, and of muscle generally.*

It is easy to conceive that the air-passages themselves are not altogether passive in the movements of respiration. Passive in the main they are, without doubt, expanding under the pressure of the air entering them when the chest opens out in inspiration, contracting passively when these walls fall in and spring back again in expiration; but this may not be all. On the contrary, it is possible that in inspiration the oxygen of the air may further the expansion of the air-passages which then happens, by acting upon the muscle of these passages as did the oxygen of the blood upon the muscle of the cardiac ventricles and of the ordinary vessels, and upon muscle generally; and that the absence of this action, when the oxygen is used up in the process of respiration, may further a state of contraction in the air passages in the same way as that in which contraction of the cardiac ventricles and of the ordinary vessels, and of muscle generally, is furthered when the oxygen of the blood is used up in the same way. It is possible, in fact, that in this way diastolic and systolic movements may be brought about by which the entrance of fresh air into, and the exit of foul air from, the air-passages may be facilitated. And if so—and that it may be so is to some extent borne out by the movements of respiration in creatures in which the action of anything like a chest is almost or altogether excluded—then it follows that the movements of the air-passages, so far as they are active and independent, instead of being at all peculiar, are in reality ruled by the one law which rules the other forms of rhythmical movement, and muscular movement generally.

III.

*The history of the respiratory and other forms of rhyth-*

*mical muscular movement is in harmony with that of ordinary muscular movement already given; and this latter history seems to supply a key by which the secret of the rhythm in rhythmical muscular movement is disclosed.*

In the preceding remarks upon rhythmical muscular movement the general drift of the evidence has been to show that there is nothing peculiar in this movement—that, in fact, one and the same law rules this movement and ordinary muscular movement; while at the same time, with each fresh step in the argument, it becomes plainer and plainer that the view taken of ordinary muscular action is one which indicates a way by which a physical explanation of the cause of the rhythm in rhythmical muscular movement may be arrived at.

## CHAPTER XI.

# ON THE NATURE OF MUSCULAR ACTION.

### I.

*THE action of blood upon muscle would seem to be, not to cause contraction by supplying a stimulus which awakens into action a vital property of irritability inherent in muscle, but to counteract contraction by keeping up that natural electrical charge which is discharged during contraction.*

The broad inference from the facts which have been adduced already, is, that the disposition to muscular contraction is inversely related to the supply of arterial blood to the muscles. There is no reason to believe that muscular action is ever brought about by the blood acting as a stimulus to a vital property of irritability inherent in muscle. There is good reason to believe that the blood acts in a totally different manner to this, producing, not contraction, but relaxation, by keeping up the natural electrical charge of the muscle, which charge has to be discharged before contraction can happen—a manner of action of which more will have to be said presently when speaking definitely of the electrical history of muscular action.

## II.

*The action of nervous influence upon muscle would seem to be, not to cause contraction by supplying a stimulus which awakens into action a vital property of irritability inherent in muscle, but to counteract contraction by keeping up that natural electrical charge of muscle which is discharged during contraction.*

There is, as it would seem, as little reason for believing that nervous influence causes contraction by supplying a stimulus which awakens into action a vital property of irritability in muscle, as for supposing that blood causes contraction by acting in this manner. As with the blood, indeed, so with nervous influence ; there is every reason to believe that nervous influence acts upon muscle through the natural electricity of the muscle, its presence producing relaxation, its absence permitting contraction. It would seem, indeed, that nervous influence acts upon muscle by the natural electricity associated with it, and that the charge imparted from the nerves to the muscle, acting like the special charge belonging to the muscle, may help in keeping the muscle in a state of elongation, for the fuller pulse of those states of the system in which there may be supposed to be more abundant innervation, may serve to show, as has been seen, that muscle is elongated in proportion to the degree of innervation.

## III.

*The action of all kinds of electricity upon muscle would seem to be resolvable into that of the charge and discharge, the case being this :—*

(1.) *That the sheath of each muscular fibre is highly elastic, and at the same time so wanting in conductibility as to allow it to act as a dielectric.*

(2.) *That a charge of one kind of electricity, usually the positive, developed continually on the outside of the sheath of the fibre by the molecular changes dependent upon oxygenation and other processes,* induces *a charge of the other kind on the inside by acting across the dielectric wall of the sheath, and that in this way, during rest, the sheath is kept in the condition of a charged Leyden jar.*

(3.) *That, during rest, the sheath is compressed at right angles to its surfaces by the mutual attraction of the opposite electrical charges disposed on these surfaces, and that in this way the fibre at this time is kept in a state of elongation which is proportionate to the amount of the charge.*

(4.) *That the charge present in muscular fibre during rest is discharged when this state changes for that of action.*

(5.) *That the discharge which happens in muscular action brings about muscular contraction by liberating the fibres from the charge which kept them in the state of elongation, and so leaving them free to yield to their natural elasticity.*

(6.) *That the contraction is increased in certain cases, as in electrotonus, not because the irritability of the muscle is augmented, but simply because the elasticity* *has greater play in these cases in consequence of the fibre having been kept more on the stretch than usual during rest.*

The drift of the electrical history of muscle, as set forth in these pages, is to show that electricity of every kind and in every case acts upon muscle, not by the continuous current, but by the charge and discharge of free electricity, the charge giving rise to the state of rest, the discharge of this charge bringing about the state of action.

During muscular rest, the sheath of each fibre is kept in the state of a charged Leyden jar, the rule being for the outside to be charged positively, and the inside negatively, the exception for these electrical relations to be reversed. The sheath is a very imperfect conductor—so imperfect as to allow it to act as a dielectric. The one kind of electricity, developed on the outside of the sheath by the molecular changes dependent upon oxygenation and other processes, *induces* a charge of the other kind on the inside by acting through the dielectric wall of the sheath, and so the sheath is kept charged as a Leyden jar is charged. As long as this charge is kept up so long is the fibre at rest. As long as this charge is kept up so long is the fibre in a state of relaxation, or elongation. Within certain limits, also, the degree of elongation thus produced is proportional to the amount of the charge. Thus, judging from the greater fulness of the pulse, the muscular fibre of the cardiac ventricles elongates most in cases where the electric activity of the system may be supposed to be most considerable. Thus, the charge acting upon the muscle in electrotonus, which is greater in amount than that which naturally belongs to the muscle, is attended by increased elongation of the muscle. Nor is this result of the action of the charge upon the fibre unintelligible. On the contrary, it is no more than the necessary physical consequence of the sheath being compressed at right angles to its surfaces by the mutual attraction of the opposite electrical charges disposed upon these surfaces during rest, these charges necessarily attracting each other, and compressing the elastic dielectric sheath between them in direct proportion to their amount. This is the view of the electrical history of muscle, during rest, which seems to be fully borne out by all the facts.

During muscular action, on the other hand, there is a discharge of the charge present in muscle during rest—a discharge analogous to that of the torpedo—and the effect of this discharge is contraction. The sheath of the muscular fibre is elastic as well as dielectric. During rest the supposition is that this sheath is elongated, or kept on the stretch by being squeezed out, as it were, under the mutual attraction of the opposite charges disposed upon its two surfaces. In action, the supposition is that the sheath, liberated by the discharge from the charge which kept the fibre elongated, shortens by virtue of its elasticity simply. This is all. No contraction resulting from the awakening into action by a stimulus of a vital property of irritability is introduced into the problem. The contraction is simply the result of an elastic tissue, which was before kept on the stretch, being left free to yield to its elasticity. And this view is as applicable to the increased contraction of electrotonus as to ordinary contraction, for in electrotonus there is an increased elongation of the fibres during rest which must leave more room for the play of elasticity in contraction. This is the view of the electrical history of muscle, during action, which seems to be fully borne out by the facts.

IV.

*The fact that muscle contracts without change of volume or loss of time is no reason for believing that muscular contraction is not a purely physical phenomenon brought about by elasticity in the way which has been indicated.*

The fact that muscle passes from the state of relaxation into that of contraction without any change of volume or loss of time, has been looked upon as a reason for thinking that the contractile force at work could not be physical, but with very insufficient reason for so doing.

In point of fact, india-rubber or any other elastic body retains the same volume in the stretched and unstretched states; and thus the absence of change of volume when muscle passes from the state of relaxation into that of contraction, or, *vice versâ*, instead of being a matter not to be accounted for physically, may only show that muscle, as an elastic body, behaves like other elastic bodies, and in this way be an additional proof of the view which sees no more than the operation of elasticity in muscular contraction.

Some evidence to the same effect, perhaps, may

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also be found in certain experiments by Dr. Joule,* in which it is shown that a bar of iron suddenly and without any change of volume gains in length and loses in breadth when it is charged with magnetism, and that it as suddenly returns to its former dimensions when the magnetism is discharged.

In one experiment, a square bar of iron, with one of its ends fixed, and with the other end in communication with a system of levers by which any change in its length is multiplied 3,000 times, is placed in the longitudinal axis of a coil composed of insulated copper wire, and after this, it is alternately magnetized and demagnetized by alternately making or breaking the connexion between the coil and a Daniell's battery of half a dozen cells. This being done, what happens is this — that when the bar becomes charged with magnetism, the finger which records on a dial the movements of the system of levers connected with the end of the bar, immediately springs forward to the extent of a quarter of an inch or thereabouts—a movement which shows that the effect of the charge of magnetism has been to cause the bar to gain in length to the extent of $\frac{1}{3000}$th of an inch; and that when the bar loses its charge of magnetism, the finger immediately springs back to the position it occupied before receiving the charge. And in addition to these sudden forward and backward movements of the finger — the movements obviously arising from the charge and discharge of magnetism—there is also a slow forward movement if the coil be kept connected with the battery—a slow

* "Philosophical Mag.," Feb. and April, 1847.

movement arising, as it would seem, from the expansion of the bar under the action of the heat radiating from the current in the coil; but this slow movement is quite distinct from the sudden movements, and that it is so is proved by the fact that the shifting of the finger on the dial to another position under the slow movement does not alter the amount of forward or backward motion which is connected with the charge and discharge of magnetism.

In this experiment, it is seen that a bar of iron suddenly gains in length when it is charged with magnetism, and as suddenly loses its length when the magnetism is discharged; in the experiment which has next to be noticed, it is seen that these changes are unaccompanied by any alteration in volume—that, in fact, the gain in length is accompanied by a compensative loss in breadth.

This companion experiment is as follows:—

A conductor consisting of ten insulated copper wires, each wire being $\frac{1}{20}$ of an inch in diameter and 110 yards in length, is coiled around a glass tube, forty inches in length and one inch and a half in diameter. One end of this tube is closed permanently in glass; the other end has a cork provided with a vent-hole and having a graduated capillary tube fitted into this hole and projecting from it. The graduation of the capillary tube is made upon a scale of which one degree is equal to $\frac{1}{4500000}$th part of the bar which has to be magnetized and demagnetized. The bar itself is of annealed iron, one yard in length and half an inch in diameter. In proceeding with the experiment, this bar is placed in the tube within the coil; then water is poured in so as to fill the tube;

then the cork is adjusted so as to force the water to a convenient height in the capillary tube projecting from the vent-hole in the cork ; and finally, the bar is alternately magnetized and demagnetized by alternately connecting and disconnecting the coil with a Daniell's battery of half a dozen elements. What happens is this—that the level of the fluid in the capillary tube is not affected by the connexion or disconnexion of the coil with the battery. The result, that is to say, is one which shows very plainly that the alternate charge and discharge of magnetism has produced no alteration in the volume of the bar ; for if it had been otherwise, the sudden changes in the length of the bar (of which there was evidence in the last experiment) would cause the level of the fluid in the capillary tube to rise twenty degrees when the bar is charged with magnetism, and to fall as many degrees when this magnetism is discharged. In other words, this experiment shows very plainly that the increase of length which the bar undergoes when charged with magnetism, and of which there was evidence in the last experiment, is accompanied by a compensative decrease of breadth—that, in fact, the changes in form are not accompanied by changes in volume.

In this last experiment, the level of the water in the capillary tube rises slowly if the coil be kept in connexion with the battery, and this slow movement is evidently owing to the same cause as that which produced the slow movement of the finger upon the dial in the first experiment, namely, the expansion of the magnetized bar under the action of the heat given out by the galvanic current in the coil.

At any rate, it is evident that this slow rising in the level of the water in the capillary tube, under these circumstances, does not invalidate the fact that the level of the water does not undergo any alteration at the moment when the bar is charged with magnetism by connecting the coil with the battery, or at the moment when this magnetism is discharged by breaking this connexion ; and this is the fact which is of interest in the present inquiry.

It is plain, then, that a bar of iron may suddenly and without any change of volume gain in length and lose in breadth when it is charged with magnetism, and that it may as suddenly return to its former shape when this magnetism is discharged,—it is plain, that is to say, that a bar of iron, under these circumstances, may undergo changes which are strictly parallel to the changes of muscular fibre which constitute the opposite states of contraction and relaxation ; and, therefore, it is fair to conclude that these changes in muscular fibre are not inconsistent with the physical theory of muscular motion which is now under consideration.

## V.

*The fact that a relaxed muscle is more readily lacerable after death may have to do, not with the loss of vital power, but with simple impairment in the physical integrity of the muscle.*

After death, the muscular fibre may be weakened by the solvent action of the fluid, more or less analogous to gastric juice—the juice of flesh—which is contained in the muscular tissue, or by the commencing resolution of the muscular molecules into their constituent elements. After death, the strain upon the muscular fibres may fail to produce that state of contraction in the fibres which it would not fail to produce during life, and in this way muscle may become more readily lacerable after death, for it is to be supposed that the molecules of the muscular fibres will oppose a greater resistance to this strain when they are nearer together, as they are in contraction, than when they are further apart, as they are in relaxation. But, be the explanation what it may, the fact that muscle is more readily lacerable after death is no reason for believing that this change is owing to the muscle having lost at death a vital property of contractility.

## VI.

*It is more easy to resolve the contractile force of muscle into elasticity than to accept the explanation, based upon the doctrine of the correlation of the physical forces, that the natural electricity of the muscle is transformed into this force.*

The electrical history of muscle from beginning to end is, as it would seem, altogether opposed to the view which would look upon the contractile force of muscle as the product of the transformation of the natural electricity of muscle. This view, no doubt, finds no little support in this doctrine which suggests it—the doctrine of the "correlation of the physical forces;" but the view which would resolve the contractile force into elasticity is, as I take it, more simple in itself, and more in accordance with the facts. If the electrical history of muscle be that which is set forth in these pages, elasticity must operate in contraction; and it is surely more philosophical to prefer a cause about the existence of which there can be no doubt to a cause of which even the existence may be called in question, for after all there is no certain proof that the natural electricity of muscle is transformed into the contractile force of muscle.

## VII.

*It is more easy to to believe that certain so-called stimuli bring about muscular action by disturbing the electric equilibrium of the muscle than to suppose that they act by awakening a vital property of irritability inherent in muscle.*

A muscle may be thrown into a state of action by several local agents, mechanical, chemical, and others, which agents are supposed to act as "stimuli" to a vital property of irritability inherent in muscle. The idea seems to be that this property is awakened into action very much in the same way that a person who has been asleep is roused and made to bestir himself by shaking him. But this is not the only view which may be taken of the matter. It is possible, indeed, that the so-called "stimulus" may disturb the electric equilibrium of the muscle, and so lead to action by bringing about a discharge analogous to that of the torpedo; and this view, to say the least, is as intelligible as the other. It is possible that this discharge may be brought about by compressing the sheath of the fibres so as to bring the two opposite charges with which the two surfaces of the sheath are charged within discharging distance. It is possible that the effect of the so-called "stimulus" may be to cause a local reversal in the electrical relatives of the insides and outsides of the sheaths of the fibres in the part acted upon, by which reversal fibres whose sheaths are positive internally and negative externally will be brought into juxtaposition with fibres the electrical relations of whose sheaths remain unchanged—which remain positive externally and negative internally, and that in this way discharge may be brought about, for to have fibres whose sheaths are negative externally and positive internally among fibres which are positive externally and negative internally, will be to bring opposite electricities together which are ordinarily kept apart, and so necessitate discharge. It is possible that a discharge may be brought about in one or other of these ways, and that there may be no sign of it without the muscle, for in reality the circuit through which it passes is merely between the two surfaces of the sheaths of the acting fibres. And, most assuredly, nothing is lost by adopting this electrical view of the action of the "stimuli" in question in place of the current view, for nothing is gained by adhering to a view which assumes as its basis an unintelligible stimulation of an unintelligible vital property of irritability capable of being stimulated.

## VIII.

*It is not necessary to call in the aid of a vital property of muscular irritability, capable of responding to certain kinds of stimulation, in order to account for the phenomena of muscular action, for a sufficient* *physical explanation may be found for all these phenomena.*

Everything that has been said has gone to contradict in some degree the current notion that muscular contraction is the result of a vital property of muscular irritability being awakened or provoked into action by a stimulus, and to bring muscular action within the domain of simple physics, by showing that during rest the muscular fibres are kept on the stretch by their sheaths being charged in the way in which a Leyden jar is charged, and that contraction is nothing more than the result of the fibres being liberated from the stretched condition in which they are kept during rest by the discharge of this charge, this discharge leaving them free to yield to their natural elasticity. There is no need to suppose that the contractile force is the product of the transformation of the natural electricity of the muscle. There is no need to assume that this force has anything to do with vitality. In point of fact, a vital property of irritability responding to stimulation of various kinds, according to the current notion, must be, to say the least, unnecessary. And, after all, what is it that is gained by supposing that muscle contracts because a vital property of irritability inherent in the muscle has been awakened or roused into action by some stimulus? What, indeed! What more, then, would be gained by saying that a muscle contracts because it has a gift of contracting, or that an animal lives because it is alive!

## CHAPTER XII.

# ON THE NATURE OF RIGOR MORTIS.

## I.

THE *fact that rigor mortis does not make its appearance until long after the time when the heart has ceased to beat, and that the muscles in which it has made its appearance may be made to relax and recover their lost impressibility by again supplying them with blood, would seem to show that in some way or other the state of rigor mortis is antagonised by the blood.*

The investigations of Dr. Brown-Séquard,* and of the late Professor Stannius, of Rostock,† shed a new light upon the history of rigor mortis, by showing that muscles which have passed into this state of rigidity may be made to relax and recover their lost impressibility by again supplying them with blood.

On the 12th of July, 1851, Dr. Brown-Séquard began an experiment which consisted in repeatedly injecting a pound of defibrinated dog's blood into the principal artery of the arm of a criminal who had been

* "Comptes Rendus," Juin 9 et 28, 1851.

† "Untersuchungen über Leistungfähigkeit des Muskeln und Todtenstarre, Vierordts-Archiv. für Phys. Heilkunde." Stuttgart, 1 Heft, 1852.

guillotined at 8 o'clock on the morning of that day. The injections were commenced at 11 p.m., the arm then being in a perfect state of rigor mortis. A moment or two afterwards, some reddish spots, not unlike those of measles, made their appearance, more particularly about the wrist. Then these spots became larger and still larger, until the whole surface acquired a reddish violet hue by their meeting and merging. A little later, and the skin generally had acquired its natural living colour, elasticity, and softness, and the superficial veins stood out distinct and full as during life. Then the muscles relaxed, and became again impressible, first in the fingers, afterwards in the shoulder. At 11.45 p.m. this impressibility was found to be more decided than it was at 5 p.m., at which time the corpse was first examined; and from 11.45 p.m. until 4 a.m., when the operator was obliged to succumb to fatigue, there was no alteration in this respect. When the experiment was commenced the temperature of the blood was 75° Fahrenheit, of the room 66°.

Another experiment was upon a full-grown rabbit which had been killed by hæmorrhage. In this case, after waiting until rigor mortis had fully set in, Dr. Brown-Séquard injected the defibrinated blood of the same animal into the principal vessel of one of the hind limbs. Fifteen minutes afterwards, the muscles of this limb had lost their stiffness, and recovered their impressibility. From this time, throughout the night, until 3 p.m. on the day following, the injections were repeated at intervals of from twenty to thirty minutes, and all this while the relaxed muscles were highly impressible. From 4.50 p.m. to 7 p.m., the injections were repeated at tolerably regular intervals, with the same results as at first, rigor mortis being fully re-established in the part from which it had been banished when the experiment was resumed. On the morning following, this part was again in a state of cadaveric rigidity, while the rest of the body, which all along had been in this state, was beginning to pass out of it. On the third morning, the body was supple, and everywhere in an advanced state of putrefaction, with the exception of the limb upon which the injections had been practised, and here no signs of the departure of rigor mortis were as yet perceptible.

About the time that Dr. Brown-Séquard was engaged in these and other experiments of the kind, Professor Stannius, without any knowledge of what was being done in Paris, was carrying out an analogous series of inquiries at Rostock.

At 7.30 a.m. on the 21st of July, 1851, this able physiologist put ligatures around the abdominal aorta and crural arteries of a puppy, and tied them. A few minutes after 10 a.m. the muscles had begun to stiffen in all parts from which the blood was excluded. At 10·45 a.m. both hind limbs were stretched out, and perfectly stiff and cool. At 11.40 a.m. the ligatures were loosened, and the blood was seen and felt to penetrate into the empty vessels. At 11.45 a.m. the natural warmth had returned in some degree to both hinder limbs, and the right limb was a little more flexible than the left. At noon both limbs had undoubtedly recovered their flexibility, and it once appeared as if the left had moved spontaneously ; but no sign of pain was caused by pinching the toes. At 12.30 p.m. the muscles which had been rigid, contracted everywhere on the application of galvanism; and at one time this application seemed to cause pain, for the animal, which was before quiet, gave a sudden plunge forward when it was made. Death happened unexpectedly at 12.45 p.m.

Early in the morning of the day following, a similar experiment was performed upon another puppy. At noon the paralyzed hinder limbs were perfectly supple, but the muscles below the knee had ceased to respond to the action of galvanism. At 2.15 p.m. both these limbs were stretched out and rigid, and all signs of impressibility were at an end. At 2.35 p.m. the ligatures were untied. At 3.35 p.m. galvanism gave rise to strong contractions in the muscles of both thighs, and to weaker contractions in the muscles of the left leg below the knee; and very few traces of rigidity were to be discovered anywhere. At 5.35 p.m. the muscles, now perfectly soft and limber everywhere, responded readily to the prick of a scalpel, as well as to the shocks of a coil-machine. On the morning following, the animal was found dead.

In another experiment in which the abdominal aorta and the crural arteries of a puppy were tied, and left tied to the end, Stannius shows very clearly that the rigidity, of which mention is made in the two last experiments, is identical with rigor mortis. In this case, four hours after the operation, the muscles *below* the ligatures were perfectly rigid and unimpressible. In the evening of the day following there was no alteration of any moment. Twelve hours later the animal was found dead, with the parts *above* the ligatures in a state of rigor mortis, and with the parts *below* the ligatures—which parts had been rigid before death—flaccid, moist, and exhaling a putrescent odour. In other words, the parts *below* the ligatures were in the state which comes on after rigor mortis; and hence it follows that the stiffness which had existed in these parts before the death of the anterior half of the animal must have been identical with rigor mortis.

And thus there is reason to believe that rigor mortis is in some way or other counteracted by the blood, for the fact is, not only that this form of contraction does not make its appearance until long after the heart has ceased to beat, but also that the muscles in which it has made its appearance, may more than once be made supple and impressible by again supplying them with blood.

## II.

*The fact that the nerves have ceased to be impressible long before rigor mortis makes its appearance, would seem to show that nervous influence can have no part to play in causing this form of contraction.*

Long before the advent of rigor mortis, the nerves have ceased to be impressible. Long before this time, that is to say, the nerves have passed into that state in which it seems impossible that any supply of nervous influence to the muscles can be kept up. In a word, the case is one in which it would seem to be impossible that nervous influence can have any part to play in the production of cadaveric rigidity.

## III.

*The fact that the nerves and muscles have both lost* *their natural electricity before rigor mortis makes its appearance, would seem to show that this natural electricity can have no part to play in causing this form of contraction.*

The signs of electricity which may be detected by the galvanometer and electrometer in living nerve and muscle, have all come to an end before the muscles set in the state of cadaveric rigidity. The case, indeed, is one which justifies the conclusion that the establishment of this state is contingent upon the departure of this form of electricity, and that in this respect the history of rigor mortis is not essentially different from that of ordinary muscular contraction already given.

## IV.

*Rigor mortis, like ordinary muscular contraction, would seem to be caused by the elasticity of the muscular fibres being allowed to come into play by the disappearance of the electrical charge which had previously kept these fibres upon the stretch, the contraction in rigor mortis being permanent simply because this disappearance is final.*

The history of rigor mortis, so far as it is known, is precisely what it should be according to the view of muscular action which has been evolving hitherto. For what is this view? It is that the state of action in muscle is counteracted by the blood, by nervous influence, and by the natural electricity belonging to the muscles and motor nerves, the blood and nervous influence acting through this latter agent. It is that ordinary muscular contraction is nothing more than the simple result of the muscular fibres being left to the play of their natural elasticity upon the momentary discharge of the electrical charge which had previously kept them elongated. According to this view, all that is wanted to account for rigor mortis is, that there should be an end to that action of the blood, of nervous influence, and of electricity, which operates in living muscle during the state of rest ; and that in this way the muscular fibres should be left permanently to the play of their natural elasticity. According to this view, indeed, the contraction in rigor mortis is permanent simply because the charge is not restored, which, by being immediately restored, makes ordinary contraction only momentary. In a word, any difference between ordinary muscular contraction and rigor mortis is to be found, not in the cause of the contraction, but simply in this, that in rigor mortis the muscle is no longer the seat of that electrical action which produces the state of elongation or relaxation, and that the contraction is brought about, not by the sudden and momentary discharge of a charge, but by the slow and final disappearance of it,

V.

*The fact that rigor mortis comes on and passes off more quickly than usual where death has been preceded by violent muscular action may be due to the damage done to the physical integrity of the muscular fibres by this action, for by this damage it is easy to see that the fibres may, at one and the same time, be made less capable of receiving and retaining the charge which antagonizes contraction and of manifesting the elasticity which produces contraction.*

Sommer and others have noticed that rigor mortis may occur without any appreciable interval of muscular relaxation after death from convulsion, and Dr. Brown-Séquard has confirmed this observation and given a definiteness to it which it had not before. He has, indeed, done more than this, for he has not only confirmed the fact that rigor mortis may occur without any appreciable interval of relaxation under these circumstances, but he has made out distinctly that in cases where death has been preceded by violent muscular action rigor mortis is quick in coming on and quick in passing off in direct proportion to the amount of such action. In many animals killed by strychnia, for example, not only does putrefaction set in with strange rapidity, but, as in the case of the rabbit referred to in the preface, it is impossible to draw any strict line of separation between tetanic stiffness and cadaveric rigidity: and so also is it with many animals whose death has been brought about by the shocks from a Ruhmkorff coil. It has been noticed also—and this, too, in a man dying from ordinary tetanus, as well as in animals poisoned by strychnia—that rigor mortis may be partially established before the heart has ceased to beat, and that in some cases, as in that of the hare which has been run down by greyhounds in coursing, putrefaction may come on so quickly as to leave no time for cadaveric rigidity to get firm hold of the muscles, the body, as I can testify, becoming quite stale and limp before it can be brought home, even though but short time was needed for this. Nor is it difficult to explain these facts in accordance with the premises. According to the premises, rigor mortis will come on as soon as there is an end to the electrical charge of the muscular fibres which antagonizes contraction, and continue as long as these fibres retain their elasticity, that is, until they are broken up by putrefaction. A certain degree of physical integrity in the fibres is necessary, as well to the receiving and retaining of the charge which antagonizes contraction, as to the manifestation of the elasticity which causes contraction; and thus there need be no difficulty in seeing how it may be that rigor mortis may come on and pass off more quickly than usual where death has been preceded by violent muscular action, for it is quite conceivable that this quickening may be the simple effect of the damage done by the action to the physical integrity of the fibres, the quickening in the coming-on to the fibres being rendered less capable of receiving and retaining the electricity which antagonizes contraction, the quickening in passing-off to the fibres being rendered less capable of continuing to manifest the elasticity which produces the contraction. And this assumption is certainly permissible, unless there be a fatal flaw in the soundness of the foundation upon which these conclusions are based.

## VI.

*There is no need to assume that muscle is endowed with a vital property of tonicity in order to account for the phenomena of rigor mortis.*

As with ordinary muscular contraction it was possible to dispense with the help of a vital property of irritability, so with rigor mortis it is possible to dispense with the help of a vital property of tonicity, simple elasticity doing the work ascribed to irritability in the one case and to tonicity in the other. This is all. It is not one cause for ordinary muscular contraction and another for rigor mortis. It is one and the same cause for both, and this a physical cause. The history of ordinary muscular contraction and that of rigor mortis are, in fact, mutually supplemental, the one running into the other by imperceptible gradations. The evidence which went to show that a vital property of irritability has no work to do in the production of ordinary muscular contraction tells equally in showing that a vital property of tonicity has no work to do in causing rigor mortis; and, on the other hand, the evidence that a vital property of tonicity need not be introduced into the explanation of rigor mortis reacts in showing that a vital property of irritability has nothing to do in the production of ordinary muscular contraction; and thus, to bring the remarks under the present head to a point, there is no lack of evidence to show that the phenomena of rigor mortis may be sufficiently accounted for without supposing that muscle is endowed with a vital property of tonicity.

CHAPTER XIII.

# ON THE NATURE OF NERVOUS ACTION.

## I.

*THE physiological history of convulsion would seem to show that muscular action in this case is connected, not with the presence, but with the absence of the nervous influence developed in the great nerve-centres by the action of blood upon these centres.*

Convulsion is the accompaniment of death by bleeding or strangling, and the immediate consequence of suddenly cutting off the supply of arterial blood to the great nerve-centres of the head and neck by stopping the great arteries in the neck. Convulsion is not the consequence of increasing the supply of blood to these great nerve-centres by tying the great vessels so as to divert the flow of the blood from the rest of the body and produce a state of things which may be spoken of as active determination of blood to these centres. Convulsion is present, that is to say, under circumstances in which the development of nervous influence in the great nerve-centres must be at zero, and absent under circumstances in which this development must be excessive; for the functional activity of these centres may be supposed to be proportionate to the activity of the circulation in these centres; and thus the conclusion from the physiological history of convulsion must be that muscular action in this case is connected, not with the presence, but with the absence, of nervous influence.

## II.

*The fact that muscles which are paralysed by cutting them off from the chief nerve-centres may be made to contract with greater force than muscles which are not so paralysed, would seem to show that contraction in this case is connected, not with the presence, but with the absence of nervous influence.*

If the hind limbs of a frog are deprived of the nervous influence derived from the brain and upper part of the spinal cord by cutting across the spinal cord in the middle of the back, the contraction which may be produced by pinching the toes, and in various other ways, is considerably increased in force. If one of the two limbs thus paralysed be still further deprived of nervous influence by cutting across its principal nerve high up near the spine, the contraction which may be produced in it becomes more marked than that which may be produced by the same means in the limb which still receives nervous influence from the lower portion of the spinal cord. In a word, the facts would seem to show that contraction in this case is connected, not with the presence, but with the absence, of nervous influence, and also this—that the degree of contraction is inversely related to the supply of nervous influence.

## III.

*The empty and quick pulse which accompanies nervous exhaustion, when taken in connection with the full and slow pulse marking the contrary state of things, may prove that muscular relaxation, not muscular contraction, is directly related to the supply of nervous influence, by simply showing that the ventricular diastole is more perfect and more prolonged in direct proportion to this supply.*

The fact that the pulse is empty and quick in cases where the innervation is defective, and full and slow where the contrary state of things obtains, has been already appealed to as a reason for believing that the diastole of the ventricle is perfect and prolonged in direct proportion to the amount of nervous influence supplied to it ; and, most certainly, what has been said since would only seem to corroborate this conclusion. No doubt the evidence upon which to form any such conclusion is not very cogent. No doubt this evidence is in the main merely circumstantial. Still the empty and quick pulse of nervous exhaustion, and the full and slow pulse of the contrary state of things, remain as facts ; and thus, as it would seem, there is reason, other than circumstantial, for believing that the ventricular diastole is more perfect and prolonged in direct proportion to the supply of nervous influence to the ventricle, and, by implication, that muscular relaxation in all cases, not muscular contraction, is directly related to the supply of nervous influence to the muscle.

## IV.

*Nervous influence may act upon muscle through the instrumentality of the natural electricity associated with it, producing rest and relaxation when present, because this presence implies a state of electrical charge, causing contraction when absent, because this absence (up to a given point) is marked by a discharge analogous to that of the torpedo.*

It is not difficult to believe that nervous influence may act upon muscle through the instrumentality of the natural electricity associated with it. Indeed, apart from this electricity, nervous influence, to say the least, is a very indefinite idea. And, if so, then it is sufficiently intelligible that nervous influence should act upon muscle as it has been seen to act, producing rest and relaxation when present, and contraction when absent, because this electricity has been seen to produce rest and relaxation when present and contraction when absent. And most assuredly the action of the nerves in causing contraction is not a little simplified by supposing that the action is accompanied by a discharge analogous to that of the torpedo, for on this view the action of the nerves within the body is precisely that which is illustrated out of the body in the experiment in which secondary or induced contraction is produced by placing the nerve of one rheoscopic limb upon the nerve of another rheoscopic limb, and then putting the latter nerve in action. The case, indeed, is plain enough. It is not that of a nerve producing action in muscle by a process which is made none the less unintelligible by calling it vital. It is simply this—that a charge of electricity present in nerve is discharged in action, and that this discharge, which is analogous to that of the torpedo, acts upon the muscle which happens to be within its circuit just as the discharge of the torpedo would act upon any muscle included in its circuit.

## V.

*The sensorial nerves may tell upon the sensorium in producing sensation of various kinds in the same way as that in which the motor nerves tell upon muscle in producing contraction, that is, by the discharge, analogous to that of the torpedo, acting directly upon the part of the sensorium included in its circuit.*

Electrically the sensorial nerves are in the same predicament as the motor nerves. There is the same state of charge during rest in each, and the same discharge of this charge during action in each. What holds good of the one kind of nerve holds good of the other kind also, and therefore it may be that the sensorial nerves may tell upon the sensorium in producing sensation of various kinds in the same way as that in which the motor nerves tell upon the muscles in producing contraction, that is, by the discharge, analogous to that of the torpedo, which accompanies the state of action, this discharge acting directly upon the part of the sensorium which happens to be within its circuit. Indeed, this view must be taken for granted, unless it can be shown that the nerves do not act upon the muscles in the manner which has been indicated.

## VI.

*The convulsion caused by suddenly cutting off the supply of red blood to the great nerve centres of the head and neck may have its explanation in a reversal in the relative position of the two kinds of electricity forming the charge of the fibres of the nerve-centres in some parts but not in others, for the effect of this partial reversal will be to cause the discharge which is the basis of action, by bringing the sheaths of fibres which are negative at the sides and positive at the ends into juxta-position with the sheaths of other fibres which are positive at the sides and negative at the ends.*

A fact was noticed when speaking of the electrical condition of nerve and muscle during rest, which may perhaps supply the key to the explanation of that state of muscular action which is set up by arresting the supply of red blood to the great nerve-centres of the head and neck, and this fact is the reversal in the electrical relations of the sides and ends of the fibres, which may sometimes happen in nerve after violent exercise or under the operation of heat, and in nerve and muscle alike shortly before the advent of rigor mortis. This reversal was not a constant phenomenon. It did not happen in all nerves. It might happen in the nerves and not in the muscles : thus, in the most familiar case of all, that of frogs, it might happen shortly before the advent of rigor mortis in the fibres of the brain and spinal cord, but not in the muscular fibres. What then ? Is it possible that such a reversal may be an effect of arresting the supply of red blood to the great nerve-centres of the head and neck ? Is it possible that this arrest in the supply of blood may act by producing a state of things which is akin to that which is produced in nerves by violent action or under the operation of heat, and in some nerves and muscles alike on the near approach of rigor mortis ? Is it possible that such reversal may be partial and not general—in certain parts of the nervous system and not in others, in certain parts of the nervous system, but not in the muscles ? Is it possible that by this partial reversal a state of unstable electric equilibrium may be set up which may issue in discharge, and that in this way muscular action may be brought about ? These are the questions which are suggested naturally by the premises, and which, once put, would seem to demand answers in the affirmative. For it may be considered that this arrest in the supply of red blood to the great nerve-centres of the head and neck may affect these centres in the way in which they are affected when the circulation is at an end, and in this way bring about the reversal in question : and, this point conceded, all the rest follows, for by this partial reversal that must happen which will bring fibres whose sides are negative and ends positive, into juxta-position somewhere with fibres whose sides are positive and ends negative, and which will in this way necessitate discharge, and the action consequent upon it, for opposite electricities cannot be thus brought together without such discharge. All this is plain enough, and also this—that the state of action thus set up must continue until the discharge ceases, either from the electrical relations of the sides and ends of the fibres, which were at first dissimilar, becoming similar in consequence of the reversal which was at first only partial becoming general, or else from the discharge itself becoming after a time too feeble to tell upon the muscles with the force sufficient to give rise to contraction, either or both of which results may well be supposed to be the natural result of the arrest in the supply of blood being still continued. In a word, there is reason to believe that the arrest in the supply of red blood to the great nerve-centres may bring about muscular action in a way which leads naturally out of the premises ; and that in this way the apparent anomaly of excessive nervous action under circumstances in which the development of all nervous influence in these centres must be suspended is done away with, and the true law of nervous action vindicated by so doing.

## VII.

*The so-called stimuli which give rise to nervous action, the will itself not excepted, may act as the same causes have been seen to act upon muscle, that is, by disturbing the electric equilibrium which obtains during rest, so as to give rise to a discharge analogous to that of the torpedo.*

There appears to be no reason why the explanation which has been applied to the action of the so-called stimuli upon muscle should not apply also to the action of these agents upon nerve. Nor need a different explanation be applied to the action of the will, for the "electric chain wherewith we are darkly bound," may well serve as the channel by which the biddings of the brain may reach the muscles.

## VIII.

*The dogma that nervous action is the result of the awakening by some stimulus of a dormant vital property of irritability inherent in nerve would seem to have no foundation in fact.*

All that has gone before is opposed to the current dogma that nervous action is the result of a vital property of irritability being awakened by the action of a stimulus of one kind or other. All that has gone before, indeed, leads directly to the same conclusion as that which was arrived at when muscular action was the subject of inquiry, nervous action, like muscular action, being resolvable into electrical action. Electricity, in fact, would seem to be everything, and vitality nothing. In saying this, however, I have no intention of elevating that which is physical at the expense of that which is vital. On the contrary, I firmly believe—and with this remark I would bring these comments on the cause of nervous action to a close—that what is called electricity is only a one-sided aspect of a law which, when fully revealed, will be found to rule over organic as well as over inorganic nature—a law to the existence of which the instincts of philosophy and the discoveries of science alike bear testimony—a law which does not entomb life in matter, but which transfigures matter with a life which, when traced to its source, will prove only to be the effluence of the Divine Life.

# DYNAMICS OF NERVE AND MUSCLE.

## PART II.

*THE SUBJECT FROM A PATHOLOGICAL POINT OF VIEW.*

## Chapter I.

# ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN EPILEPSY AND OTHER FORMS OF CONVULSION.

## (A.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE STATE OF THE RESPIRATORY AND CIRCULATORY SYSTEMS DURING EPILEPSY AND OTHER FORMS OF CONVULSION.

### I.

*THE epileptic and epileptiform convulsion is not unfrequently ushered in by unmistakable failure in the respiration.*

The habit of sighing, which is not very unfrequent among epileptics, and which is often most marked when a fit is at hand, would seem to show that insufficient breathing has to be made up now and then by some breaths which are more deeply drawn than usual. At times, too, especially when the fit happens during sleep, the respiratory movements may come to a complete standstill for a few moments before the paroxysm: and more than once, in my own experience, this pause has been long enough to make me think that the patient was dying, or actually dead. Indeed, I believe it is not too much to say that the epileptic and epileptiform paroxysm is always ushered in by signs which betoken failure of the respiration to a greater or less degree.

## II.

*The epileptic and epileptiform convulsion is usually accompanied by a state of unmistakable suffocation.*

The livid, black, and bloated head and neck, the throat-sounds suggestive of death by strangling, the evident suspension of all proper respiratory movements, are symptoms of no doubtful meaning, and these are the symptoms which are rarely absent in the fully-developed epileptic or epileptiform paroxysm. The signs of strangling are so plain as to suggest death by the bowstring at the hands of some invisible executioner. Nor is it really different in those varieties of general or partial epileptic or epileptiform disorder in which the face remains pale and shrunken from the beginning to the end of the paroxysm ; for in these cases the face has always a ghastly pallor or lividity, which shows very plainly that the convulsive symptoms are accompanied by some grave interruption in the proper aëration of the blood.

## III.

*The convulsion of hysteria and chorea is associated with very defective respiration.*

In hysterical and choreic convulsive disorders the breathing is not arrested as it is in the epileptic or epileptiform paroxysm, but it is shallow, embarrassed, often broken by sighs, and generally accompanied by a distressing sense of want of breath. It is hampered and interrupted in no inconsiderable degree.

## IV.

*In the chronic forms of convulsive disorder the inter-paroxysmal condition is usually marked by evident signs of a feeble circulation.*

In common epilepsy the inter-paroxysmal condition is usually marked by a weak and slow pulse, and by cold and clammy hands and feet; and in all chronic forms of epileptiform disorder, so far as my experience goes, this same state of things holds good to a greater or less degree. The state, in fact, is the very reverse of true vascular excitement. Instead of predisposing to these disorders, all febrile excitement would, indeed, seem to have a contrary effect; and so also in the acute forms of epileptiform disorders, for, as will appear in due time, even here the time of the convulsion is found to be before and after a state of fever, not during it.

Where there is a liability to hysterical convulsion everything goes to show that the circulation is wanting in ordinary vigour. The difficulty is to keep the hands and feet warm, or to avoid chilblains when the weather is at all cold; and frequent palpitation shows how often the heart is called upon to make up for work which the "capillary force" has left undone. There is, in fact, something in the condition of the circulation which seems to be not remotely akin to that which is noticed in hybernating animals—a condition for which radical weakness is no inapt name.

Nor is it otherwise in chorea. A disposition to rheumatic fever would seem to be not uncommon in this affection—at least in this country; but in this fact there is no reason for supposing that the febrile and choreic symptoms are in any way concurrent. The place of the choreic symptoms is not with the febrile symptoms, but before them or after them; often long before or long after; and the only sound conclusion possible is that chorea in itself is essentially a feverless malady. Not unfrequently, also, there are signs which point to a condition of circulation the very opposite to that which is met with in fever, such as coldness and clamminess of the hands and feet, pastiness and puffiness of certain parts of the skin, anæmic vascular murmurs, and the rest. Indeed, the very predisposition to rheumatic fever may be taken as an argument that the circulation in chorea is radically weak; for it is a fact, that valvular disease of the heart and various indications of feeble capillary action, one or both, are often present in persons who are liable to rheumatic fever. Moreover, it not unfrequently happens that the symptoms of chorea are suspended by the accidental development of scarlet fever, or some other true febrile disorder, and that they return again when the state of feverishness passes off.

## V.

*The epileptic and epileptiform paroxysm is usually, if not invariably, ushered in by signs of failure in the circulation.*

The immediate precursor of the perfect form of the paroxysm is a sign which it is somewhat difficult to catch—corpse-like paleness of the countenance. M. Dalasiauve* was the first to notice this phenomenon ; and M. Trousseau insists upon it as a mark which distinguishes true epilepsy from feigned epilepsy. "Il est une signe," he says,† "qui se produit au moment de la chute, qui n'est imitable pour personne ; c'est la pâleur très prononcée cadavérique, qui couvre pour un instant la face épileptique. Nous ne la voyons pas, parceque nous arrivons toujours trop tard, alors que la face est déjà d'une ronge très prononcée." In fact, the general form of the epileptic or epileptiform paroxysm begins in the same way as the partial form ; for it is allowed by all that cadaverous pallor of the countenance is the initial symptom in the latter case.

## VI.

*At the height of the epileptic or epileptiform paroxysm the pulse is usually full, strong, and frequent, not because the arteries are then receiving an increased supply of* red *blood, but because they are then labouring under a load of* black *blood, as they are found to labour in suffocation.*

In some cases the pulse at the wrist is almost or altogether imperceptible from the beginning to the end of the paroxysm ; in others it rallies speedily, and, when the fit is at its height, it beats with considerable force, fulness, and frequency. How then ? What is the true meaning of this vascular over-

* "Traité de l'Épilepsie." 8vo. Paris, 1855.
† "L'Union Médicale," 28th April, 1855.

action? The current belief on the subject is that an increased quantity of *red* blood is injected into the arteries during the convulsion, and that the increased quantity of red blood produces the convulsion by provoking a state of increased functional activity in one or more of the great nerve-centres; and but lately Schroeder van der Kolk has given distinct expression to this belief.* In reality, however, there is reason to believe that the pulse acquires power during the epileptic or epileptiform convulsion, because the condition of the circulation at the time is one of suffocation, and for this reason simply. For what is the condition of the circulation in suffocation? It is *not* one in which, as is generally supposed, the arterial pulse rapidly fails for want of blood, and the venous system as rapidly becomes gorged with un-aërated blood; on the contrary, it is one in which the arterial system becomes gorged at the expense of the venous system, and in which the pulse in the arteries becomes stronger and fuller as the blood within these vessels becomes more and more venous in its character. Proof of all this, as has been already found (p. 162), is contained in the experiments of Reid and Dr. Draper, the younger. It is certain, indeed, that the strong pulse of the epileptic or epileptiform paroxysm may be nothing more than the natural pulse of the state of suffocation which obtains at the time—the pulse of black blood—the *apnœal* pulse, as it may be called; nay, this is the only conclusion which can be drawn; for with the respiration completely, or all but completely, arrested, as it is in fact, it is simply impos-

* "On the Proximate Cause, and Rational Treatment of Epilepsy." New Sydenham Society Series, 8vo., London, 1859.

sible that there can be an increased injection of *red* blood into the arteries during the paroxysm. Nor is it only with reference to the condition of the pulse in convulsion that these facts are of interest. On the contrary, these facts explain many apparent anomalies in the pulse. For example, they explain how it is that blood drawn from the temporal artery in a fit is often black in colour, and projected to an unusual distance; and how, in cases of congestion of the lungs, and in some other cases where the aëration of the blood is greatly interfered with, the pulse may beat with seemingly contradictory power in the very last moments of life. They show, in fact, that the pulse may derive a fictitious value from the admission of black blood into the arteries when the respiration is at fault, and that mere power of pulse, apart from the condition of the respiration, is a very unsafe criterion by which to gauge the degree of true vascular activity.

## VII.

*Convulsion is never coincident with a state of active febrile excitement of the circulation.*

In the fevers of infancy and early childhood, in the exanthemata especially, convulsion not unfrequently occupies the place taken by rigor in the fevers of later years. It occurs in the cold stage of the fever, when the circulation is very wanting in power, and it is confined to this stage, except there happen to be certain brain or kidney complications, of which more will have to be said presently. It is rather the fever which is a consequence of the convulsion, than the the convulsion which is a consequence of the fever.

The constant rule would seem to be that the convulsion now and then associated with inflammation of the brain, or rather of its membranes, is connected, not with the hot stage of the sympathetic fever, but with the cold stage before the hot stage, or with the cold stage at a later period, which too often is the immediate precursor of death. Indeed, there would seem to be something altogether incompatible between convulsion and sympathetic fever in any case; for it is a fact, not unfrequently verified, that the fits of common epilepsy are often suspended for the time by sympathetic fever, as, for example, by that resulting from the burn caused by falling upon the fire in a fit.

And certainly there is nothing to lead to a contrary conclusion in the history of the convulsion connected with teething or worms, or with any other manifestation of the state which is called "morbid irritability," for here most assuredly in the few instances where convulsion goes with anything like fever, its place is, not side by side with the fever, but before it or after it, precisely as it was in the instances of the kind already cited.

In a word, the result of careful observation has been to convince me in the very fullest manner that the true place of convulsion in connection with any form of febrile disorder is in the cold stage before the hot stage, or in the cold stage after the hot stage, and not in the hot stage—that, in fact, there is something incompatible between convulsion and an excited condition of the circulation.

## VIII.

*The convulsion which may attend upon certain forms of kidney-disease is often associated with a pale and watery condition of the blood, and with unmistakable signs of want of vascular vigour, as well as with symptoms of uræmic poisoning.*

It is not easy to theorize upon the way in which convulsion is brought about when urea is retained in the blood, or rather when this urea is resolved into carbonate of ammonia in the blood. It may be that the uræmic poisoning acts by destroying the blood corpuscles. It may be that the great deficiency of blood corpuscles, which is a marked feature of the advanced stages of Bright's disease, is independent of uræmic poisoning, and more concerned in the production of the head-symptoms than the uræmic poisoning. Sir Thomas Watson is of opinion that the pale and watery condition to which the blood is at last reduced in albuminuria may have something to do in bringing about the stupor and coma which mark the close of the disorder, and he bases this opinion upon the fact that similar symptoms are apt to ensue, in conjunction with a similar deficiency of hæmatosin, in spurious hydrocephalus, and I am quite disposed to subscribe to this opinion, and to apply it to the interpretation, not only of the stupor and coma, but of the convulsion also. At all events this is certain, that the convulsion of kidney-disease is associated with a state which is in every way the reverse of that which may be spoken of as vascular vigour—a state in which the blood is pale and watery, and the circulation carried on in a very inadequate manner.

## IX.

*Epileptiform convulsion is a direct consequence of sudden and copious loss of blood.*

What happens in the convulsion attending fatal hæmorrhage in child-birth—a fact too terribly familiar—requires no comment. The convulsion is as evidently the consequence of the loss of blood as is that which attends death by the knife in the shambles, and the inference from the fact is as evidently the same as that which has been already drawn from it.

## X.

*The condition of the respiration and circulation in epilepsy and other forms of convulsion shows very plainly that muscular action in this case is connected with want of red blood, and with a state of things in every way the reverse of feverish activity—a condition intelligible enough upon the previous view of muscular motion, but utterly unintelligible upon the supposition that the convulsion is a sign of "exalted irritability" in any organ.*

The history of muscular motion, as exhibited in the state of the respiration and circulation during epilepsy and other forms of convulsion, goes to show that muscular action in this case is connected with want of red blood, and with a state of things which is in every way the reverse of feverish activity. As in the former cases, so in this, the blood would seem to antagonize this action, not to cause it; and, in short, the facts are as much opposed to the notion that the blood produces convulsion by rousing into a keener life a vital property of irritability as they are in harmony with the view, set forth in the premises, according to which muscular motion is not caused, but antagonized, by the action of the blood.

### (B.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE STATE OF THE NERVOUS SYSTEM DURING EPILEPSY AND OTHER FORMS OF CONVULSION.

I.

*A liability to epilepsy and other forms of chronic convulsion is usually marked by unmistakable want of brain-power.*

In confirmed and aggravated cases of epilepsy a short examination will suffice to prove that a terrible blight has fallen upon all the faculties which distinguish man from the mere animal. Indeed, a single glance at the countenance will often serve at once to detect this blight and to connect it with epilepsy, even though there be none of those dark specks about the eyes and temples which are certain signs that the face has been "black and full of blood" in some recent paroxysm. Apparent at all times, these signs of mental inadequacy are most conspicuous after a fit. After a fit, indeed, the faculties of the mind may be so blunted that the features of the epileptic may become blended with those of the demented person, or symptoms of mental or moral aberration may show themselves, and the epileptic for the time may be transformed into the lunatic. Not unfrequently, also, there is the very gravest degree of mental infirmity from the very first, and instead of only tending to dementia, the history of the epileptic may begin in sheer idiotcy. Indeed, it cannot be looked upon as a mere accident that idiotcy and epilepsy should so often go together, and that the head of the epileptic should be so frequently wanting in proper size and proportions as to suggest to the least imaginative observer its suspicious kinship to the head of the idiot.

Often, it is true, very often, no doubt, a careful search is necessary to detect in ordinary cases of epilepsy any signs of mental infirmity; but never, as I believe, are such signs altogether wanting. As I believe, too, the seeming exceptions to this rule are more apparent than real; for who shall say that the brains of men like Julius Cæsar or Napoleon I, who are known to have been epileptic at one time of their lives, had not broken down in other respects also before the attacks of epilepsy made their appearance?

And certainly there is no lack of evidence to show that the subjects of hysterical and choreic convulsions are the reverse of strong-minded.

The subjects of hysterical convulsion, for the most part, are undecided, irresolute, fickle, purposeless, yielding easily and almost passively to every impulse either from within or without, and but little capable of any continuous effort. They do what they ought not to do, and they leave undone what they ought to have done, and their only excuse is that they cannot help it. The mere creatures of feeling, *will* is to them an empty name. The temper, also, is as little under control as the feelings, and impatience, perverseness, obstinacy, and anger are only too common occurrences. There is no lack of imagination, but, as a rule, the ideas are allowed to take their own erratic course unchecked; and hence fancies and whims of all kinds in endless succession, or, what is worse, some one whim or fancy in possession of the mind, and the reason unequal to the task of ejecting it. Often, too, there is a disposition to exaggeration and deceit, which must betoken not a little bluntness in the moral sense. In a word, the subjects of hysterical convulsion are so constituted mentally as to make it difficult at times to hold them altogether accountable for all they do or leave undone.

In bad cases of chorea the expressionless features and the abortive attempts at speaking would seem to show that the brain is acting very imperfectly. It may be that the features are expressionless, less because they are not required to lend themselves to expression, than because the grimacing muscles refuse so to lend themselves, or that the speech is difficult or impossible, less because there is nothing to say, than because the organs of articulation are rebellious; but, be this as it may, there is no doubt that blankness rather than briskness must be looked upon as the mental condition which is likely to be associated with choreic convulsion.

In a word, there is no one case in which a liability to choreic convulsion is not more or less marked by unmistakable signs of wanting brain-power.

## II.

*All signs of mental life are abolished, or all but abolished, during the paroxysm of convulsion.*

With very few exceptions in which faint glimmerings of sense and feeling may remain, the mind is a perfect blank in every form of the epileptic or epileptiform paroxysm, partial as well as general: and this perfect blankness is insisted upon as the chief characteristic of the epileptic as distinguished from the hysteric paroxysm. In fact, however, it is possible to insist too much on this distinction, for in the hysteric convulsion the will is as much in abeyance as in the epileptic, and as for the remaining mental faculties, all that can be said of them is this, that their suspension is not altogether absolute. Nay, cases are not at all uncommon in which it is more than difficult to find in the mental condition alone a means of diagnosing between the epileptic and the hysteric paroxysm. And so also in chorea it is in the paroxysm that the mental blankness, of which mention has just been made, is most apparent. Whether, indeed, the convulsion be epileptic, or epileptiform, or hysteric, or choreic, the same law seems to hold good, namely this, that all signs of mental life are abolished, or all but abolished, during the paroxysm.

## III.

*The belief that convulsion is associated with an overactive condition of the circulation in the brain is contradicted by clinical evidence.*

Convulsion most certainly is not a common symptom of inflammation of the brain or its membranes. Now and then, as has been already pointed out, it may happen, in children especially, at the onset of the disorder, in the cold stage before the hot stage, or at the end of the disorder, in the stage of collapse after the hot stage, when the patient has all but ceased to strive in the " struggle called living ;" but, so far as my experience goes, it never happens in the proper hot stage, when general febrile reaction with determination of blood to the brain is fully established.

Neither does convulsion take rank among the symptoms of acute mania. Acute mania may be a consequence of the convulsion, and convulsion may return when the maniacal excitement has subsided, but the convulsion and the acute mania are not co-existent. Indeed, the simple fact appears to be that here also the convulsion is incompatible with anything like active determination of blood to the brain.

On the other hand, convulsion and apoplexy may go together, and in apoplexy there may be active determination of blood to the brain. In this case, without doubt, the convulsion is connected with active determination of blood to the brain, but it is connected also with pressure, and therefore it may be that the convulsion, like the coma, is connected, not with the active determination of blood to the brain, but with the pressure caused by the effused blood, the part pressed upon being somewhere or other in the excito-motor tract ; and most assuredly this and no other is the conclusion which is warranted by the premises.

## IV.

*The appearances after death are not calculated to show that epilepsy and other forms of convulsion are connected with inflammation in any one of the great nerve-centres.*

The morbid appearances after death from ordinary epilepsy are very obscure, if the case have really not been one of epileptiform convulsion connected with some special disease. In cases fatal during the fit the brain has been found to be congested ; but this appearance is clearly owing to the mode of death, and it is allowed to be so. In cases where epilepsy has been complicated with insanity, the brain or its membranes may present various signs of inflammation, or of changes more or less akin to inflammation ; but these signs are clearly referrible to insanity rather than to epilepsy, and for this reason,—that they are as common, or more common, in insanity without epilepsy. In other cases there are signs of degeneracy of the brain, such as pallor of its grey matter, softening, granular induration, atrophy, dropsical effusion—the very signs, indeed, which belong to the demented state. And it is this fact which furnishes some ground for supposing that signs of this character, and not signs of inflammation, may have something to do with epilepsy. For is it not true that the demented state is intimately connected with the epileptic disorder ? And is it not equally true that a demented person is almost sure to be affected with palsied shakings, or cramps, or spasms, in one form or another, if he escape the graver affliction of epilepsy ?

In other cases, again, the skull may be thicker and heavier than usual, and the several internal projections, the clinoid processes, for example, considerably developed, or various parts of the dura-mater ossified; but there are in the brain proper or its membranes no changes of sufficient constancy to be necessarily connected with epilepsy—not even that change in the pituitary body of which so much was said by Wenzel;* for, writing of it, Professor Rokitansky† says that he has "frequently failed to discover it in those who had notoriously suffered from epilepsy and convulsions," and that he has "met with it in those who were perfectly healthy." It is in the medulla oblongata alone, indeed, that there appear to be any changes after death which have any pretensions to constancy. In early cases of epilepsy, it is true, this organ may present no signs of disease: in confirmed cases, on the other hand, it is often hardened by the interstitial deposit of a minutely granular albuminous matter, or else softened, swollen, and presenting signs of evident fatty degeneration. The posterior half of the organ, moreover, is redder and more vascular than it ought to be, even when the patient has not died in a fit; and, on making a more minute examination of this part, the blood-vessels are seen to be dilated to twice their natural dimensions, and with their walls much thickened—in the course of the hypoglossus nerve and corpus olivare, in epileptics who were in the habit

* "Boebacht, über den Hernauhang fallsüchtiger Personen," &c., 8vo. Mainz, 1810.

† "Manual of Pathological Anatomy." Translated for the Sydenham Society by Dr. C. H. Moore, vol. iii, p. 434.

of biting their tongue in a fit, and in the course of the roots of the vagus in epileptics who were not in this habit.* These facts, for the knowledge of which we are indebted to the late Schrœder van der Kolk, are not altogether unintelligible, and they must show, if they show anything, that the medulla oblongata of the epileptic is damaged in structure, and in a proportionate degree rendered incapable of discharging its proper offices efficiently. The signs of fatty degeneration have but one significance. The interstitial deposit, also, implies an equivalent absence of healthy nerve-structure, and so, in a measure, does the dilated condition of the blood-vessels. In a word, the appearances after death in the medulla oblongata of epileptics, are more in accordance with the notion that epilepsy is connected with a depressed state of functional activity in this organ than with the contrary notion.

Nor are the disclosures of pathological anatomy in chorea such as to connect this malady with an inflammatory condition in one or other of the great nerve-centres.

In half the whole number of fatal cases, perhaps, traces of inflammation more or less vague, and always of most uncertain seat, are met with in the brain or spinal cord, one or both, and quite as frequently in the cord as in the brain; but in the remaining half the most careful search fails to detect them. Traces of inflammation in these parts are not always present; this is plain: and to be ever absent is in itself a cer-

* "On the Proximate Cause and Rational Treatment of Epilepsy." Translated for the New Sydenham Society, by Dr. C. H. Moore. 1859.

tain proof that the inflammation which left them cannot be regarded as the cause of the chorea. Indeed, there is reason to believe that certain parts of the brain, the sensorio-motor ganglia and the neighbouring convolutions, instead of being inflamed, are actually starved for want of blood by the plugging of their minute vessels; this state of plugging, or embolism, arising either, as Dr. Kirkes supposes, from the passage into the circulation of minute warty vegetations detached from the cardiac valves—and hearts with valves covered with such vegetations are so common in chorea as to have led to their being spoken of as choreic hearts—or else, as Dr. Bastian conjectures, to white blood corpuscles, altered somewhat, and cohering to the walls of the vessels. In point of fact, the pathology of chorea is, to say the least, quite as much in accordance with the notion of certain parts of the brain being starved for want of blood, from the vessels being plugged in one or both of these ways, as with the notion that these or other parts are inflamed. Nor is there any difficulty in accounting for the traces of inflammation which are met with in some of the cases, and which are not to be ignored. On the contrary, all that is necessary to do this is to suppose that they are the effects of the disease, and not the cause—that when they are present, the disease has been prolonged until the nerve-centre has broken down from sheer fatigue, vessels which were contracted up to a certain point, because the vaso-motor nerves shared in the action of the ordinary motor nerves, having become relaxed when the capability of this action had ended in paralysis. Nay, it is not too much to imagine that in the very cases in which these traces of inflammation are met with the choreic symptoms may have been mitigated when the inflammation was established, for all the evidence so far has tended to show that the action of the blood and nervous influence is to counteract muscular action, not to cause it.

And if this be so with respect to chorea and epilepsy, there is no reason to suppose that a contrary conclusion is necessary with respect to hysteric and other forms of convulsive maladies.

## V.

*Convulsion cannot be looked upon as the result of a congested condition of the cerebral veins.*

The clinical history of disease, as it would seem, is altogether opposed to the theory which ascribes the convulsion to a congested condition of the cerebral veins. In whooping-cough these veins are often conjested in a very high degree during the paroxysm, and yet convulsion is only an occasional accompaniment of this disorder. In extreme congestion of the lungs, also, these veins are greatly gorged with black blood, and the consequences of this engorgement are dreamy sleepiness, stupor, coma it may be, not convulsion. Nor is the case different where extreme venous congestion of the brain is brought about by straining or in any other way, for here the symptoms are coma simply, not coma and epilepsy. In these cases indeed, it is virtually as it is in certain recent experiments of MM. Kussmaul and Tenner, in which the external and internal jugulars of rabbits were tied, for here the effect of the operation is, not to convulse the animals, but only to stupify them for the first twenty-four or thirty hours, and, in some instances, to cause some grinding or gnashing of the teeth in addition. Indeed, there is nothing in the evidence, clinical or experimental, to nullify the conclusion already drawn when speaking of the convulsion connected with suffocation—nothing to show that venous blood has any special action in producing convulsion.

VI.

*The depressed condition of the respiration and circulation in epilepsy and other forms of convulsion resulting from disease involves a corresponding depression in the vital activity of all nerve-centres at the same time, for the vital activity of any organ must be directly proportionate to the vigour with which the respiratory and circulatory processes are carried on.*

The depressed condition of the respiration and circulation which has been seen to be a part of the history of epilepsy and other forms of convulsion, must necessitate a corresponding depression in the vital activity of the nerve-centres, great and small, for the vital activity of any organ is directly proportionate to the amount of arterial blood supplied to it. Indeed, the condition of the circulation and respiration being what it is, the vital activity of the nerve-centres must flag to a degree which is scarcely above that at which it ceases altogether.

VII.

*There is reason to believe that epilepsy and other forms of convulsion resulting from disease are connected,* *not with a state of inflammation in any nerve-centre, but with a state which may or may not issue in such inflammation—a state to which the name of "irritation" is given, and which is marked, not by relaxation of vessels and hyperæmia, but by contraction of vessels and anæmia.*

After what has been said, it is evident that epilepsy and the other forms of convulsion which have been commented on, are connected, not with the state of inflammation in any nerve-centre, but with a state which may now and then issue in inflammation. This state is usually spoken of as "*irritation.*" It is marked, not by relaxation of the vessels and hyperæmia, but by contraction of the vessels and anæmia. In relation to the *hot stage* of fever or inflammation, it is the *cold stage.* It may end in the hot stage, because after a time the vaso-motor nerves, which were in a state of over-action while it continues, become exhausted, or, it may be, paralysed, by this over-action, and so leave the vessels free to relax and receive more blood. As a rule, this stage of vascular contraction and emptiness does not end in the hot stage of fever or inflammation. It may issue in feeble reaction, which is the first step towards the hot stage, and, as a rule, this is all, the convulsion coming to an end when this step is taken. In point of fact, the history of epilepsy and other forms of convulsion the result of disease is no more than the counterpart of that of the convulsion caused by bleeding or strangling or by stopping the arteries leading to the brain, in that want of red blood to certain nerve-centres is found to be the common cause in every case.

## VIII.

*The connection of epilepsy and other forms of convulsion resulting from disease with a very defective supply of red blood to certain nerve-centres, supplies a fatal objection to the view which refers the convulsion to a state of "exalted or morbid irritability" dependent upon an increased supply of red blood to these centres.*

It is not easy to define precisely what is meant by a state of "exalted or morbid irritability" in a nerve-centre having to do with movement, and all that can be said is, that increased movement in this case is supposed, in some way, to depend upon the increased vitality resulting from a more liberal supply of red blood to the part. This theory of exalted or morbid irritability, in fact, presupposes a state of circulation in the nerve-centres which is the exact opposite of that which is found to exist; and, therefore, the state of the circulation being what it is, this theory must fall to the ground. Nor is there anything in the background to make it desirable to raise it up again. For what is this state of so-called exalted or morbid irritability? It is not inflammation; it is not fever; it is some undefined and negative state occurring frequently in teething, in worm-disease, in uterine derangement, in spinal irritation particularly, and in many other cases—a state in which the patient easily over-balances on the side of excitement or on that of depression, as the case may be, in which exhaustion is very readily brought about, and for which nervous exhaustion would seem to be as good a name as any other—a state, in fact, which, to say the least, is as readily accounted for on the supposition that certain nerve-centres are starved for want of blood, as upon the supposition that these centres are excited by being over-fed with blood. In a word, there is nothing in the facts which together go to make up the idea of "exalted or morbid irritability," which is in any degree calculated to overrule the objection that has been already taken to the view which connects convulsion with this state of "exalted or morbid irritability."

## IX.

*The connection of epilepsy and other forms of convulsion resulting from disease with a very defective supply of red blood to certain nerve-centres is intelligible on the view which has been already offered in explanation of the convulsion caused by bleeding or strangling or by stopping the great arteries leading to the brain, which view is this—that the defective supply of blood disturbs the electrical equilibrium of the nervous system so as to bring about the discharge which is the basis of muscular action, by causing a partial reversal in the electric relations of the sides and ends of the fibres, for by this partial reversal opposite electricities which were kept apart are brought together in consequenee of fibres whose sides are made negative and ends positive being brought into juxtaposition with fibres whose sides remain positive and ends negative.*

In order to account for the connection of epilepsy and the other forms of convulsion resulting from disease with a very defective supply of red blood to certain nerve-centres, all that is necessary is to apply the view already applied to the explanation of the convulsion caused by bleeding or strangling, or by stopping the great arteries leading to the brain, and to suppose that this defective supply has disturbed the electrical equilibrium of the nervous system by causing a partial reversal in the electric relations of the sides and ends of the fibres, and so leading to the discharge which is the basis of muscular action, for by this partial reversal, opposite electricities which were kept apart so long as the electrical relations of the sides and ends of the fibres were everywhere similar, will be brought together, fibres which by the reversal have become negative at the sides and positive at the ends, being brought into justa-position somewhere with other fibres which remain positive at the sides and negative at the ends. No different explanation is needed. The two cases are the same intrinsically; and, therefore, it would be needlessly going out of the way to seek another explanation so long as the one already found and applied appears to be that to which all the evidence, physiological and pathological, would seem to point naturally.

## X.

*The history of epilepsy and the other forms of convulsion resulting from disease is as consistent with the view of muscular motion set forth in the physiological portion of this inquiry as it is inconsistent with the current view of muscular motion.*

The history of epilepsy and the other forms of convulsion resulting from disease is precisely what it ought not to be according to the current view of muscular action. According to this view, there ought to be increased activity of the circulation in one or other of the great nerve-centres, for the convulsion is the sign of increased vital activity in this part. What there ought to be is precisely the very opposite of what there is. What there is, is precisely what there ought to be to bring about convulsion according to the view of muscular action evolved in the physiological portion of this inquiry. And with this general remark the chapter may be brought to a close, for if all that has been said does not make this conclusion self-evident, it would be hopeless now to try and make the matter clear by any recapitulation of the heads of the argument.

## CHAPTER II.

# ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN COMMON TREMBLING AND OTHER FORMS OF TREMOR.

### (A.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE CONDITION OF THE RESPIRATION AND CIRCULATION IN COMMON TREMBLING AND OTHER FORMS OF TREMOR.

I.

THE *condition of the respiration in common trembling is one of unmistakable depression.*

The respiration is carried on very imperfectly in all forms of tremulous disorders. This is evident in the want of vital warmth, as well as in the comparatively small amount of air which passes in and out of the chest during a bout of trembling; and this, as will be seen immediately, is also what may be inferred from the state of the circulation.

II.

*The condition of the circulation in common trembling and other forms of tremor is one of unmistakable depression.*

In an attack of common trembling the circulation is greatly depressed, and the pulse does not recover itself until the paroxysm is over. In paralysis agitans, the paleness and chilliness of the surface of the body, and the decided relief afforded by wine, tell a similar story. In delirium tremens, the cold perspirations, the quick and fluttering pulse, the moist and creamy tongue, are all significant facts. The initial rigor of fever, moreover, is coincident with wanting warmth, miserable pulse, sunken countenance, blueness of nails, *cutis anserina*, and other signs of vascular collapse, and subsultus with the most utter prostration of the powers of the circulation. And in mercurial tremor an inference as to the real state of the circulation may be drawn from the fact that the subjects of this disorder are not unfrequently in the habit of resorting to gin and other stimulants for the purpose of making themselves steady.

## III.

*There appears to be something uncongenial between tremor and an excited state of the circulation.*

The state of the circulation in the delirium of which trembling is the distinctive feature—delirium tremens—is quite different from the state of the circulation in the delirium in which there is no trembling. In the latter case—in the delirium of acute meningitis, for example—the skin, especially the skin of the head, is hot and dry, not cold and damp; the pulse is hard and strong, not weak and fluttering; the tongue is parched and dry, not moist and creamy—the condition, in short, is one of high fever, and not one which, as in delirium tremens, is more akin to collapse than to high fever. And it is not less certainly a fact that delirium tremens loses its characteristic trembling if acute head-symptoms and high fever make their appearance in the course of the disorder. Moreover, it must be borne in mind, as pointing to the same conclusion, that the initial rigors of fever disappear *pari passu* with the establishment of the vascular reaction of the hot stage, and that they return in the form of subsultus when this state of reaction has died out, and left the patient utterly prostrate and helpless. In a word, there are certain facts which appear to show that there is something uncongenial between tremor and an excited condition of the circulation.

## (B.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE STATE OF THE NERVOUS SYSTEM DURING COMMON TREMBLING AND OTHER FORMS OF TREMOR.

### I.

*The condition of the brain during trembling is one of unmistakable functional depression.*

The subjects of common tremulousness have a certain delicacy of constitution which cannot be overlooked, and, if not women, they have very generally a feminine habit of body and mind. It is also evident that they are altogether *unnerved* during the paroxysm, and that their thoughts and words are as little under control as their muscles. In old age and in paralysis agitans, every mental faculty has given way under the wear and tear of life ; and during an actual bout of trembling, the mind loses for the time the command of the small stock of vital energy which is not yet expended. In delirium tremens, the mental state is passive in every point of view. The mind is confused, irritable, despondent, anxious, and tortured with gloomy forebodings or spectral delusions. Everything and everybody are objects of mistrust, or fear, or dread. In the initial rigors of fever, the mental state is one of dejection, languor, stupor ; in subsultus it is one of dreaminess or apathetic drowsiness. In slow mercurial poisoning, the failure of the mental powers keeps pace with the failure of the bodily powers, and the condition is one of premature old age. In every case, in fact, the manifestation of brain-power is all but absolutely suspended during the act of trembling.

## II.

*The depressed condition of the respiration and circulation during tremor, involves a corresponding depression in the vital activity of all nerve-centres at the time, for the vital activity of any organ is directly related to the activity of the circulation and respiration.*

In the different forms of tremor, the condition of the nervous system, as reflected in the state of the mind, has been seen to be one of weakness rather than strength. Nor is it possible to suppose that the condition of the cerebral hemispheres is different from that of other parts of the nervous system ; for the condition of the respiration and circulation, which has just been described, is one which must necessitate a state of things in which the development of nervous influence must be suspended, or all but suspended, not in one nerve-centre only, but in all nerve-centres indifferently.

### III.

*The fact that tremor generally comes to an end during sleep is no objection to the conclusion that this disorder is associated with deficient nervous energy.*

If there be a connection between tremor and depressed nerve-power, it may seem, at first sight, that the trembling ought to be aggravated during sleep, when all brain-power and almost all nerve-power is more or less dormant; but this is not the conclusion which is arrived at after a few moments' reflection. For what is the state of the nervous system during sleep? It is, with the exception of a few centres here and there, a state of sleep. It is a state not remotely akin to paralysis. It is a state in which the muscles will become relaxed, and remain relaxed; for, upon either theory of muscular action, living muscles must become relaxed as soon as they are left to themselves, and must remain relaxed as long as they are left to themselves. And thus the fact that there is an end to tremor during sleep is no objection to the conclusion that this disorder is associated with a depressed condition of nerve-power in general, and of brain-power in particular.

### IV.

*The key to the history of tremor would seem to be that which is found, not in the current view of muscular* *motion, but in the view unfolded in the physiological portion of this inquiry, and which has also served to interpret the history of convulsion.*

It is impossible to connect tremor with a state of over-excitement in any nerve-centre. It is plain, indeed, that tremor, instead of being connected with such a state of excitement, is actually antagonized by it. In a word, the key to the history of tremor would seem to be that which is found, not in the current view of muscular motion, but in the view which is set forth in the first part of this inquiry, and which has also served to interpret the history of convulsion ; for, as in convulsion, so in tremor, the state of the three functions of circulation, respiration and innervation, is one which is in every way the reverse of excitement.

## CHAPTER III.

# ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN TETANUS AND OTHER FORMS OF SPASM.

## (A.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE CONDITION OF THE CIRCULATION AND RESPIRATION IN TETANUS AND OTHER FORMS OF SPASM.

### I.

*THERE is reason to believe that tetanus and other forms of spasm are associated with a state of insufficient respiratory activity.*

In tetanus the breathing, never free, becomes more and more laboured as the spasm gripes with firmer hold upon the walls of the chest, and there are many moments in which the struggle for breath amounts almost to mortal agony. And in the tetanus arising from strychnia the respiration is still further interfered with, as is rendered evident by the fact, discovered by Dr. Harley, that one action of the poison is to prevent the blood from becoming as fully aërated as it ought to be. In catalepsy the play of the lungs is almost or altogether imperceptible. In hydrophobia there is an abiding sense of suffocation, as from some impediment in the throat, and the breathings are hurried and often interrupted by sighs. In acute general myelitis and spinal meningitis, dyspnœa is a prominent phenomenon, and want of breath is evidently one main reason why the vital powers of the system so speedily succumb. In laryngismus stridulus the spasm is accompanied by actual suffocation, and in a lesser degree so also in whooping-cough. In every form of spasm, indeed, there is reason to believe that the condition of the respiration is one which must be spoken of as the very reverse of increased activity.

## II.

*There is reason to believe that tetanus and other forms of spasm are associated with a condition of the circulation which is the very reverse of increased or feverish activity, and that the increased temperature accompanying spasm in some cases is no proof to the contrary.*

In tetanus the pulse has no semblance of increased power except at those moments when dusky lips and other signs of deficient respiration show that it is acquiring fictitious strength from the admission of imperfectly aërated blood into the arteries (p. 162). Nor is the fact that in the fits of spasm, and in a lesser degree in the intervals between the fits, the skin is often very hot and perspiring—the heat rising in some cases as high as 110·75° Fahr., and the sweat having now and then a peculiar pungent odour—a reason for believing that fever in the true sense of the word is an integral part of the history of tetanus, for as death approaches the temperature, instead of falling as it might be expected to do, actually rises higher and higher, and, what is stranger still, this rise may not be at its maximum until the patient has been dead for some time. This fact is not so familiar as it should be, but fact it is, as is abundantly shown by several cases, especially by the three recorded by Dr. Wunderlich,* who was the first to call attention to the subject.

The patient in the first of these cases was a butcher, aged 29. The disorder, which was idiopathic or rheumatic tetanus without anything peculiar as to symptoms, ran its course in five days, death happening in the state of exhaustion following a bout of spasm of no special severity, after an earlier change of short duration in which there was some delirium with marked abatement in the spasmodic symptoms. Putrefaction was unusually rapid. The brain was healthy, but the spinal cord was injected and considerably broken up in substance. The temperature of the ward at the time of death was 77° Fahrenheit; the notes taken of the temperature of the body at different times before and after death, and of the beats of the pulse and the number of the breathings up to death, are these :—

| *Date.* | | *Temperature.* ° Fahr. | *Pulse.* | *Respiration.* |
|---|---|---|---|---|
| 24th July, 1861 | | 102 | 96 | 24 |
| 25th ,, | | 102 | 82 | 22 |
| 26th ,, | 9.0 a.m. | 104·45 | 96 | 20 |
| | 6.0 p.m. | 103·55 | 112 | 32 |
| | 9.20 p.m. | 110·1 | 180 | 36 |
| (*Death*) | 9.35 p.m. | 112.55 | | |

* "Archiv. der Heilkunde." Bd. ii, iii, and v (1861, 1862, and 1864).

| | | | ° Fahr. |
|---|---|---|---|
| After death | 2 minutes | ... | 112·77 |
| ,, | 5 ,, | ... | 113 |
| ,, | 20 ,, | ... | 113·22 |
| ,, | 35 ,, | ... | 113·55 |
| ,, | 55 ,, | ... | 113·67 |
| ,, | 60 ,, | ... | 113·65 |
| ,, | 70 ,, | ... | 113·22 |
| ,, | 90 ,, | ... | 113 |
| ,, | 100 ,, | ... | 111·8 |
| ,, | 6 hours | ... | 106·25 |
| ,, | 9 ,, | ... | 104 |
| ,, | 12 ,, | ... | 102 |
| ,, | 13½ ,, | ... | 101 |

The second case was one of traumatic tetanus in a man aged 20, fatal on the tenth day. Up to twenty-four hours before death, the spasms were well marked, and the mind was quite clear: from this time, and especially during the last six hours, unrest, talkativeness, jactitation, and slight delirium, were the most prominent symptoms. The appearances after death agreed with those met with in the first case. The notes of the temperature are these :—

| | | *Temperature.* ° Fahr. |
|---|---|---|
| Three hours before death | ... ... | 105·8 |
| *At death* ... ... | ... ... | 107·6 |
| 10 minutes after death | ... ... | 107·8 |
| 15 ,, ,, | ... ... | 108 |
| 20 ,, ,, | ... ... | 107·8 |
| 48 ,, ,, | ... ... | 106·45 |
| 58 ,, ,, | ... ... | 105·8 |
| 68 ,, ,, | ... ... | 105·35 |
| 80 ,, ,, | ... ... | 104·45 |
| 95 ,, ,, | ... ... | 103·55 |
| 120 ,, ,, | ... ... | 101·75 |
| 240 ,, ,, | ... ... | 99·3 |

The third case was one of well marked idiopathic or rheumatic tatanus, proving fatal on the third day, from, as it would seem, pneumonia beginning on the second day, rather than from the spasmodic disorder, the only appearances met with after death pointing to the pneumonia. Here the notes of the temperature are:

| | *Temperature.* ° Fahr. |
|---|---|
| 3½ hours *before* death ... ... | 102·85 |
| 10 minutes *after* ,, ... ... | 103·32 |
| 21 ,, ,, ... ... | 103·55 |

Along with these cases also may be ranked others by Drs. Wunderlich, Erb,* Ringer,† Weber,‡ Murchison,‡ Sanderson,‡ and others, which complete the story by showing that this strange rise in temperature up to the time of death and afterwards is not peculiar to tetanus, and of which two or three by Dr. Erb, and one recently noticed by myself, may serve as illustrations.

One case, recorded by Dr. Erb, is that of a man, aged 22, who died from tubercular inflammation of the base of the brain without convulsion, profuse perspiration, unconsciousness, respirations from 44 to 60, and an uncountable pulse being the more prominent symptoms of the last 24 hours of life. In this case the notes of the temperature are—

| | *Temperature,* ° Fahr. |
|---|---|
| 24 hours before death ... ... | 102.65 |
| At death ... ... ... ... | 104·9 |
| 13 minutes after death ... ... | 105·12 |
| 15 ,, ,, ... ... | 104·67 |
| 55 ,, ,, ... ... | 104 |

* "Deutsches Archiv. für Klin. Med.," vol. i, 1866.
† "Med. Times and Gaz.," vol. ii, 1867.
‡ "Clinical Soc. Trans.," vol. i, 1868.

Another case, also recorded by Dr. Erb, was one of purulent meningitis, the patient being a woman, aged 22, six months gone in pregnancy, the more prominent symptoms being, not convulsion, but coma setting in suddenly an hour and a half before death, with very laboured breathing and a full and frequent pulse. In this case the notes taken of the temperature are these—

| | Temperature. ° Fahr. |
|---|---|
| 6 minutes after death ... ... | 103·45 |
| 10 ,, ,, ... ... | 104 |
| 15 ,, ,, ... ... | 104·67 |
| 20 ,, ,, ... ... | 104·9 |
| 35 ,, ,, ... ... | 105·12 |
| 45 ,, ,, ... ... | 105·12 |
| 100 ,, ,, ... ... | 104 |
| 160 ,, ,, ... ... | 101·22 |

A third case, which came under my own notice in the course of last summer, and of which the notes taken of the temperature before and after death are subjoined, was one of sunstroke, fatal in 24 hours, in a man, aged 60, the symptoms being sudden coma, with great oppression of the breathing and pulse, without convulsion.

| | Temperature. ° Fahr. |
|---|---|
| 12 hours before death ... ... | 103·25 |
| 3 ,, ,, ... ... | 104 |
| At death ... ... .... ... | not ascertained |
| 7 hours after death ... ... | 105·5 |

Moreover, it is very well known, though the fact has not been verified in the same exact way by the thermometer, that the body may become very hot shortly before death, and remain very hot for some time after death, in cholera, in scarlet fever, and in several other cases, which in reality occur so frequently as to have little claim to be regarded as exceptional.

If, then, the temperature rises in this manner under these circumstances, it is more than difficult to connect the increased heat of tetanus with increased activity of the circulation—with anything like fever in the true sense of the word. Rising as death draws near the temperature continues to rise after actual death ; and thus the facts would seem to show that the increased heat must be connected, not with increased activity of the circulation, not with anything like true fever, but with a contrary state of things. Nor is it more easy to connect the increased heat with the spasms. A part of the addition may be accounted for in this manner, but only a small part. Indeed, the simple fact that in one of the cases which has been instanced the mercury continued to rise coincidently with a decided abatement in the severity of the spasms, and that in all the cases the rise continued after death, when all spasm was at an end, is in itself a proof that it is not in the excessive muscular action that the explanation of the increased heat of tetanus is to be found. Moreover, the fact that the temperature rises in the same way before and after death in cases where neither convulsion nor spasm were among the symptoms during life, must lead of necessity to the same conclusion. How to explain the phenomenon in question is another matter. Increased heat is an effect of injuries by which the cord or medulla oblongata is torn or cut across. Increased heat, as is shown in some of the cases which have been cited, is an accompaniment of certain diseases which annihilate more or less completely cerebral action, without causing convulsion. It seems as if one condition of this rise in temperature might be the removal of some cerebral regulating power, and beyond this it is difficult to see further, except it be that this paralysis, reaching to the vaso-motor nerves, allows the minute vessels to dilate and receive more blood, and that this state of congestion, even though the blood be stagnant, as it is after death, may lead to increased molecular changes, of which the additional degree of heat is the effect. What is necessary, however, is not to find the cause of the increased heat in tetanus, but simply to point out the fact that this phenomenon does not imply increased activity of circulation—that true fever in the ordinary sense of the word is no part of the history of tetanus. And this, as it seems to me, is the legitimate inference from the evidence which has been cited.

And certainly there is nothing in the history of other forms of spasmodic disorder to set aside the conclusions which are to be drawn from the history of tetanus.

During the attack of catalepsy the appearance of the patient is not unlike that of a corpse, and it may even be necessary to apply the ear to the chest to know of a certainty that the heart continues to beat.

In cholera, the cramps are coincident with a state of almost pulseless collapse, and any increase of temperature before and after death is evidently to be accounted for in the same way as is the analogous phenomenon in tetanus. In hydrophobia the condition of the circulation is the very opposite of true fever. In spasmodic ergotism there is no evidence of vascular excitement throughout the whole course of the malady. And, certainly, no contrary inference with respect to the state of the circulation is to be drawn from the history of the seizures of cramp in the leg and elsewhere which are so often met with in aged people and in those in whom the nerveless and marrowless period of old age is anticipitated by softening of the brain.

In a word, there is reason to believe that tetanus and other forms of spasm are all associated with a condition of the circulation which is the very reverse of increased or feverish activity, and that the increased temperature which accompanies spasm in some cases is no evidence to the contrary.

### III.

*There is reason to believe that spasm is antagonized rather than favoured by an excited condition of the circulation.*

In tetanus it appears to be the rule for the spasm to gain ground almost in exact proportion to the degree in which the pulse loses true power. In hydrophobia it would seem as if the same law held good, for on analyzing the histories of a considerable number of cases, I find that there was less agitation, less convulsion, less spasm, where the circulation was less depressed than it is in the ordinary run of cases. Nor is a different conclusion to be drawn from the history of spasm as it is set forth in whooping-cough. For what is the fact? The fact is simply this—that the whoop, which is the audible sign of the spasm, does not make its appearance until the febrile or catarrhal stage has passed off; that it disappears if pneumonia, bronchitis, or any other inflammation be developed in the course of the malady; and that it returns again when the inflammation has departed. Taken by itself this evidence, it is true, may not amount to much; taken in connection with what has gone before, and with what has still to come, it justifies the notion that spasm, like convulsion and tremor, is a disorder which is antagonized, rather than favoured, by an excited condition of the circulation.

## (C.) ON THE HISTORY OF MUSCULAR MOTION AS EXHIBITED IN THE STATE OF THE NERVOUS SYSTEM DURING TETANUS AND OTHER FORMS OF SPASM.

### I.

*There is reason to believe that spasm is associated with failure of brain-power.*

In the more severe forms of spasmodic disorder, the mental state during the spasm is one of abstraction, exhaustion, or prostration. In catalepsy the mind is either in a deep sleep, or else rapt in some dreamy vision. In tetanus the patient is alarmed, agitated, alive only to suffer. The cramps of cholera are attended by indifference to the future and by hopelessness, than which are no surer signs of mental prostration. In hydrophopia, the mind is in a state which may be said to be the exaggeration of delirium tremens. In spasmodic ergotism the state borders very closely upon fatuity. And in the minor forms of spasm, the evidence, so far as it goes, is to the same effect. Thus, for example, cramp in the calf of the leg is a common accompaniment of general or partial dementia, and thus again, spasm in the stomach and bowels is not unfrequently the immediate result of sudden mental depression.

## II.

*There is reason to believe that spasm is connected, not with a state of inflammation in the spinal cord or any other part of the nervous system, but with a state which may or may not issue in such inflammation—a state to which the name of "irritation" is given, and which is marked not by relaxation of vessels and hyperæmia, but by contraction of vessels and anæmia.*

It is a common impression that spasm is in some especial manner a characteristic symptom of certain inflammatory conditions of the spinal cord, but it may be doubted whether this impression is justified by the facts.

There is no good reason to connect the spasms of tetanus with inflammation in the spinal cord or elsewhere. "Serous effusion with increased vascularity," says Mr. Curling, "is generally observed in the membranes investing the medulla spinalis, and also a turgid state of the blood-vessels about the origin of the nerves," and the same changes may be met with within the cranium, but not in so marked a degree or so frequently. Out of 70 fatal cases collected by Mr. Curling, there were only two in which changes in the nervous system, unequivocally the result of inflammatory action, were discovered after death, and these two were cases where there had been a blow or a wound in the back, where the symptoms had plainly to do with the inflammation of the cord or its membranes rather than with the tetanus, and where the signs of inflammation found after death were, to say the least, as easily referrible in the injury as to the tetanus; and at the same time it is pointed out as a fact, not to be overlooked, that the turgid state of the vessels of the pia-mater, together with the effusion of serum which is met with in the spinal cord and brain after death from tetanus, is also met with in those persons who have been poisoned by opium, hydrocyanic acid, and other powerful drugs —agents often employed in the treatment of tetanus— as well as after death from delirium tremens, hydrophobia, epilepsy, and many other diseases. It is also a fact, to be remembered in relation to this point, that Majendie, Ollivier, and Orfila failed to detect any perceptible lesion in the spinal cord of animals killed by the tetanus resulting from poisoning by strychnia. Nor do recent microscopic investigations into the condition of the spinal cord in tetanus bring to light any clearer signs of inflammatory changes in this organ. Mr. Lockhart Clarke* finds the vessels injected, and the substance of the cord in a state varying from simple softening to complete solution, the softened or dissolved portions forming irregular "areas of disintegration" filled with the *débris* of blood-vessels and nerves, or with a finely granular or perfectly pellucid fluid. These areas of disintegration were chiefly in the grey substance around the canal, but they were also in the white substance. They were, in fact, in no one part particularly and exclusively. Here and there were extravasations of blood and

* "Med. Chir. Trans.," vol. xlviii, 1865.

"other exudations," but pus corpuscles are not mentioned. "In the walls of the blood-vessels," Mr. Clarke says, "there was no morbid deposit, nor any appreciable alteration of structure, except where they shared in the disintegration of the part to which they belonged; but the arteries were frequently dilated at short intervals, and in many places surrounded, sometimes to a depth equal to double their diameter, by granular and other exudations, beyond and amongst which the nerve-tissue, to a greater or lesser extent, had suffered disintegration." And elsewhere Mr. Clarke adds, "the appearances met with are exactly similar in kind to the lesions or disintegrations which are found in various cases of ordinary paralysis, in which there has been little or no spasmodic movement." In short, the cord is broken up, as at a certain time in all cases it is broken up, by ordinary putrefaction, and, the dilated vessels, and, certain exudations of blood and serum excepted, this is all that is noticed. The facts point, not to inflammation, but to disintegration, and what Mr. Clarke finds in six cases is substantially the same as that which is found by Dr. Dickinson in the one case recorded by him,† for the only peculiarity in this case is in the presence, in addition, of an excessive quantity of a translucent, structureless, or finely granular, carmine-absorbing material, evidently the sero-fibrinous plasma of the blood, which had escaped from the minute arteries into various parts of the substance of the cord where the nerve tissue had broken down, and which lay in pools here and there between the cord and its membranes, a state of things pointing evidently,

† "Med. Chir. Trans.," vol. li, 1868.

not to inflammation, but to œdema or dropsy. Nor is a contrary conclusion to be drawn from the condition of the sympathetic ganglia or of the nerves at the wound where there is a wound. In some cases, there is the preternaturally injected state of the minute vessels supplying the sympathetic ganglia, especially the cervical and semi-lunar, met with by Mr. Swan, but these cases are few in number compared with others in which all signs of the kind are absent. In some cases, also, there may be traces of inflammation in the wound, and these cases are more numerous than those in which such traces are met with in the spinal cord or other great nerve-centre; but here again these traces, instead of being constant, are not even common. In the great majority of cases, indeed, the wound, if there be one, is to all appearance perfectly healthy, and healing or healed. In a great number of cases, in the majority perhaps, the primary wound was completely healed and almost forgotten when the symptoms of tetanus made their appearance, and Dr. Rush, who had extensive opportunities for observation in the military hospitals of the United States, and who was unquestionably a most competent observer, remarks that there was invariably an absence of inflammation in the wounds causing the disease. John Hunter also says: "The wound producing tetanus is either considerable or slight. * * * When I have seen it from the first, it was after the inflammatory stage, and when good suppuration had come on; in some cases when it had nearly healed, and the patient was considered healthy. Some have had locked-jaw after the healing was completed. * * * When tetanus comes on in horses, as after docking, it is after the wound has suppurated and began to heal."

Again, the history of true inflammation of the spinal cord or its membranes, would only seem to lead to the same conclusion by a different way, that is, by showing that where this inflammation is really present, the symptoms are not those of tetanus.

Acute general spinal meningitis is often obscure enough in its symptoms at first, and this obscurity is generally increased by the presence of head-symptoms in one form or another, for, in the majority of cases, the spinal disease is only a part of an affection in which the cranial nerve-centres are all in some degree implicated. As symptoms of primary importance may be enumerated:—fits of pain along the spine and in the extremities, produced by movement, accompanied by fits of muscular stiffness in the painful parts, intervals of comparative or complete freedom from pain and stiffness as long as movement can be avoided, *absence of marked spasmodic symptoms*, absence of paralysis, some exaltation of sensibility, loss of power over the bladder, partial loss of power over the bowels, and absence of spinal tenderness: as symptoms of secondary importance, these—difficulty of mastication and deglutition, difficulty of breathing, no increased reflex excitability, no priapism, fits of perspiration, no active inflammatory fever, and no marked head-symptoms. The pain along the spine and in the extremities produced by movement, must, as I think, be regarded as the most prominent symptom of all. It may be confined to the region of the spine, but more generally it shoots into the extremities, into the legs especially. As a rule, it does not shoot belt-wise round the trunk. It is brought on by any movement of the trunk, and in great measure at least it may be prevented by avoiding such movement. It is brought on also by moving the extremities, and in this case it is very likely to begin in the limb and shoot thence to the spine. It seems to depend, in part at least, upon the same cause as the pain of pleurisy, viz., the dragging of an inflamed and therefore exquisitely tender serous membrane, and its character is certainly more like that of pleurisy than of rheumatism (to which latter pain it has been likened), for it occurs in the same sharp, sudden, breath-stopping catches. Along with these fits of pain are fits of muscular stiffness in the painful parts, about which latter fits it is desirable to have very clear notions. It is usual to regard this stiffness as analogous to the spasm of tetanus; it is necessary, I believe, to look upon it as expressing an instinctive act of muscular contraction, of which the object is to prevent pain by preventing the movements which produce pain. The spine and extremities cannot be moved without causing pain: the stiffness prevents the pain by preventing the movement: this would appear to be the true view. This explanation, originally given by Dance as applying to the muscular stiffness in a case of acute spinal meningitis observed by him and recorded by Ollivier, would seem to apply with the same exactness to all cases of the kind. Indeed, as I believe, there can be no greater mistake than to confound the stiffness in question with the spasm of tetanus, or to regard, with Ollivier, spasm "comme indignant positivement la phlegmasie des membranes de la moelle," for the rule is, that as long as the patient can keep still, so long is he, comparatively at least, free, not only from fits of pain, but from fits of stiffness also, these intervals of freedom being sometimes of considerable length, even for days—a rule which is very different from that which obtains in tetanus. The differences between acute spinal meningitis and tetanus, in respect of spasm, are indeed so marked as to prevent the possibility of a mistake in diagnosis, if only a moderate degree of attention be paid to the subject. Muscular rigidity continuing without any marked relaxation from the time of its first appearance is the most characteristic symptom of tetanus. It would seem to be the rule for this state of rigidity to begin in the muscles of the jaws, causing trismus, and to extend from thence as a centre, first to the muscles of the face and neck, then to those of the back, causing opisthotonus, then to those of the lower extremities, and, lastly, to those of the upper extremities, the progress in both extremities being from above downwards, but there are exceptions to this rule. Thus, the tetanus caused by strychnia, if, at least, the dose of the poison be large, is not only very speedily fatal, the time varying from 15′ to 20′, but, according to Mr. Poland, it differs also from ordinary tetanus, in the absence of lock-jaw, and in the presence of specially strong spasms in the extremities, the arms being stretched out stiffly and the hands clenched, and the legs being widely apart and rigidly extended. Again, in ordinary tetanus there are some cases in which the muscles of the neck are affected before those of the jaws, and others in which the muscles near a wound, as in the stump after an amputation, have been the first to become rigid.

Even in the most extreme cases the hands and tongue remain limber, and it is but very rarely, except perhaps in children with "head-symptoms" in addition to the ordinary phenomena of tetanus, that a squint or a fixed stare shows that the deep muscles of the orbit are affected. Fits of spasm may seize upon the tongue, as they do frequently upon the muscles of the throat in attempts to swallow, but there is no proof that either the tongue or the muscles of the throat are ever in a state of permanent rigidity. Neither is it probable that the heart or any other involuntary muscle is in any degree permanently contracted. The affected muscles are very hard, curiously so, feeling very much as they do in rigor mortis, and not unfrequently they are found to be somewhat tender when pressed upon or squeezed. In the great majority of cases, without question, the first effect of tetanic rigidity is to cause lock-jaw, and the next to bend the body backwards as it is bent in opisthotonus, which backward bending, by the way, is almost as constant and characteristic a phenomenon as trismus. Now and then, it is true, instead of the body being bent backwards it may be bent sideways, causing pleurosthotonus, or forwards, causing emprosthotonus; but these bendings are quite exceptional, and opisthotonus may therefore be looked upon as the one position which the body always takes or tends to take in tetanus. Besides this rigidity, tetanus is also marked by fits of painful spasm in the permanently contracted muscles, which fits become more frequent as well as more violent and painful as the disease progresses, recurring, when at the worst, every ten or fifteen minutes, lasting from one to two and a half minutes, and sometimes being violent enough to crush the teeth out of their sockets, or to break the thigh bones, or to cause great muscles like the psoas and rectus femoralis to tear across. In acute spinal meningitis, on the other hand, the jaw, if it be set at all, is rather at the close of the disease, and then only to a very inconsiderable degree, and muscular rigidity and spasm are neither constant nor conspicuous phenomena. In acute spinal meningitis, indeed, it is plain that the muscular rigidity and the seeming spasms are in great measure voluntary or semi-voluntary acts to prevent the pain in the back and limbs which is produced by movement, and that the muscles are relaxed, with the exception perhaps of those behind the neck, almost as long as the patient can keep perfectly still. *In a word, the true involuntary fits of spasm and the permanent muscular rigidity which are constant and characteristic phenomena in tetanus are not met with in acute spinal meningitis.*

Among the symptoms of acute general myelitis, no place is found for trismus, or convulsion, or spasm in any form. Paraplegic anæsthesia, ushered in by tingling or some similar sensation in the parts which eventually become anæsthetic; paraplegic paralysis, ushered in by uncontrollable restlessness; a disagreeable feeling of tightness around the waist and elsewhere; absence of pain in the spine or extremities—of pain produced by movement especially; retention of urine; involuntary stools; absence of spinal tenderness; increased sensitiveness to differences of temperature, by which moderately warm or iced water gives rise to a feeling of burning over the vertebra which marks the upper limit of the myelitis; annihilation of reflex excitability in the paraplegic parts; priapism; acidity of urine; comparative voicelessness; impeded respiration; engorgement of lungs and other viscera; tendency to bed-sores; loss of electro-contractility and electro-sensibility in the paralysed muscles; absence of "head-symptoms"; absence of fever; absence of trismus, or any other convulsive or spasmodic symptoms are, in fact, the points which call for special notice in the history of general acute myelitis. The symptoms are very different from those of spinal meningitis—so different as to make it difficult to confound them, if only moderate care be taken to realize them. In spinal meningitis, the most prominent symptom is pain in the back and extremities, produced or aggravated by movement; in myelitis, pain of any kind has scarcely a claim to be reckoned among the symptoms, pain produced by movement certainly not. In spinal meningitis the sensibility is somewhat exalted; in myelitis it is abolished. In spinal meningitis there is muscular weakness, and the movements are fettered by pain, but there is no true paralysis; in myelitis paralysis is the symptom of symptoms. In spinal meningitis there is occasionally a state of muscular stiffness, half voluntary in its character, of which the object is to prevent certain movements which give rise to pain. In myelitis there is, for the most part, an utter absence of any symptom akin to spasm or tremor, or convulsion. Ollivier, it is true, speaks of continuous contraction of the limbs as being met with, "assez ordinairement," in chronic myelitis; but the cases cited by this excellent observer do not substantiate this statement. Thus, out of nineteen cases of myelitis, complicated and uncomplicated, acute and chronic, there are three only in which these contractions were present, and not one of the three can be correctly cited as a case of myelitis. Thus, in one of the three (89), the sensibility was intact, and the disease of the cord confined almost exclusively to the anterior columns ; in the second (93), there was obtuse sensibility, and the disease was chiefly in the grey matter ; and in the third (94), sensibility remained, and there was no post-mortem examination to show what the disease in the cord really was. In each one of these cases, also, there were head-symptoms which do not figure in uncomplicated myelitis. Again, prolonged contraction of the extremities is not an unfrequent symptom in cases in which there is neither myelitis nor spinal meningitis—cases in which the state of the cord is that which is spoken of as "spinal irritation." Nay, even in those exceptional cases of myelitis in which there is increased reflex excitability in the paralysed limbs, it is difficult to connect these spasmodic symptoms with inflammation. Dr. Brown-Séquard says—"When the dorso-lumbar enlargement is inflamed, reflex movements can hardly be excited in the lower limbs, and frequently it is impossible to excite any. On the contrary, energetic reflex movement can always be excited, when the disease is in the middle of the dorsal region, or higher up." And again, when speaking of the reflex convulsions which may happen in the cases where the inflammation is in the middle of the cord, or higher up, he says, "convulsions do not take place at the beginning of the inflammation, but some time after, and they recur by fits for months and years after." And this is precisely what happens. The truth, in fact, would seem to be, that these reflex spasmodic movements must be referred, *not* to inflammation in the lumber enlargement of the cord, nor yet to inflammation higher up in the cord, for in this latter case, to enforce what has just been said by repeating it, the "convulsions do not take place at the beginning of the inflammation, but some time *after*, and they recur by fits for months and years *after*." They happen, as it would seem, *after* the inflammatory disorganization has interrupted the continuity of the cord, and produced a state of things analogous to that witnessed in the guinea-pig, whose cord has been cut across experimentally—a state of things in which increased reflex excitability in the paralysed parts is one of the consequences. Nor is a different conclusion to be drawn from the occasional presence in the paralysed muscles of a state which is analogous to it, not identical with the "late rigidity" of Todd. This "late rigidity" is very different from "early rigidity." In "early rigidity," the electro-motility of the muscles is increased, and the muscles relax during sleep, and to a less degree under the influence of warmth. The contraction is evidently of the nature of spasm. In "late rigidity," on the contrary, the muscles are wasted, their electro-motility is annihilated, and sleep and warmth do not tell in causing relaxation. This form of contraction, indeed, if not identical with rigor mortis, is, as it would seem, more akin to this state than to spasm. In a word, absence of spasmodic symptoms would seem to be the rule in all cases of myelitis, acute or chronic. In children, it is true, myelitis may be ushered in by convulsions—in which case the convulsion may be supposed to take the place of the rigor which may usher in the same disorder in adults, and to belong to the precursory stage of irritation, and not to the stage of actual inflammation—but, even in children, unless there be some meningeal complication along with the myelitis, this preliminary convulsion would seem to be of rare occurrence.

Prolonged muscular contraction, on the other hand, is one of the many symptoms belonging to the state which is known under the name of spinal irritation. The lower extremities appear to be the parts most commonly affected, one or both of them ; but the upper extremities can claim no exemption, nor yet the muscles of the jaws and neck, trismus and torticollis being among the forms it may take. This contraction, which is generally painless, may be prolonged for weeks or even months continuously, even during sleep, or with occasional intermissions of uncertain duration ; and the attacks, secondary as well as primary, are usually found to begin and end suddenly and unexpectedly. It cannot well be confounded with tetanus ; it may in some instances be difficult to distinguish between it and the somewhat vague disorder to which Trousseau gave the name of tetany (tétanie). In tetany as in tetanus, the contraction is painful, but the order in which the body is attacked, is different from that which is observed in tetanus, centripetal not centrifugal, first the extremities, then the trunk or head, the contraction in fact being confined to the extremities except in cases of unusual severity. In the way in which it affects the extremities first, and often exclusively, the contraction of tetany agrees with the contraction under consideration, but in other respects it differs. It differs especially in being ushered in and accompanied by symptoms which do not seem to be part and parcel of simple spinal irritation, viz., tingling and some degree of anæsthesia, and also (so it is said) in the form of the hand being peculiar when the contraction is in this part, this form being like that which is assumed in putting on a tight glove, and also in the possibility of bringing on the contraction when it is absent by firm pressure upon the principal arteries and nerves of the part in which the contraction is about to be manifested. It may be questioned, however, whether there are absolutely fixed lines of division between these different forms of prolonged muscular contraction, and whether the differences which exist may not be accounted for as the result of different degrees of irritation, affecting, it may be, different parts of the spinal cord. It may be questioned, also, whether a sufficient case is made out for describing tetany as a distinct disorder, and whether it is not rather a form of spinal irritation complicated with some graver spinal disease—myelitis, meningitis, or congestion—in varying proportions. The association of tingling and numbness with the prolonged contraction is, as it would seem, a reason for an affirmative conclusion. At any rate, be its significancy in tetany what it may, prolonged contraction in various sets of muscles must be looked upon as a not unfrequent symptom in simple spinal irritation, a state which points, not to organic, but to functional disorder, of which one most characteristic feature is the way in which one symptom or group of symptoms may change, and change suddenly, into another symptom or group of symptoms. In spinal irritation, indeed, it is now this disease which is simulated, now that, there being scarcely any disease which may not be copied. At one time the head is affected, at another the chest, at another the abdomen or the extremities, and the only thing constant among these ever-shifting phenomena appears to be the presence of spinal tenderness, of which the seat changes from one part to another as this or that set of spinal nerves is chiefly affected. The pain or disorder of any particular organ is altogether out of proportion to the constitutional disturbance; and the local tenderness of the spine, in the simple fact of its sudden changeableness as to its seat, has plainly nothing to do with a cause so mechanical and fixed in its nature as inflammation. In point of fact, the subjects of spinal irritation, with few if any exceptions, may be spoken of as hysterical, hypochondriacal, or nervous. They have that *nervous constitution* which Whytt, following in the steps of Sydenham, showed to be the common basis of hysteria and hypochondriasis, and of which the signs are sufficiently obvious. First in order among these signs comes that sign which Sydenham regarded as pathognomic of hysteria and hypochondriasis—a proneness to pass, under or after strong emotion, large quantities of pale, limpid urine. Then come other signs scarcely less characteristic: proneness to tenderness, not only in some part of the spinal column, but also in the epigastrium and left hypochondrium—*le trépied hystérique* of Briquet; proneness to sudden and distressing flatulent distension of the stomach and bowels, with loud rumblings and explosions, and with a feeling of a ball rolling about, first in the left flank, and then mounting, or tending to mount, into the throat, where it gives rise to a sense of choking and to repeated acts of swallowing; proneness to bursts of laughing or crying and sobbing; proneness to yawning, sighing, and stretching of the arms, which phenomena are rarely ever present in acute organic disease; proneness to fits of convulsive agitation and struggling. Then come a promiscuous series of signs, namely, these: erratic pains of a neuralgic character, breathlessness, nervous cough, palpitation, throbbings in the temples, epigastrium, and elsewhere, "flushes and chills," syncope, hiccough, nausea, vomiting, aversion to or unnatural craving for food, heartburn, oppression at the præcordia, languor, debility, fidgetiness, tremulousness, vertigo (especially on rising hastily), ringing in the ears, "animus, nec sponte, varius et mutabilis," fancifulness and inability to discriminate between fact and fiction, undue lowness of spirits or the contrary, and other symptoms whose name is legion. Not only, indeed, is the name of these symptoms legion, but there is ever going on a process of mutual metamorphosis in the symptoms themselves; and, in short, it is this very variability and mutability of the symptoms which must be looked upon as the great characteristic of the nervous constitution, with which, and not with any inflammation or structural change, the prolonged muscular contraction, which has to do with spinal irritation, is associated.

The vagueness in the seat of the inflammation which may be developed in the course of various spasmodic disorders, would also seem to show that spasm is not to be regarded as a symptom of inflammation of the spinal cord, or of any other part of the nervous system. In tetanus, for example, the traces of inflammation met with after death are not in the spinal cord exclusively, but in various parts of the brain, in the nerves, and in other parts as well. And so also in hydrophobia. Thus, in 46 cases, of which the histories were carefully analysed by my brother, J. Netten Radcliffe,* "the morbid appearances after death were in the dura mater in 8, in the arachnoid membrane in 10, in the pia mater in 16, in the velum interpositum in 2, in the choroid plexus in 12, in the cerebral hemispheres in 28, in the spinal cord and membranes in 18, in the medulla oblongata and pons varolii in 4, in the tongue in 8, in the palate in 3, in the salivary glands in 2, in the pharynx in 19, in the œsophagus in 16, in the stomach in 20, in the intestines in 6, in the larynx, trachea, and bronchial tubes in 31, in the ultimate ramifications of the air-passages in 24, in the heart in 4. These lesions consisted of every grade of injection of the blood-vessels, from the slightest blush to the most vivid red or dark black congestion; of alteration of the consistency of the tissues, principally softening; of effusion of blood and certain products of perverted secretion and nutrition. In several of the cases the lesions were of such a character that they have been classed with those resulting from common idiopathic inflammation; in a greater number of cases they were of that character which is found in the structural changes occurring in asthenic conditions of the system." Now, this vagueness in the seat of these inflammatory and other structural changes, I look upon as a very curious and significant fact—a fact which, perhaps, more clearly

* "Lancet," Sept., 1856.

than any other single fact, is calculated to show the true relation of spasm to inflammation. It is calculated to show that inflammation of one particular nerve-centre cannot be essential to the existence of the spasm. It is calculated to show that the cause of the inflammation may be as general as the cause of the febrile symptoms which are developed along with the inflammation—that, in fact, it is little more than an accident, which fixes the seat of the inflammation in one nerve-centre rather than in another, or in one part of the organism rather than in another. In the case of hydrophobia, indeed, it is calculated to put the inflammation which may be developed in the course of the malady in the position of a depurative process—a process which, like the inflammation developed in connection with the fever of small-pox, is intended to rid the system of a morbid virus. And thus, as with convulsion and tremor, there is reason to believe that spasm is connected, not with a state of inflammation in any part of the nervous system, but with a state which may or may not issue in such inflammation—a state to which the name of "irritation" is commonly given, and which is marked, not by relaxation of vessels and hyperæmia, but by contraction of vessels and anæmia; for the arguments which were of avail in the former cases hold good in this case also.

## III.

*The key to the history of spasm would seem to be that which served to unlock the histories of convulsion and tremor, and which is to be found, not in the current* *view of muscular motion, but in the view of this motion which is unfolded in the physiological portion of this inquiry.*

The preceding remarks evidently lead to the same conclusion as that already arrived at when speaking of convulsion and tremor, and by the same way. Indeed, what was said when dismissing the subject of convulsion and tremor, will equally serve for the dismissal of the subject of spasm, if only the word spasm be inserted in the places where the word convulsion or tremor was inserted.

## Chapter IV.

# ON THE HISTORY OF SENSATION AS EXHIBITED IN NEURALGIA AND OTHER FORMS OF NEURALGIC DISORDER.

## (A.) ON THE HISTORY OF SENSATION AS EXHIBITED IN THE CONDITION OF THE CIRCULATION AND RESPIRATION DURING NEURALGIA AND OTHER FORMS OF NEURALGIC DISORDER.

### I.

*PAIN of a neuralgic character may be associated with a very depressed condition of the circulation.*

It is a well-established fact that neuralgia in its most excruciating form may occur again and again without either fever or inflammation. It is also a well-established fact that the majority of persons who suffer from neuralgia are of a feeble and excitable constitution, with the circulation in keeping with this state of things. Judging, also, from the pale and perspiring skin, and the miserable pulse, which are so generally met with in the actual paroxysm of neuralgia, it may be supposed that this paroxysm is associated with a state of the circulation in which the habitual depression is exaggerated. Indeed, the appearances during such a paroxysm are often calculated to remind one of the cold stage of ague, especially in that form of neuralgia which is met with in aguish districts, and in which malaria seems to figure conspicuously as a cause of the malady; for in this case the neuralgia is often obedient to the same law as ague so far as this—that it is associated with rigors, that it begins and ends punctually at a given time, and that it is followed by an obscure hot fit. It would seem, indeed, as if the neuralgia and the rigors were companion symptoms, both belonging to a cold stage, both associated with a depressed state of the circulation—a state of anæmia, and not a state of hyperæmia. And this view of the matter derives some additional support from the fact that in all cases of neuralgia the patient is apt to shiver and shudder during the paroxysm. There is, in fact, abundant evidence to show that pain of a neuralgic character is associated with a state of circulation which is altogether opposed to the state of inflammation and fever: at any rate there will be no lack of such evidence when what has just been said is taken in connection with what still remains to be said.

## II.

*Pain of a neuralgic character would seem to be antagonized rather than favoured by an over-active condition of the circulation.*

In rheumatic fever the rule, I believe, will be found to be this—that the pains which had been torturing the patient for days, or weeks, or months previously, preventing him from being comfortable when up, and causing him to toss about in sleepless misery at night, come to an end when the feverish reaction and local inflammation of the fully formed disorder make their appearance. After this, the joints are *tender* enough, but if the patient keep as still as he is very likely to do under the circumstances, he is comparatively at peace so far as pain is concerned. Or, if it be otherwise, the pain will generally be found to be in a part in which the signs of rheumatic inflammation are imperfectly established or absent, or else at a time in which there is a decided remission in the feverish symptoms—an event which happens more frequently in this disorder than is commonly supposed.

It is also difficult to look upon the local inflammation of gout as essential to the existence of the racking pain of this disorder. "About two o'clock in the morning," says Sydenham, who from personal experience knew full well what to say, "the patient is awakened by a severe pain in the great toe, or, more rarely, in the heel, ankle, or instep. This pain is like that of a dislocation, and yet the parts feel as if cold water were being poured over them. Then follow chills and shiverings, and a little fever. The pain, which was at first moderate, becomes more intense; and with its intensity the chills and shivers increase." After tossing about in agony for four or five hours, often till near daybreak, the patient suddenly finds relief, and falls asleep. Before falling asleep, the only visible change in the tortured joint is some fullness in the veins: on waking in the morning, this part has become swollen, shining, red, tender in the extreme, and more or less painful, but this painfulness is as nothing when compared with the torture of the night past. It seems, indeed, as if the pain which now exists must be referred to the mere tension and stretching of the inflamed ligaments, for it may be relieved, or even removed, by judiciously applying support to the toe and to the sole of the foot. On the night following, and not unfrequently for the next three or four nights, the sharp pain in all probability returns, reappearing and disappearing suddenly, or almost suddenly, and resulting in the discovery of additional inflammatory swelling upon awaking in the morning. The pain in these relapses, like the primary pain, is accompanied by chills and shivers, and by the most distressing irritability and excitability, but until unequivocal signs of inflammation are developed in it the painful part is not tender in the true sense of the word. The inflammation is attended by no fever, or by very little ; or, if it be otherwise, as it is occasionally, the inflammation runs higher than usual, *and the characteristic pain is less urgent than usual.* Dr. Garrod points out this latter fact in his excellent work on Gout,* and says that he has seen several illustrations of it. From its history, then, it would seem as if the inflammation of gout were not essential to the pain of gout. It would seem as if the pain went hand in hand with the rigors which are preliminary to the development of the inflammation. It would seem as if the inflammation had little to do with the pain, for if it were otherwise, it is scarcely to be supposed that the pain should be least urgent in the cases of gout in which the inflammation is most marked, and that the unequivocal signs of inflammation should make their

* "Gout and Rheumatic Gout." Post 8vo. London: Walton and Maberly, 1859, p. 39.

appearance during sleep without waking the patient. Nay, it would even seem as if the pain were put an end to by the establishment of the inflammation—as if, in fact, the pain were antagonized rather than favoured by the inflammatory condition. Moreover, the suddenness with which it begins and ends in the majority of cases must be looked upon as a reason for referring the pain to the category of neuralgia—a category in which, to say the least, it is not a little difficult to find any place for inflammation.

There is also reason to believe that pain holds the same relation to fever and inflammation in other kinds of fever besides the rheumatic, and in other kinds of inflammation besides the gouty.

Six or seven years ago, I had a patient in the Westminster Hospital who, when I saw him first, complained of violent pains all over the body, especially in the back and loins, and also of chills and shivers. A few hours afterwards he was hot and feverish, and the pains and chills and shivers had all taken their departure. The case was one of small-pox; and the lesson I gathered from it was that the pains and the rigors were symptoms which ought to be classed together, and considered as belonging to the cold and not to the hot stage of the fever. And this case would seem to be a fair illustration of what happens in other fevers; for it seems to be the rule rather than the exception for the pains which attend upon the onset of these disorders to pass away or to become greatly mitigated when the cold stage gives place to the hot stage. Nay, it would even seem as if pain gave place for the time to what may be called artificial feverishness. At any rate, I have more than once felt tic-douloureux pass away as soon as I could set my blood fairly in motion by violent bodily exercise; and on two occasions I have derived a similar benefit from a practice which is not unfrequently adopted in the hunting field, and put an end summarily to a sudden attack of lumbago by leaning forwards in the saddle and beating the loins with the two hands until the whole body was aglow and the perspiration dropped from the forehead.

Nor is it different with inflammation. In the case of a dislocation or sprain, for example, the acute pain of the accident—the pain to which Sydenham likens that of gout—does not, as a rule, remain after the parts have begun to be hot and swollen and tender; and this case is certainly no exception in the history of inflammation. It would seem, in fact, as if the proper place for the pain was among the phenomena of the preliminary cold stage—the stage of "shock," and not among the phenomena of actual inflammation. And it is not impossible that the efficacy of blisters in the relief of many kinds of pain may furnish another passage in a similar story; for it is a fact, which is as well established as any fact in therapeutics, that blisters are most effectual means of relieving pain, and that this relief is usually coincident with the blistering—that is, with the inflammation set up by these agents. Nor is a contrary conclusion to be drawn from the history of certain cases in which pain continues as a permanent symptom after the full establishment of inflammation, as, for example, in deep-seated inflammation of the mamma, for in these cases it is a fact that this persistent pain is immediately relieved or removed by those operative measures which diminish the tension or stretching arising directly or indirectly from the inflammation. It is a fact, that is to say, that the persistent pain in these cases is an accidental and not an essential accompaniment of the inflammation—a consequence, as I have just said, of tension or stretching of the tender tissues, and not a necessary part of the inflammation itself.

How far these inferences will be confirmed or set aside by the histories of those forms of pain in which the nervous system is more especially implicated remains to be seen; but, so far, there seems to be good reason for believing that pain of a neuralgic character is connected with a depressed state of the circulation rather than the opposite state of febrile in inflammatory excitement.

### III.

*Pain, the result of tenderness, not pain of a neuralgic character, is associated with the state of active congestion or inflammation.*

Severe and prolonged pain may be the result of touching or otherwise interfering with an inflamed part, but this pain, which is the result of the tenderness which accompanies the inflammation, is not to be confounded with pain of a neuralgic character. Pain, the result of tenderness, in fact, is only the sign of exalted sensibility, which exalted sensibility is the effect of the increased vascularity—a phenomena to be explained in the same way as the sensation produced by the prick of a pin, or by any other local means. It does not occur if the inflamed part be let alone. It is not essential, like pain of a neuralgic character; it is only accidental. In a word, evidence is not wanting which would seem to show that there is something uncongenial between pain of a neuralgic character, and pain the result of tenderness, the former pain disappearing when the latter makes its appearance. Thus in the cases of neuralgia in which it may be presumed that neuritis is developed in the course of the disorder, the nerve changes from a state of comparative indifference to pressure into a state of exquisite tenderness, and at the same time the previous torture comes to an end, and the patient is comparatively at ease if the nerve be let alone.

## IV.

*The condition of the respiration in neuralgia and other neuralgic disorders, like the condition of the circulation, is one of deficient activity.*

The condition of the respiration in neuralgia and neuralgic disorders generally presents nothing which can be spoken of as at all remarkable. A patient suffering from severe tic-douloureux will often sigh in a way which suggests the notion that he is far from happy at the time, when in reality the sighs only show that insufficient breathing has to be made up now and then by breaths which are more deeply drawn than usual; and other signs to the same effect may be found if they are carefully looked for in this and in analogous disorders. But it is unnecessary to adduce special illustration in support of this point, for if the condition of the circulation in neuralgia and analogous disorders be what it has been stated to be, one, that is, of wanting activity, it is plain that the accompanying condition of the respiration must also be marked by wanting activity.

(B.) ON THE HISTORY OF SENSATION AS EXHIBITED IN THE CONDITION OF THE NERVOUS SYSTEM DURING NEURALGIA AND OTHER NEURALGIC DISORDERS.

I.

*Neuralgia and pain of a neuralgic character would seem to be connected, not with a state of inflammation in any part of the nervous system, but with a state which may or may not issue in such inflammation—a state to which the name of irritation is commonly given, and which is marked, not by relaxation of vessels and hyperæmia, but by contraction of vessels and anæmia.*

Pain is no very conspicuous symptom in the common form of cerebral meningitis—that is, the tubercular form ; and in simple meningitis there is reason to believe that any severe pain in the head is the precursor of, rather than the attendant upon, the actual inflammation. Not long ago, for example, I had in the Westminster Hospital a well-marked case of acute simple cerebral meningitis in a boy aged fifteen. On my first visit, the face was pale and perspiring, the ears and head generally somewhat below the natural temperature, the pupils dilated, and the pulse contracted and feeble, and what was complained of was agonising pain in the head, with frequent chills and shivers. On my second visit, eight hours afterwards, the face was flushed, the head burning hot, the pupils contracted, the eyes ferrety, the skin hot and dry, the pulse quick and hard ; and now fierce delirium had taken the place of the pain. And this, so far as my experience goes, is the regular history of the pain in this disorder. It is pain ceasing, not pain beginning, as the signs of active determination of blood to the head make their appearance. It is pain in association, as it would seem, with an anæmic rather than with a hyperæmic condition of the membranes of the brain.

Nor is it otherwise when the membranes of the spinal cord are the seat of the inflammation. For example, I had also in the Westminster Hospital, about the same time as the last case, another case in which, after death, was found unmistakable evidence of recent spinal meningitis of an acute character, the patient being a young man aged 23. The illness began three days before admission with sharp pain in the back and legs, shivering and retention of urine, the patient beginning to suffer in this way shortly after sleeping for some time flat on his back on the grass. Upon examination the back was found stiff, with the head drawn back, and on any attempt at movement, and now and then without such attempt, severe pain was experienced along the whole course of the spine, in the legs, in the lower part of the abdomen, and, to a lesser extent, in the head also, this pain being always accompanied by increase of stiffness. Death happened at the end of a week. During the last three days of life the bouts of pain and contraction were very occasional and of very short duration ; and in many instances even these there is reason to believe might have been avoided if the patient could have been kept perfectly still. The pain, in fact, obeyed the same rule as that obeyed by the contraction, of which enough has been said already, and the conclusion would seem to be that the pain is, not of a neuralgic character, but the result of tenderness, and that pain of a neuralgic character in this case is antagonized rather than favoured by the inflammation.

And certainly this is the conclusion which must be drawn from the history of those painful disorders which come under the head of spinal irritation, and which are so often met with in hysterical patients, for here severe pain of a neuralgic character is a prominent symptom, and yet the collateral symptoms and the issue of the disorder in nineteen cases out of twenty make it impossible to ascribe the pain to inflammation of the substance or membranes of the cord.

With respect to neuralgia in all its manifold forms one thing is certain, and this is, that neuritis is not necessary to its production.

In the cases where the extreme local tenderness with some degree of swelling along the track of the sciatic nerve would seem to show that sciatica has become complicated with neuritis, the neuralgic pains are not aggravated. On the contrary, the plain fact would seem to be rather this—that these pains, which had been such prominent symptoms previously, come to an end when the local tenderness and swelling give evidence of the establishment of inflammation in the course of the sciatic nerve, if only the affected limb be kept still and all pressure upon the tender parts be avoided.

It is also the rule, rather than the exception, for toothache to come to an end when the face becomes swollen and inflamed, and so likewise with the stabbing pains which so generally precede the inflammatory eruption of herpes, for these pains scarcely ever remain after the eruption is fully established.

Again, I can testify to this being the true history of facial neuralgia, or tic-douloreux, in many cases: first, neuralgia without local tenderness and swelling and redness, and with frequent chills and shiverings, and a decidedly depressed condition of the circulation; then, after an interval more or less prolonged, local tenderness, redness and swelling, with general feverish reaction, without chills and shivers, and without neuralgia, the true neuralgia for the most part coming to an end coincidently with the establishment of the local inflammation.

In short, neuralgia and pain of a neuralgic character would seem to be connected, not with a state of inflammation in any part of the nervous system, but with a state the reverse of this, which may or may not issue in such inflammation—a state to which the name of irritation is given, and which is marked, not by relaxation of vessels and hyperæmia, but by contraction of vessels and anæmia, the case, indeed, being no other than that of convulsion, or tremor, or spasm, if only the scene of action be shifted from the parts concerned in motion to the parts concerned in sensation.

## II.

*The key to the history of neuralgia and pain of a neuralgic character would seem to be that which is found, not in the current view of sensation, but in the* *view which is unfolded in the physiological portion of this inquiry, and which has also served to unlock the mystery of muscular motion.*

Neuralgia and pain of a neuralgic character, according to the current view of sensation, are associated with a state of increased vascularity in some part of the nervous system. The pain is looked upon as a sign of excited vital action, and, because the manifestations of life in a part are proportionate to the supply of blood to the part, it is assumed that there is this state of increased vascularity. Neuralgia and pain of a neuralgic character, according to the view of sensation set forth in the premises, is nothing more than the result of a disturbance in the electrical equilibrium in some parts of the nervous system, of which the result is discharge, this disturbance being brought about, not by excess of blood in some parts of the nervous system, but by the contrary state of things. In short, the key to the history of neuralgia and pain of a neuralgic character would seem to be that which is found, not in the current view of sensation, but in the view which is unfolded in the physiological portion of this inquiry, and which has also served to unlock the mystery of muscular motion. The physiology explains the pathology, and the pathology establishes the physiology. In physiology and in pathology it is one and the same story throughout.

# DYNAMICS OF NERVE AND MUSCLE.

## PART III.

*A FEW WORDS IN CONCLUSION.*

## IN CONCLUSION.

A FEW words will serve to say all that remains to be said in conclusion.

Looking back at the evidence brought forward in the previous investigations, it is impossible to accept as truth the dogma which ascribes to nerve and muscle a special life, of which action is the expression, which is fed by the blood, and which is prone to act in direct proportion to the amount of blood it has to feed upon. It is impossible to reconcile with this dogma the true history of convulsion, or spasm, or tremor, or increased muscular contraction in any form, or neuralgia, or any pain of a neuralgic character. These phenomena, according to this view, are signs of undue vital excitement in one or other of the parts concerned in the production of muscular motion and sensation. They point to a bloodshot or congested state in one or other of these parts, for without the additional blood, it is believed, there could not be this undue vital excitement. In fact, however, the actual case proves to be the very opposite of what in theory it ought to be. Where the state ought to be one of afflux of blood, it is one of efflux; and, in point of fact, the signs of increased action which have been indicated, convulsion and the rest, are met with under the very circumstances in which they ought not to exist if they are the signs of undue excitement in a special life of nerve or muscle which expresses itself in action. This is the conclusion arrived at when the subject is regarded from a physiological point of view, and this no less is the conclusion when the point of view is shifted from the side of physiology to that of pathology, physiology and pathology in this matter telling one and the same story.

Instead of regarding the state of action in nerve and muscle as a manifestation of vitality, there is, indeed, reason to believe that it must be brought under the dominion of physical law in order to be intelligible, and that a different meaning, also based upon pure physics, must be attached to the state of rest.

There is reason to believe that all kinds of electricity act upon nerve and muscle by way of charge and discharge, the charge antagonizing, the discharge permitting, the state of action.

There is reason to believe that the blood acts upon nerve and muscle, not by causing the state of action, but by antagonizing it.

There is reason to believe that "nervous influence" acts upon nerve and muscle, not by causing the state of action, but by antagonizing it.

The whole case is simple enough. It would seem, indeed :—

(1) That the sheaths of the fibres in nerve and

muscle are capable of being charged like Leyden jars, and that during the state of rest they are so charged.

(2) That the sheaths of the fibres in muscle are highly elastic.

(3) That the fibres of muscle are elongated during the state of rest by the charge with which their sheaths are charged, the mutual attraction of the two opposite electricities, disposed Leyden-jar-wise, upon the two surfaces of the sheaths, compressing the elastic substance of the sheaths and so causing elongation of the fibre in proportion to the amount of the charge.

(4) That the muscular fibres contract when the state of rest changes for that of action, because the charge which caused the state of elongation during rest is then discharged, and because this discharge leaves the fibres free to return, by virtue of their elasticity simply, from the state of elongation in which they had been previously kept by the charge, and that the degree of contraction is proportional to the degree of elongation previously existing.

(5) That the fibres of nerve are not affected in the same way as the fibres of muscle by the charge and discharge of electricity, because the sheaths of the fibres may be wanting in the requisite degree of elasticity.

(6) That the blood antagonizes the state of action in nerve and muscle by helping to keep up the natural electrical charge which antagonizes action.

(7) That "nervous influence" antagonizes the state of action in nerve and muscle by helping to keep up the natural electrical charge which antagonizes action.

(8) That diminished efflux of blood to certain nerve-centres leads to excessive action in nerve and muscle by disturbing the electric equilibrium of the nervous system which is maintained during the state of rest, this disturbance causing a partial reversal in the relative position of the two electricities with which the sheaths of the fibres are charged, and so necessitating the discharge which is the basis of the state of action; for by this partial reversal sheaths of which the charge has become negative at the sides and positive at the ends are brought into juxta-position with sheaths of which the charge remains positive at the sides and negative at the ends—are brought into a relation which necessitates discharge, for discharge must happen when opposite electricities come together.

These are the broad conclusions which are deducible from the facts; these are the more salient points which are made out by a retrospective glance at the subject as a whole. Everything is in opposition to the dogma which ascribes to nerve and muscle a life of which the state of action is the expression. Everything, indeed, points to a solution of the problem of which the effect is to bring phenomena which have been regarded as exclusively vital under the dominion of physical law.

THE END.

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BY THE SAME AUTHOR,

*Will be shortly Published,*

SKETCHES OF CEREBRAL, SPINAL, AND OTHER DISORDERS OF THE NERVOUS SYSTEM.