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VITAL MOTION

AS A MODE OF PHYSICAL MOTION.

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BY

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CONTENTS.

PROLOGUE.

		Page
	VITAL MOTION REGARDED HISTORICALLY ...	1

THE ARGUMENT.

I.

	VITAL MOTION REGARDED PHYSIOLOGICALLY ...	29
Chapter I.	On the Electrophysics of Vital Motion	31
,, II.	On the Electrophysics of Simple Muscular Movement and Simple Nervous Action	51
,, III.	On the Electrophysics of Cardiac and other forms of Rhythmical Vital Motion	71
,, IV.	On the Work of Artificial Electricity in Vital Motion	88
,, V.	On the Electrophysics of Rigor Mortis	137
,, VI.	On the Work of the Blood in Vital Motion	139
,, VII.	On the Work of Nervous Influence in Vital Motion	153
,, VIII.	On the Removal of certain Objections to the View here taken of Muscular Motion	177

THE ARGUMENT.

II.

	VITAL MOTION REGARDED PATHOLOGICALLY ...	185
Chapter I.	On Vital Motion as exhibited in Epilepsy and other Convulsive Disorders	187

Page

Chapter II. On Vital Motion as exhibited in Tetanus and other Spasmodic Disorders 204

,, III. On Vital Motion as exhibited in various forms of Tremor 231

,, IV. On Vital Motion as exhibited in various forms of Neuralgic Disorders 234

EPILOGUE.

A GLANCE RETROSPECTIVELY 245

PROLOGUE

VITAL MOTION REGARDED HISTORICALLY.

"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.'"

VON HUMBOLDT.

MORE than five and twenty years ago my faith in all that I had been taught to believe about vital motion received a rude shock in this way. I happened to be present at an experiment in which a rabbit was killed by injecting a solution of strychnia under the skin: I watched the strong cramps produced by the poison, and wished again and again for death to come and put an end to them: I was amazed to find that the spasm seemed to keep firm hold *in spite of death:* I had to wait until the evening of the fourth day after death, when putrefaction had evidently set in, before any unmistakeable signs of muscular relaxation were to be detected. The animal, when the spasms were at their height, stood tip-toe on its up-stretched hind legs, leaning against a hamper which happened to be within reach, pawing the air, and with the body arched backwards until the ears lay over the scut—a rampant position from which it must have fallen down at once if the muscular contraction had yielded for a moment to relaxation ; and yet it did not so fall until the muscles were made limp by putrefaction. This is what I witnessed. It seemed as if the spasmodic rigidity which existed before death had passed without any interval of relaxation into the cadaveric rigidity which always comes on, sooner or later, after death, and which is only relaxed by the actual decomposition of the muscular tissue. It seemed as if the spasm had passed at once into *rigor mortis*. At first all my prejudices were against such a notion ; in the end, I came to believe, most unhesitatingly, that a radical change was necessary in the doctrine of vital motion,—that the interpretation of spasm was to be sought, not on the side of life, but on that of death,—that spasm and *rigor mortis* were to be regarded, not as signs of vital action in certain vital properties of contractility, but as physical phenomena akin to, if not identical with, the return of an elastic body from a previous state of extension,—that muscular contraction in all its forms might be the simple consequence of the operation of the natural attractive force or forces inherent in the physical constitution of the muscular molecules,—that life is concerned in antagonizing contraction rather than in causing it,—that this antagonizing influence itself might have a physical basis,—that, in short, vital motion might have to be regarded as a mode of physical motion.

And yet more did this conviction grow in strength on the food supplied by two other facts to which my attention was called at a later period.

Of these two facts the first was brought to light in an epileptic patient in whom it had been thought expedient to try and cut short a succession of very violent convulsions by taking blood from the temporal artery.

The artery was divided when the fit was at its height, and the blood escaped by jets in the usual way, but not of the usual colour. Instead of being *red,* the blood was *black:* instead of being *arterial,* that is to say, it was *venous.* The state during the convulsion was evidently that of suffocation; and, on this account, black, unaërated blood had found its way into the arteries, and was being driven through them at the time. The case was intelligible enough as regards the suffocation, for in this state the simple fact is, that black blood does for a time penetrate into and pass along the arteries; but it was not intelligible as regards convulsion, if convulsion were, as it is assumed to be, a sign of exalted vital action. I could connect such exaltation with increased supply of *red* blood to certain nerve-centres, but not with the utterly contrary state of things involved in the actual circulation of *black* blood; and, do what I would, I could see no other conclusion than that which had been already forced upon me by the history of the poisoned rabbit, namely this, that the convulsion pointed to a state of things which had to do with death rather than with life,—that, in short, this state of muscular contraction was due, not to the *black* blood having acted as a stimulus, but to the withdrawal of an *inhibitory* influence which had served to keep up the state of muscular relaxation as long as certain nerve-centres were duly supplied with *red* blood.

And so likewise with the second of the two facts to which I have alluded. I had the good fortune to be present on one occasion when Matteucci was watching the action of strychnia upon the common electric ray of the Mediterranean. I saw very plainly that this action was marked by involuntary electric shocks as well as by involuntary spasms, and I was much struck by what was said by this excellent physiologist in support of the notion that muscular contraction was attended by a discharge analogous to that of the torpedo, and that there was much in common between the action of the electric organ and the action of the muscles: and, so seeing and hearing, I could not help wondering whether muscular relaxation might not be the consequence of the muscular molecules being kept in a state of mutual repulsion by the presence of an electrical charge, and whether the discharge of this charge might not bring about muscular contraction by allowing the attractive force or forces inherent in the physical constitution of the muscular molecules to come into play. I could, indeed, bring myself to adopt no other conclusion than this: and thus it was that this experiment upon the torpedo proved to be the means of adding not a little strength and definitiveness to the conviction at which I had already arrived respecting vital motion.

Looking back, I can now see plainly enough that there are not a few faults and shortcomings in the argument by which hitherto I have hoped to bring others to the same way of thinking with myself in this matter. About the first published statement* of this argument, I may say, in the words of Dryden, that 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:" and, most certainly, no feeling of complacency is called up by the remembrance of any other statement published subse-

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

quently*,†,‡. I should, in fact, be very glad if much that I have written on this subject at different times could be cancelled. What has been done, however, has been done, and, after all, if it were still to do the argument in all essential particulars would be the same. Indeed, except in being clearer and far more comprehensive, the view now taken is that I have always taken ; and this is all that I would now say about it except this—that I have thought it expedient to prolong these introductory remarks so as to bring in certain points in the history of vital motion which can scarcely fail to be of service in preparing the way for the just appreciation of the more serious work with which I propose to occupy myself presently.

Very misty notions about vital motion prevailed in olden times, and, most assuredly, some of this very mistiness still clings to the notions which are now in the ascendant.

In the beginning, as it would seem, all motion was believed to be essentially vital.

Thales talked about the world as being animated by a soul and actuated by demons, and looked upon motion as being due to the working of one or other of these two causes.

Hippocrates believed in the universal presence of a living, intelligent, active principle, to which he gave the name of *nature* (φύσις), and to him, as to many in the present day, it was enough to refer motion to *nature*—to

* "Epileptic and other Convulsive Affections of the Nervous System." (Incorporating the Gulstonian Lectures for 1860.) 3rd edition. Post 8vo. Churchill, 1861.

† "Lectures on certain Diseases of the Nervous System." Delivered at the Royal College of Physicians. Post 8vo. Churchill, 1864.

‡ "Dynamics of Nerve and Muscle." Post 8vo. Macmillan & Co., 1871.

regard it as *natural.* The power of motion, indeed, was one of the faculties with which the principle of nature was endowed.

Plato says little to the point. With him all philosophy merged in theology: to him vital motion, and motion generally, when traced to its source, resolved itself into a display of divine power.

Aristotle, the great contemporary of Plato, recognized, not a Divine Being as Plato did, but a *First Moving Cause,* a *primum mobile,* one in essence, eternal, immaterial, at once immoveable, and the spring of all movement. According to him, this First Moving Cause worked in the living body (ζῶον) through the instrumentality of a principle which was distinctive of this body, and to which he gave the name of soul (ψυχή)—a principle possessing various energies or faculties of its own, distinct from the organs in which it was manifested, and yet requiring these organs for its manifestations. To this soul, when most developed, belonged several faculties (δυνάμεις)—the faculty of receiving nourishment (δύναμις θρεπτική), the faculty of sensation (δ. αἰσθητική), the faculty of motion in place (δ. κινητική), the faculty of impulse or desire (δ. ὀρετική), the faculty of intelligence (δ. διανουτική). Vegetables even, by having the lowest of these faculties, the threptic, were supposed to have souls. Moreover, it is hinted that the seat of this kinetic faculty in animals is in the muscles, and that—a conjecture for which Praxagoras, who lived two hundred years previously, ought to have credit—there were nerves, some of which had to do with movement and others with sensation. Nay, it is scarcely just to speak of the localization of the kinetic faculty in the muscles as being only hinted at, for this was the definite conclusion at which Aristotle arrived after witnessing the working of the intercostal muscles of a living chameleon as displayed under the transparent pleura.

After this time, for a thousand years and more, when anything was done in this direction it was little beyond a servile copying of what had been said by Hippocrates and Aristotle. Even Galen had nothing to say that was really new; nor yet the schoolmen of the middle ages, with many of whom the notions chiefly in the ascendant were those of alchemy and magic and astrology. At the revival of letters, indeed, the only light of importance was that derived from the old Greek fathers in science; and at the end of this epoch no new light had arisen to dissipate the darkness. No new light, for instance, was shed by the doctrine of occult causes which found most favour in these times, for this doctrine was no more than a copy of the doctrine of Hippocrates or Aristotle, that various bodies had various powers by which they were able to act in the various ways *natural* to them. And still less did new light radiate from the notion, which in too many instances was associated more or less closely with that of occult causes—that there were elementary spirits, intermediate between material and immaterial beings, in the four elements of air, water, fire, and earth—sylvans or fairies in the air, nymphs and undines in the water, salamanders in the fire, gnomes, trolls, pigmies, spirits of the mine, little folks, little people, cobolds, in the earth,—that the body had its double or dæmon, called Archæus, whose primary function was to superintend the work of the stomach, and who managed the various functions of the body, that of motion included, through the instrumentality of a legion of underling deputies undignified by any distinctive names.

Indeed, it was not until Von Helmont, Stahl, and

Hoffman appeared on the scene that the notions handed down from the ancients began to be materially modified, and to take the forms belonging to modern times.

With Paracelsus, Von Helmont held that the Archæus and its underlings were the agents in all vital manifestations, and he also thought for himself a little, for to him belongs the credit, if credit it be, of being the first to maintain that the living body had powers of a specific character altogether different from those belonging to inanimate nature.

Accepting the doctrine that there was one law for animate and another for inanimate nature, Stahl went further, and maintained that matter is essentially and necessarily passive and inert, and that all its active properties or powers are derived from a specific and immaterial animating principle imparted to it—a principle to which he gave the name of *anima*. The body, he held, as body, has no power to move itself. All vital motion is the result of animation. The physical powers of matter, which have only free play after death, are in every way opposed to, and counteracted by, the *anima*, of which he further says, as the followers of Hippocrates said of *nature*, that "it does without teaching and without consideration what it ought to do;"—a remark which makes it evident that the *anima* of Stahl is not to be confounded with the conscious personal Archæus of Paracelsus and Von Helmont.

What Stahl explained in this way, Hoffman, who took the next noticeable step in advance, explained on the hypothesis of *nervous influence*, or *nerve-fluid*, whatever that may mean. By this influence or fluid, according to him, the moving fibres have a certain power of action, or tone, which may be increased or diminished. If increased unduly, spasm is the result: if decreased, atony.

Next in order have to be named Glisson, Haller, and *the* Brown, known as the author of the Brunonian system of medicine, men whose speculations form the basis of the doctrine of vital movement now in favour.

Glisson, an eminent professor at Cambridge in his day, was the first to advance the present doctrine of *muscular irritability.* He asserted that there was in muscle a specific vital property, to which he gave this name, and that contraction was due to this property being in some way put in action.

Haller expanded this idea, and drew for the first time a line of distinction between the special vital property of muscle and the special vital property of nerve. He retained the name of irritability for this property in muscle: he gave the name of sensibility to this property in nerve. Each property was something vital, something departing at death, and therefore in no wise akin to any power in inanimate nature. The property was a *life* of which muscular contraction and nervation were *acts.*

Brown, starting from this point, added another idea—that of *stimulation.* Everything acting upon the vital property of irritability or sensibility (to which he gave the common name of *excitability*), according to him, acted as an excitant or stimulus. Action is caused by a process of stirring-up, as it were, the capacity for action being asleep, or at rest, until it is so stirred-up. The idea would seem to be none other than that all vital movement in its nature is identical with that which is produced by teasing a sleeping man until he wakes up and strikes about him in anger.

And this doctrine of vital motion, which thus took form in the speculations of Glisson, Haller, and Brown, is, with little change, the doctrine at present in favour.

In point of fact, the position taken at present has but little shifted since the days of the schoolmen, when occult qualities of one kind or another were thought to be a sufficient explanation for everything—when, for example, terreity, aqueity, and sulphureity, the occult qualities of the three elements, earth, water, and sulphur, of which, in varying proportions, according to Paracelsus, all bodies are composed, were supposed to account for all that was general in these bodies,—when Petreity was thought to be a sufficient explanation of the peculiarities distinguishing Peter from Paul or other men,—when the answer of Argan* to the question, "quare opium facit dormire," in the mock examination for the diploma of physician, would have been listened to without a smile if it had been given in sober earnest before the examiners of a real faculty of medicine:—

Mihi a docto doctore
Demandatur causam et rationem *quare*
Opium facit dormire.
Et ego respondeo
Quia est in eo
Virtus dormitiva
Cujus est natura
Sensus assoupire.

For in referring vital motion to a property of irritability, what more is done than to say, that the moving body moves because it is actuated by an occult quality which is suspiciously akin to terreity, aqueity, or sulphureity, or to Petreity, or to the "virtus dormitiva" of opium in the comedy? "To tell us," as Newton said, "that every species of thing is endowed with an occult specific quality, is to tell us *nothing*." Even to say that the phenomenon is *vital*, is, as Whewell remarks, "very

* Molière, "La Malade Imaginaire:" 3ième intermède.

prejudicial to the progress of knowledge by stopping enquiry *by a mere word.*" Moreover, the very assumption upon which the doctrine in question is based—that vital motion is altogether distinct from physical motion —is itself not altogether satisfactory. "At the best," as Coleridge says,* "it can only be regarded as a hasty deduction from the first superficial notions of the objects that surround us, sufficient, perhaps, for the purpose of ordinary discrimination, but far too indeterminate and diffluent to be taken unexamined by the philosophic enquirer. * * * * By a comprisal of the *petitio principii* with the *argumentum in circulo*—in plain English, by an easy logic which begins by begging the question, and then, moving in a circle, comes round to the point where it begins—each of the two divisions has been made to define the other by a mere re-assertion of their assumed contrariety. The physiologist has luminously explained $y + x$ by informing us that it was a somewhat that is the antithesis of $y - x$, and if we ask what then is $y - x$, the answer is, the antithesis of $y + x$;—a reciprocation that may remind us of the twin sisters in the fable of the Lamiæ, with one eye between them both, which each borrowed from the other as either happened to want it, but with this additional disadvantage, that in the present case it is, after all, but an eye of glass."

But this glance at the history of vital motion is not yet ended. Up to the time of Von Helmont, the idea of a well-defined gulf between animate and inanimate nature was not clearly defined: nor yet after this time did this idea gain universal acceptance.

* "Hints towards the Formation of a more Comprehensive Theory of Life." By S. T. Coleridge. Ed. by Dr. Seth B. Watson. Churchill, 1848.

At the time of Paracelsus the facts of chemistry began to occupy a large share of the attention of philosophers, and soon afterwards a school, called the iatro-chemical school, propounded various physiological doctrines founded upon chemistry. The opposition of acid and alkali, and the workings of ferments of one kind or another, were supposed to supply the solution of many problems in vitality. Then came the hope, kindled naturally by the splendid discoveries of Galileo and Newton in physical science, that the mechanical principles of the macrocosm would supply the key to all requiring interpretation in the microcosm—a hope which called into existence the so-called iatro-mathematical or mechanical physiologists. The question was of the cohesion, the attraction, the resistance, the gravity, which operate in inert matter, and of mechanical impulse and elasticity, not of powers of a higher order. It was believed that all the various bodily functions were problems to be solved, as so many hydraulic or hydrostatic problems chiefly, partly by gravitation and the laws of motion, and partly by chemistry, which itself, as far as its theory was concerned, was but a branch of mechanics, working exclusively by imaginary wedges, angles, and spheres. The restoration of ancient geometry, aided by the modern invention of algebra, had placed the science of mechanism on the philosophical throne. It was thus, for example, that Borelli dealt with the problem of muscular motion, and after him Bellini.

As far back also as the time of the great Bacon, Gilbert had struck out a new path in the same direction, the following out of which has led to more satisfactory results than any of those arrived at by the iatro-mathematical school in their own particular lines of inquiry.

He had investigated the phenomena of magnetism with much success, and by continually poring over this subject had come to believe that magnetism supplied the key to vital movement, and to vital and physical problems in general; but his speculations bore little or no fruit, and are chiefly of interest as being the first step in an inquiry which did not really begin until two hundred years later, when an event occurred in a house in Bologna which marks the birth of a new epoch in the philosophy of vital motion, and on which it may be well to insist for a moment or two. The house is in the Via Ugo Bassi, gia Strada Felice. The event is commemorated on a marble slab over the doorway in these words:—

LUIGI GALVANI
in questa casa
di sua temporaria dimora
al primi di settembre
dell' anno MDCCLXXXVI
scoperse dalle morte rane
LA ELETTRICITA ANIMALE
Fonte di maraviglie
a tutti secoli.

The actual event was this. Experimenting with an ordinary electrical machine at no great distance from a plate on which lay a number of frogs' legs prepared for cooking, and noticing that these legs jumped whenever he drew a spark from the prime conductor, it occurred to Galvani that the parts which had been intended simply as a dish for dinner might be made to do good service as electroscopes in some experiments on atmospheric electricity in which he was then engaged. Thereupon, he and his nephew Camillo Galvani, who had been turning the machine, ascended to a belvedere which served the purpose of an electrical observatory, and at once proceeded to put the idea in practice. It was expected that the limbs which had jumped in obedience to discharges of franklinic electricity might also jump in obedience to discharges of atmospheric electricity; and in order to see whether they would do so or not, they were suspended, by means of small hooks of iron wire, upon certain iron bars or stays which stretched horizontally across the upper part of the arched openings with which three sides of the belvedere were pierced. The time was a calm and cloudless evening in which there seemed to be little chance of meeting with any of the latter discharges; and yet the limbs were found to jump whenever the iron hooks by which they were suspended were pressed upon by the finger, and not unfrequently when they were let alone. Describing what happened, 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."* The house, the ricketty wooden flight of steps leading from the principal staircase to the belvedere, unmended, unpainted, almost unswept, the belvedere itself, the iron bars upon which the limbs were suspended, are still there, or were there the other day when I made a pilgrimage to the spot; and even the presence of Galvani himself may be recalled by the help of a portrait which hangs in the open

* "De Viribus Electricitatis in motu musculari Commentarius," 1791.

landing upon the wall facing the locked entrance to the stairs leading to the belvedere. In this place, and in this way, was the discovery made which is commemorated on the slab in the front of the house as the well-head of wonders for all ages, "fonte di maraviglie a tutti secoli," and of which, 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 Brandès, ils reconnaîtront que la physiologie doit à Galvani et à Harvey ses deux bases principales."* At this time, then, and in this place, Galvani saw the contractions he describes, and discovered, or rather divined, in them the existence of animal electricity. How, he asked himself, were these contractions to be accounted for? They could not be due to discharges of atmospheric electricity, for the sky at the time presented no indications of electric disturbance: they could not be due to the discharges which gave rise to them within the house, for the electric machine, which remained behind, was not then in action: they could not be due, that is to say, to discharges of either of the two kinds of electricity then known; and having arrived at this point, he jumped from it to the conclusion, that the limbs themselves must have an electricity of their own, and that the contractions were brought about by discharges of this electricity. It never occurred to him to doubt that electricity was the agent at work in causing these contractions: and, in short, he did not hesitate to conclude, not only that the contractions were in themselves abundant proof of the existence of animal electricity,

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

but also that the muscular fibres are charged during rest as Leyden jars are charged, and that muscular contraction is the sign and effect of the discharge of this charge, the discharge, in one way or another, being brought about by an electrical action of the nerves upon the muscles.

From this time until the day of his death, Galvani went on performing experiment after experiment, sacrificing hecatombs of frogs, and never wavering in his belief in the existence of animal electricity, or in the conclusion he had come to respecting the action of this electricity in vital motion: but during his lifetime he was destined to be foiled in his hopes to bring others to the same mind with himself, 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 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,

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

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 loins, would contract at the moment when the end of the nerve below the line of section 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 hetereogeneous 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 frog 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.*

* "Ann. de Chim.," T. xxiii, pp. 276 and 301.

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 regarded as thoroughly satisfactory at the present day. 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 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 franklinic electricity, but which would altogether prevent that of voltaic electricity—would allow, that is to say, 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 under ordinary circumstances.

In 1803, Aldini, Galvani's nephew,† published an account of certain experiments which furnish further

* Op. cit.

† "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.

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 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 Professor Du Bois-Reymond and others, by Sir William Thomson 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

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

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 frog, and in this he erred ; but he did, nevertheless, a great thing, for, by this experiment, he furnished 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 Matteucci has demonstrated in the most unequivocal

* "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.

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 will always give Matteucci a place in the very foremost rank of physiological discoverers—that muscular 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 most conclusively 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 muscle in man, rabbits, guinea-pigs and mice, in pigeons and sparrows, in tortoises, lizards, adders, toads, tadpoles, and salamanders, in tench, in freshwater crabs, in glow-worms, in earthworms—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

* "Cours d'Electro-Physiologie." Paris. 1858.

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

during muscular contraction, during nervous action, under the influence of continuous and interrupted voltaic currents, and so on, which changes, 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 nearly so, and the attention diverted from the static to the current phenomena of electricity. And herein, as I believe, was an unmixed misfortune. In making out the electrical history of living creatures there is work to be done which, as will be seen in due time, can only be done with the electrometer; and, for my own part, I am disposed to assign to the new quadrant electrometer of Sir William Thomson a position in these investigations which is every whit as important as that which can be assigned to the galvanometer, and to think that the apparatus of any physiological laboratory would, to say the least, be far from complete in which this instrument was wanting.

And thus, by the fact of the existence of animal electricity being now established beyond question, the way is more prepared than it was in the days of Galvani for the adoption of any view of vital motion in which animal electricity has to serve as the basis.

There are also others who must be named as taking what is substantially the same view as that taken by Galvani, and who have a just claim to be commemorated in these introductory remarks, about whose views I would say what I would also say about the views of Galvani, that I was in complete ignorance of them for long after the time when my own thoughts on the subject had taken definite shape.

The name to be mentioned first in order here is that of the late Dr. West, of Alford, in Lincolnshire. As early as 1832,* in some remarks upon the influence of the nerves upon muscular contractility, this writer maintains, "that the nervous influence which is present in relaxed muscular fibre is the only influence which the nerves of volition possess over that tissue; that its office there is to restrain or control the tendency to contract which is inherent in the muscle; and that contraction can only take place when by an act of the will this influence is suspended, the muscle being then left to act according to its own innate properties;" . . . and again, "that nervous influence is imparted to muscular fibre for the purpose of restraining its contraction, and that the action of the will, and of all other disposers to contraction, is simply to withdraw for a while this influence, so as to allow the peculiar property of muscular fibre to show itself." The co-existence of spasmodic action with nervous debility, the efficacy of stimulants as antispasmodics, and the postponement of rigor mortis until all traces of nervous action have disappeared, are the principal facts which are advanced in support of the probability of this theory.

A similar idea appears to have been also hinted at by Sir Charles Bell in a lecture at the Royal College of

* "On the Influence of the Nerves over Muscular Contractility," "London Medical and Surgical Journal," edited by Michael Ryan, M.D. Vol. i. 1832.

Surgeons of England, for, after premising that the question could never be settled, the lecturer said, "that *relaxation* might be the act, and not contraction, and that physiologists, in studying the subject, had too much neglected the consideration of the mode by which relaxation is effected." This remark is preserved by Dr. West in the essay to which reference has just been made.

Six years later, in a chapter of his classical work on comparative anatomy,* Professor Dugès, of Montpellier, argues with much clearness that all organic tissues are the seat of two opposite movements—expansion and contraction—and that "la contraction musculaire ne consiste que dans l'annihilation de l'expansion." The muscle is supposed to contract in virtue of its elasticity, just as a piece of caoutchouc might contract when set free from a previous state of extension; and an analogy is hinted at between the expanded state of the muscle and the fluid state of the fibrine of the blood, and between rigor mortis and the coagulated state of this fibrine. Analogous in its effects to electricity, the vital agent is supposed to accumulate in the muscles, and to produce expansion by causing the muscular molecules to repel each other; and contraction is supposed to be brought about either by the sudden discharge (as in ordinary contraction) or by the gradual dying out (as in rigor mortis) of the vital agent. And, further, it is supposed that the rhythmical movements of muscle are caused by successive discharges of the vital agent, which discharges are brought about whenever this agent acquires a certain degree of tension; and that the cramps of cholera, or the spasms of tetanus or hysteria, are con-

* "Traité de Physiologie comparée de l'Homme et des Animaux," 8vo. Montpellier and Paris. 1838.

sequent upon the development of the vital agent being for the time suspended.

More recently still, namely in 1847, Professor Matteucci communicated a paper to the Parisian Academy of Sciences* upon the influence of the nervous *fluid* in muscular action, in which he writes:—" Ce fluide développé principalement dans les muscles, s'y répand, et, doué d'une force répulsive entre ses parties, comme le fluide électrique, il tient les éléments de la fibre musculaire dans un état de répulsion analogue à celui présenté par les corps électrisés. Quand ce fluide nerveux cesse d'être libre dans le muscle, les éléments de la fibre musculaire s'attirent entre eux, comme on le voit arriver dans la roideur cadavérique. . . . Suivant la quantité de ce fluide qui cesse d'être libre dans le muscle, la contraction est plus ou moins forte." Professor Matteucci appears to have framed this hypothesis, partly, in consequence of certain considerations which seemed to show that the phenomenon of "induced contraction" was owing to the *discharge* of electricity in the muscle in which the "inducing contraction" was manifested—an idea originating with M. Becquerel—and, partly, in consequence of the analogy which he himself had found to exist between the law of contraction in muscle and the law of the discharge in electrical fishes; but he does not appear to have attached much importance to the hypothesis. Indeed, his own comment at the time is—"j'ai presque honte d'avoir eu la hardiesse de communiquer à l'Académie des idées si vagues, et apparemment si peu fondées, et contre lesquelles on pourrait faire bien des objections, mais je pense que, parmi les théories physiques les mieux fondées aujourd'hui, il en existe qui ont débuté de cette manière, et il est certain que des

* "Comptes Rendus." March 17, 1847.

hypothèses, aussi peu fondées que celles-ci, ont quelquefois pu produire ensuite des découvertes remarquables."

Next in order, and almost contemporaneously with the date of my own first publication on the subject, Professor Engel, of Vienna, wrote :*—" So hat der Nerve die Aufgabe, nicht die Zusammenziehungen des Muskels zu veranlassen, sondern den Zusammenziehungen bis auf einen geringen Grad entgegenzuwirken. Im lebenden Organismus, in welchem Ruhe etwas unmögliches ist, ist auch ein ruhender Muskel eben so wohl wie ein ruhender Nerv undenkbar, der Muskel in seinem beständigen Streben, sich zusammenzuziehen, wird vom Nerven daran verhindert, im Nerven macht sich das fortwährende Streben kund, die Zusammenziehung des Muskels auf ein gerechtes Mass zurückzuführen ; das Ergebniss dieser zwei einander entgegengesetzten Eigenschaften des Nervens und des Muskels ist das, was man gemeinhin Zustand der Ruhe, Zustand des Gleichgewichtes, oder an Muskeln auch Tonicität nennt. Das Verlassen dieses Gleichgewichts ist die Bewegung einerseits, die Lähmung andererseits. Die Bewegung wird aber erzeugt, indem entweder der Einfluss des Nervens auf den Muskel herabgesetzt wird, oder indem die Contractionskraft des Muskels unmittelbar gesteigert wird. Lähmung des Muskels findet sich gleichfalls entweder durch unmittelbare Vernichtung der Contractionskraft des Muskels oder durch eine übermässig gesteigerte Einwirkung des motorischen Nervens auf den Muskel. Sollen daher abwechselnde Muskelcontractionen zu Stande kommen, so ist die Gegenwart des lebendigen Nervens im Muskel unerlässlich, und auch

* "Ueber Muskelreizbarkeit," "Zeitschrift der Kais. Kön. Gesellsch. der Aertze zu Wien," Erster Band, pp. 205–219, and pp. 252–270. 1849.

bei unmittelbaren Muskelreizen können abwechselnde Zusammenziehungen nur erfolgen, so lange noch die Nerven lebensfähig sind; hört letzteres auf, so zieht sich der Muskel ohne Hinderniss zusammen. Diesen Zustand nennen wir die Todtenstarre." The chief grounds for this opinion are, first, certain original experiments, some of them very remarkable, which afford additional proof that the muscles of frogs are more prone to contract when they are cut off from the influence of the great nervous centres; secondly, the frequent spontaneous occurrence of cramps and other forms of excessive spasmodic contraction in paralysed parts; and, thirdly, the supervention of the permanent contraction of rigor mortis when all signs of nervous irritability are completely extinguished.

And, last of all, I find Professor Stannius, of Rostock,* arriving at the conclusion:—" dass es eine wesentliche Aufgabe der sogenannten motorischen oder Muskelnerven sei, die natürliche Elasticitätsgrösse der Muskelfasern herabzusetzen und ihre Elasticität vollkommener zu machen; dass anscheinende Ruhe des Muskels, zum Beispiele, während des Schlafes, das Stadium solchen regen, den Muskel zu seinen Aufgaben wieder befähigenden Nerveneinflusses anzeige; dass active Muskelzusammenziehung einen geregelten und begrenzten momentanen Nachlass des Nerveneinflusses auf den Muskel bezeichne; dass endlich die Nachweisung einer Muskelreizbarkeit, in der üblichen Auffassungsweise, ein durchaus vergebliches Bemühen sei." M. Stannius was led to this conclusion by certain original experiments, in which he found blood to have the power of relaxing

* "Untersuchungen über Leistungsfähigkeit der Muskeln und Todtenstarre," "Vierordt's Archiv für Physiol. Heilkunde." Stuttgart, 1 Heft, p. 22, 1852.

rigor mortis and restoring muscular irritability, and these experiments are advanced in evidence. Reference is also made to arguments to be brought forward on another occasion, which will prove—"dass diese Anschauungsweise, so paradox sie immer auf den ersten Anblick sich anlassen mag, mit unserem thatsächlichen Wissen über Nerven- und Muskelthätigkeit keineswegs im Widerspruch steht." The essay from which these quotations are taken was published towards the end of 1852—about two years after the date of my own first publication on the subject.

I do not stand alone, then, in thinking that a great change is necessary in the theory of *vital motion*—a change amounting to no less than a complete revolution; and I am glad that it is so, for, thus supported, I have more courage than I otherwise should have to prosecute the inquiry upon which, without further preamble, I now venture to enter, and about which I have only to add that the plan pursued in it will be that marked out in the table of contents—a plan according to which the problem of vital motion will be regarded, first, from a physiological, and then, from a pathological, point of view.

THE ARGUMENT.

I.

VITAL MOTION REGARDED PHYSIOLOGICALLY.

CHAPTER I.

ON THE ELECTROPHYSICS OF AMŒBOID MOVEMENT.

I.

IN the course of the summer before last, while at work, or rather at play, with a Thomson's new-quadrant-electrometer, it occurred to me that this very sensitive instrument might be the means of enabling me to arrive at some definite conclusions respecting the electrical condition of living protoplasm. I had by me a stale infusion of hay, in which were very many amœbæ, and with this I at once made a beginning: afterwards, as opportunity offered, I proceeded to examine pus, and mucus, and fresh-water sponge, and myxomycetes. I made this selection because I knew that the amœbæ in the infusion of hay, and the amœboid bodies in the pus and mucus, and the sarcode of the sponge, and the jelly-like fungus (so long at least as it remained in the nascent condition), were all of them little, if anything, else than so many forms of unmodified protoplasm, and because I could think of no other way by which I could arrive at the end I had in view so readily. Again and again I examined each of these substances in the dead as well as in the living state; and, concurrently (in order that I might be able to compare the electrical condition of protoplasmic bodies with that of substances which never had any claim to vital endowment)

I also made similar examinations of sculptors' clay, and distilled water. The inquiry itself was simple enough; and for its successful prosecution all that was needed was a little patience.

In each experiment the same mode of proceedure was adopted. The electrometer was set in the usual way, that is, with one pair of quadrants remaining in the state of insulation, and with the other pair "put to earth;" the object under examination was insulated by the glass of the bottle holding it, and by a tolerably thick plate of paraffin upon which the bottle rested; and then all that had to be done was to touch this object here and there successively, first, at one point only, with the one electrode belonging to the pair of insulated quadrants, and then at two points, somewhat apart, with both electrodes together. What I proposed to do when the electrode belonging to the insulated pair of quadrants was used singly, and when, consequently, nothing was done to break the insulation of the object under examination, was to ascertain whether this object was, or was not, in the same potential condition as the earth—whether it was in the state of zero or not. What I proposed to do when the two electrodes were used together, and when (because it was touched by the electrode belonging to the pair of uninsulated quadrants) the object that was being tested was no longer insulated, was to try and find out whether there were, or were not, in this object any potential differences akin to those which are known to exist in muscle and nerve. And this is all that I need say respecting the method of procedure except this—that in the case where the electrodes were used together, each of the two points touched was tested in turn by bringing it under the electrode belonging to the insulated pair of quadrants, and that the key used in making and breaking and changing the necessary connections with the electrometer was the ordinary four-part-plug-key.

Many experiments were made in this way, and all of them with a result which is fairly exemplified in the two or three which I take, almost at haphazard, as examples of the rest.

EXP. I.—*On infusion of hay containing amœbæ, and also on pus and mucus and distilled water.*

The particular results arrived at in this experiment are thus tabulated in my note-book.

	With one electrode singly.				With two electrodes together.			
	Infusion containing amœbæ.	Pus.	Mucus.	Distilled water.	Infusion containing amœbæ.	Pus.	Mucus.	Distilled water.
1874. Dec. 5.								
11·20 a.m. ...	+13	+13	+13	+13	0	0	0	0
1·25 p.m. ...	+10	+10	+10	+10	0	0	0	0
2 ,, ...	+ 9	+ 9	+ 9	+ 9	0	0	0	0
10·30 ,, ...	+11	+11	+10	+10	0	0	0	0
6.								
10·50 a.m. ...	+10	+10	+10	+10	0	0	0	0
5 p.m. ...	+ 7	+ 7	+ 7	+ 7	0	0	0	0
10·10 ,, ...	+ 9	+ 9	+ 9	+ 9	0	0	0	0
11·5 ,, ...	+ 6	+ 6	+ 6	+ 6	0	0	0	0
11·15 ,, ..	+ 5	+ 5	+ 5	+ 5	0	0	0	0
7.								
10·30 a.m. ...	+ 9	+ 9	+ 9	+ 9	0	0	0	0
1·30 p.m. ...	+ 5	+ 5	+ 5	+ 5	0	0	0	0
6·25 ,, ...	+ 4	+ 4	+ 4	+ 4	0	0	0	0
7·5 ,, ...	+ 7	+ 7	+ 7	+ 7	0	0	0	0
10·20 ,, ...	+ 8	+ 8	+ 8	+ 8	0	0	0	0
10·45 ,, ...	+ 9	+ 9	+ 9	+ 9	0	0	0	0
11·15 ,, ...	+ 7	+ 7	+ 7	+ 7	0	0	0	0

	With one electrode singly.				With two electrodes together.			
	Infusion containing amœbæ.	Pus.	Mucus.	Distilled water.	Infusion containing amœbæ.	Pus.	Mucus.	Distilled water.
1874. Dec. 8.								
9·30 a.m. ...	+ 7	+ 7	+ 7	+ 7	o	o	o	o
6·30 p.m. ...	+ 9	+ 9	+ 9	+ 9	o	o	o	o
7·45 ,, ...	+ 8	+ 8	+ 8	+ 8	o	o	o	o
10 ,, ...	+ 5	+ 5	+ 5	+ 5	o	o	o	o
11 ,, ...	+ 5	+ 5	+ 5	+ 5	o	o	o	o
9.								
8·15 a.m. ...	+ 5	+ 5	+ 5	+ 5	o	o	o	o
9 ,, ...	+ 4	+ 4	+ 4	+ 4	o	o	o	o
8 p.m. ...	+ 8	+ 8	+ 8	+ 8	o	o	o	o
9·20 ,, ...	+ 5	+ 5	+ 5	+ 5	o	o	o	o
10.								
8·20 a.m. ...	+ 6	+ 6	+ 6	+ 6	o	o	o	o
9·55 ,, ...	+ 4	+ 4	+ 4	+ 4	o	o	o	o
8 p.m. ...	+ 7	+ 7	+ 7	+ 7	o	o	o	o

The experiment, it will be seen, extended over several days. It was, in fact, carried on long enough for the pus and mucus, which were at first as fresh as possible, to become altogether stale and offensive—to change from the state of life into that of death; and yet there is nothing to indicate the occurrence of a change so momentous. On the 5th of the month, when the experiment was commenced, the electrometer tells one and the same story for the infusion containing the amœbæ, for the pus and mucus, and for the distilled water, and the story then told agrees in every particular with that told on the 15th day, when the observations were discontinued. There is nothing to show that what had been once living had become dead. Indeed, all that is noticeable is that the fluids containing protoplasm are all along in precisely the same case as the distilled water. When the examination is made with the electrode belonging to the insulated pair of quadrants only, it is seen that each of these substances, at the same time, is to the same degree positive in relation to the zero of the earth, and that in all of them the potential is subject to precisely the same fluctuations. When, on the other hand, the examination is made with the two electrodes together (and when consequently the insulation is not preserved as it was in the last case), the constant result at all times is seen to be a settling down of all signs of potential to the zero of the earth—a result which could not happen if there were in the object under examination any inherent differences of potential akin to those which are met with in muscle and nerve. This, as it would seem, is the necessary inference from the facts in this particular case, and this only; and, most certainly, no other inference is to be drawn from several cases of like nature of which I have the notes.

EXP. 2.—*On fresh-water sponge, myxomycetes and sculptors' clay.*

When first examined beautiful amœboid movements were continually taking place in the hyaline portion of the myxomycetes, and, to a lesser degree, in certain fragments of the sarcode of the sponge; later on, all these movements had come to an end, and other changes had happened which of themselves were sufficient to show that the last traces of life had died out in the fungus no less than in the foraminifera. The case, that is to say, resembles that of which I have just been speaking, in that the protoplasmic substances, which at first were full of life, were dead enough before the end of the experiment; and the resemblance also extends to the result, for, as will be seen in the accompanying table, the electrometer still fails to mark any distinction between the living and the dead state of the protoplasmic substances, or between these substances, living or dead indifferently, and the sculptors' clay. And with these remarks I leave the table to speak for itself, only adding—for the sake of those who may care to repeat the experiment—that the fresh-water sponge was found in the bottom of a half-dried-up timber pond in the New Commercial Docks at Rotherhithe, and the myxomycetes in a heap of old tan in a tan-yard on the river bank at Kingston-on-Thames.

	With one electrode singly.			With two electrodes together.		
	Fresh-water sponge.	Myxomycetes.	Sculptors' clay.	Fresh-water sponge.	Myxomycetes.	Sculptors' clay.
1873. Sep. 16.						
10 p.m. ...	+ 6	+ 6	+ 6	0	0	0
11·30 " ...	+ 5	+ 5	+ 5	0	0	0
17.						
9 a.m. ...	+ 6	+ 6	+ 6	0	0	0
10 " ...	+ 7	+ 7	+ 7	0	0	0
4 p.m. ...	+ 5	+ 5	+ 5	0	0	0
10·30 " ...	+ 7	+ 7	+ 7	0	0	0
18.						
8·30 a.m. ...	+ 6	+ 6	+ 6	0	0	0
5 p.m. ...	+ 4	+ 4	+ 4	0	0	0
9 " ...	+ 7	+ 7	+ 7	0	0	0
26. ...						
9 a.m. ...	+ 7	+ 7	+ 7	0	0	0
4 p.m. ...	+ 5	+ 5	+ 5	0	0	0
9·30 "	+ 8	+ 8	+ 8	0	0	0

EXP. 3.—*On fresh-water sponge, myxomycetes, and distilled water.*

This experiment is exceptional in that the potential of which there is evidence when use is made of one electrode singly is sometimes positive and sometimes negative. As a rule the potential is positive, but now and then, as in this case, it is negative. This is the simple fact. What may be the meaning of this fact is a question for future enquiry ; now I would only direct attention to the fact, as a fact, and say that several claps of thunder were heard not long before the time when the potential had a negative sign, and when consequently the ordinary electrical relations of the earth to the object under examination had been reversed. The actual experiment is thus tabulated in my notebook.

	With one electrode singly.			With two electrodes together.		
	Fresh-water sponge.	Myxo-mycetes.	Distilled water.	Fresh-water sponge.	Myxo-mycetes.	Distilled water.
1873. Aug. 26.						
8·30 a.m. ...	+ 7	+ 7	+ 7	0	0	0
5· p.m. ...	+ 5	+ 5	+ 5	0	0	0
10·30 ,, ...	+ 8	+ 8	+ 8	0	0	0
27.						
8 a.m. ...	+ 6	+ 6	+ 6	0	0	0
5·30 p.m. ...	+ 4	+ 4	+ 4	0	0	0
10 ,, ...	+ 7	+ 7	+ 7	0	0	0
28.						
8·15 a.m. ...	+ 6	+ 6	+ 6	0	0	0
4 p.m. ...	+ 4	+ 4	+ 4	0	0	0
10 ,, ...	− 2	− 2	− 2	0	0	0
10·15 ,, ...	− 1	− 1	− 1	0	0	0
11 ,, ...	+ 5	+ 5	+ 5	0	0	0
29.						
8 a.m. ...	+ 7	+ 7	+ 7	0	0	0
5 ,, ...	+ 5	+ 5	+ 5	0	0	0
10·30 ,, ...	+ 7	+ 7	+ 7	0	0	0

II.

1. With these facts to indicate the direction in which the eyes ought to be turned there is no great difficulty in obtaining a tolerably clear insight into the electro-physics of the vital movements belonging to proto-plasmic substances. Indeed, all that is necessary is to take the facts as so many reasons for believing that there is one and the same electrical condition in living proto-plasmic substances and in lifeless bodies generally, to be at the trouble to learn what this common electrical condition really is, and to apply the knowledge so gained.

2. In dealing with these matters it is, as it seems to me, a point of primary importance to avoid the mistake of supposing that the electrical condition of the earth is a state of zero, and of speaking of atmospheric electricity as if it were something separate from terrestrial electricity. The evidence, as Sir William Thomson has pointed out, goes to show that, under ordinary circumstances, the surface of the earth is in the very same predicament electrically as the thin vacuum-like medium which forms the outskirts of the atmosphere, that is, in the state of charge, with this difference only, that the two charges have opposite signs —that, in fact, the case is precisely as if a positive charge, acting across the dielectric air from without, had induced a negative charge on the surface of the earth, and in so doing had left the atmosphere itself charged to a certain depth by the inducing charge on the outside and by the induced charge on the inside. And, without question, the facts lead naturally enough to this conclusion. The atmosphere is certainly a dielectric—the best of all dielectrics by the way—and, as certainly, up to a certain height at least, the potential of the atmosphere becomes more and more positive as the distance from

the earth increases. Thus, if any one will take a Peltier's electrometer to a place which is more elevated than any other place in the neighbourhood, and keep his eye upon the needle, he may, by simply placing the instrument at different levels in the atmosphere, soon satisfy himself that the air at the higher level is positive in relation to air at a lower level, and most positive of all at the highest level within reach; and, at the same time, he may, if he will, by simply reflecting upon the working of the instrument, find a very cogent reason for believing that induction has a very important part to play in determining the electric relation of the earth and air. And so, likewise, at still higher levels. "I have made," says Mr. Sturgeon, "upwards of five hundred kite experiments, under almost every circumstance of weather, at various times of the day and night, in every season of the year, and in very many different places, and in every place I have found the atmosphere positive in relation to the ground. I have floated, for example, three kites at the same time at very different altitudes, and have uniformly found the highest positive to the other two, and the centre kite positive to that which was below it, and, consequently, the lowest negative to the two above it, but still positive to the ground on which I was standing."

Nor is it otherwise at still greater heights. Thus, in the case, where he let down a long wire from the car of his balloon, Gay Lussac found that the stratum of air in which he then happened to be, was, however high, always positive in relation to that into which the lower end of the wire dipped down.

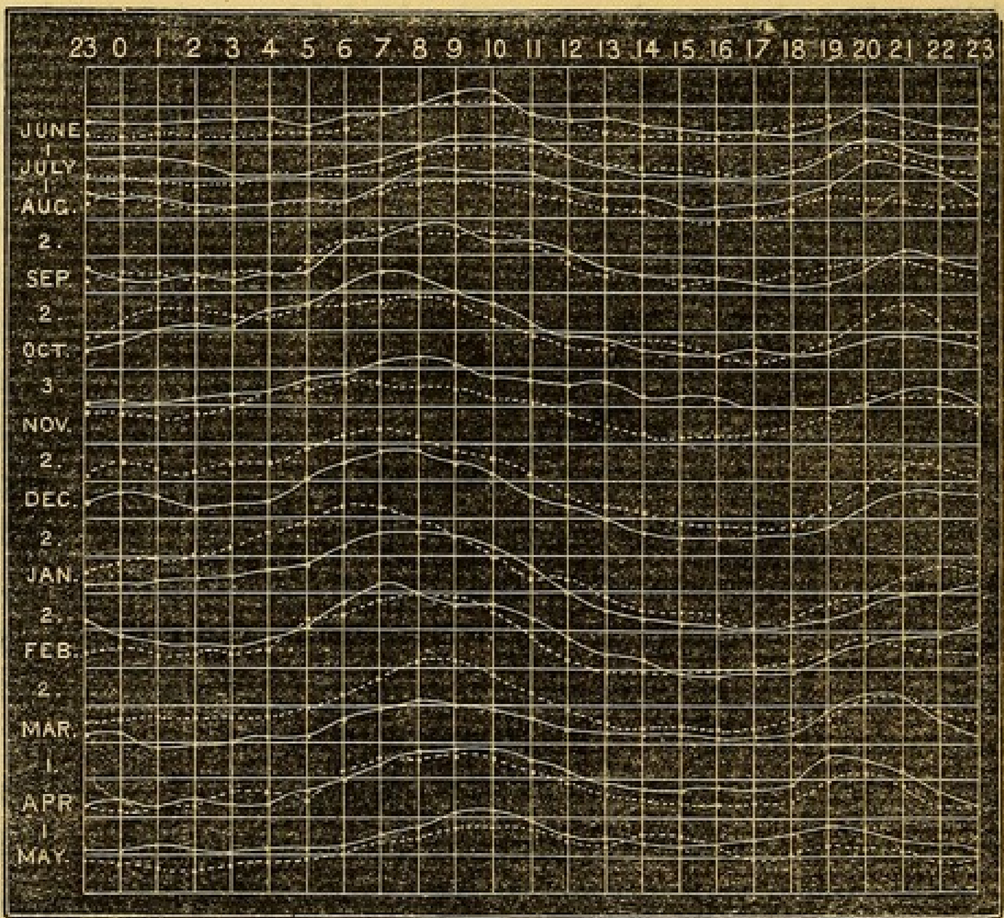
Whether the same rule holds good for still greater heights remains to be proved, and can only be proved by properly conducted balloon experiments; and all that can be said now is, that within reach of the earth the potential of the atmosphere becomes more energetically positive as the distance from the earth increases. Indeed, there is good reason to agree with Sir William Thomson in believing that the layers of extremely thin air in the atmospheric outskirts must agree with the earth in playing the part of a good conductor, for the behaviour of such thin air in respect of conduction must be supposed to be the same as that of the thin air which is left in an exhausted receiver; and if so, then a way opens out by which it is possible to come round to a very definite conclusion in these matters. For what is the case under consideration? It is that the atmosphere is interposed as a dielectric between two conducting surfaces, of which one is formed by the earth, and the other by the thin extra-aërial medium of which I have just been speaking. It is that this latter medium is charged positively. It is that this positive charge, acting through the air as a dielectric, may *induce* a negative charge on the surface of the earth. The case, indeed, is one in which it is necessary to suppose that the electrical condition of the earth is one, not of zero, but of charge, and that the inducing and induced charges must be equally potent.

All this must follow if the atmosphere be a dielectric, and if its outer surface be charged positively in the way in which it seems to be charged; for under these circumstances induction must operate so as to charge the surface of the earth to an equal degree negatively. And this, too, is the conclusion which must be drawn from the experiment in which Peltier's electrometer is made use of, or from that in which Gay Lussac let down a long wire from a balloon, for these make it evident that the electrical condition of the surface of the earth is not the zero which it appears to be, but what it actually is, that is, relatively negative. At the surface of the earth, indeed, what is called zero, is really a state of equilibrium. There is—owing to the comparative freedom of conduction in all directions along the earth—no potential differentiation, and, therefore, no electrical manifestation: that is all. Viewed from the earth, the far off heights of the atmosphere appear to be highly charged positively, but this would not be the view taken of the state of things there by an observer who had his station in these heights, unless he took care to insulate himself, and to exercise his reason freely. He, in point of fact, would be in precisely the same case as the observer on the surface of the earth; and unless he did what I have said, he would look upon the electrical condition of his own station as that of zero, and upon that of the distant surface of the earth as highly charged negatively. And, in short, there is every reason for believing that the electrical condition of the earth is one, not of zero, but of charge,—that this charge, under ordinary circumstances, is not positive, but negative in its character,—and that the full measure of this negative charge is none other than that of the positive charge of which there is evidence in the atmosphere.

3. The investigations of the last few years have also brought to light certain fluctuations and oscillations in atmospheric potential which are full of significance in the present inquiry.

Every day there are two maxima and two minima of potential, one maximum between 8 A.M. and 11 A.M., the other between 7 P.M. and 11 P.M., one minimum between 3 P.M. and 7 P.M., the other between 11 P.M. and 3 A.M. The case, indeed, is one which may be made plain enough by the help of the two following figures (Fig. 1 and Fig. 2), with a word or two in explanation. The figures are borrowed from a paper,* in which Dr. Everett, of Windsor, in Nova Scotia, records the results of the observations in atmospheric electricity which were begun in the Royal Observatory at Kew, under the direction of Mr. Balfour Stewart, and carried on in America afterwards under his own eye. The first figure (Fig. 1) gives the diurnal changes in

FIG. 1.

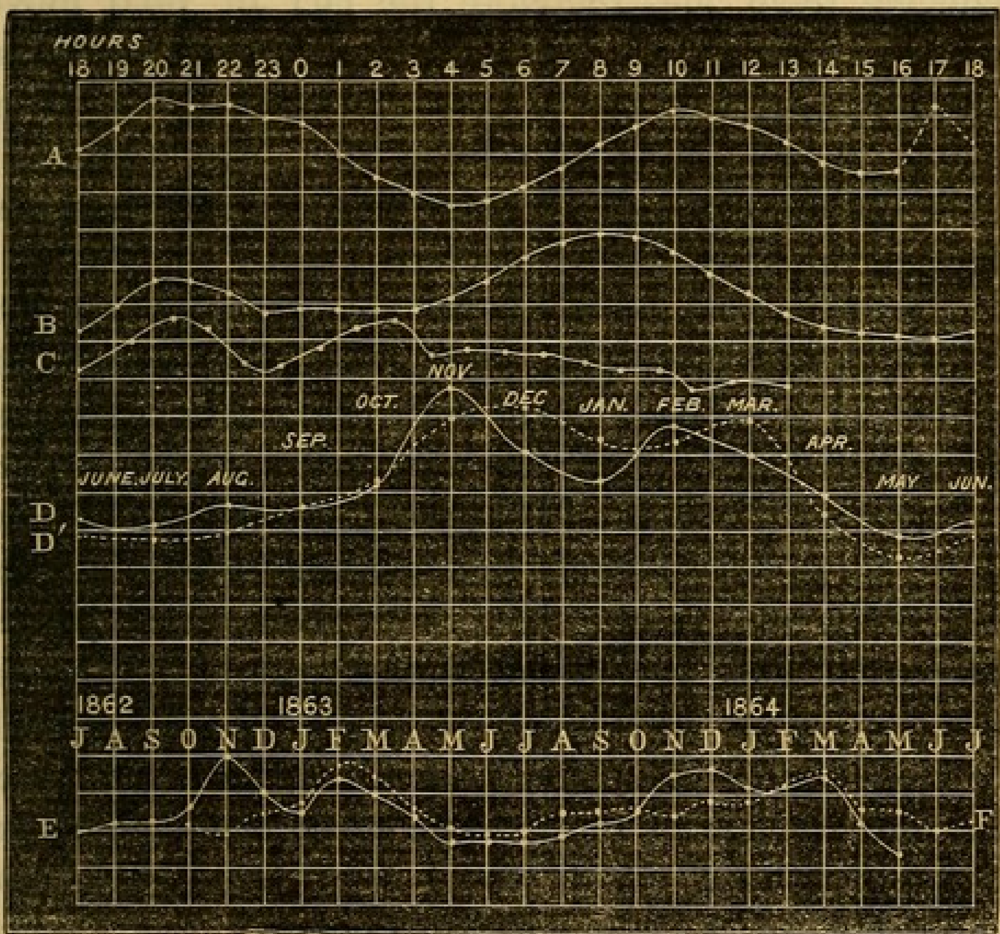


atmospheric electricity noticed at Kew hour by hour, and month by month, from June, 1862, to May, 1864, inclusive; the vertical lines figured at the top from 0, or midnight, to 23 (after the astronomical mode of reckoning), marking the time in hours, the horizontal continuous, and dotted curves, to which the names of the

* "Transactions of the Royal Society of London," 1868.

months are prefixed, doing the same office for the months, the former curves for the months of the first, and the latter for those of the second year. The second figure (Fig. 2) gives in its different curves, lettered from A on to F, a more comprehensive view of the facts set forth in this first figure, and brings into prominence other important facts as well. A gives the mean of all

FIG. 2.

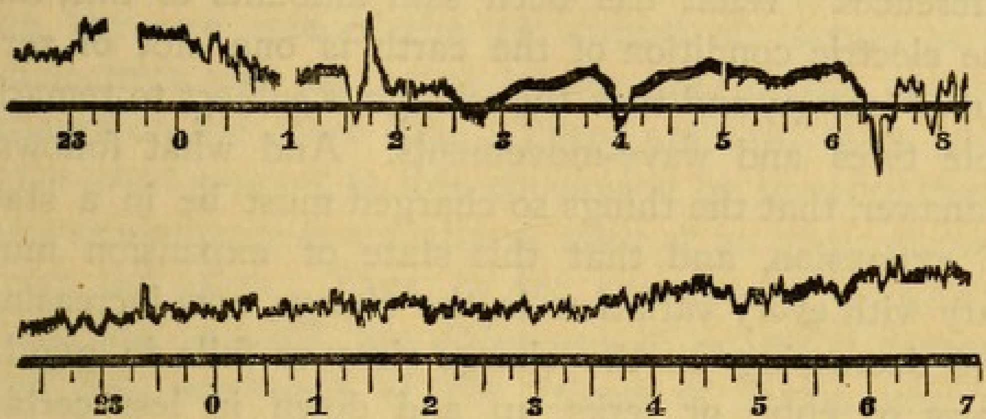


the diurnal changes in potential noticed at Kew, and recorded in the first figure; B is the mean diurnal barometric curve at Hallé, as worked out by Dr. Everett, from data in Kaemtz' "Meteorology;" C is the mean diurnal curve in potential belonging to the observations made at Windsor, in Nova Scotia; D, D′, and F are the mean annual curves in potential at Kew for the two years over which the observations extended, D belonging to the first year, D′ to the second, and F to the two years together; while the remaining curve, E, gives the mean annual electrical curve at Windsor, in Nova Scotia.

The electrical observations to which these curves relate were registered photographically by instruments with automatic action, and great care was taken in reducing them, and therefore the fact of tidal movements in atmospheric electricity is put beyond question. Upon this point there is evidence which can have no double meaning, and also upon this—that there is a close correspondence between these movements and those recorded by the barometer.

These curves, however, do not represent all that must be noticed in these matters. They have to do with tides in potential, but not with waves, which, as will appear in due time, may prove to be of no secondary importance. In point of fact, the indications of electricity in the atmosphere are continually varying, and even in serene weather these variations are very considerable. This fact is satisfactorily established by the observations of Sir William Thomson, with his portable electrometer, in the Island of Arran, and elsewhere; and further evidence to the same effect, even more conclusive than that which would have been gained by witnessing the experiments with the portable electrometer, may be found in the following figure (Fig. 3), which figure is an actual copy of a portion of the photographic record of two days' working (the same paper is, for the sake of economy, generally made to bear the record for *two* days) of Sir William Thomson's self-registering electrometer at Kew, under the direction of Mr. Balfour Stewart. The prepared paper on which these records were photographed was made to move vertically at an uniform rate, by means of clock-work, while a spot of light (the image of a gas flame reflected from a mirror fixed to the needle of the divided-

FIG. 3.



ring electrometer) moved horizontally across it, this way or that, in obedience to the continually varying potential of the atmosphere; while at the same time a datum line, showing the position the spot of light would take if it remained at its mean value, was produced by a beam from the same source of light, reflected from another mirror fixed to the case of the electrometer. Thus, the distance of the zigzag line from the straight datum line, on one side or the other, records for each instant of time (the numbers below the lines representing, as in the former figure, the hours from 0, or midnight, up to 23) the electrical potential, + or −, of the atmosphere at the point where the stream of water broke away from the nozzle of the water-dropping collector used in the experiment.

And if this be so with respect to the potential of the atmosphere, it must be so also with respect to the potential of the various bodies which together go to make up the surface of the earth. Indeed, after what has been said, it is manifestly wrong to speak of these fluctuations and oscillations in potential as belonging to the air in a sense in which they do not equally belong to the earth.

4. Here then is a definite state of things from which it is difficult not to draw certain very definite inferences. What has been said amounts to this, that the electric condition of the earth is one, not of zero, but of *charge*, and that this charge is subject to remarkable tides and wave-movements. And what follows? I answer, that the things so charged must be in a state of expansion, and that this state of expansion must vary with every variation in the charge, now increasing, now decreasing, as the charge rises or falls in regular flows or ebbs, or jerks up and down in less certain oscillations. The component molecules of a charged body are kept in a state of mutual repulsion by the presence of the charge, and to say this is only to say in other words that the body is expanded by the charge. In short, it is impossible to disassociate the idea of expansion from that of charge, or to think that variation in the amount of the charge will not be attended by corresponding variations in the amount of expansion.

Nor is it to be supposed that the expansion which may be thus brought about is altogether insignificant in amount. Thus, in the extremely thin, vacuum-like, out-lying portions of the atmosphere, the charge present, for anything that appears to the contrary, may be sufficient to keep up a very high degree of expansion in the materials charged—to keep up, it may be, that state of utter tenuity in which these out-lying portions of air are found to be. And if this be so, then it follows almost as a matter of course that the counter-charge at the surface of the earth may, in the same way, help to keep up that state of expansion without which there would be neither air nor water, nor anything except absolute adamantine solidity, the charge of electricity, in fact, having to do much of the work which is commonly ascribed to latent heat. All this is possible; nay, all this is not altogether improbable; for if induction play the part which it has been supposed to play, the charge on the one side of the air (which air serves as the dielectric) will be equal in amount to the counter-charge on the other side. There are also facts which give great support to this conclusion by showing that a powerful charge is actually at work at no great distance from the earth. Thus, in the experiment to which I have just referred, the difference of potential between the flat sea-beach of the island of Arran and the burning match nine feet above the portable electrometer, was equal to the potential of a Daniell's battery of from 200 to 400 elements, in fair weather, and of not less than from six to ten times this amount in foul and blustering weather. And further evidence to the same effect is supplied by the more recent investigations of Sir William Thomson with his water-dripping cistern, and with his divided ring electrometer; for these go to show that in ordinary weather the potential of the air (+) is equivalent to no less than 430 volts (the volt being from 5 to 10 per cent. less than the electromotive force of a Daniell's cell) at a level of nine feet above the earth, and that in exceptional weather, as when rain or hail was falling heavily, or when a strong wind was blowing from the east or north-east, it was equally high at not more than a foot above the earth. And what is this manifestation of potential in comparison with that which accompanies a storm of thunder and lightning, or which is present in such a case as that of the damp, dreary November fog, without thunder or lightning, described by Mr. Crosse—a case in which, (by means of 500 or 600 yards of insulated wire raised aloft, like telegraphic wires upon tall poles, fixed to the highest trees in the grounds at Broomfield,) as much electricity was collected as served to charge up to the point of discharge, twenty times in a minute, a Leyden battery consisting of 50 pairs, containing in all 73 square feet of coating, and requiring to charge it up to the point of discharge 230 vigorous turns of the wheel of a 20-inch cylindrical machine—a battery the discharge of which was attended by a report equal to that of a cannon! And thus, though it may be very difficult to form an exact estimate of the force of the charge with which various terrestrial bodies are charged, it is plain that it must be anything but trifling in amount, and that it may be sufficient to counterbalance, as has been suggested, the action of ordinary molecular attraction, or any other mode of attraction, to the extent of causing softness, liquifaction, or even aërifaction. And if so, then this too must follow, that the state of rarefaction or expansion thus kept up may be supposed to fluctuate perceptibly as the potential rises or falls, now on the side of rarefaction, now on that of condensation.

Indeed, when the subject is better known, it may appear that much of the work which is supposed to be done on the surface of the earth by heat is really done by electricity; or rather it may be that heat and electricity are inseparably correlated, and bound to act in very much the same way, what appears as electricity at one moment appearing as heat at another, and *vice versâ*. For, to mention only one among many other reasons for so thinking—a reason, too, to which attention has not yet been directed—it is surely a significant fact that the electricity and heat of the air are inversely related to each other in the summer and winter months of the year, the potential being lower and the temperature higher in the summer months, the temperature being lower and the potential higher in the winter months.

5. In what has just been said, as it seems to me, there may be the key to the electrophysics of vital motion as exhibited in simple protoplasm. It may be safely assumed that the degree of expansion consequent upon charge will not be the same in all bodies—that it will be greater in aëriform bodies than in bodies which are fluid like water, and greater in these latter than in those which are of the nature of solids. It may be safely assumed also that the expansion will operate unequally in bodies which are unequally made up of portions which are more or less solid, and of portions which are more or less fluid. It may be, indeed, that in these latter bodies the only *apparent* expansion and shrinking may be, not in the portions which are more or less solid, but in the portions which are more or less fluid. In the case of the amœbæ and amœboid bodies, for example, it may be in the hyaline portions, and not in the portions which have become more or less granular, both of which portions are distributed irregularly, that the apparently irregular expansions and shrinkings are manifested, and that herein is the key to amœboid movements so far as their electrophysics are concerned. And the facts bear out this conclusion with tolerable fidelity. In all cases, the movement would seem to be connected, not with the granular condition which becomes developed eventually in the protoplasmic matter, but with the hyaline condition which marks the very earliest phase of development. It is the least granular amœba, or pus corpuscle, or mucus-corpuscle, or cornea

corpuscle, or bit of sarcode, which is most likely to be the scene of amœboid movement; and only let the granular condition be decidedly developed in any of these bodies, and the chances are that there are no longer any such movement. It is with these bodies, indeed, as it is with the protoplasmic matter of the myxomycetes; for the simple fact is that amœboid movements are seen in this matter, not when it has become semi-solid and granular, but only while it retains its original cream-like consistence and appearance, and remains hyaline under the microscope. In a word, there is reason to believe that amœboid movements, so far at least as their electrophysics are concerned, may be nothing more than the simple consequence of the expansion and shrinking of the irregularly distributed more expansile hyaline portions of the amœboid bodies, in obedience to the oscillations in potential, of which there is written evidence in the photographic lines which are copied in the last figure (Fig. 3). And with this hint I take leave of the electrophysics of vital motion as exhibited in simple protoplasm.

CHAPTER II.

ON THE ELECTROPHYSICS OF SIMPLE MUSCULAR MOVEMENT AND SIMPLE NERVOUS ACTION.

PASSING on from protoplasmic substances to muscle and nerve the signs of natural electricity are found to stand out more prominently, and to need, as it would seem, a somewhat different interpretation. Up to this point the signs noticeable were merely those which belonged equally to protoplasmic substances and to the earth as a whole. Now other signs are superadded which show plainly enough that living muscle and nerve have an electricity of their own, which is present at one time and absent at another, and which is capable of acting upon the galvanometer as well as upon the electrometer.

Now, indeed, I have to deal with actual currents, and with actual changes in these currents, as well as with certain statical phenomena which evidently keep time and step with the changes in the current phenomena. In a word, the path along which I have next to move is clearly indicated; and what I have now to do is to get along it from stage to stage as best I may, first, through the facts which have to be mastered, and then through the inferences lying beyond these facts.

I.

1. A current, to which the name of muscle-current is given, is easily detected in quiescent muscle if the

examination be conducted in a certain manner. It is detected if the two electrodes of the galvanometer are so applied that one is in contact with the side and the other with either one of the two ends of the fibres, and also, though less conspicuously, at the sides singly, or at either of the ends singly, provided the two points of contact are not at the same distance from the edge between the sides and the ends. It is *not* detected if the electrodes are made to connect the two ends of the fibres, or two points upon the sides singly or upon the ends singly which are equidistant from the edge between the sides and the ends of the fibres.

A current is, or is not, detected under these circumstances, and, when detected, it is found to be so directed as to show that, under ordinary circumstances, the surface made up of the sides of the fibres is positive in relation to that made up of either one of the two ends of the fibres, and that the former surface is more positive and the latter more negative in direct proportion to the distance from the line between the two surfaces. All this is made out most conclusively by Dr. Du Bois-Reymond, with this in addition—that the currents so detected while muscle is alive and at rest, disappear gradually as death gains the mastery over life, and that this disappearance is complete, or all but complete, when *rigor mortis* is fully established.

2. Along with these current phenomena there are also certain statical phenomena to which, as I believe, it is necessary that attention should be directed particularly—phenomena about which I am able to speak with more exactness since the time when I began to make use of the new quadrant electrometer of Sir William Thomson in these investigations. Thus, if I examine with this instrument the muscles of the

thigh of a frog while they are alive and at rest, I find marked potential differences between the longitudinal surface of the fibres and their transverse section, the surface being positive and the section negative; and so likewise with the fresh muscle of mouse and sparrow. And this, too, I find — that these potential differences are equal in degree, and that this degree, in the frog, is equivalent to about the third of the potential, + or —, of a Daniel's element in the state of open circuit; and also that, in the frog at least, this state of things may continue, with no very material diminution, long after the setting-in of *rigor mortis,* even until the fifteenth day after the death of the animal to which the muscle belonged. And thus the electrometer supplies an important sequel to the story told by the galvanometer. For this story was merely about potential differences (these of course are implied in the very existence of the current), without a word in it to show whether these were differences in degree only or in kind also; whereas, the sequel, as deciphered by means of the electrometer, is to the effect that the potential differences met with in muscle while alive and at rest are differences of kind as well as of degree, and that the differences between the sides and ends of the fibres are in reality differences of kind akin to those of + and —, which are developed in electromotive action and by induction. And this is a fact of no secondary importance, for, as the evidence unfolds itself, it will be seen to be not improbable that the actual condition of each muscular fibre while alive and at rest is one and the same with that of an electromotive element in the state of open circuit.

3. All that holds good of the electrical history of muscle while alive and at rest, would seem to hold good

equally of the electrical history of nerve under similar circumstances. If the electrodes of the galvanometer are applied to points in the nerve corresponding to the points in the muscle between which the muscle-current is, or is not, detected, a current, now called the nerve-current, is, or is not, detected. If the nerve be placed between the two electrodes of the new quadrant electrometer, so that one is applied to a point on the sides of the fibres, and the other to a point on the surface made by cutting the nerve across, it is found that the former surface is positive and the latter negative, and that the degree of potential, which is the same for the two surfaces, is very much what it was in the corresponding case of the muscle, that is, about a third of that which belongs to a Daniel's element in the state of open circuit. The story of the nerve, indeed, is found to be the exact counterpart of that of the muscle, even to that part of it which concerns the way in which the electricity takes its departure. And this is all that need be said on the subject now.

II.

1. When muscle passes from the state of rest into that of action, there is a change in the muscle-current to which its discoverer, Dr. Du Bois-Reymond, has given the name of "negative variation." If, for example, the muscle-current of the gastrocnemius of a frog be examined in the usual way with the galvanometer, first while the muscle is quiescent, and, then during the opposite state of things, it is found that as soon as the state of contraction is brought about (this is done usually by applying a drop of strong solution of salt to the nerve, but it may be done in any other way), the needle swings back from the point of divergence at which it had been kept standing as long as the muscle

is at rest, and, after oscillating widely for a moment or two, it takes up a position close to zero, and always on the same side as that on which it had diverged while the muscle was at rest.

On setting up the state of contraction, that is to say, there is evidence of a considerable weakening of the muscle-current, for the movement of the needle points simply to this, and not (as it might seem to do if the matter were inquired into hastily) to reversal of the current.

About this there can be no doubt. The notion of reversal may, it is true, seem to receive some support from the violent way in which the needle swings back to the other side of zero when contraction is first set up ; but this notion is set aside by the simple fact that a moment or two later, after the oscillation is over, the position taken up by the needle close to zero is, not on the other side, but on the same side as that on which it had been standing while the muscle was quiescent.

And if this be not sufficient, additional evidence to the same effect is elicited by again examining the muscle-current of the same gastrocnemius in the two opposite states of rest and action, but with these two states separated by an interval in which the needle of the galvanometer is brought to rest at zero by short-circuiting the instrument ; for, in this case, what happens is simply this, that during muscular contraction the needle moves in the same direction as that in which it moved while the muscle was at rest, but not to anything like the same distance from zero. Indeed, the movement caused by the current of the contracting muscle is at most only sufficient to carry the spot of light, which marks on the scale the motion of the needle, to a very few degrees from zero, whereas the movement caused by the current of the quiescent muscle invariably carries this spot altogether off the scale, unless means are taken to shunt off a considerable portion of the current. In a word, the facts go to show that the movement of the needle which indicates the change of current called "negative variation," is nothing more than that which marks a state of weakening or negation, and that instead of any good end being answered by continuing to speak of it as "negative variation," the only effect of so doing is to add unnecessary complication to a problem which is in itself sufficiently simple.

2. When muscle passes from the state of rest into that of action, there is also a change in the charge to which the electrometer bears testimony, which change is precisely equivalent to the change in the current of which I have been speaking. On repeating the last experiment with the new quadrant electrometer in the place of the galvanometer there is what seems to be unequivocal discharge of the charge which is present in the quiesent muscle: and what I find to be true in this case, I also find to be true in all cases in which I have examined with the electrometer the electrical condition of muscle in the two opposite phases of rest and action. At all events, there is in all cases a more or less complete disappearance of the charge belonging to the quiesent state, when muscle passes into the state of action; and so the facts which are brought to light by means of the new quadrant electrometer are found to be strictly parallel to the galvanometric facts about which I have been speaking.

3. In nerve too there is the same disappearance of current and charge when the state of rest changes into that of action.

In one experiment in which it is made evident that the state of nervous action is accompanied by disappearance of "nerve-current," the plan pursued is to fix a decapitated frog upon a frame of some sort, 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, to inject a few drops of solution of strychnia under the skin, and to wait a little and watch. The procedure is simple enough, and the results are unequivocal. Before the poison takes effect the needle is kept at a considerable distance from zero by the current of the quiescent nerve; after the poison takes effect the needle falls back towards zero, but not beyond it, and takes up a position not far from this point. When, that is to say, the nerve is thrown into a state of action by the poison, there is a movement of the needle which shows most unequivocally that this state is accompanied by disappearance of "nerve-current." And so it is also with purely motor nerve and with nerves of special sensation, as well as with other mixed nerves, for in all of these cases equally the state of action is found to be accompanied by disappearance of nerve-current previously present in the nerve.

Nor is it otherwise with the charge which is present in the quiescent nerve, for the simple result of inquiry into the matter with the new quadrant electrometer is to show most clearly that this charge is no longer to be detected when the nerve has passed from the state of rest into that of action. This, for example, is seen to be the case when the last experiment is repeated with the electrometer in place of the galvanometer; and it is, as I can also testify, seen no less plainly in other experiments of like nature; and, in short, the history of nervous action, as elucidated by the electrometer, and the history of muscular contraction from the same point of view, are found to be identical in all particulars.

III.

1. More than one view has been taken, and may be taken, of the electrical condition of muscle and nerve during the state of rest. The fibres of muscle and nerve may be looked upon as made up of *peripolar molecules* —of molecules, that is to say, which are negative at the two poles, and positive in the interpolar regions, or, in exceptional instances, the reverse of this. And, taking this view, it may be supposed that the potential differences between the ends and sides of the fibres are due to the fact of these molecules being so arranged in the fibres as to have their poles pointing endwise, and their interpolar belts turned sideways, and that the muscle-current and nerve-current are derived from infinitely stronger currents which are ever circulating in closed curcuits around each peripolar molecule. The view taken, in fact, may be that of Dr. Du Bois-Reymond. Or, following in the steps of Galvani, it may seem more easy to believe that each fibre (or cell) in quiescent muscle and nerve is a charged Leyden-jar, in which the coat, which is a very bad conductor, does the work of a dielectric,—that the potential differences of the two surfaces, longitudinal and transverse, of the fibre are due to the fact that these two surfaces correspond, directly or indirectly, with the two oppositely charged surfaces of the dielectric coat,—and that the "muscle-current," and "nerve-current," are merely accidental consequences of the two charged surfaces of the dielectric coat being imperfectly connected by means of the galvanometer. The view taken, that is to say, may point to the conclusion that the

electrical condition of muscle and nerve during the state of rest is, not fluent as in the current, but statical. Or else the view taken may be one, according to which each fibre or cell in quiescent muscle and nerve is looked upon as an electromotive element in the state of open circuit,—a view which leads, as did the last, to the conclusion that the electrical condition of muscle and nerve, during rest, is statical, and that the "muscle-current," and "nerve-current," are nothing more than the accidental consequences of the closure of the otherwise open circuits of the electromotive elements by means of the galvanometer. And the latter view is that which now seems to me to be in every way the most simple and the most probable. Indeed I now see plainly enough that I was assuming too much in supposing, as I once did, that the coats of the fibres and cells in muscle and nerve were sufficiently bad conductors to allow of the dielectric action which is required upon the Leyden-jar hypothesis, and that my present choice must lie, not between this view and the electromotive element view, but between this latter view and that which is based upon peripolar molecules. This, then, is the case as it now stands. I prefer the electromotive element view as the most simple and the most probable; and the question is whether I can justify this preference.

2. Amid much that is different, there is something in the structure of muscle and nerve which betokens a protoplasmic nature, and it may be well to try and realise this fact, for so far as their structure is protoplasmic it may be supposed that the electrical history of muscle and nerve will be that of protoplasm.

Instead of saying that voluntary muscle is composed of a large number of extremely fine filaments collected in fibres, each of which is enclosed in a transparent, elastic (the composition agreeing strictly with that of yellow elastic tissue), homogeneous, nucleated sheath, to which the name of sarcolemma or myolemma is given, it may be more correct to say that the fibre is composed of a mass of granular, amœbic, protoplasmic matter, enclosed in such a sheath; for the contents of the sheath, instead of splitting up longitudinally into filaments, may split up horizontally into discs (the sarcous elements of Bowman),—may split up either way, or any way, as they would do, in fact, if they were made up neither of fibrils nor discs, but of granules, which may aggregate into fibrils, or discs, or not aggregate at all. It may indeed be with the fibres of voluntary muscle, as it is more obviously with the fibre-cells of which involuntary muscle is composed, for each of these fibre-cells consists of a core of granular matter, sometimes arranged so as to form imperfect fibrils, and of a nucleated cell-membrane, without any proper sarcolemma, but with each cell-membrane connected with other cell-membranes by a homogeneous, transparent, uniting medium. In this case the cell membrane really takes the place of the sarcolemma, for each cell is nothing more or less than a rudimentary fibre. Indeed, in long voluntary muscles there are fibres which seem to partake somewhat of the character of voluntary and somewhat of the character of involuntary fibres, fibres which instead of running continuously from one end of the muscle to the other, are made up of several elongated fusiform cells overlapping each other at their ends, and in which, therefore, the cell-membrane and sarcolemma are more or less confounded. The facts, indeed, go to show that the contents of the sheath in voluntary muscular fibre, and of the cell-membrane in the involuntary muscular fibre, are but little removed from the condition of simple protoplasm, and that each fibre may be likened to an ensheathed amœba—a likeness which in the latter case receives a further touch in the fact that the shape of the fibre-cell, instead of being fusiform, with pointed or truncated ends, may be branched at the ends as if amœbic processes had been put out there and not retracted.

What holds good of muscle structurally, would also seem to hold good, in great measure, of nerve tissue. Central ganglionic cells, as seen in the ganglia of the sympathetic system especially, consist of a round, oval, or pyriform mass of soft translucent granular substance, with which two or more nerve-fibres communicate, and of an enclosing capsule formed of a transparent membrane with attached or embedded nuclei. In the ganglionic cells of the sympathetic system, indeed, the distinction between the central granular substance, with which the nerve-fibres communicate, and the investing capsule, is well defined, but not so in those of the brain and cord. In the brain and cord, indeed, there is the same central substance, but not the proper capsule. Moreover, the central substance, instead of being a round, oval, or pyriform mass, with which the nerve-fibres are connected at one point only, branches out into several processes which in all probability are continuous with the nerve-fibres. At the same time these cells and fibres are surrounded and supported by connective tissue called reticulum by Kölliker, and neurilogia by Virchow —a tissue which, as Sharpey points out, "is not merely an open mesh-work, but consists of fine laminæ formed of the close investment of fibrils disposed as membranous partitions, and tubular compartments for supporting and enclosing the nervous bundles:" so that in the brain and spinal cord, as in the sympathetic ganglia, there is good reason for believing that the structure of the ganglionic cell is virtually the same, namely, a central granular mass, more or less protoplasmic in constitution, with which nerve-fibres are connected, and a nucleated capsule, of one kind or another, investing this mass. A central mass of granular substance, with which nerve-fibres are intimately connected, and an investing capsule, simple or complex, as the case may be, is, in short, the structural plan, not only of the ganglionic cell, but also of all the peripheral parts of the nervous system without exception—end-bulbs, touch corpuscles, pacinian bodies and the rest. Nor is the plan of the nerve-fibres altogether different, either in those fibres which are white with dark borders, or in those which are grey, pale, non-medullated, or gelatinous. The white or tubular fibres, when quite fresh, appear perfectly homogeneous, like threads of glass, but afterwards, when coagulation has taken place, they are found to consist of an axis or primitive band, as it is called, a white medullary coating, strongly refractive of light, which gives to them the appearance of having dark borders, and an outer membranous sheath or tube, with nuclei in it, analogous to the sarcolemma both in composition and in position. The gray, pale, gelatinous fibre, indeed, would seem to consist of the axis, or primitive band of the others, with obscure sheaths, in which are nuclei, but without medullary coating. In nerve fibres, therefore, as in nerve-cells, the same plan is perceptible, and this is that which is met with in muscle-fibres. In each case, that is, there is the central, soft, more or less granular, protoplasmic core, and the investing capsule.

However altered, therefore, there is still sufficient in muscle and nerve to indicate a protoplasmic origin, and to allow the assumption that what holds good of protoplasm electrically may, up to a certain point, hold good of muscle and nerve likewise—to allow the assumption that the cores of these fibres and cells may be usually in the same state of negative charge as that in which amœbæ and protoplasmic bodies generally, and inorganic substances without exception, are found to be – in the common electrical condition of the surface of the earth, that is to say. Nor does this assumption rest merely on indirect evidence, for the simple fact is this—that when muscle and nerve are examined with a single electrode of the new quadrant electrometer—that is, with the electrode belonging to the pair of insulated quadrants—the electrical indications are found to be precisely the same as those which belong to sponge or myxomycetes, or water, or sculptors' clay, namely those, and those only, which belong to the earth generally.

3. It is also easy to see that the structural peculiarity of muscle and nerve may supply the key to any peculiarity in the electrical history of muscle and nerve. This structural peculiarity consists, partly at least, in the addition of the sheath or cell-membrane. The substance, that is to say, has ceased to be homogeneous. And this change is that which may carry with it all that is needed for the construction of the electromotive elements of which I have spoken. In virtue of its physical composition simply, each fibre or cell in muscle and nerve may well be an electromotive element, the contents, like protoplasm generally, remaining negative, and the sheath or cell-membrane, by reason of juxtaposition with the contents, together with oxidization, or some equivalent operation, becoming positive; and, further, the condition of these electromotive elements,

while the muscle and nerve are at rest, may well be that, not of the closed circuit, but of the open circuit. All that is necessary to the constitution of an electromotive element is the juxtaposition of two heterogeneous substances under circumstances which will allow of the setting up of oxidization, or some equivalent change, in one or other of them; and, therefore, there is good reason for believing that the contents of the fibre or cell may be one of these heterogeneous substances, that the sheath or cell-membrane may be the other, and that the oxygen in the blood, or in the air, may be the agent needed for the excitation of electromotive action. Indeed, the history of electromotive action being what it is, the *onus probandi* rests with those who maintain that fibre or cell so constituted and so excited is not an electromotive element, and not with those who are opposed to them in opinion. Nor is it difficult to advance another step onwards, and come to the conclusion that such electromotive elements may, nay must, be in the state of open circuit while the muscle or nerve is at rest, for if the state were not this it is surely difficult to account for the degree of potential difference, + and −, in muscle and nerve, of the existence of which there is unequivocal evidence in the experiments with the new quadrant electrometer.

5. Nor does this view become more visionary when it is looked into more closely. At all events, the more I so look the more am I inclined to think that there is a value attaching to it which does not belong either to the peripolar view, or to the Leyden-jar view, and that I am justified in giving, as I do, the preference to it.

The electrical condition of any electromotive element in the state of open circuit is eminently statical, the one-half being charged positively and the other negatively. The case is one in which as long as the circuit remains open the idea of current is excluded as far it can be. If, then, the fibres and cells of the quiescent muscle and nerve are electromotive elements in the state of open circuit, of which the negative polar portions are represented by the contents, and the positive by the coats, the inevitable inference is that the electrical indications which are of primary importance are those which act, not upon the galvanometer, but upon the electrometer—are those, not of the "muscle-current" and "nerve-current," but of free positive and free negative electricity. Thus regarded, indeed, the two currents so often named, sink down into mere accidental consequences of the galvanometer being used in a particular way; and the attention which has hitherto been paid to current phenomena will have to be transferred to tensional phenomena. And a right view in this matter is of more than merely theoretical importance. For if the fibres and cells of quiescent muscle and nerve are so charged, it is not difficult to gain a glimpse of the way in which muscular relaxation or elongation is brought about. The inevitable effect of the charge is the setting-up of a state of mutual repulsion among the molecules of the charged body; and there is no reason to suppose that muscle and nerve are affected differently in this respect. But it does not follow that the animal tissue should expand everywhere, and in all cases to the same degree. On the contrary, it is quite conceivable that expansion may be perceptible in the contents of the muscular fibres and cells and not in the coats and that the contents as well as the coats of the fibres and cells in nerve may be in the same case as the coats of the muscular fibres; for it may well be that the parts which so expand are more rudimentary, more protoplasmic, less elaborated, more like the young amœba in which amœboid expansion is met with, and that the parts which do not expand are less rudimentary, less protoplasmic, more elaborated, more like the elderly amœba, in which there are no perceptible expansile movements. And so there may be much in common between muscular relaxation or elongation, and that movement which shows itself in the protrusion of an amœbic process. But it does not do to confound the two movements. A charge operates in causing expansion in both cases, but not the same charge. In the amœba the only charge operating is the common terrestrial charge, and the amœbic movements simply follow the up and down changes which are continually happening in the amount of this charge. In the muscle, on the other hand, the principal charge acting upon the fibre or cell may be the result of electromotive action inherent in the animal tissue; and, in fact, this may be the sole charge so acting perceptibly. At all events the simple fact that in muscle there are no irregular fluctuations of expansion corresponding to those which are witnessed in the amœba, is in itself sufficient to show that the action of the inconstant extrinsic charge which operates in the production of amœbic movement, and which may operate to some extent upon muscle likewise, is in muscle over-ridden by the action of some constant intrinsic charge. And thus the operation of the charge from the constant electromotive action in the quiescent muscle may be to keep up a constant state of muscular elongation, and, consequent upon this, a constant readiness to enter into the state of contraction; while at the same time it is easy to see that this elongation and contraction must operate in a given direction, for the simple reason that the mobile contents of the muscular fibres and cells are so confined by the coats as to be only free to move in one direction, their case in this respect being in fact very similar to that of the mercury within the tube of the thermometer.

6. Again, it is to be supposed that the different electromotive elements which enter into the composition of living muscle and nerve, will interact reciprocally as do the different electromotive elements in the common voltaic pile, or in the pile made by Matteucci out of the femoral muscles of frogs, or in the pile made by myself, in imitation of the latter, out of separate portions of the sciatic nerves of frogs ; and that one result of this interaction will be a change, by which the charge may be intensified to a great degree, and muscular elongation proportionately increased. Moreover, another result of the same interaction—and with this remark I will bring to an end what I have now to say upon the electro-physics of muscle and nerve during the state of rest—may show itself in the counteraction or *inhibition* of action in both muscle and nerve,—the electromotive elements in muscle reacting upon each other, and upon the like elements in nerve, and counteracting or *inhibiting* action in muscle and nerve equally, the electromotive elements in nerve reacting in like manner and with the same results, upon each other, and upon the electromotive elements in muscle.

IV.

1. When muscle or nerve passes from the state of rest into that of action, there is a disappearance, more or less complete, of the electricity which is always present in the quiescent muscle; when the muscle or nerve returns from the state of action into that of rest,

at that moment the electricity which had vanished reappears in full measure. That happens, in fact, which must lead to the development of instantaneous currents of high tension—*extra-currents* in the actual circuit, and *induced currents* in the neighbourhood of the circuit; for it is a law of electricity that such currents are developed whenever there is any falling or rising electrical movement, and at these moments only. And if so, then, in some measure at least, the state of action in nerve and muscle may be stripped of its mystery; for these instantaneous currents of high tension, which are known to have a remarkable power of setting up the state of action, are also known to have the power of discharging the charge which is associated with the state of rest, and which may act by counteracting, or *inhibiting*, the state of action. That they have this latter power is evident in the fact that the "muscle-current" and "nerve-current" are permanently put an end to, and the state of spasm driven on at once into that of rigor mortis, by exposing the body of a dead rabbit or frog, for a sufficient time, to the action of a Ruhmkorff's coil. And what is seen in an extreme degree in this case is also seen in a lesser degree in every case in which muscle or nerve is put in action by induced currents, for here this state of things is always found to be accompanied by the temporary discharge of the charge which always attends upon the state of rest. And this, according to the premises, may be all that is wanted to bring about the muscular contraction; for, by the discharge of this charge, the muscle is left free to yield to the play of the attractive force which is inherent in the physical constitution of the muscular molecules. This is all. There may be an electro-physical reason for muscular contraction no less than for muscular elongation or relaxation; and to account for muscular contraction there may be no reason to call in the aid of a property of muscular irritability, and to suppose that this property has in some mysterious way been stimulated into action during muscular contraction by the extra-currents, or by the induced currents.

2. Again; it is quite conceivable that these instantaneous currents of high tension may be greatly intensified by inductive interaction, and that the extra-currents and induced currents attending upon the state of action in muscle and nerve, would prove to be very powerful, even as powerful as the discharge of the torpedo, if they were not in great measure lost by being short-circuited within the body. And certainly the anatomical and physiological analogies between the muscles and their nervous system and the electric organ and its nervous system, are close enough to justify this conjecture. Like the nerves of the muscles, the nerves of the electric organ originate in the same track of the spinal cord, and terminate in the same manner; like the muscles, the electric organs are paralysed by dividing their nerves; like the muscles, the electric organs, after having been so paralysed, may be made to act by pinching the nerves below the line 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 beyond a certain point without intervals of rest. And, lastly, the nerves of the electric organs, like the nerves of the muscles, when somewhat exhausted, respond in the same curiously alternating way to the action of the "inverse" and "direct" voltaic current, if only discharge be taken as the equivalent of contraction. In a word, these analogies are so close as almost to necessitate

the conclusion to which Matteucci was led in regarding them, namely this—that muscular contraction is actually attended by a discharge of electricity analogous to that of the torpedo, as well as to justify the further conclusion, that, instead of being utterly feeble and of no practical consequence, the instantaneous currents of high tension which attend upon the state of action in muscle and nerve alike, would, if they were not in great measure lost by being short-circuited within the body, prove to be as potent as the discharges of the torpedo.

3. And if so, then it ceases to be matter for wonder that nerve should be capable of setting up a state of action in a neighbouring muscle or nerve, or that muscle should react in the same way upon muscle or nerve; for it is easy to believe that the extra-current developed in the first instance *in* the nerve or muscle at the moment when the state of rest is made to change into that of action, must of necessity lead to the development of induced currents *in the neighbourhood* of the nerve or muscle. In short, the difficulty is to understand, not how action in one part may set up action in another part, but how the action so set up should be *limited* as it is in the living body.

CHAPTER III.

ON THE ELECTROPHYSICS OF CARDIAC AND OTHER FORMS OF RHYTHMICAL VITAL MOTION.

1. In the Croonian Lecture delivered by Sir James Paget, in 1857, "on the cause of the rhythmic motion of the heart," it is made evident that this motion is connected with the action of certain nerve-ganglia, discovered by MM. Bidder and Rosenberger within the substance of the heart, and, at the same time, much light is shed upon the cause of rhythmic vital motion generally; and I cannot do better now than follow in the track which is thus marked out for me.

It is not necessary to go far along this track in order to see that the cause of the cardiac movement lies within the heart itself, for the simple fact that the heart of the frog or tortoise goes on beating for some time after removal from the body is in itself a sufficient proof that it is so. Nor is it necessary to do more than attend to the evidence advanced in this lecture in order to be satisfied that the cause of this rhythmic motion may be further localised in the cardiac ganglia, to some of which the name of "rhythmic centres" has been given.

If, for example, the heart of a frog or tortoise be removed from the body, and divided into two portions by cutting transversely through the substance of the ventricle a little below the line of junction with the auricles, it is found that rhythmic motion goes on in the portion which comprises the auricles and the rim of the cup of the ventricle, but not in the rimless cup of the ventricle itself. When pricked, or otherwise "stimulated," the latter portion may be made to contract like any ordinary muscle, but nothing serves to recall the lost rhythmic motion; whereas, except in being somewhat retarded, this motion goes on as it did before, and that, too, for some time, in the other portion. Again, if the heart of a frog be placed upon a board, with some of its own blood within it and around it, and scissors be then used (as Heidenhain used them) so as to snip away bit by bit, from above downwards, the auricles first of all, and the rim of the ventricle afterwards, the removal of the auricles is found to have little or no effect upon the pulsation, but not so the mutilation of the ventricle, for, as this is proceeded with, the pulsation is seen to diminish progressively in force and frequency until—an event which usually happens when the rim of the ventricle has been snipped away to the depth of a couple of lines or thereabouts—it ceases altogether:—so that in this case it is evident that the cause of the rhythmic motion is not in the rimless cup of the ventricle, nor yet in the auricles, but in the rim which is cut away from the cup of the ventricle. Moreover, there is another way to the same conclusion through certain experiments (always upon frogs or tortoises) in which, without removing the heart from its place, ligatures are tied in various positions—experiments devised by the late Professor Stannius, and often repeated with the same results by Sir James Paget and others. In one of these experiments the ligature is tied around the great veins at some distance from the auricles, with the effect of producing little or or no disturbance in the rhythmic motion which is going on in the heart on the one side, and in the great veins on the other side. In another experiment, what is done is to put a ligature around the auricular orifices of the great veins ; and then, after waiting for a few minutes, to place a second ligature around, or rather a little below, the boundary ring between the auricles and the ventricle, so as to include the bulbus arteriosus of the frog, or the two aortæ of the tortoise ; and what follows is—(1), *upon tying the first ligature*, cessation of rhythmical motion in both auricles and ventricle, and, a moment or two later, a return of this motion (with the rate of rhythm considerably retarded) in the ventricle, *but not in the auricles*, while all the while the slow beating of the great venous sinuses goes on as if nothing had happened ; and (2), *upon tying the second ligature*, acceleration of the rhythmic motion of the ventricle (or renewal if this had just come to a stop), and nothing else, for all along the slow beating goes on as it did previously in the great venous sinuses, *and all along the auricles remain motionless*. Now, the nerve-ganglia, to some of which the name of " rhythmic centres " is given, lie chiefly in the boundary lines between the auricles and the ventricle, and between the auricles and the great veins ; and, therefore, it is not difficult to see how herein may be found what may prove to be the key to these experiments,—how the cutting-away of the rim of the cup of the ventricle may put an end to rhythmic motion in the ventricle by removing rhythmic centres,—how the crushing of rhythmic centres under the ligature, may have put an end to rhythmic motion in the auricles in the experiment where the ligature is tied around the boundary ring between the auricles and great veins,—and how even the apparently contradictory fact of acceleration of rhythmic motion in the ventricle, which is brought to light when the second ligature is tied around the boundary ring between the auricles and ventricle, may be explained, for here it may be supposed that the rhythmic centres are not wholly crushed by the ligature, that the parts which remain uncrushed are not sufficient to *inhibit* action to the full extent, and that by the lessening of the periods of diastole thus brought about, the systolic beats may be quickened. And thus, the facts being as they are, it becomes almost a matter of certainty that the source of the rhythmical action of the heart must be sought, not in the cardiac muscular tissue alone or independently, but in the nerve-ganglia, to which the name of rhythmic centres has been given.

2. In speculating upon the *modus operandi* of these rhythmic centres in the production of cardiac motion, Sir James Paget has much to say which cannot be disregarded, but I find it difficult to accept the conclusion at which he arrives. He believes that the rhythmic action of the heart in the vertebrata "is due to the 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 contraction," and that these discharges are themselves brought about by the nutrition of these ganglia "being, in certain periods, by nutritive changes of composition, raised, with regulated progress, to a state of instability of composition, in their decline from which they discharge nerve-force, or change their shape, contracting." For my own part, I am disposed to think that the cause of the rhythm has to do less with nutritive than with electrical changes in these ganglia, which changes have their origin in the respiration or aëration of the parts ; and, most assuredly, the result of reflection is to confirm me in this disposition rather than to unsettle me.

The fact of the occurrence of electrical changes during cardiac action is put beyond question by Dr. Burdon Sanderson in an experiment upon the palpitating heart of a frog, which may easily be repeated. What has to be done in this case, after arranging the clay electrodes of the galvanometer, not side by side in the usual way, but one above the other, is to place the heart between them, with its apex resting in a hollow upon the upper surface of the one, and with the auricles, or the bare rim of the cup of the ventricle, so arranged as to be in contact with the under surface of the other, and then to watch the spot of light upon the graduated scale, which spot tells of what is happening in the current thus introduced into the coil from the heart as the heart goes on beating. And what happens is simply this—that as the heart goes on beating this spot of light moves forwards and backwards on the scale synchronously with the beats, forwards at the diastole, backwards at the systole, forwards in obedience to the current proceeding from the cardiac muscles and nerves during the state of quiescence, backwards when this state of quiescence changes into that of action in consequence of that withdrawal or deprivation of current to which the name of "negative variation" has been given. It is so when the entire heart is experimented on; it is so equally when a ventricle, from which all traces of the auricles have been removed, is made to take the place of the entire heart. In either case indifferently the ray moves forwards and backwards in the same way and to the same degree coincidently with the diastolic and systolic movements of the ventricle; indeed so it must be, for in the cut-out heart the only unmistakable rhythmic movements met with are, not in the auricles, but in the ventricle singly. And so in supposing, as I do, that the cause of the rhythm of the heart is to be sought in certain electrical rather than in certain nutritive changes within the heart, I may make a firm stand on the fact that the beatings of the heart are really accompanied by synchronous changes in the cardiac electricity.

It is also a fair inference from the facts to which attention has been already directed that these electrical changes must originate in the nervous rather than in the muscular apparatus of the heart. Indeed, until these facts are set aside, the only inference which would seem to be permissible is that the primary seat of all rhythmic changes in the heart, electrical and the rest, must be referred to the rhythmic centres of the heart.

As far as I can see, also, I am at liberty to assume that the electrical changes which accompany cardiac action are traceable to changes in the aëration rather than to changes in the nutrition of the rhythmic centres. It is not difficult to believe that a state of electromotive action may be set up in these centres by the oxygen in the arterial blood, and that this state may be kept up until this oxygen is used up, and no longer. It is not difficult to believe that the state of diastole (which is one of muscular relaxation) should continue as long as this electromotive action is sufficiently kept up, and that the state of systole (which is one of muscular contraction) should follow at the moment when the electromotive action fails for want of red blood, and also at the moment (immediately succeeding the former moment) when this action is being restored by the injection of a new supply of red blood into the cardiac vessels at the systole. And so, likewise, in the case of the heart, which goes on pulsating in the air after its removal from the body; for here it may be supposed that fresh air penetrates to the rhythmic centres through the open vessels, and through various pores and interstices, partly by atmospheric pressure, partly by capillary attraction, and partly by the diastolic suction to which it gives rise, and that in due time, in consequence of this air being used-up, the systole follows and drives out this used-up air,—so that, in this way, the diastole is again brought about, as at first, by the cardiac tissue being again exposed to the action of fresh air. It is, of course, a matter of indifference whether the oxygen acting on the heart be supplied in the arterial blood, or in the air which takes the place of the arterial blood in the case to which I am now alluding. Nor is there anything in the background to make it necessary to change the view which is here taken of the action of oxygen in the production of cardiac motion. On the contrary, I find more than one cogent reason for not changing it. I find one such reason in the fact that a heart, or a fragment of a heart, which is beating in the air, will cease to beat when it is placed in a vacuum, or in an atmosphere of hydrogen, or carbonic acid, or nitrogen, and resume its beatings when air is re-admitted into the vacuum, or substituted for the gases which have just been named. I find another such reason in the fact that a heart which has just ceased to beat in common air will begin to beat again, and go on beating for some time afterwards, when it is removed into an atmosphere of oxygen or nitrous oxide gas. And this is not the only evidence to the same effect which might easily be cited if it were needed.

In this way, step by step, I am able to advance, without stumbling, far enough to see that a key to the beatings of the heart may be found in electrical changes in the " rhythmic centres," to which I have been directing attention. For the case in question is one which has only to be interpreted in accordance with the principles already laid down. During the state of diastole the electrical condition of the heart is constant—is that which counteracts or inhibits the state of systole. During the state of systole there is first a falling and then a rising movement in the electrical condition belonging to the state of diastole, which movements must be attended by the development of the instantaneous currents of high tension, extra-currents and induced currents, which are concerned in causing the muscular contraction which is here called systole. This is all. While the cardiac electricity remains constant the cardiac muscles must be in that state of relaxation to which the name of diastole is given; at the moment when there is an ebbing or flowing movement in this electricity, the cardiac muscles (by reason of the action of the instantaneous currents of high tension which are then developed) must be thrown into the state of muscular contraction which is spoken of as systole. In a word, there is no need to go beyond the electrical changes of which there is certain evidence, in order to hold the key which will unlock the secret of the rhythmic motion of the heart.

Nor is this conclusion invalidated by the fact that the movement of the auricle is in seeming opposition to that of the ventricle, the diastole in the one corresponding to the systole of the other, and *vice versâ*. At first sight, no doubt, it seems as if it were so, but on looking into the matter a little more attentively, it soon becomes evident that the movements of the auricles are in the main resolvable into mere passive consequences of the movements of the ventricles, and that the only real difficulty in connexion with the rhythmic motion of the heart is to account for the movements of the ventricles.

The absence of valves at the auricular openings of the great veins is to me a strong reason for believing that the systole of the auricles cannot, by possibility, play that active part in the propulsion of the blood which is played by the systole of the ventricles; and, do what I will, I can come to no other conclusion than this—that the auricular systole is in the main due to the passive falling-in of the thin auricular walls upon the blood being suddenly gulped away from the auricles into the ventricles at the ventricular diastole,—and that the auricular diastole is, in the main, due to the simple passive filling-out of the auricles, arising partly from the regurgitation of blood produced by the backward movement of the auriculo-ventricular valves at the ventricular systole, and partly from the pressure of the column of blood which is continually setting-in towards the auricles through the valveless openings of the great veins. I cannot well be in doubt here. If the auricular systole had to drive the blood into the ventricles in the same way as that in which the ventricular systole has to drive it into the arterial system of vessels, then surely there *must* have been valves at the openings of the great veins into the auricles; and, therefore, the simple fact that there are no such valves is, as I think, a good reason for concluding that the auricles may be little more than elastic cisterns for feeding the ventricles,—that their work, like that of the great venous trunks, may be chiefly that of balancing the moving column of blood,—and that, in short, the auricular movements are, in the main, merely passive consequences of the movements of the ventricles.

Moreover, I find another and perhaps still more cogent reason to the same effect in another direction. Thus, as in one of the experiments already mentioned, the effect of tying a ligature around the boundary ring between the auricles and the great veins in the heart of a tortoise, is this—that rhythmic motion goes on in the ventricle on the one side, and in the great veins on the other, *but not in the auricles*. And thus, as in an experiment which I have more than once tried upon the heart of a tortoise, the effect of tying a broad ligature tightly around the ventricle, a little below its line of junction with the auricles, is to put a stop to rhythmic motion in the ventricle, and to leave the auricles and the great veins opening into them beating at the same slow rate. It is evident, that is to say, that the rhythmic motion in the ventricle may go on without any such movement in the auricles, and also that the rhythmic motion which is manifested in the auricles when there is no such movement in the ventricle, has nothing in it to distinguish it from the slow rhythmic motion which belongs to the great venous sinuses; and, therefore, I do not see that there is anything to prevent me from finding in these experiments much that is corroborative of the conclusion which I have ventured to draw from the absence of valves at the auricular openings of the great veins.

In this way the explanation of the rhythmic motion of the heart generally may be made to resolve itself into the explanation of the rhythmic motion of the ventricles simply, and this again, perhaps, into that which applies equally to capillary movement and to vascular movement generally. For, after what has been said, a very natural conclusion appears to be this, that the ordinary vaso-motor centres may act upon the coats of the capillaries and other simple vessels, as do the rhythmic centres of the heart upon the walls of the ventricle, keeping up a kind of diastolic dilatation as long as the electricity supplied by them to the coats of the vessels remains sufficiently constant, and causing a kind of systolic contraction, by the instantaneous currents then generated, at the moments when there are ebbing or flowing movements in this supply.

3. And so likewise when the inquiry is extended so as to include other cases of rhythmic motion, as, for example, the case of the movements of the chest in mammals, or that of the mantle in the cuttle-fish, or that of the gelatinous body-disk of the pulmonograde acalephæ, or that of the oscillatoria, or that of ciliary movement, or that of the vacuoles in certain minute algæ, or that of the lateral leaflets of the desmodium gyrans ; for in each and all of these cases it is easy to see that there is no occasion to call in any other principle of interpretation than that which has been made use of hitherto.

The respiratory movements in mammals, no less than the cardiac movements, must be referred to the action of rhythmic nerve-centres ; indeed, the more evident dependence of the former movements upon the action of the medulla oblongata as a rhythmic centre is pointed to as an additional reason for believing that the cardiac movements are really connected with the action of the rhythmic centres of Bidder and Rosenberger. Without changing the point of view, also, it is possible to see why it may be that the rhythmic action of the medulla oblongata may be slower than that of the rhythmic centres within the heart. For really there is no necessity that the beatings of the chest should synchronize with the beatings of the heart. The heart, without question, is a very important agent in carrying on the circulation, and so also is the force to which the name of capillary movement is given. Even in the higher animals it may be a mistake to suppose that the action of the heart is merely supplemented by that of the capillary movement, and that the latter action might be dispensed with without much loss ; nay, it may be necessary to believe that in certain parts of the economy, even in mammals, the capillary movement is of greater importance in carrying on the circulation than the cardiac movement, and that in these parts the blood is transmitted through the minute vessels in obedience to the slower rhythmic action of these vessels, rather than in obedience to the quicker rhythmic action of the heart. It is, indeed, quite conceivable that capillary action, which is all important in carrying on the circulation of creatures which have no heart, may be, even in mammals, very far from being altogether subject to that of the heart, in certain parts of the system at least. And if this be so,—and I really see no reason why it may not be so,—then there is no difficulty in seeing why it is that the heart may beat more quickly, and that the chest may move more slowly, for in order to this all that is necessary is to suppose that the circulation is carried on chiefly by cardiac action in the rhythmic centres of the heart, and chiefly by capillary action in the medulla oblongata, and that, for this reason, the different rate of rhythm in the two cases is merely the natural consequence of fresh blood being supplied to the rhythmic centres more quickly in the one case than in the other. Electrically, indeed, the two cases are one and the same. In both cases electromotive action is set up in the rhythmic centre by the oxygen of the blood, and the only difference between the two cases is that this oxygen is used up more quickly by the cardiac ganglia than by the medulla oblongata. As long as the oxygen is supplied in quantity sufficient to keep up the electromotive changes in the centre to a given pitch of constancy, so long the state of action is inhibited in the nerves and muscles belonging to the centre; as soon as the oxygen so acting becomes insufficient to do this (by being used up), the electromotive action of the centre fails, and in failing, sets up, by means of the instantaneous currents of high tension which must be developed in and around the centre at the moment of such failure, a state of action in the nerves and muscles belonging to the centre; while at the same moment, or rather an instant later, these nerves and muscles are again thrown into a state of action by the momentary currents of high tension which are also developed at the moment when the electromotive action of the rhythmic centre is being restored to its original pitch by the re-admission of fresh blood. And thus there need be no difficulty in subjecting the rhythmic motion of the heart and of the chest in mammals to what is substantially one and the same rule.

This explanation would also seem to be readily applicable to the rhythmic motion of the mantle of the cuttle-fish—a motion by which, as may now-a-days be seen in more than one aquarium without any trouble, the water is alternately drawn into the branchial cavities through the slit-like aperture at the base of the funnel, and ejected through the funnel; and, in fact, I only refer to this instance of rhythmic motion in order to point out in it what seems to me to be a cogent proof that the state called muscular relaxation is something more than simple cessation of contraction—something very like active expansion. I have often watched this rhythmic motion in the mantle of the cuttle-fish, and I am satisfied that the diastolic movement by which the water is sucked in has something in it which must be of the nature of active dilatation or expansion. It cannot be due to the resiliency of elastic fibres upon the cessation of a previous state of contraction, for such fibres form no part of the texture of the mantle. It cannot well be due to simple cessation of a state of contraction, for, so far as I can see, this would lead, not to the water being drawn underneath the mantle, but simply to the limp mantle being firmly pressed inwards by the weight of the circumjacent water. In point of fact, I can only see in this movement of the mantle of the cuttle-fish a proof that the state of muscular fibre to which the name of relaxation is given is, not mere cessation of contraction, but something more akin to active elongation or erection. Nay more, I cannot look at the movement in question without jumping to the conclusion that the diastolic movement may be equally active in other forms of rhythmic muscular motion as well—that, for example, the blood may be *drawn into* the ventricle of the heart at the diastole as well as *driven out* at the systole, and that the force concerned in carrying on the circulation may be *doubled* in like manner in the capillaries and minute vessels, the blood being *drawn into* the vessels at the time of vascular dilatation, as well as *driven out* of them at the time of vascular contraction.

To a certain extent, also, the same explanation may meet the case of rhythmic motion as exhibited in the jelly-like body-disk of a pulmonograde acalaphe, for here there is a neuro-muscular apparatus which may be supposed to behave in the same way as that in which this apparatus has been seen to behave in the cases of rhythmic motion which have just been under consideration. In this case, however, it is not easy to believe that the neuro-muscular system is sufficiently developed to do all the work which must be done in order to account fully for the rhythmic motion which has to be accounted for, and it is difficult to escape the conclusion that here there is another cause at work which is the sole cause of rhythmic motion in the cases which have yet to be noticed—in the cases, namely, of the oscillatoria, of vibratile cilia, of pulsating vacuoles, and of the mobile parts of the lateral leaflets of the desmodium gyrans ; or rather the probability is that here, at the foot of the scale of being, each cell or fibre, in which the contents and coats are distinctly differentiated, is an electromotive element which, so far as the capacity for originating and responding to rhythmic motion is concerned, is potentially at once a rhythmic nerve centre and the neuro-muscular system belonging to it.

In the genus oscillatoria, and in the kindred genera of confervoid algæ, the plant is composed of cylindrical filaments of protoplasmic substance, invested by a continuous cellulose sheath, or tubular cell-membrane, each filament, as a rule, becoming transversely striated as it advances in age, and the filaments collectively cohering into definite fronds. These filaments, in those parts which are free to move, are continually writhing in one direction or another in a kind of oscillatory movement ; and in the older filaments, where the striated appearance is well marked, it is difficult not to think that there is something in common between them and striated muscular fibres, both as regards structure and function. Indeed, there seems to be no reason why the analogy should not also extend to electromotive capacity, for, in reality, there is in the filament of the oscillatoria a differentiation into contents and coats identical with that which is met with in the muscle-fibre or nerve-fibre, which distinction may be all that is wanted in order to convert the fibre into a true electromotive element. And this is all that need now be said; for if the filament of the oscillatoria be an electromotive element, it is easy to go on step by step, and suppose—that a state of electromotive action will be set up in every filament by the oxygen in the surrounding air or water, which action will continue until this oxygen is more or less used up; that the effect of this electromotive action will be to keep up a state of rest, with perhaps elongation or erection in the filament; that the failing of electromotive action, for want of oxygen, may be attended by the development of an instantaneous current of high tension in and around the filament, which current may have the effect of causing the filament to move in one direction, and to contract, it may be, at the same time; that, in this way the used up air or water will be dispersed, and the filament brought into close relation with fresh air or water; that electromotive action will thus, by oxygenation, be again brought about in the filament, and with it the state of rest, and, perhaps, that of elongation or erection; and that the moment in which the electromotive action is being thus re-established, may be attended by the development of another instantaneous current of high tension, which current takes the opposite course to that taken by the former current, and so causes the filament to move in the opposite direction to that in which it was made to move by the former current. Thus, in each interval between the times of rest, it may be that the filament of the oscillatoria is made to move in opposite directions by the two instantaneous currents which must be developed whenever the electromotive action in the filament sinks and rises, for the simple reason that the course taken by these two currents is in opposite directions.

The same explanation may also serve for the movements of vibratile cilia. Indeed, it could only be in a bout of differentiating phrensy that anyone could hesitate to bring the movements of vibratile cilia and those of the filaments of the oscillatoria into the same category, or to think that the same explanation did not apply equally in the two cases.

And, as in these several cases of rhythmic motion, so in the two cases which yet remain to be noticed, that is, in the case of the vacuoles in certain confervoid algæ, and in that of those parts of the desmodium gyrans which have to do with the movements of the lateral leaflets. For what is the fundamental supposition throughout? It is that wherever there is a cell or fibre in which there is a distinct difference between contents and coats, there is there that heterogeneity of structure which is necessary to the conversion of that cell or fibre into an electromotive element, the action of which remains constant or sinks and rises as the oxygenation on which it is dependent remains constant or sinks and rises. It is that the more mobile parts of the cell or fibre, the contents, expand or shrink as the charge set up in them by electromotive action happens to be imparted or withdrawn. This is all. And, therefore, the changes in the size of the vacuoles, and of the mobile parts of the desmodium gyrans, may be merely the natural consequences of the changes in bulk of the contents of certain cells, which changes are thus brought about.

CHAPTER IV.

ON THE WORK OF ARTIFICIAL ELECTRICITY IN VITAL MOTION.

In what I have to say upon the *modus operandi* of artificial electricity in vital motion, I propose to examine, in turn, the way in which muscle and nerve are affected by franklinic electricity, by faradaic electricity, and by voltaic electricity. I shall say nothing upon the action of artificial electricity in the non-muscular and non-nervous modes of vital motion, for the simple reason that at present too little is known of this action to make it possible to say much to the purpose; and, in what I do say, I shall concern myself chiefly with the action of voltaic electricity upon muscle and nerve, as manifested in connection with the workings, (1) of the so-called "inverse" and "direct" currents, and, (2) of electrotonus. I select these two problems as being in themselves of paramount interest and importance, and as containing the very essence of all that has to be said respecting the action of voltaic electricity upon muscle and nerve; and I choose to dilate upon the action of voltaic electricity upon muscle and nerve rather than upon the action of franklinic or faradaic electricity, because, in this direction, it is more easy to find the common key to the *modus operandi* of artificial electricity in vital motion.

A.—ON THE WAY IN WHICH MUSCLE AND NERVE ARE AFFECTED BY FRANKLINIC ELECTRICITY.

It is a fact that a living body may be charged with positive or negative franklinic electricity (if only care be taken so to manage the charging as to avoid a spark) without the production of either motion or sensation, and that there is neither sensation nor motion while this charge remains. It is also a fact that the sudden discharge of this charge is marked by motion and sensation. Charge, that is to say, is plainly associated with the state of rest, and discharge, not less plainly, with the state of action.

Again. It is a fact that the muscular action attending upon the discharge of franklinic electricity will go on for a longer time when the charge is positive than when it is negative. Thus, if a galvanoscopic frog, which has been insulated by putting it astride upon the end of a glass rod, be charged at an ordinary electrical machine, and then discharged by touching one of the toes with a finger, contractions happen from 15 to 20 times before the discharge is complete, if the charge be positive, but not oftener than from 4 to 5 times, if the charge be negative. The contraction is repeated in either case, as it would seem, because the toe is twitched away from the finger by the contraction before there has been time for the charge in the limbs to be wholly discharged ; but it is not so easy to say why it should be repeated more frequently with the positive charge than with the negative. Indeed, all that may be done now is to hint, in passing, that in one way or another the positive charge must be more favourable than the negative to the preservation and manifestation of muscular activity, and to direct attention to a fact which is in some measure parallel to the one in question, namely, this, that Sir Wm. Thomson charges the aluminium needle of his new quadrant electrometer with positive rather than with negative electricity, because he finds "that when a conductor with sharp edges or points is surrounded by another, presenting everywhere a smooth surface, a much greater difference of potential can be established between them, without producing disruptive discharge, if the points and edges are positive than if they are negative."*

And thus, as it would seem, the action of franklinic electricity upon muscle and nerve may be resolved into that of charge and discharge, either charge maintaining the state of rest, the positive charge being favourable to the preservation and manifestation of vital activity in a way in which the negative charge is not favourable, and the discharge coinciding with the state of action. In a word, the case of the action of franklinic electricity upon muscle and nerve is not dissimilar from that of the action of natural electricity upon muscle and nerve, for in this latter case the whole tenor of the evidence went to show that living nerve and muscle during rest are in a state of charge, and that the state of action is attended by discharge.

B.—ON THE WAY IN WHICH MUSCLE AND NERVE ARE AFFECTED BY FARADAIC ELECTRICITY.

When a muscle is faradized, a state of action is invariably set up in it: when a nerve is treated in the same manner, this may or may not be the effect, the state of action being present or absent in this case, as the currents in use happen to be under or over a certain degree of strength.

The beats of the heart of a rabbit are arrested in the stage of diastole by faradizing the pneumogastric nerve (E. and F. Weber†). The peristaltic movements of the intestine of the same animal are brought to a stop in the state opposed to contraction, when the grand

* "Papers on Electrostatics and Magnetism." Macmillan. 1872. 8vo., p. 267.

† "Handwörterbuch der Physiologie." Art. "Muskelbewegung." Vol. iii, p. 42. 1846.

sympathetic nerve or the spinal cord is subjected to the same treatment (Pflüger*). In order to this, however, the faradaic currents must be of a certain strength, for when weaker currents are used in place of stronger, the result is to quicken, as the case may be, the cardiac beatings or the intestinal workings (Lister†). These are the facts. By faradizing nerve, nervous action may be arrested or suspended, and the difference in the result is manifestly due to difference in the strength of the currents made use of.

A similar conclusion is also necessitated by the facts which have been brought to light by the investigations in which the names of Claude Bernard‡ and Brown-Séquard§ are so closely associated, for these, when supplemented by a fact which I am able to supply, go to show that faradaic electricity acts upon vaso-motor nerves in precisely the same way as that in which it acts upon other nerves, provoking or paralysing action as it happens to be under or over a certain degree of strength. Thus it is that the evidences of vaso-motor paralysis—the elevation of temperature, the blood-shot state of the conjunctiva and lining membrane of the nostril and ear, the fuller and firmer pulse, the contracted pupil, and the rest—which are produced on one side of the head and neck of a rabbit when the superior cervical ganglion is

* "Ueber das hemmungs Nervensystem für die peristaltischen Bewegungen der Gedärme." Berlin. 1856.

† "Prelim. Inquiry into the Functions of the Visceral Nerves, with special reference to the so-called 'Inhibitory System.'"—"Proc. of Royal Society," 13th Aug., 1858.

‡ "Comptes Rendus de la Soc. de Biologie," Dec., 1851; "Gaz. Méd. de Paris," 1852, p. 72; "Comptes Rendus de l'Académie des Sciences," March 29, 1852; "Leçons sur la Physiologie et la Pathologie du Système Nerveux," 8vo., Paris, vol. ii, Leçons 15 and 16, 1858.

§ "Philadelphia Med. Examiner," Aug., 1852; "Exp. Researches applied to Phys. and Pathology," New York, 1853; "Lancet," Oct. 30, 1858.

removed, or the cervical filament of the sympathetic is cut across, on that side, are made to disappear by faradizing the upper end of the sympathetic nerve. This is true enough, but it is not the whole truth. It is true if the faradaic currents made use of are comparatively feeble: it is not true if these currents be at all strong, for in this case, as I can testify, a contrary result is brought about. Thus, in the experiment on the rabbit to which reference has just been made, the effect of faradizing the upper end of the divided sympathetic nerve with strong currents, is altogether different from that which is brought about in this way with weaker currents, the symptoms of vaso-motor paralysis passing off under the action of the weaker currents, but not under that of the stronger. Indeed, time after time, these symptoms of vaso-motor paralysis may be made to disappear and re-appear, by simply altering the distance between the primary and secondary coils, so as to weaken or strengthen sufficiently the currents which are acting upon the vaso-motor nerves.

And this too is what happens in other cases of the kind, the faradaic currents always provoking or paralysing nervous action as they happen to be under or over a certain degree of strength.

Indeed, the whole case would seem to be sufficiently simple, if only it be supposed that the nerves are thrown into a state of action by the *shock* caused by the passage of the faradaic currents, for then all that is wanted in order to account for the paralysing effects of the stronger currents, is to suppose that the *shock* of these currents has made nervous action impossible, either by *stunning* the nerves or nerve-centres, or, (to adopt a more mechanical view of the matter,) by putting their electromotive apparatus altogether out of gear.

C.—ON THE WAY IN WHICH MUSCLE AND NERVE ARE AFFECTED BY VOLTAIC ELECTRICITY.

I.—ON THE ACTION OF THE SO-CALLED "INVERSE" AND "DIRECT" CURRENTS UPON MUSCLE AND NERVE.

I.

1. The action of the so-called "inverse" and "direct" currents, and of voltaic electricity generally, is most clearly and conveniently demonstrated upon an *ordinary galvanoscopic frog*—that is, upon the two hind limbs of the animal, without their skin, and connected only by the lumbar nerves and the intervening bit of spine—if the results obtained in this case are checked by those obtained in the case of a second galvanoscopic frog, which may be distinguished as the *extraordinary galvanoscopic frog*, and which differs from the first in this—that all the natural pelvic and lumbar connections of the two hind limbs are allowed to remain intact.

In showing the action of the "inverse" and "direct" currents upon the muscles, what is done is to connect the voltaic poles with the feet, the positive with one foot, the negative with the other, and to notice what happens in each of the two forms of galvanoscopic frog, when the circuit is closed and opened, and while it remains closed. The arrangement is one which makes it possible to observe the action of the "inverse" and "direct" currents upon the muscles, for it is evident that the voltaic current must take an "*inverse*" or upward course in the limb connected with the positive pole, and a "*direct*" or downward course in the limb connected with the negative pole. And this is what has to be noticed when this is done. In each of the two forms of galvanoscopic frog, the broad results are the same. In each there is contraction on closing and opening the circuit, or at one or other of these moments. In each the muscles are relaxed while the circuit remains closed. In each the contraction consequent upon opening and closing the circuit continues longer in the limb in which the current is "inverse," or upward, than in the limb in which it is "direct," or downward. In each the contraction, after it has come to an end in the limb in which the current is "direct," or downward, may—as is seen in the so-called "voltaic alternatives"—be again and again brought back to that limb, by reversing the direction of the current, provided only the contractions have not yet come to an end in the limb in which the current is "inverse," or upward. At first, it seems to be matter of indifference whether it is the *ordinary* galvanoscopic frog or the *extraordinary* galvanoscopic frog which is experimented upon: afterwards it becomes evident that the results 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 singly.

In both forms of galvanoscopic frog, as I have said, the results of the action of the "inverse" and "direct" currents are in the main the same. In both cases, under both currents, the muscles are relaxed as long as the circuit remains closed. In both cases the contraction consequent upon the closing and opening of the circuit continues longer under the "inverse" than under the "direct current." In both cases there may be an exhibition of "voltaic alternatives." It is indeed only in the results of closing and opening the circuit that *after a time* a difference creeps in between the two cases. In the case of the *ordinary galvanoscopic frog*, the result of closing and opening the circuit is contraction, first at both these moments under both currents alike, and afterwards, when the activity of the parts is somewhat impaired, at only one of these moments, under each current differently, the contraction eventually being at the moment of opening, and not at the moment of closing, under the "inverse" current, and at the moment of closing, but not at that of opening, under the "direct" current, thus:—

What happens in the case of the somewhat exhausted *ordinary galvanoscopic frog* at the Opening and Closing of the Circuits of the "Inverse" and "Direct" Currents.

	Under the "Inverse" Current.	Under the "Direct" Current.
At the *Closing* of the Circuit.	o	Contraction.
At the *Opening* of the Circuit.	Contraction.	o

Whereas, in the case of the *extraordinary galvanoscopic frog*, under the "inverse" and "direct" currents alike, the result of closing and opening the circuit is contraction, first, at both these moments, afterwards, when the parts have become somewhat exhausted, at the moment of closing only, thus:—

What happens in the case of the somewhat exhausted *extraordinary galvanoscopic frog* at the Opening and Closing of the Circuits of the "Inverse" and "Direct" Currents.

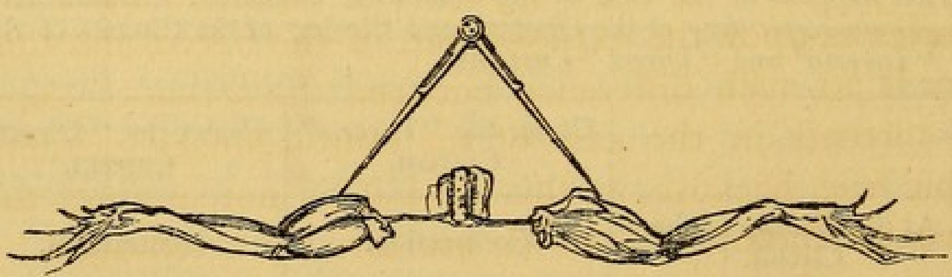
	Under the "Inverse" Current.	Under the "Direct" Current.
At the *Closing* of the Circuit.	Contraction.	Contraction.
At the *Opening* of the Circuit.	o	o

2. How, then, are these facts to be accounted for? How is it that there should be these differences as to contraction between the two forms of galvanoscopic frog? Why are the muscles relaxed while the circuit remains closed? Why is the contraction at the moment of closing or opening the circuit, at both these moments at first, at only one of them afterwards? Why does this contraction alternate eventually as it is found to do in the case of the ordinary galvanoscopic frog? And these questions are certainly not unanswerable if only a reasonable amount of patience and attention be expended upon them.

3. It is easy to see how it is that the action of the "inverse" and "direct" currents upon the muscles should eventually differ as it is found to do in the two forms of galvanoscopic frog.

If a voltaic current be passed along an *ordinary galvanoscopic frog* from one foot to the other until the contraction is only at the opening of the circuit under the "inverse" current, and only at the closing 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:—

FIG. 4.



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. With the compasses placed in this position, that is to say, the order of contraction changes to that which is observed in the case in which the current is passed in the same direction along the *extraordinary galvanoscopic frog;* and thus a point is gained from which it is possible to see why it is that the current should act differently in the two forms of galvanoscopic frog. For the action of the compasses can be only that of a good conductor, by which the current previously passing is more or less diverted from the nerves, which are very bad conductors, in comparison, not only with the compasses, but also with the muscles, while at the same time, in consequence of the resistance to its passage being diminished, more current is admitted to act upon the muscles. And if so, then it may also follow, that in the case of the extraordinary galvanoscopic frog, the voltaic current may in the same way be diverted from the nerves, and left to act chiefly or solely upon the muscles, for in reality muscles are far better conductors than nerves. In a word, the facts would seem to justify the conclusion that there is a difference between the action of the "inverse" or "direct" currents upon muscle in the case of the ordinary galvanoscopic frog, because the current in this case is acting directly, not so much upon the muscles as upon the exposed part of the nerve, and that there is no such difference between the action of these two currents in the case of the extraordinary galvanoscopic frog, because in this case it is not so much the nerves as the muscles which are directly acted upon by the current—a conclusion from which it follows that muscle, apart from nerve, must be looked upon as responding to the action of the "inverse" and "direct"

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current in precisely the same way, the contraction in each case equally being at first at the closing of the circuit and at the opening also, and afterwards at the closing of the circuit and *not* of the opening.

The simple fact that contraction happens, *not* while the circuit remains closed, and the constant current is passing, but only at the moment when the circuit is closed and opened, one or both, would serve to connect the contraction, not with the constant current, but with the instantaneous currents of high tension, to which their discoverer, Faraday, gave the name of *extra-currents.* Moreover, this power of producing contraction is precisely that power which these extra-currents may be supposed 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 allied to induced currents as to be often confounded with them. Like induced currents, extra-currents are two in number, the one at the closing, the other at the opening of the circuit of the constant current. Like the two induced currents, the two extra-currents are in contrary directions and of unequal strength. In fact, extra-currents are only unlike induced currents (of the first order) in this, that their direction and relative strength is not the same. With induced currents, it is the current at the *opening* of the circuit which passes in the same direction as the constant current, and 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. With extra-currents, indeed, as with induced currents, there are special differences in strength and direction, which may, nay must, tell upon the production or non-production of muscular motion under the action of the "inverse" and "direct" current; and, in fact, much that is at first perplexing in the order of contraction under these circumstances receives a simple explanation, if only these differences be remembered and applied.

Remembering and applying these differences, and not forgetting what has been said before, there is no difficulty in explaining the occurrence of contraction at the closing of the circuit, and at the opening also, under both currents; for, in order to this, all that is wanted is that the muscles in the case of the extraordinary galvanoscopic frog, and the exposed nerves in the case of the ordinary galvanoscopic frog, should be sufficiently "irritable" to be capable of responding to the weaker as well as to the stronger of the two extra-currents.

And in the case of the extraordinary galvanoscopic frog, there is no difficulty in explaining why contraction should linger longer at the closing of the circuit than at the opening; for after a time it is to be supposed that, in consequence of failing "irritability," the muscle will respond only to the extra-current which is the strongest of the two, namely, to that which is at the closing of the circuit.

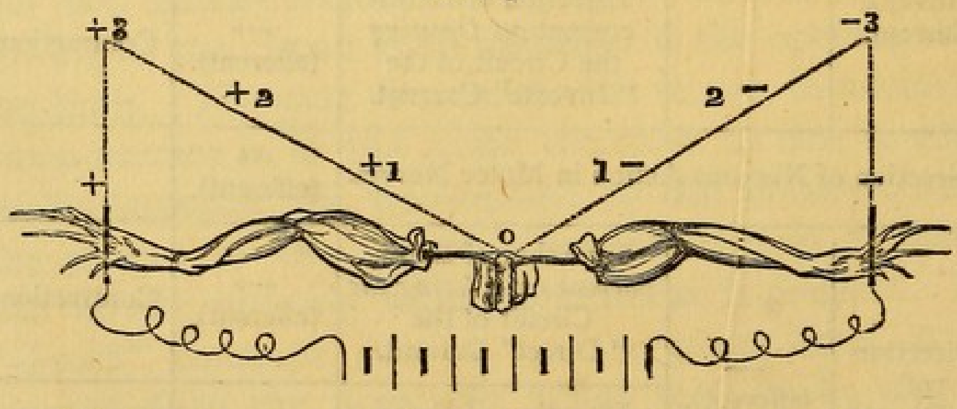
The difficulty is in accounting, not for these phenomena, but for the differences in the order of contraction which afterwards creep in in the case in which the ordinary galvanoscopic frog is operated on.

The question is, how is it that in this case the contraction after a time comes to be 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?" How is this strange alternation in action to be accounted for? Has the *direction* of the extra-current along the exposed nerve anything to do in the matter? Is it that the nerve, now that it is somewhat spent, is only capable of responding to that extra-current which happens to pass along it in the same direction as that in which the natural motor impulses are transmitted to the muscles? That the nerve is somewhat spent 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 have to be in the affirmative. The extra-current, to repeat, is in the same direction as the constant current at the closing of the circuit, and in the opposite direction at the opening; and, therefore, it is easy to see what must happen under the "inverse" and "direct" currents. The "inverse" constant current is the current *up* the limb; and hence it must be that of the two extra-currents belonging to it, the one at the closing of the circuit is *up*, and that at the opening *down* the limb. 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 hence it must be that the extra-current connected with the closing of the circuit, must be likewise *down* the limb, while that at the opening is *up* the limb—must be, that is to say, 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, and not at the opening, under the "direct" current, if only the nerve at this time be in that state of impaired activity 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. For the case is simply that which is represented in the following tabular summary of the facts :—

<table>
<tr><td colspan="5">Showing that the Direction of the Extra-currents belonging to the "Inverse" and "Direct" Currents agrees with that of Nervous Action in Motor Nerves when there is Contraction, and disagrees in the contrary case.</td></tr>
<tr><td colspan="4"></td><td>Results.</td></tr>
<tr><td rowspan="2">Direction of "Inverse" Current.</td><td rowspan="2">← (afferent).</td><td>Direction of Extra-current on Closing the Circuit of the "Inverse" Current.</td><td>← (afferent).</td><td>o</td></tr>
<tr><td>Direction of Extra-current on Opening the Circuit of the "Inverse" Current.</td><td>→ (efferent).</td><td>Contraction.</td></tr>
<tr><td colspan="3">Direction of Nervous Action in Motor Nerves.</td><td>→ (efferent).</td><td></td></tr>
<tr><td rowspan="2">Direction of "Direct" Current.</td><td rowspan="2">→ (efferent).</td><td>Direction of Extra-current on Closing the Circuit of the "Direct" Current.</td><td>→ (efferent).</td><td>Contraction.</td></tr>
<tr><td>Direction of Extra-current on Opening the Circuit of the "Direct" Current.</td><td>← (afferent).</td><td>o</td></tr>
</table>

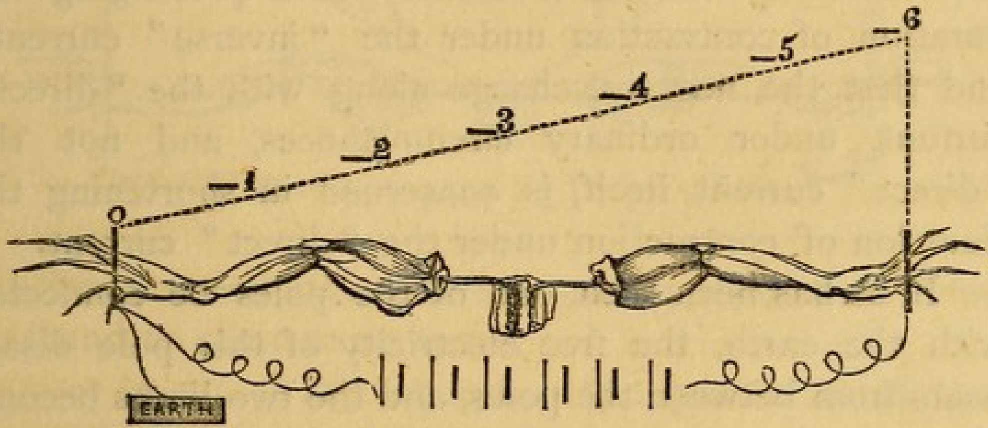
5. Nor does it appear that the different direction of the two constant currents can supply any explanation of the fact that the contraction on opening or closing the circuit is found to continue for a longer time under the "inverse" current than under the "direct" current. On the contrary, there is good reason to believe that the difference in question is in some way connected, not with the constant current, but with the charge of free electricity associated with this current. The fact of the charge is not to be called in question. If either of the two forms of galvanoscopic frog be tested by the new quadrant electrometer, or any sufficiently sensitive instrument of the kind, while a voltaic current is passing as in the ordinary experiment for showing the action of the "inverse" and "direct" currents, the movement of the ray upon the scale shows very unequivocally—provided only the circuit be properly insulated—that a positive charge is associated with the "inverse" current, and a negative charge with the "direct" current—shows, in fact, that each limb receives a charge from the pole in contact with it, of which the potential falls regularly from the pole, where it is highest, to a point midway between the poles, where it is at zero, thus:—

FIG. 5.



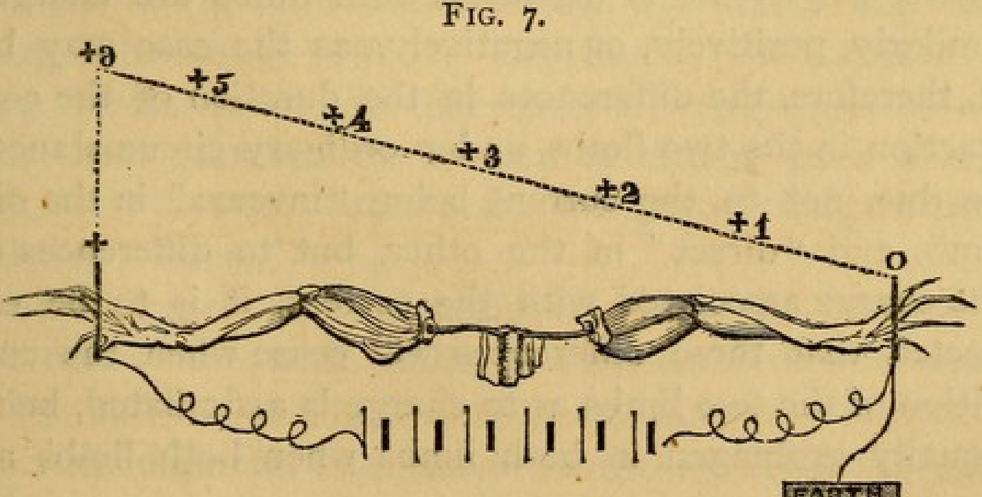
This is the case where the circuit is insulated, but not in the contrary case. When, for example, an earth-wire is put to either pole, the free electricity of that particular pole disappears, and the parts between the poles become wholly charged with the free electricity of the other pole. If the positive pole be put "to earth," positive electricity disappears altogether from the space between the poles, and both limbs become charged negatively from the negative pole, the potential of the remaining charge falling regularly from the negative pole, where it is highest, to the positive pole, where it is at zero, thus :—

FIG. 6.



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

FIG. 7.



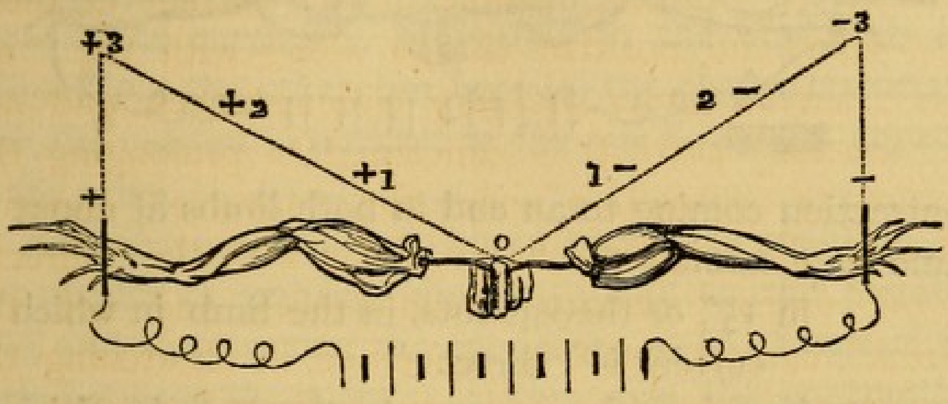
It is with the prepared limbs, indeed, as Kohlrausch and others have shown it to be with any imperfect conductors placed in the same position between the poles.

Nor are these facts beside the present purpose. On the contrary, it is easy to show that the state of the two limbs as to charge has more to do in modifying the condition of the limbs as to contractility than the "inverse" or "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 concerned in prolonging the duration of contraction under the "inverse" current; and that the negative charge along with the "direct" current, under ordinary circumstances, and not the "direct" current itself, is concerned in shortening the duration of 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 similarly 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 the current being "inverse" in the one limb, and "direct" in the other, but to differences in the charge associated with the current, it is to be expected that these differences will cease when the condition of the two limbs as to charge is assimilated, being equally prolonged in both limbs when both limbs are charged positively, being equally shortened in both limbs when both limbs are charged negatively. And this expectation is not contradicted by the results.

In the ordinary case in which the circuit is insulated the state of the two limbs as to free electricity is that which is represented in the accompanying figure (Fig. 8),

FIG. 8.



and the contractions on closing and opening the circuit are found to come to an end

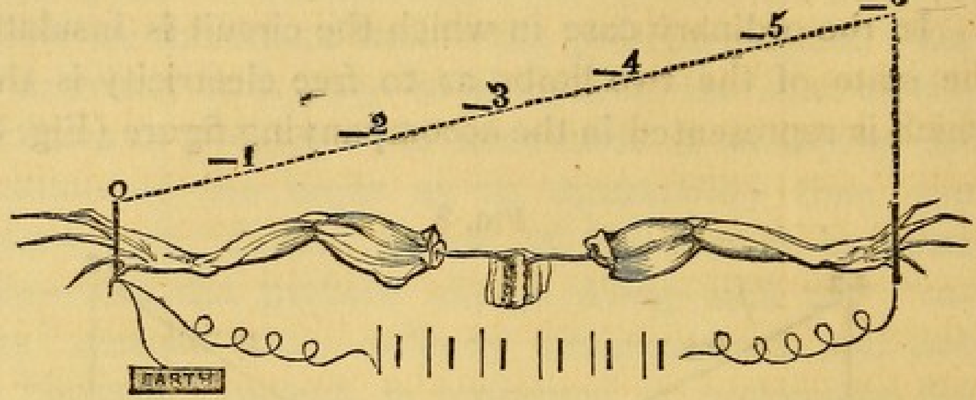
in 15′, or thereabouts, in the limb in which the current is "direct;"

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

In this case, that is to say, the state of things is one in which it is possible to believe that the difference in the duration of the contraction in the two limbs may be owing either to the different direction of the currents in these limbs, or to the difference of charge associated with these currents, for a positive charge goes along with the "inverse" current, and a negative charge along with the "direct."

In the case where 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, both limbs being now charged negatively, as in the accompanying figure (Fig. 9), and the

FIG. 9.



contraction coming to an end in both limbs at about the same time, namely :—

in 15′, or thereabouts, in the limb in which the current is "direct ;"

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

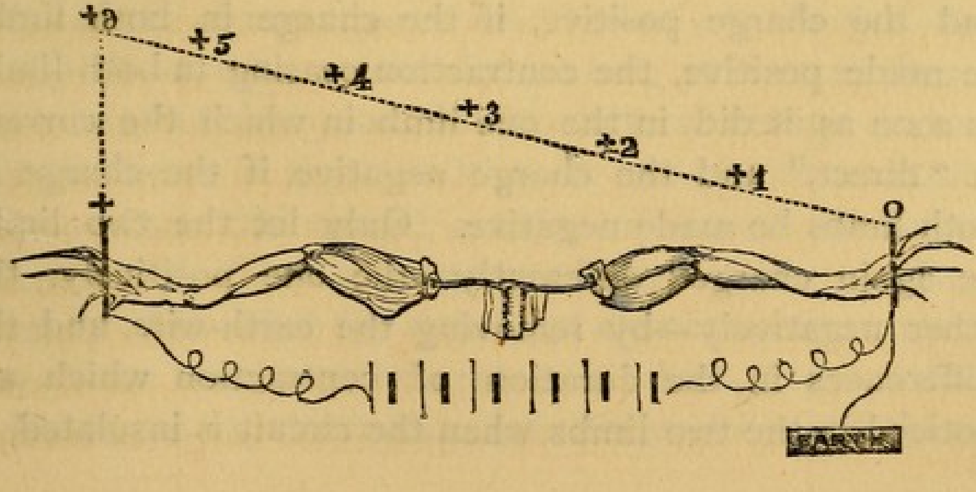
These are the facts to be noticed in this case. The contraction comes to an end in both limbs in about 15′, and not as it did in the last case, in 15′ in the one limb, and in 60′ in the other. The alteration in the duration of the contraction is not in the limb in which the current is "direct," and the negative charge 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 into 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 into negative, the alteration being this— that the duration of the contraction in the limb in which the charge is changed from positive into negative is made to agree with that belonging to the limb in which the charge has throughout remained negative. In a word, everything in this case goes to show that in the former case, where the circuit is insulated, the duration of the contraction is different in the two limbs, not because the current is "inverse" in the one limb and "direct" in the other, but because the charge associated with the current is positive in the one limb and negative in the other.

And these conclusions are in every way confirmed by the results of putting the earth-wire to the negative pole, and so reversing the conditions as to charge of the parts between the poles. 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 circuit is "inverse,"
>
> in 60′, or thereabouts, in the limb in which the circuit is "direct:"

in this case the two limbs are in the electrical condition represented in the accompanying figure (Fig. 10),

FIG. 10.



both being charged positively. As in the last case, so in this, the charge in the two limbs is assimilated, and so is the duration of the contraction, and the only difference is one which gives point to the same lesson. In this case, the alteration in charge is, not in the limb in which the charge was positive before the earth-wire was applied, this charge remaining positive throughout, but in the limb in which the charge has changed 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 has changed from negative to positive, the alteration being this—that the duration of the contraction is, not equally shortened in the two limbs, as in the last case, but equally prolonged.

It is plain, therefore, that the differences in the duration of contraction which are noticed when the circuit is insulated, are to be referred, not to the "inverse" and "direct" currents, but to the charges of different electricity associated with these currents. There is no flaw in the evidence. Only let the limbs be charged similarly by putting one of the poles "to earth," and the differences between the two in relation to contraction 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. Only let the two limbs be again charged differently—the one positively, the other negatively—by removing the earth-wire, and the differences in the duration of contraction 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 contraction 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 way indicated, must be regarded as a conclusive proof that the longer or shorter duration of the contraction in the two limbs is due, not to the current being "inverse" in the one limb, and "direct" in the other, but to the charge associated with the current, under ordinary circumstances, being positive or negative, as the case may be, the positive charge in some way lengthening the duration of the contraction, and the negative in some way shortening it.

6. And herein also, in all probability, may be found the key to the fact that after contraction has ceased in the limb in which the current is "direct," and the charge negative, it may, as in the so-called "voltaic alternatives," be more than once re-called by reversing the position of the poles so as to make the current in the limb "inverse," and the charge positive; for if the latter charge has the power of prolonging contractility, it is not difficult to advance a step and suppose that the power which is thus preservative may be restorative also.

7. Nor is it altogether unintelligible that the two charges, positive and negative, should act thus differently, if only the charged parts are themselves charged naturally as well as artificially (so long at least as they are in a state of rest), and if this natural charge is to be looked upon as the condition, or one of the conditions, upon which the special activity of muscle or nerve is dependent: for the case, according to the premises, is simply this—that the structural elements of muscle and nerve are electromotive centres in which, under ordinary circumstances, the investments are positive and the con-

tents negative,—that these electromotive centres are in the state of open circuit while the muscle or nerve is quiescent,—and that, for this reason, the state of the structural elements (the fibres and cells) is one of charge during rest—a state in which, under ordinary circumstances, the investments are charged positively, and the contents negatively. The case is one, indeed, in which it is easy to see that the investments of the cells and fibres, with their natural positive charge, may be affected one way with the artificial positive charge, and another way with artificial negative charge, and that the one way may be favourable, and the other unfavourable to the continuance of the natural activity of the charged parts, if only the natural charge of these parts be the condition, or one of the conditions, upon which the natural activity of the parts is dependent; or else the key to the difficulty in question may be in the supposition that the artificial electricity may react with the natural electricity as two voltaic batteries must do, in which the arrangement of the elements is made to agree in the one case, and to disagree in the other.

8. The continuance of the state of rest during the time the circuit remains closed is a problem which cannot be fully dealt with until the phenomena of electrotonus have passed 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 charge associated with the current, for if the nerve and muscle remain at rest so long as they retain their natural charge, it is to be expected that rest, not action, may also be their state under the action of the artificial 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 *charge*, whether natural or artificial, has an actual power of counteracting or inhibiting action in both muscle and motor nerve—of producing rest, in fact.

9. So far, then, the general drift of the evidence would seem to show that muscular motion is affected by electricity, not by the muscle or nerve being polarized in one way when the current is "inverse," and in the other way when the current is "direct," but simply by the muscle or nerve being acted upon by the positive charge ordinarily associated with the "inverse" current, and by the negative charge ordinarily associated with the "direct" current. In point of fact, there is no need to fly for help to the so-called "inverse" or "direct" current, or to the constant current in any of its aspects or workings. The case, indeed, is one which resolves itself into an action of charge and discharge, the positive charge in some way preserving and restoring the state of activity, the negative charge in some way having a contrary action, and both charges producing the state of rest; while contraction is in all cases connected with discharge, by means of the extra-currents, which are virtually discharges. Nay, after all, it may be that the true action of voltaic electricity upon muscle and motor-nerve may be one in which even the constant current and the extra-currents are alike put out of court—an action in which, as is illustrated by the following experiment, the only possible ways of acting left are those of charge and discharge. This experiment consists simply in putting the prepared limbs of a frog astride upon a glass rod, and in bringing them first to one and then to the other of the poles of a voltaic battery of which the circuit is always kept open. What is done is to charge the limbs at one pole, and then to

alter this charge by carrying them so charged to the other pole. And when this is done, contraction happens, not when the charge is received in the first instance, but when it is discharged by altering it. The case is one in which, the circuit being always open, extra-currents and constant currents are both excluded, and the only modes of action left are those of charge and discharge. The case, too, is of interest as suggesting that the action of the open voltaic circuit and of the closed voltaic circuit may after all be one and the same,—that the action of the extra-currents and of the constant current may be no more essential in the one case than in the other,—that what is only essential in both cases is the charge and the discharge, the charge counteracting the discharge causing action,—that extra-currents may be no more than discharges taking place between the poles,—that even the constant current itself, as Mr. Gassiot suggests, may be made up of a succession of similar discharges without sensible intervals between them,—and that, by thus resolving everything into charge and discharge, the action of voltaic electricity upon muscle and motor-nerve may be assimilated to that of franklinic electricity, and even to that of faradaic electricity, so far at least as the production of muscular motion is concerned, for there is no reason to think that the action of induced currents (the only action to be considered in faradaic electricity) differs in any wise from that of the discharges of franklinic electricity, or of the extra-currents of voltaic electricity.

II.

1. There are also several experiments of Lehut, Bellingheri, and Matteucci, which go to show that

these different conclusions are in no way contradicted by those which have to be drawn from the action of the "inverse" and "direct" currents upon sensory nerves.

This is what happens, for example, when a short portion of the sciatic nerve of a rabbit is acted upon by a voltaic battery, if only the nerve be exposed without disturbing its connections with the sensorium on the one hand and with the limb on the other. On closing the circuit of the "*inverse*" current and also on opening it, there are cries and convulsions; while the circuit is kept closed there are neither cries nor convulsions. At first the cries are 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 the convulsions attend upon the closing of the circuit and also upon the opening, and they are general; afterwards, when the activity of the nerve is somewhat impaired, the convulsions in the muscles *below* the part of the nerve experimented on, and the convulsions in other parts of the body, are at different times, the latter being at the closing of the circuit only, the former at the opening only. Afterwards, in fact, the contractions in the leg experimented on alternate with the cries, the contractions being at the opening of the circuit, the cries at the closing. And these are the contractions to which attention is now called exclusively. Indeed, these contractions are plainly those which have here the sole right to be regarded as essential, the others (those in the muscles whose nerves arise *above* the part of the nerve acted upon by the "inverse" current) being reflex phenomena which may here be regarded as merely accidental.

On closing the circuit of the "*direct*" current, and also on opening it, there are cries and convulsions; while the circuit is kept closed there are neither cries nor convulsions. At first the cries are at the closing of the circuit, and at the opening also; afterwards, when the activity of the nerve is somewhat impaired, they are at the opening only. At first the contractions *below* the part experimented on (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 cries and convulsions alternate under the "direct" current as they did under the "inverse" current, but in a different order, the cries 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:—

The Alternation of Sensation and Contraction at the Closing and Opening of the Circuit of the "Inverse" and "Direct" Currents, when the somewhat exhausted Sciatic Nerve of a Rabbit is acted upon by these Currents.

		Results.	
Under the action of the "Direct" Current.	At the *Closing* of the Circuit.	o	Contraction.
	At the *Opening* of the Circuit.	Sensation.	o
Under the action of the "Inverse" Current.	At the *Closing* of the Circuit.	Sensation.	o
	At the *Opening* of the Circuit.	o	Contraction.

Once arrived at this point, these cries and convulsions 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 cries no less than the convulsions come to an end sooner under the "direct" than under the "inverse" current.

2. As regards contraction, there is nothing in the experiment which does not tally perfectly with what has gone before, and about which all that is necessary has been said already. The facts to be dealt with, indeed, are only new so far as sensation is concerned, and therefore all that has to be done now is to inquire whether the conclusions drawn respecting alternating contraction are also applicable to the history of alternating sensation.

The fact that sensation agrees with contraction 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, must have 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 special activity 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 contraction in the order set forth in the preceding table. On the contrary, this very alternation, when it is inquired into particularly, supplies 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 under the "direct" current, and not at the opening, was explained by supposing that the nerve at this time had lost so much of its peculiar activity as to be only capable of responding to the extra-current which passed in the same direction as that in which motor impulses 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, in fact, 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 alternating sensation and motion is, in fact, precisely the same, if it be supposed, as it may well be, that the peculiar activity of sensory nerve, no less than that of motor nerve, is at the time so far impaired as to leave the nerve in a state in which it is capable of responding only to the extra-current which passes in the same direction as that in which its natural impulses are transmitted: for the case is simply that which is represented in the following tabular summary of the facts:—

1. Showing that the Direction of the Extra-currents at the Closing and Opening of the Circuit of the "*Inverse*" *Current* agrees with that of nervous action in *Sensory Nerves* when these Currents give rise to *Sensation*, and disagrees in the contrary case.

		Results.
Direction of Extra-currents at the *Closing* of the Circuit of the "*Inverse*" Current.	← (afferent).	Sensation.
Direction of nervous action in *Sensory* Nerves.	← (afferent).	
Direction of Extra-currents at the *Opening* of the Circuit of the "*Inverse*" Current.	→ (efferent).	o

2. Showing that the Direction of the Extra-currents at the Closing and Opening of the Circuit of the "*Inverse*" *Current* agrees with that of nervous action in *Motor Nerves* when those Currents give rise to *Contraction*, and disagrees in the contrary case.

		Results.
Direction of Extra-currents at the *Closing* of the Circuit of the "*Inverse*" Current.	← (afferent).	o
Direction of nervous action in *Motor* Nerves.	→ (efferent).	
Direction of Extra-currents at the *Opening* of the Circuit of the "*Inverse*" Current.	→ (efferent).	Contraction.

3. Showing that the Direction of the Extra-currents at the Closing and Opening of the "*Direct*" *Current* agrees with that of nervous action in *Sensory Nerves* when these Currents give rise to *Sensation*, and disagrees in the contrary case.

		Results.
Direction of Extra-currents on *Closing* the Circuit of the "Direct" Current.	→ (efferent).	o
Direction of nervous action in *Sensory* Nerves.	← (afferent).	
Direction of Extra-current at the *Opening* of the Circuit of the "Direct" Current.	← (afferent).	Sensation.

4. Showing that the Direction of the Extra-currents at the Closing and Opening of the Circuit of the "*Direct*" *Current* agrees with that of nervous action in *Motor Nerves* when these Currents give rise to *Contraction*, and disagrees in the contrary case.

		Results.
Direction of Extra-currents at the *Closing* of the Circuit of the "Direct" Current.	→ (efferent).	Contraction.
Direction of nervous action in *Motor* Nerves.	→ (efferent).	
Direction of Extra-currents at the *Opening* of the Circuit of the "Direct" Current.	← (afferent).	o

And thus, instead of showing that nerve is affected differently by the "inverse" and "direct" currents in the production of sensation and motion, these alternating sensations only supply additional reason 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 in which the alternating contractions were explained, may be looked upon as an additional reason for believing that these alternating contractions have been explained in the right way.

II.—ON THE INFLUENCE OF ELECTROTONUS UPON MUSCULAR MOTION.

I.

1. Electrotonus is the name given to a condition in nerve produced by the action of a voltaic current upon nerve. It makes itself known by certain movements of the needle of the galvanometer, and by certain modifications of nervous activity. It is described as consisting of two opposite phases, the one called anelectrotonus, in which the nerve-current is strengthened and the nervous activity suspended, the other called cathelectrotonus, in which the current is weakened and the activity exalted. Much has been done to elucidate these phenomena. Much, in particular, has been done by Du Bois-Reymond, who discovered the changes in the nerve-current; by Eckhard, who discovered the accompanying modifications of nervous activity; and by Pflüger, whose complicated and careful investigations would seem to have left but little to be done by others. In point of fact, however, much work remains to be done, which can only be done by going over the whole ground 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 nervous activity.

2. In order to exhibit the electrotonic movements of the needle, the plan commonly pursued is to take a long piece of fresh nerve,—the entire sciatic nerve of a large frog most commonly and most conveniently,—and to arrange it between the electrodes *e e'* of a galvanometer, and across the poles *a c* (*a* for anode, *c* for cathode) of a voltaic battery, as it is arranged in the two following figures (Fig. 11 and Fig. 12).

FIG. 11.

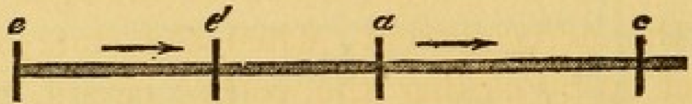
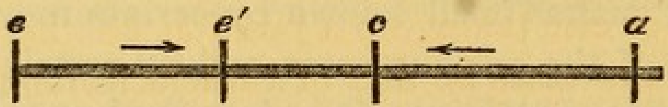


FIG. 12.



When, as in the first of these two figures (Fig. 11), the movements of the needle under the action of anelectrotonus have to be investigated, the position of the + pole, or anode, is next the galvanometer, and the direction of the voltaic electrotonizing current along the nerve between the voltaic poles is that which is indicated by the arrow there placed.

When, as in the second of the two figures (Fig. 12), the action of cathelectrotonus upon the needle is under consideration, the position of the — pole, or cathode, is next the galvanometer, and the direction of the voltaic current along the nerve between the poles is that of the arrow which is placed in that position. The direction of the voltaic electrotonizing current along the nerve is altogether different in the two electrotonic states. The direction of the nerve-current, as is indicated in the two figures by the arrow between the electrodes *e é* of the galvanometer, is, on the other hand, the same in the two electrotonic states, namely, from the cut ends of the fibres of the nerve to the uncut sides: and so it must be for this simple reason, that in both these states the two electrodes of the galvanometer are always applied to the nerve in the same way, the electrode *e* being applied to the cut ends of the fibres, and the electrode *é* to the uncut sides. Before setting up the state of electrotonus by closing the voltaic circuit, the needle has taken up the position into which it diverges under the action of the nerve-current: when the state of electrotonus is set up by closing the voltaic circuit, the needle moves this way or that, becoming more divergent if this state be that of anelectrotonus, becoming less divergent, or else taking up a position on the other side of zero, if this state be that of cathelectrotonus. These are the facts. It is as if the nerve-current were invigorated under the action of anelectrotonus, because it happens to pass in the same direction as the voltaic electrotonizing current: it is as if the nerve-current were weakened or reversed under the action of cathelectrotonus, because it does not happen to pass in the same direction as the voltaic electrotonizing current. At any rate, this is the explanation which Dr. Du Bois-Reymond has to offer of the facts to which attention is now directed.

3. In truth, however, there is good reason to believe that the electrotonic movements of the needle must be interpreted in a very different way to this.

One such reason—which is brought to light by simply going on a little further with the same experiment—is to be found in the fact that the movement in question continues for a long time after the final disappearance of the nerve-current.

Another reason to the same effect is contained in the fact, that movements of the needle, in all respects like those belonging to electrotonus, are to be observed, not only when there is no nerve-current in the nerve to be modified, but even when the nerve itself is taken away, and another bad conductor used in its stead. If, for example, a piece of common hempen string, moistened with water or saliva, be made to take the place of the nerve, the needle is found to move as it moves under the action of electrotonus when the nerve was present—as it moves in anelectrotonus if the anode be nearest the galvanometer, as it moves in cathelectrotonus if the cathode be in this position. And so, also, if a piece of cotton thread, or silk thread, or a thread-like strip of gutta-percha, similarly moistened with water or saliva, be substituted for the piece of common hempen string, the movements of the needle belonging to electrotonus being always produced if only the voltaic conditions for producing them are provided.

Here, too, as between brackets, mention may also be made of another fact which must not be lost sight of when the time comes for commenting upon the other facts, namely this, that electrotonic movements of the needle are *not* met with when the experiment for producing them is repeated upon a copper, or silver, or platinum wire.

When I made these discoveries I did not know that I had been anticipated by any one. In truth, however, Matteucci had pointed out a short time previously, first, that electrotonic movements of the needle are obtainable, not only from living nerve, but also from dead nerve, as well as from strips taken from the substance of brain, or from the coats of the bladder; and, secondly, that such movements are not obtainable 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 on my own observations simply, there is, in addition, the highest testimony which can be had as to the correctness of the facts to which I am now directing attention.

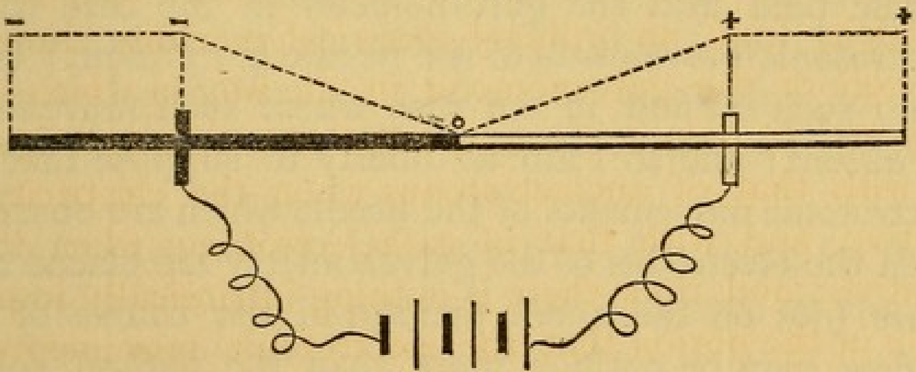
4. It is very plain, then, that the electrotonic movements of the needle cannot be referred to modifications of the nerve-current produced by the voltaic current, but it is not so easy to go beyond this negative conclusion. Matteucci refers them to polarization, and accounts for their absence, in the case of the amalgamated zinc wire, coated with thread soaked in saturated solution of sulphate of zinc, by supposing that such wire is unpolarizable; but this view is clearly untenable, for the simple fact is that the movements in question are equally absent when the wire employed in the experiment is made of copper, or silver, or platinum, or any other metal, without anything being done to it to prevent polarization. Be this as it may, however, I prefer to account for the presence and absence of the movements in question in another way:—for the presence of the movements, by supposing that free electricity, which is liberated at the voltaic poles when the circuit is closed by nerve or other bad conductor, may so overflow towards the galvanometer, and into the coil, as to act upon the needle and make it move, as it moves in anelectrotonus if this free electricity be positive, as in cathelectrotonus if negative; for the absence of these movements when a good con-

ductor like wire is placed between the poles, by supposing that in this case there is no overflow of free electricity from either of the poles towards the galvanometer, and into the coil, for the simple reason that the presence of the good conductor between the poles prevents the liberation of free electricity at the poles. And there is, to me at least, good reason for this preference in the fact that similar movements of the needle may be actually brought about in this way, and that free electricity is actually present when electrotonic movements of the needle are present, and absent in the contrary case.

Movements of the needle strictly corresponding with the electrotonic movements may be caused by passing electricity from an ordinary electrical machine through the coil of the galvanometer in the way in which it is supposed to pass in electrotonus, the movement agreeing with that of anelectrotonus when the electricity is positive, and with that of cathelectrotonus when it is negative; and thus there is nothing intrinsically improbable in the notion that the electrotonic movements of the needle may be brought about in the way which has been suggested, that is, by the action of free electricity.

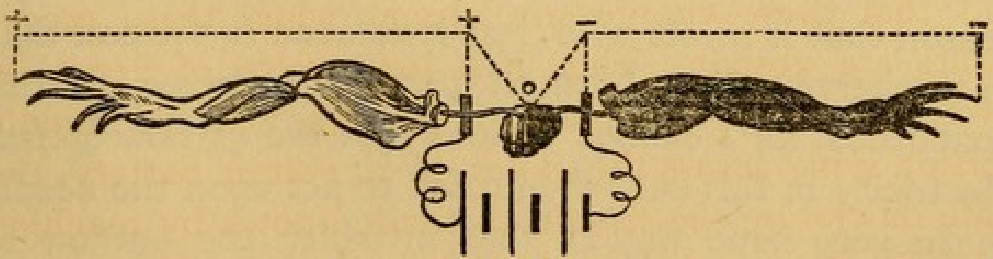
There is also very conclusive evidence to show that the two electrotonic regions are actually charged, as they are supposed to be charged, in the case where the electrotonic movements of the needle are present, the anelectrotonic positively, the cathelectrotonic negatively, and that these charges are absent when these movements are absent. Thus, if a long piece of nerve be stretched out upon a plate of paraffin, with its middle portion lying across the poles of a voltaic battery, the state on closing the circuit, as made known by touching the nerve here and there with the electrode belonging to the insulated pair of quadrants of the new-quadrant-electrometer, is found to be that which is shown in the accompanying figure (No. 13), the anelectrotonic region, which is left unshaded, being charged positively, to a degree which is indicated by the dotted line with the + signs over it, the cathelectrotonic region, which is shaded, being charged negatively, to a degree which is also indicated by the dotted line with the — signs over it—a state of charge, the degree of which outside the pole is equal to that of the pole, but not so inside the pole, for inside the pole it falls regularly from the pole, where it is at its height, to a point midway between the poles, where the two electrotonic regions meet, and where the charge is at zero.

FIG. 13.



And as in this case, so in that in which a galvanoscopic frog is arranged and examined in the same manner as that in which the nerve was arranged and examined in the last instance; for here, as is seen on comparing the figure following (Fig. 14) with the one

FIG. 14.



preceding (Fig. 13), it is found that the two corresponding halves of the limbs and of the nerve are in precisely the same predicament as to charge.

On the other hand, if a wire be made to take the place of the nerve or galvanoscopic frog, all these evidences of charge are wanting, unless it be that there is (from insufficient contact at the poles, from want of conductive capacity in the wire, or from some other cause,) some impediment to the free passage of the current between the poles.

5. These are the facts. Movements of the needle, like those belonging to electrotonus, are caused by the passage of free franklinic electricity through the coil. There is an outflow of free electricity from the voltaic pole into the galvanometer in the case where electrotonic movements of the needle are present: there is no such outflow in the case where such movements are absent: and so I am at liberty to suppose that the electrotonic movements of the needle which are observed when the electrodes of the galvanometer are placed *anywhere* (not on the nerve merely) in the course of the outflow, may be nothing more than the natural consequence of the passage of free electricity into the coil from the voltaic pole which happens to be nearest to it, this free electricity passing through the coil from one electrode of the galvanometer to the other (from that which is nearest to the voltaic pole to that which is furthest from this pole), rather than directly along the nerve from one electrode to the other, for the simple reason that it meets with less resistance in the coil than in the nerve. Nay, I am in a measure driven to this conclusion, for I cannot believe that the outflow of free electricity in this case should fail to act upon the needle in the very same way as that in which the outflow of

free electricity from an ordinary electrical machine has been found to act.

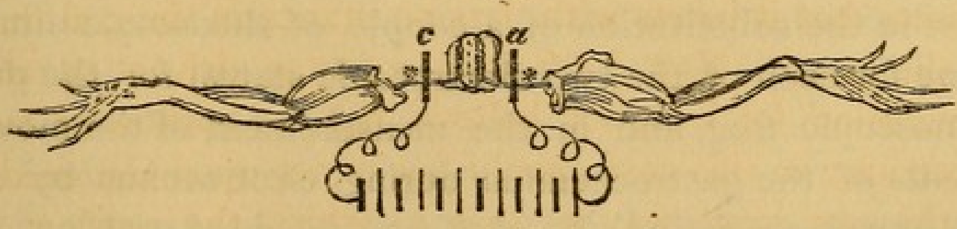
II.

1. Passing on from the electrotonic movements of the needle to the electrotonic modifications of neuro-muscular activity, the question of questions is as to the facts which have to be dealt with. Is it enough to say that this activity is suspended in anelectrotonus and exalted in cathelectrotonus, or is this only a partial statement of the truth? Are there points of resemblance between the two electrotonic states as well as points of difference? What, in short, are the actual facts? Any-one who has gone into the subject practically will know full well that it is no work of supereroga-tion to put these questions, and that I am quite in order in beginning, as I now do, by trying to find in actual experiment the answers of which I stand in need.

2. In carrying out this intention of seeking for these answers in actual experiment two methods of inquiry have been employed.

The first of these methods is that which is carried out more simply than the second, but it is less exhaus-tive. A galvanoscopic frog is arranged upon a plate of paraffin, with its two limbs stretched out in opposite direc-tions, and with the two exposed lumbar nerves resting at their middle, as in the accompanying figure (Fig. 15),

FIG. 15.



upon the two voltaic poles, and then a state of slight tetanus is set up in both the limbs simultaneously by applying a drop of strong salt water to the nerve on each side where it lies beyond the pole—at the points marked in the figure by asterisks—or else by faradizing with very feeble currents at the same spot. Until the limbs are slightly tetanized the voltaic circuit is open, and there is consequently no electrotonus; when the limbs are slightly tetanized the voltaic circuit is closed, first with two or more elements included in it, and then with only one element, and in this way electrotonus is set up—anelectrotonus in the limb and nerve on the one side, cathelectrotonus in the limb and nerve on the other side. And these are the results. If there be two or more voltaic elements in the circuit, the establishment of electrotonus is attended by the cessation of tetanus in both limbs—in the limb on the side of cathelectrotonus as well as in the limb on the side of anelectrotonus; if only a single element be used, the tetanus is suspended in the limb on the side of anelectrotonus when this state is set up by closing the circuit, but in the limb on the side of cathelectrotonus, instead of being suspended, it is usually (not always) exaggerated. These are the facts. And this is all that may be gathered by means of this method of inquiry, except this, that the contraction consequent upon closing and opening the voltaic circuit continues for a longer time on the side of anelectrotonus than on that of cathelectrotonus.

3. The second method of inquiry differs from the first in the substitution of a couple of rheoscopic limbs (one for each of the two electrotonic states) for the galvanoscopic frog, and in the measurement of the movements of the gastrocnemius during electrotonus by an apparatus consisting of a platform, provided with an in-

sulating gutta-percha top and with binding-screws for holding the electrodes belonging to the electrotonizing and faradizing apparatus, together with a set of multiplying wheels, a graduated scale, an index, and a string which has to be attached at one end to the tendo-achillis, and at the other to a hanging weight just heavy enough to put the gastrocnemius gently on the stretch, and which, by resting at its middle in the grooved rim of the wheel by which the other wheels are driven, obliges the index to move in face of the graduated scale this way or that whenever the muscle happens to lengthen or shorten.

When the object is to exhibit the action of anelectrotonus, one of the rheoscopic limbs (the other is reserved for the experiment in cathelectrotonus) is fixed on the platform by passing a pin through the knee into the gutta-percha top; and then, after tying the string of the measuring apparatus to the tendo-achillis, and arranging the sciatic nerve so that it lies across the voltaic poles, with the anode next the muscle, a drop of solution of salt is applied to the nerve between the anode and the knee, or else the action of feeble faradaic currents is brought to bear upon the same spot. When the object is to exhibit the action of cathelectrotonus the same arrangements are made for the other rheoscopic limb, with this single difference that the cathode, not the anode, is now next the muscle. And this is all that has to be done in these two cases, except to wait until the muscles are slightly tetanized, and then to set up the state of electrotonus by closing the voltaic circuit, first with two or more elements included in it, and then with one element only.

And this is what happens.

In the case of the limb in which the action of anelectrotonus is exhibited, the setting up of this state is seen to have the effect of suspending the tetanus, and of causing, in addition, the index of the measuring apparatus to move in a way which plainly shows that the gastrocnemius is slightly elongated ; and the only perceptible difference between the action of the stronger and weaker electrotonizing currents is in this, that the muscular elongation produced by the latter is somewhat less marked in degree.

In the case of the limb which has to respond to the action of cathelectrotonus, the results are the same as those which were noticed in the limb which was subjected to the action of anelectrotonus, if two or more voltaic elements are employed in producing the electrotonic state. The tetanus is suspended in both cases alike, and in both cases alike there is also muscular elongation, to the same degree even. But the results are not the same when a single voltaic element is made use of in producing the cathelectrotonic state ; for, in this case, instead of being suspended, the tetanus, as is plainly seen in the movements of the index of the apparatus, is often found to be considerably exaggerated by the electrotonus.

4. These, then, are the facts which are brought to light by carrying out these methods of enquiry, and the question is how to account for them. How is the suspension of tetanus, and the muscular elongation consequent upon electrotonus, to be accounted for ? What is the explanation of the exaggeration of tetanus which may happen in cathelectrotonus ? How is it that anelectrotonus has a greater power of suspending contraction than cathelectrotonus ? Why do the contractions, consequent upon opening and closing the voltaic circuit, come to an end sooner under the action of cathelectrotonus than under that of anelectrotonus ? And, certainly,

these are not to be looked upon as unanswerable questions.

The natural electrical state of quiescent nerve and muscle, according to the premises, is one in which the fibres and cells are electromotive centres in the state of open circuit, the coats being charged positively and the contents negatively by electromotive action. While charged in this manner, the condition of the muscle is that of relaxation or elongation ; when this charge is discharged contraction is the result ; and the broad conclusion is that the state of relaxation or elongation is consequent upon the presence, just as the opposite state of contraction is consequent upon the absence of this charge, the charge operating by setting up a state of mutual repulsion among the muscular molecules, the discharge doing its work by allowing the attractive force, or forces, inherent in the physical constitution of these molecules, to come into play. Hence it is not altogether unintelligible that the artificial charge belonging to the electrotonic states may set up a state of muscular elongation which is more marked than that which is natural to the muscle, for it may be supposed that the artificial charge is more powerful than the natural charge, and also that it may at the same time act like the natural charge in antagonizing or *inhibiting* contraction. Hence the exaggeration of tetanus in cathelectrotonus may be not altogether unintelligible, for if contraction be only the passive return from a previous state of elongation consequent upon the presence of charge, it follows that the contraction in electrotonus may be greater than that which is natural to the muscle, for the simple reason that there is in electrotonus a greater degree of elongation before the contraction than that which is natural to the muscle. And hence, too, the explanation of the fact that contraction is *inhibited* more effectively by anelectrotonus than by cathelectrotonus, and that the contraction on closing and opening the circuit continues for a longer time under anelectrotonus than under cathelectrotonus, for these differences, after all, may be nothing more than the natural consequence of the way in which the action of the artificial charge agrees with that of the natural charge in the one case, and disagrees in the other—the positive artificial charge imparted to the coats of the fibres in anelectrotonus agreeing with the positive charge which is natural to these coats, and so favouring the continuance of that natural state of charge which at once inhibits contraction and allows of contraction; the negative artificial charge imparted to these coats in cathelectrotonus disagreeing with the natural positive charge, and being, on this account, unfavourable to the continuance of that natural state of charge which at once counteracts contraction, and makes contraction a possibility. In a word, the explanation of the longer or shorter continuance of the contraction which happens in this case on the closing and opening of the circuit is the same as that which has been applied to the longer or shorter continuance of the same contraction under the "inverse" and "direct" current; for, in this latter case, the evidence goes to show that the differences in question have to do, not with differences in the direction of the current, but with differences in the charge associated with the current, the positive charge associated with the "inverse" current being favourable to the continuance of the contraction, the negative charge belonging to the "direct" current being unfavourable.

III.

Several experiments, of which the one following may serve as an example, all go to show that in sensory nerve the state of activity is suspended by electrotonus, and that, in this respect at least, the two kinds of nerve are in precisely the same case.

The nerve operated upon in this experiment is the sciatic of a small rabbit. The plan pursued is, first, to faradize the nerve with *weak* currents in the part which lies between the sensorium and the part included in the voltaic circuit, and, afterwards, to set up, in turn, first one electrotonic state, and then the other, by closing the voltaic circuit with the poles arranged so that the anode is in the position next the sensorium in the one case, and the cathode in the other. The faradaic currents made use of are only just strong enough to cause the animal to cry out a little and to struggle a little. The voltaic battery which supplies the electrotonizing current is made up of not less than three elements. Before the state of electrotonus is set up the effect of the weak faradaic currents is to cause the animal to cry and struggle faintly; when the state of electrotonus is set up these weak faradaic currents cease to tell in this manner, and the animal becomes perfectly still. In other words, that action of weak faradaic electricity which issues in sensation as well as motion, when there is no electrotonus, is suspended by electrotonus—by cathelectrotonus as well as by anelectrotonus, though not quite to the same extent by cathelectrotonus, for more voltaic elements are necessary to bring about this result in this state than in the anelectrotonic state. If the faradaic currents are over a certain strength, the animal may continue to cry 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 serves to counteract the action of weak induced currents, may itself be counteracted by increasing somewhat the strength of these currents; and thus, in order to the successful performance of this experiment, it is essential that the faradaic currents acting upon the nerve should be reduced to the necessary degree of weakness before the state of electrotonus is set up in the nerve.

IV.

What has been said upon the action of the "inverse" and "direct" currents upon sensory nerves, is, so far as it goes, 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 supplements the old story in a part which most needed confirmation. And what has been said upon 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 upon the action of voltaic electricity upon sensory nerves, agrees with all that has been said respecting the action of voltaic electricity upon motor nerve and muscle, for what seems at first to be disagreement, in the end changes into a still stronger proof of agreement. And certainly nothing which would lead to a contrary conclusion has been knowingly left unsaid.

V.

In what has just been said upon the workings of voltaic electricity upon muscle and nerve everything has helped to direct attention, *not to the constant current,* but to the *charge of free electricity,* positive or negative, associated with this current, and to the instantaneous currents of high tension, extra-currents or induced currents, which are developed at the moment when the voltaic circuit is closed or opened, or when (which is practically the same thing) the current fluctuates perceptibly in strength. The charge is seen to counteract or inhibit the state of action in both nerve and muscle, and to cause in muscle, not relaxation merely, but actual elongation—elongation which is greater than that which belongs naturally to muscle—while at the same time it keeps up in muscle and nerve alike the capacity for action ; and, with not less certainty, the instantaneous currents of high tension (extra-currents and induced currents) are found to have to do with the production of the state of action in nerve and muscle. In point of fact, the conclusion to which the evidence points broadly is not different from that which has been already drawn from the working of the other forms of electricity in vital motion. It is not different as regards franklinic electricity, for here it is manifest that charge has to do with the state of rest in both muscle and nerve, and discharge (which agrees with the extra-current or induced current in being an instantaneous current of high tension) with the state of action. It is not different as regards taradaic electricity, so far at least as the bringing about of the state of action is concerned, for this result is evidently a consequence of the working of instantaneous currents of high tension. Nor is it otherwise with the natural electricity which belongs to living muscle and nerve, for here the whole tenor of the evidence goes to show that the state of rest is coincident with the state of charge, and that this charge is discharged when this state of rest changes into that of action. In a word, the ruling of the same law is discernible in all these cases, and the sum of the whole matter amounts to this, that the different forms of electricity, the voltaic, the franklinic, the faradaic, and that which is natural to living substances, all agree in acting, not by polarization, or by any other working of the constant current, but by the charge or discharge of free electricity, the charge (the negative as well as the positive, but not to the same degree) causing the state of rest, and with it more or less expansion, by keeping the charged molecules in a state of mutual repulsion, the discharge bringing about action, and with it the state opposed to expansion, by leaving the now chargeless molecules free to yield to simple molecular attraction.

CHAPTER V.

ON THE ELECTROPHYSICS OF RIGOR MORTIS.

IN rigor mortis a great change has come over both muscle and nerve electrically. The muscle-current and the nerve-current are no longer present, or, if present, they are scarcely perceptible; and the signs of electricity which are brought to light by the help of the new-quadrant-electrometer, or any other instrument of the kind, are only those which belong to muscle and nerve in common with inorganic substances. The facts, indeed, would seem to show that muscle may have passed into the state of cadaveric rigidity because the attractive force which is inherent in the physical constitution of the muscular molecules is then no longer counteracted by the electricity which belongs specially to living muscle and nerve. Nay, so it must be if living muscle be kept in the state of relaxation by the presence of this electricity; for, in this case, it follows as a necessary consequence that muscle must pass into the state which is opposed to relaxation, and remain in this state, if this electricity be, as it is, withdrawn permanently. And thus, instead of there being anything at all peculiar in the electrical history of rigor mortis, there is good reason to believe that this state is nothing more than the result of the muscle being left permanently to the unresisted operation of molecular attraction by being permanently deprived of the electricity which belongs specially to living nerve and muscle, unless it be that the fibres of muscle and nerve have now, in addition to this, become more or less solidified by reason of a change in their contents which may be akin to that granulation which happens in those amœbæ in which amœbic movements are no longer manifested.

Nor is this conclusion respecting rigor mortis to be set aside by anything that remains to be said in one or other of the following chapters. On the contrary, all the evidence, implied or explicit, which has yet to be cited, is to the same effect ; and, so far as I can gather, there is nothing in the electrical history of rigor mortis which is in the least degree at variance with the conclusions already drawn respecting the electrophysics of vital motion.

CHAPTER VI.

ON THE WORK OF THE BLOOD IN VITAL MOTION.

1. THE facts which belong to this chapter have nothing enigmatical about them, and it matters little how they are dealt with. Indeed, unless the judgment be biassed to begin with, they must, however placed, serve as safe stepping-stones by which it is easy to get across any boggy ground in the argument without having to make a long and uncertain jump anywhere.

2. The fact that death by bleeding is attended by violent convulsion of every muscle in the body is, to my mind, altogether unintelligible if it be taken for granted that the work of the blood in the system is that of a stimulus to vital motion. Privation of blood and convulsion stand together in the relation of cause and effect, and, so far as I can see, there is no escape from the conclusion that, in this case at least, the action of the blood, be this what it may, must have told upon the muscles *before* the convulsion rather than *in* it—that this action may have helped to keep off the convulsion rather than to bring it about.

3. The same story is also told, though not quite so plainly, by the fact that death by strangling is in like manner attended by general convulsion. For, as in the last case so in this, it is surely more easy to suppose that the convulsion has to do with want of arterial blood simply, than to agree with Dr. Brown-Séquard in believing that it is due to the stimulating action of the

black blood with which the arteries as well as the veins are than filled to the full. Indeed, while one cause will serve equally for both cases, I cannot bring myself to think that two causes are really at work; and for this reason I shall, until I am obliged to shift my position, continue to believe that, in death by strangling no less than in death by bleeding, want of arterial blood is the common cause of the convulsion, and that the possibility of finding such common cause is in itself a sufficient ground for rejecting the notion that in death by strangling the neuro-muscular system is stimulated into convulsion by the carbonic acid in the venous blood with which all the vessels in the system are then loaded.

4. In perfect keeping with these familiar facts are also three experiments, one by Sir Astley Cooper,* two by Drs. Kussmaul and Tenner,† in each of which it appears very plainly, not only that convulsion is connected with want of arterial blood, but also that this want tells directly upon certain great cranio-cervical nerve-centres, while in one of them it is further shown that convulsion has no connection with the opposite state of things—a state, that is, of things in which these centres are overloaded with blood.

"I tied," says Sir Astley Cooper, "the carotid 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 thumbs, the trachea being carefully excluded. Respiration stopped almost directly, convulsive struggles succeeded, the animal lost consciousness, and appeared dead. The pressure

* Guy's Hospital Report, No. III, 1836.

† Untersuchungen z. Naturlehre der Menchen, u. d.. Thiers: Moleschott, vol. ii.—Frankfort, 1859.

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, with the same effect. In seven hours it was cleaning its face with its paws. In nine hours, the vertebrals 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."

In the first of the two experiments by Drs. Kussmaul and Tenner, the plan is to put ligatures, with slip-knots, around the common innominate and left subclavian arteries of a rabbit,—the only two great vessels, that is, proceeding from the arch of the aorta in this animal—to tie them, to leave them tied for about a couple of minutes, and then to untie them. And this is what happens. On tying the ligatures, convulsion is at once set up everywhere. On untying them a couple of minutes later, when, that is, the convulsion is still raging at its height, all the muscles are suddenly and completely relaxed—as suddenly and completely as if the animal had then been struck down by a stroke of paralysis. There can be no doubt that the convulsion comes on when the blood is shut off from one or other of the great cranio-cervical nerve-centres. There can be no doubt that the convulsion ceases at the very moment when the blood is allowed to return to these centres. No other conclusion is possible.

In the second of the two experiments by Drs. Kussmaul and Tenner, the supply of blood is cut off from the trunk and limbs, and diverted to the head, by tightening ligatures around the descending aorta and the two subclavian arteries of a rabbit, and then the supply of blood to the head and neck is successively shut off and turned on by compressing the carotids between the fingers, and by ceasing to do so. And what happens is this. When the blood is shut off from the parts below the neck, by tying the ligatures, all these parts, so far as they have to do with movement, are at once paralysed. When the blood is also shut off from the head, by compressing the carotids between the fingers, general convulsion follows instantly. When the blood is allowed to return to the head by removing the fingers from the carotids, the former state of paralysis returns as instantly. As in the two last cases, so in this, convulsion comes and goes concurrently with the moments when the flow of blood to the head is barred and unbarred. The more plentiful supply of blood to the head in this case makes no difference in this respect: it only serves to give point to the same story, by making it evident that the effect of an unusually plentiful supply of blood to the great cranio-cervical nerve-centres—a state which may be supposed to be not unlike that to which the name of "active determination of blood to the head" is given—is, not convulsion, but paralysis.

5. Want of arterial blood would also seem to have to do with the spasms arising from the poisonous action of strychnia or brucia, for one way in which these poisons work mischief is, as is seen in the experiments of Dr. Harley,* by making the blood less capable of aëration—by bringing about, that is to say, a change which is virtually equivalent to loss of arterial blood.

In one of the experiments in which this important fact is brought to light, air which had been over simple blood for twenty-four hours, and air which had been for the same time over blood containing a small quantity of strychnia, is examined by Bunsen's method, with this result:—

	Composition of common air.	Composition of air after having been over *simple blood* for 24 hours.	Composition of air after having been over blood containing strychnia for 24 hours.
Oxygen	20·960	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 blood made use of was collected as it flowed from an opening in the jugular of a calf, and the air and it were in about equal quantities. The plan pursued was to take two tolerably large test-tubes, to fill them half full of blood, to add to one of them two or three drops of solution of strychnia, to cork them both carefully, to set them aside with their mouths downwards, and, except at certain times when they are taken up for the purpose of shaking the air and blood well together, so to leave them until it was time to examine the air

* "Lancet," June and July, 1856.

which had been bottled up all this while. At the end of the twenty-four hours, when the air was removed and examined by Bunsen's method, it was found that the air which had been over the blood containing strychnia, had been less acted upon by the blood than the air which had been over the simple blood, in that it contained more oxygen and less carbonic acid and nitrogen—a state of things which may be looked upon as pointing to a change in the blood which is equivalent to loss of blood; for it is evident that the strychnia has had the effect of diminishing those respiratory reactions between the blood and the air which issue in the absorption of oxygen and the elimination of carbonic acid and nitrogen, and that blood so altered is as good as lost for all vital uses. Nay, it may be supposed that the change so wrought in the blood in fatal cases of poisoning by strychnia or brucia (for the only difference in the action of the two poisons is in the last-named being the less energetic of the two) is equivalent to copious loss of blood, for in the experiment under consideration, as may be seen in the table, a very minute quantity of the poison had the effect of lessening the respiratory reactions between the blood and the air to the extent of full two-thirds of the natural amount.

And thus, as in the cases of convulsion already described, so in the case of the spasms produced by strychnia or brucia, there is reason to connect the excessive involuntary muscular action with the withdrawal of arterial blood,—to suppose, in fact, that the action of the blood tells in preventing muscular action rather than in producing it.

6. There are also two experiments, one by the late Dr. John Reid,* of Aberdeen, the other by Professor

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

Draper,* the younger, of New York, in which it is seen that in asphyxia black blood gets through the smaller vessels less readily than red blood, and from which, for this reason, it may be inferred that the state of vascular contraction is more effectually counteracted by red blood than by black blood.

Taking a rabbit, and laying bare the windpipe and the great vessels at the root of the neck, Reid proceeds, first to connect a hæmadynamometer with one of the carotids, and then to suffocate the animal by putting a ligature around the windpipe, and tying it. Before suffocation, the hæmadynamometer marks what may be spoken of as the ordinary force and frequency of the pulse, and the artery is easily distinguished from the vein by being somewhat smaller, and by (the difference in the colour of the arterial and venous blood showing very plainly through the coats of the vessels) its red colour: after suffocation, the red colour of the artery rapidly changes into the black colour of the vein, and in about two minutes, when, that is, the process of suffocation is at its height, the artery is as black as the vein, and there is obviously more distension of the artery and less of the vein than there was at first, while, at the same time, the force of the pulse, as registered by the hæmadynamometer, is well nigh doubled. This is what is noticed before and after the establishment of suffocation. Black blood finds its way from the veins into the arteries, and is detained there because it does not get through the intermediate vessels with the same readiness as red blood. The state of things, indeed, is one which justifies the conclusion that the minute vessels between the arteries and the veins are kept less open by black blood

* "Lectures on the Physiol. of the Circulation." "Amer. Med. Monthly," April, 1860.

than by red blood—a conclusion too in which this seems to be involved, that the red blood has a more marked power of counteracting or inhibiting a state of contraction in the coats of these minute vessels than red blood.

In Draper's experiment, the heart and its great vessels are laid bare in a rabbit, and then a ligature is put around the trachea and tied tightly. At first, while the ligature remains untied, the red and black sides of the heart, and the great vessels near the heart, are of their natural dimensions and colours; afterwards, when the trachea is closed by the ligature, 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 arterial blood darkens into venous.

And so the fact that the arteries are more and more distended with black blood as the process of suffocation goes on, and that the pulse becomes fuller and firmer as the blood in the arteries becomes more and more venous —a fact which is altogether at variance with the current belief that the arterial pulse fails rapidly in suffocation, and that the venous system as rapidly becomes gorged with black blood—may serve to show that the action of the blood is to prevent rather than to provoke a state of contraction in the minute vessels between the arteries and the veins; for, after what has been said, it is more easy to refer the contraction in these vessels (in consequence of which the arteries are filled at the expense of the veins) to the simple absence of red blood, than to the stimulating presence of black blood.

7. A similar conclusion may also be drawn from certain experiments by Dr. Spiegelberg,* of Gottingen, on the action of the blood upon the peristaltic movements of the bowel of a rabbit. In some of these, such movement is seen to be increased by pressing upon the abdominal aorta so as to prevent the admission of red blood into the vessels of the bowels, and to be diminished when, by removing this pressure, the blood is allowed to return into the empty vessels. In others, the same movements are seen to be increased, though not to the same extent, when the intestinal vessels are kept full of venous blood by pressing upon the vena cava or vena porta, and to be diminished when, by removing this pressure, these vessels are allowed at one and the same time to get rid of their load of black blood and to receive fresh supplies of red blood. Relaxation, not contraction, is associated with the presence, and contraction, not relaxation, with the absence of red blood: it is, indeed, as if the disposition to peristaltic movement were inversely related to the supply of blood to the coats of the alimentary canal.

8. Nor is a different conclusion to be drawn from the action of the blood upon the muscle of the cardiac ventricles; for here the simple fact appears to be—that the minute vessels of the muscle receive a fresh supply of arterial blood as the diastolic state sets in; that this diastolic state continues until this arterial blood is used up; and that the state of systolic contraction is coincident with the moment when the arterial blood becomes venous. Here, also, it seems as if the action of the blood, be this what it may, has to do with muscular relaxation rather than with muscular contraction. And if so, then no different conclusion need be drawn from

* Henle u. Pfeuffer's Zeitschrift.—3 Reihe ii, 1857.

the action of the blood upon the muscle of the cardiac auricle; for, as has been sufficiently pointed out already in the chapter on cardiac and other rhythmical movement, the movements of the auricles must in the main be resolved into mere passive consequences of the movements of the ventricles.

9. Again: the muscles which are less vascular are more prone to enter into, and remain in, the state of contraction than the muscles which are more vascular. Thus, the less vascular voluntary muscles of reptiles and fishes are more prone to contraction than the more vascular muscles of birds and animals. Thus, the less vascular involuntary muscles of any animal are more prone to contraction than the more vascular voluntary muscles of the same animal. And thus, again—a fact which shows that in the two former cases comparative bloodlessness and proneness to contraction, are really connected as cause and effect—the muscles of a dormouse, or any hybernating animal, are more prone to contraction in the state of hybernation, when the circulation is almost at a standstill, than in the period of 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, in which the action of the blood upon muscle and nerve, be this what it may, is exhibited as being favourable to the state of rest, and unfavourable to the state of action; and, so far as I know, the facts which are in any way at variance with these have yet to be discovered.

10. And lastly, another version of the same story may be found in certain experiments in which Drs. Brown-Séquard* and Stannius† tested the action of the blood

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

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

upon muscle which had passed into the state of rigor mortis.

On the 12th of July, 1851, Dr. Brown-Séquard began an experiment which consisted in again and again injecting a pound of defibrinated dog's blood into the principal artery of the arm of a criminal who had been 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 little later, some reddish spots, not unlike those of measles, made their appearance, about the wrist more particularly. Then these spots became larger and larger, until, by their meeting and merging, the whole surface acquired a reddish violet hue. Shortly afterwards, 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 recovered their electro-contractility, first in the fingers, afterwards in the shoulder. At 11.45 p.m., this contractility 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 contractility. 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 irritable. 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 muscles everywhere were soft, and 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, the late 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, ligatures were put around the abdominal aorta and crural arteries of a puppy, and tied tightly. 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 cold. 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 fully recovered their flexibility,

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

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 under the action of electricity, and once there seemed to be pain, for the animal, which was before quiet, gave a sudden plunge forward, when it was electrified. 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 paralysed hinder limbs being still perfectly supple, the muscles below the knee had ceased to respond to the action of electricity. At 2.15 p.m. both these limbs were stretched out and rigid. At 2.35 p.m. the ligatures were untied. At 3.35 p.m. electricity 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 detected anywhere. At 5.35 p.m. the muscles, now 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 rigid and inactive. In the evening of the day following there was no alteration in this respect. 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 putrescent., 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 to recover their lost suppleness and contractility by again supplying them with blood.

The whole drift of the present chapter is strictly in accordance with that of the preceding chapter, and the sum of the whole matter so far seems to be this—that the action of the blood in vital motion may be really resolvable into that of electricity—that, in short, the blood may antagonize the state of action in nerve and muscle, because its oxygen has to do with the keeping up of that electro-motive condition in nerve and muscle which antagonizes the state of action in nerve and muscle, and which in muscle keeps up, in addition, the state of relaxation. The evidence, in fact, is as much in harmony with this view as it is in opposition to the dogma that blood produces contraction by acting as a stimulus to a vital property of irritability inherent in living muscle and motor nerve.

CHAPTER VII.

ON THE WORK OF THE NERVOUS SYSTEM IN VITAL MOTION.

I.

1. IN order to arrive at anything like exact knowledge respecting the work of the nervous system in vital motion, the way which to me seems least roundabout, and which is selected for that reason, is to jot down some of the chief points in the anatomy and physiology of the nervous system, and to comment upon them as far as may be necessary, without any particular regard to the order of the remarks.

2. A few words will serve for what need here be said about the *cerebral hemispheres.* These parts are evidently anatomically distinct from the corpora striata and thalami optici from which they spring. From them start the impulses which issue in voluntary movement. If one be removed, the opposite side of the body is paralysed; if its convolutions be faradized in certain districts, as in the recent experiments of Dr. Ferrier, certain groups of muscles are set in action, not directly, perhaps, but indirectly; for the experiments of Dr. Dupuy and Dr. Burdon Sanderson, as it seems to me, go to show that this localization is really due to the faradaic action having penetrated downwards until it reaches certain parts of the corpora striata and thalami optici, which parts are, as it were, played upon from the brain proper

as are the keys of an organ by the fingers. There is, indeed, reason to agree with Dr. Carpenter in believing that the cerebral hemispheres communicate with the muscles, not directly, but indirectly, and that "the movements which are usually designated as 'voluntary,' are only so as regards their original source, the stimulus which calls the muscles into contraction being even then immediately issued from the cranio-spinal axis, as it is in the movements prompted by the original reflex stimulation of an external impression."* In a word, there is reason to believe that the radiating fibres of the cerebral hemispheres originate at the corpora striata and thalami optici—that their office, in fact, is mainly, if not wholly, commissural.

3. The parts of the nervous system which underlie the cerebral hemispheres—the cranio-spinal axis (consisting of the corpora striata and thalami optici, the corpora quadrigemina, the crura cerebri, the medulla oblongata, and the spinal cord), the cerebellum and pons varolii, and the vaso-motor ganglia, are not so easily disposed of; for it is here that the manifold workings of the nervous system in vital motion are made known in a way which can only be understood by going into some detail.

4. The *corpora striata* and *thalami optici* are in connection superiorly with the radiating fibres of the cerebral hemispheres, and inferiorly with the crura cerebri, upon which they are implanted. They are also connected with each other by grey matter as well as by white, the two on the same side so closely as to make it somewhat difficult to conceive of them as really separate, the two on opposite sides less closely by certain

* "Principles of Human Physiology," 6th vol., p. 460.

commissural fibres, by those of the soft commissure more especially. Indeed, as Dr. Todd suggested, it is not improbable that the two on the same side have much the same relation to each other as that which exists between the anterior and posterior peaks of vesicular matter in the spinal cord. The corpus striatum is implanted on the motor tract of the crus cerebri through which the orders of the will are transmitted—the will-tract; and it seems to be the great motor centre of the cranio-spinal axis upon which the will plays in producing voluntary movement; the thalamus opticus, on the other hand, is said to hold the same relation to the sensory tract of the crus cerebri and its sensory nerves as that which the corpus striatum holds to the motor tract and its motor nerves; but upon no very sufficient grounds, as it would seem. At all events, only this appears to be clearly made out, that injury to either corpus striatum is followed by paralysis, with more or less loss of sensation on the *other* side of the body; and that injury to one of the thalami optici results in strange turning or rolling movements, towards the right if the part injured be to the front, towards the left if this part be behind, together with more or less permanent contraction of the muscles, on the *same* side as that of the injury generally, and without any very perceptible paralysis or loss of sensation anywhere.

5. The *corpora quadrigemina* are connected, anteriorly, with the back of the thalami optici and with the crura cerebri, and, posteriorly, with the superior peduncles of the cerebellum; and injury to the pair on one side is followed by turning or rolling movement, with more or less permanent contraction in the muscles on that side.

6. The *crura cerebri* connect the corpora striata and

thalami optici with the medulla oblongata, and they also receive some fibres from the corpora quadrigemina. They contain the two tracts concerned in the transmission of the impulses which lead to voluntary motion and sensation, the former tract being the hindermost of the two. In addition to the grey central mass to which the name of locus niger is given (and which, as it would seem, is directly continuous with the grey matter in the corpora striata and thalami optici in one direction, with that of the corpora quadrigemina and corpus dentatum of the cerebellum in another, and with that of the spinal cord in a third), they also contain many fibres—passing chiefly from the olivary bodies to the corpora quadrigemina—which do not belong to either of these two tracts, and which, when injured, respond by giving rise, not to voluntary motion or sensation on the opposite side of the body, as is the case when the part injured is in the will-tract or sensory tract, but to turning or rolling movements, with more or less permanent contraction in the muscles on the side to which these movements are directed. It would also seem that the direction of this turning or rolling varies with the part injured, the movement being (when the right crus is damaged) towards the right side when the part is near the pons varolii (Schiff), and towards the left if it be near the thalamus opticus (Majendie).

7. The *cerebellum* is connected with the restiform bodies of the medulla oblongata by the inferior cerebellar peduncles, and with the corpora quadrigemina and optic thalami by the superior cerebellar peduncles, and it receives through the middle cerebellar peduncles the fibres of the *pons varolii*, which is the commissure uniting the two lateral lobes of the cerebellum. Injury to the cerebellar lobes shows itself in various ways, but

never in true paralysis or loss of feeling or unconsciousness. A constant result is loss of that power by which co-ordinated muscular movement is made possible—a loss which, when extreme, carries with it utter inability to stand or move about A frequent result is persistent tremulousness in the muscles of the body generally, resembling paralysis agitans, or else a state of tonic spasm, which may manifest itself in one case as torticollis, and in another in a stiff and out-stretched state of the legs. A less frequent result is an irresistible impulse to turn or roll, or to rush forwards or backwards. Turning or rolling in one direction or another, with some slight degree of permanent contraction in certain muscles, or else more marked spasm, without turning or rolling, is also found to be caused by injury to the pons varolii and to the middle cerebellar peduncle ; and, as in the case of the cerebellar lobes, so here, it is difficult to say beforehand whether any particular injury will result in turning or rolling rather than in spasm, or in spasm rather than in turning or rolling. Indeed, all that can be affirmed as yet with certainty is, that in the pons varolii, any injury, in the lateral regions more especially, is almost sure to give rise to more or less persistent spasm on the same side of the body as that of the injury, and that this tonic contraction is unaccompanied by any appreciable paralysis or loss of sensation on the other side, or anywhere.

8. The *medulla oblongata* consists of many tolerably well defined parts, to each of which attention must be directed. Each one of the *anterior pyramids* is continuous, above, with the will-tract of the crus cerebri on the *same* side, and, below, with the lateral column (and to some small extent, also, with the contiguous parts of the posterior column) of the spinal cord on the

other side, the decussation of each with the other being effected at its lower extremity; and injury to either shows itself in complete paralysis of the muscles below the head on the *other* side of the body, without any alteration of sensation anywhere, and without any symptom of a spasmodic or convulsive character. The two anterior pyramids, indeed, seem to be the sole channels for the transmission of the impulses which lead to voluntary motion. The *olivary bodies* are in connexion, superiorly, with the corpora quadrigemina, and hinder portions of the optic thalami, through the tracts of the crura cerebri which have to do with automatic, instinctive, reflex movement, and, inferiorly, with the anterior columns of the spinal cord, and with the edges of the lateral columns contiguous to them. Transverse section of either is not followed by appreciable paralysis or alteration of sensation anywhere; a deep puncture, on the other hand. gives rise, without loss of time, to contraction in the muscles below the line of injury on the same side of the body, which contraction may continue without abatement, not only for hours or days, but for weeks and months even. Each of the *lateral columns* is continuous, at its upper end, with bundles of fibres of which some pass into the middle cerebellar peduncle and some into the crus cerebri, while others intercross commissurally with their fellows behind the corpora quadrigemina, and, at its lower end, with the anterior column of the spinal cord, the two lateral columns and the two anterior pyramids decussating in the same place ; and as yet no light is thrown upon their particular office, either by physiology or pathology. The *tubercle of Rolando* is simply the expanded and exposed portion of the posterior cornu of the grey substance of the spinal cord, and its special office, if it have one, has yet to be discovered. The *restiform bodies* are the connecting links between the inferior cerebellar peduncles, on the one hand, and the posterior (and partially, also, the lateral) columns of the spinal cord ; and marked incoordination of movement, without paralysis, without loss of sensation, without spasmodic or convulsive disturbance, is, as it would seem, the sole and invariable result of an injury to them. The *posterior columns* are in connexion, at their upper ends, with fibres which pass into the sensory tracts of the crura cerebri, and, at their lower ends, partly with the lateral and partly with the posterior columns of the spinal cord; and their office has yet to be determined. Turning and rolling movements, with more or less muscular contraction, is also the result of irritating the parts near the roots, or the roots themselves, of the great nerves which proceed to or from the medulla oblongata—the glossopharyngeal, the vagus, and the spinal accessory, or the facial and the auditory; indeed, in the case of the latter nerve, irritation of the expansion of the nerve within the ear itself is followed by the same result as that which attends upon irritation of the trunk of the nerve lower down. Moreover, the effect of a marked injury to almost any part of the medulla oblongata is to arrest or greatly disturb the breathing, and to bring about strong convulsion of an epileptiform character. This is certain. What is less so is the notion that this result is brought about by the injury having acted directly upon some one focus of respiratory rhythmic power, which one focus is seated somewhere in the grey matter of the medulla oblongata. In fact, the investigation of Dr. Brown-Séquard have made it evident that at most here is only a chief focus of such power, and that there are others, and scarcely minor foci, elsewhere in the cranio-spinal axis, even in all those parts which are directly connected with the nerves going to all the respiratory muscles.

9. The *spinal cord* is a continuation and simplification of the medulla oblongata. The fibres composing the anterior and lateral columns, in the uppermost part of the cervical region, descussate laterally, so that, on the one hand, the upper end of the anterior column is made up of fibres which elsewhere went to form the lateral column, and, on the other, the upper end of the lateral column is made up of fibres which elsewhere are gathered together in the anterior column. After this, the fibres forming the upper ends of the two columns again alter their course, those of the lateral column on one side passing over to the anterior pyramid of the other side, those of the anterior column entering the olivary body of the same side. The fibres forming the posterior columns, on the other hand, are in direct connexion with those which go to make up the corresponding restiform bodies, without any descussation beyond that which seems to take place, to a greater or less degree, everywhere along the lines which separate contiguous columns, or which mark the boundaries between the white and grey matter.

Thus it is that the effect of injuries differs in different parts of the anterior and lateral columns, but not so in different parts of the posterior columns. At the upper end of the *anterior column* this effect is, not paralysis or loss of sensation, but more or less peramanent spasm in the muscles below the head on the same side: whereas in the anterior columns elsewhere, it is paralysis in the muscles below the line of injury on the same side, and paralysis only. At the upper end of the *lateral columns*, the effect of injury is paralysis in the muscles below the head, on the same side, and paralysis only ; whereas in the lateral columns elsewhere, it is more or less permanent spasm below the line of injury, with some paralysis, on the same side—a result which seems to point to some connexion between the lateral columns and the olivary tracts, on the one hand, and between the lateral columns and the anterior pyramids on the other. In the *posterior columns*, the effect of injury anywhere is, not paralysis, or loss of sensation, or persistent muscular contraction, but incoordination of movement and hyperæsthesia. This is what happens constantly in this case. The facts are in flat contradiction to the notion that the posterior columns are merely bundles of fibres going up from the posterior roots of the spinal nerves to the encephalon. In the *grey matter*, the effect of dividing one lateral half is paralysis, with hyperæsthesia, in the parts below the line of section on the *same* side, and anæsthesia, without paralysis, in the corresponding parts on the *other* side. The effect of this injury, that is to say, is to make it evident that very many of the motor conductors which have to do with voluntary motion, and nearly all the sensory conductors belonging to the senses of touch, temperature, tickling, and pain, as well as to the muscular sense, are contained in the grey matter; and this also, that the sensory conductors ascend, not in the lateral half of the grey matter into which they first enter, but in the other lateral half, the decussation with their fellows being effected at or about the level of their entrance into the cord; and that the motor conductors descend all the way from the medulla oblongata to the anterior roots of the nerves belonging to them in the same lateral half of the grey matter, without decussating anywhere with their fellows in the other half. It is not possible, however, to localise these various effects with strict exactness, and, in fact, the different columns of the cord, and the grey matter as well, are so connected as to make it impossible to injure one part without injuring other parts also.

There are also certain experiments which bring to light other functions of the spinal cord, and of these it is expedient to mention three or four particularly.

One of these, which is Marshall Hall's old and fundamental experiment of cutting across the spinal cord of a frog in the middle of the back, and of setting up reflex contractions in the paralysed hinder limbs by pinching the toes, shows that a marked increase of reflex excitability is the result of this operation.

Another, by Dr. Brown-Séquard, is one in which, after cutting across the spinal cord of a frog in the middle of the back, and dividing the lumbar nerves on one side, but not on the other, the lumbar nerves on both sides are feebly faradized to exactly the same extent, with the result of finding that the contractions in the hind limbs, so caused, are more powerful in the limb whose nerves are cut off from the spinal cord than in the limb whose nerves are not so cut off.

A third experiment, also by Dr. Brown-Séquard, brings out in a very striking manner the fact that the reflex contraction which is produced in the hind leg of a frog by pinching the toes, is capable of raising a heavier weight after the cord is cut across in the middle of the back than that which was raised by contraction produced in the same way in the same limb before the operation, with this in addition, that the contractile power so manifested goes on increasing for some time, and is slow to decrease, the grammes actually raised at different times in two frogs, A and B, being :—

		A.	B.
Before division of the cord		60	60
After 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 46 "	150	140

At this highest figure the force of the contraction remained stationary for several days. Afterwards, it began to decline slowly, but so slowly that a month later the weight raised was still somewhat heavier than that which had been raised before the division of the cord. And it is suggested that even this slow loss of power would have been prevented if due care had been taken to keep the muscles in good trim by exercising them electrically.

The other experiments, which show that the destruction or removal of the spinal cord is followed by loss of muscular tension, are thus briefly sketched by their author, Dr. Marshall Hall, but nothing is really omitted which is necessary to complete the picture. "Two rabbits were taken; from one the head was removed; from the other, also, the head was removed, and the spinal marrow cautiously destroyed by a sharp instrument; the limbs of the former retained a certain degree of firmness and elasticity; those of the second were perfectly lax." Again: "The limbs and tail of a decapitated turtle possessed a certain degree of firmness or tone, recoiled on being drawn from their position, and moved with energy on the application of a stimulus. On withdrawing the spinal column gently out of its canal, all these phenomena ceased. The limbs were no longer obedient to stimuli, and became perfectly flaccid, having lost all their resilience. The sphincter lost its circular form and contracted state, becoming lax, flaccid, and shapeless. The tail was flaccid and unmoved on the application of stimuli."

10. The *vaso-motor or sympathetic system of nerves* has very wide and intimate connection with the cranio-spinal axis, and almost any injury to any part of this axis, in addition to the other effects which have been noticed, is also accompanied by signs of vaso-motor paralysis or "irritation." Hyperæsthesia, for example, which really points to a bloodshot state depending upon vascular relaxation from vaso-motor paralysis, is the effect of dividing any part of the cranio-spinal axis from the corpora quadrigemina down to the lower end of the spinal cord, and, to a less degree, of making a transversal incision into the cerebellum or superior cerabellar peduncle. It would seem, indeed, as if vaso-motor nerves entered everywhere into the composition of the great nerve tracts which are concerned in the production of automatic, instinctive, reflex movements. At all events, the effects of injury to the vaso-motor nerves themselves, and to the parts of the cerebro-spinal axis with which they are specially connected, are one and the same.

The removal of the inferior cervical ganglion, or the division of the cervical filament of the sympathetic in a rabbit, results in rapid increase of warmth and vascularity in the corresponding side of the head and face—the temperature rising several degrees, the white of the eye, the lining membrane of the nostril and ear, and the skin of the side of the head and face generally, becoming bloodshot and tender, the pulse gaining considerably in force and fulness, and so on—and this state of things may continue, wirh little or no change, for weeks, perhaps for months. The faradization of the trunks of the nerves so paralysed puts an end for the time to all these signs of vaso-motor paralysis, provided only the faradaic currents themselves are not so strong as to help to perpetuate the state of vaso-motor paralysis. And what happens in this case is precisely what happens when the side of the spinal cord in the immediate neighbourhood of the cervical sympathetic is cut or electrified to a certain extent; and in all other cases in which vaso-motor nerves, or their cranio-spinal connexions, are acted upon in the same way, the results are substantially the same. Moreover, in some instances at least, vaso-motor nerves would seem to act, not steadily, but rhythmically. In the case of the rhythmic centres of the heart (which belong to the vaso-motor system), it is certainly so in the case of the vaso-motor nerves belonging to the ordinary minute vessels there is good reason for thinking that in a lesser degree it may be so; and thus it is that the vaso-motor centres, like the respiratory nerve centres, may have very important work to do in the initation and regulation of rhythmic movement of various sorts.

II.

1. Here, then, are facts in connexion with which question after question must arise in the mind of any one who is wishful to know anything of the work of the nervous system in vital motion. Why is it that the muscles of the hind leg of a frog may be made to contract more readily and more powerfully when they are cut off from the brain and the upper half of the spinal cord

by dividing the cord in the middle of the back, and more so still when they are also cut off from the lower half of the cord by dividing the lumbar nerves high up? Why is loss of muscular tension consequent upon the destruction or removal of the spinal cord? How is it that the action of the nerve-centres is intermittent in some cases, and not in others? How is it that injury to different parts of the cerebro-spinal apparatus may give rise to spasm in one case, to convulsion in another, to trembling in a third? In what way is voluntary movement brought about? These and other questions crowd upon the mind, and I know of no better way of attaining the end I have in view than by taking each of them in turn and making it a text for what I wish to say.

2. Viewed apart from theory, the fact that the muscles of the hind leg of a frog may be made to contract more readily and more powerfully when they are cut off from the brain and upper part of the spinal cord by dividing the cord in the middle of the back, and more so still when they are also cut off from the lower half of the cord by dividing the lumbar nerves high up, would seem to show that one way in which the great cerebro-spinal apparatus must work upon the muscles is, not by provoking muscular contraction, but by preventing it. And this view is that which is necessitated by the premises, for according to these all that is necessary to account for the increased disposition to muscular contraction, is to suppose that the muscles have been left more free to yield to the attractive force inherent in the physical constitution of their molecules, by the withdrawal of an influence which previously kept these molecules in a state of mutual repulsion,—an influence which, in this particular case, may be none other than the electricity imparted to the muscles from the great

cerebro-spinal nerve-centres. Upon this view the case is not unintelligible; upon the current view of nervous action in muscular motion, as it seems to me, it is utterly unintelligible.

3. Upon the same view, the loss of muscular tension which follows the destruction or removal of the spinal cord is not less readily accounted for. For the case, according to the same premises, is simply this—that the action of the natural electricity belonging to nerve and muscle during the state of rest shows itself in causing, not relaxation merely, but actual elongation of the muscular fibres—shows itself, that is, in causing a state which may well be spoken of as muscular tension. And if so, then it follows, as a matter of course, that the destruction or removal of the spinal cord, by the simple withdrawal from the muscles of the electricity imparted to them from the cord, must show itself, not in muscular contraction, but in an equivalent loss of muscular tension; for, until the occurrence of rigor mortis, the muscle, if quiescent, is prevented from passing into the state of contraction by the action of its own natural electricity.

4. In the chapter which treats of cardiac and other rhythmical movements a good deal was said which, when followed out a little further, may supply the answer to the question why it is that some nerve-centres act intermittently and others not. It was supposed that a state of electromotive action was set up in the rhythmic centres within the substance of the heart by every fresh jet of arterial blood, and that as long as this action remained constant the muscle of the ventricle was in the state of diastole; and further, it was supposed that there was a failure in this action when the blood ceased to be arterial, that this failure was attended

by the development of an instantaneous current of high tension, that a moment later when electromotive action was being restored by a fresh jet of arterial blood a second instantaneous current of high tension was developed, and that these two instantaneous currents, the second following so quickly upon the heels of the first as to be almost confounded with it in point of time, gave rise to the ventricular systole. And so also in the case of the rhythmic centres which have to do with the respiratory movements, with this difference only, that the electro-motive action "runs down" less quickly. The view taken was one which has only to be a little widened in order to take in the nerve-centres which do not act intermittently, as well as those that do. Indeed, the sole difference between the two cases may be this—that the electromotive action of the centres which do not act intermittently does not "run down" so quickly as that of the centres which do act intermittently;—that, in fact, there may *not* be time in the former case for any appreciable "running down" of electromotive action in the intervals between the moments in which fresh arterial blood is supplied to the centres, and, consequently, no opportunity for the development of the instantaneous currents of high tension which give rise to muscular contraction; for if the electromotive action remain constant, there can, of course, be no such development, and, according to the view under consideration, no contraction. And, certainly, there is nothing in this notion which can be set down as altogether fanciful; for all that is needed, in order to make it practical enough, is to suppose that there are differences in the constitution of the electromotive elements of the different nerve-centres which are analogous to those which are met with in batteries of which the voltaic elements are composed of different heterogeneous materials.

5. Why permanent spasm should result from injury to certain parts of the cerebro-spinal axis is a wider and more difficult question: but here, also, the line of inquiry which has to be followed out is laid down with tolerable clearness. It is plain that this line is always somewhere within the tract of the nervous system which has to do with automatic, instinctive, reflex movement, and which takes in the hindermost portions of the thalami optici, the corpora quadrigemina, a great part of the superior and middle cerebellar peduncles, some undefined portions of the cerebellar lobes, a great part of the crura cerebri, a great part of the pons varolii, the olivary bodies more especially, in part the lateral columns of the medulla oblongata, the upper ends of the anterior columns of the spinal cord, and very probably the entire length of the lateral columns of the cord with the exception of their uppermost extremities. It is plain that the fibres belonging to the two lateral halves of this tract do not, to any marked extent, decussate with those belonging to the other half—that, in fact, they are distributed to muscles on the same side of the body. It is plain, too, that the permanent spasm in question comes on immediately, and continues without abatement, not only for hours and days, but for weeks and months. Beyond this, it is not easy to know in what direction to pass on with any certainty. The locality of the injury which leads to spasm is defined with tolerable exactness; the *modus operandi* of the injury is as yet only matter of conjecture. After what has been said, however, the question naturally arises, whether the natural electrical action of this tract of the nervous system may not be to

inhibit muscular contraction—to keep up, in other words, a state of muscular tension—and whether an injury to either of the two lateral halves of this tract, by diminishing this action in that particular half, may not lead to contraction on the same side of the body, not because the muscles are subjected to any kind of irritation, but simply because the lessening of their electrical tension, caused by the injury, leaves them more at liberty to yield to the attractive force inherent in the physical constitution of the muscular molecules. Moreover, it may be asked whether another and still more important cause may not be at work in the production of spasm in this case, for it may be supposed that the *duration* of electromotive action in the injured nerve-centres is shortened and so made less constant, that this inconstancy shows itself in the development of instantaneous currents of high tension, that these instantaneous currents give rise to muscular contraction, and that these contractions are unusually prolonged, for the simple reason that the muscles, owing to their lessened electrical tension, are less prompt to get out of the state of contraction when once in it. All this follows very naturally if it be allowed, as a point to start from, that the electromotive action of the nerve-centres is reduced by the injury from a state of constancy to a state of inconstancy—a state in which they take, so to speak, the lower level which belongs naturally to "rhythmic centres;" and so far as I can see, there is nothing at all far fetched in such a notion, for all that it really involves is this—that the electromotive action of the injured part of the nervous system should be less persistent than that of the same part when uninjured; and that, owing to this lessened persistency, it should be subject to the fallings and risings which give occasion to the development of instantaneous currents of high tension, by not being carried continuously or constantly through the whole intervals between the moments in which the electromotive action is renewed by fresh supplies of arterial blood to the electromotive elements of the part. Upon this view, it is not altogether unintelligible that permanent contraction may set in immediately, and, once set in, may continue, with little or no abatement, for any length of time; upon the current view of nervous action in muscular motion the facts remain, as it seems to me, utterly enigmatical. As it seems to me, there is nothing in the facts which is in any way inconsistent with the conclusions already drawn respecting the history of vital motion—nothing to make me shrink from adopting, at least provisionally, the physical view of spasm which is here proposed.

6. The history of trembling is substantially the same as that of spasm. This form of muscular disturbance is one of the consequences of injury to certain not very clearly defined districts of the cerebellum. It may also be caused by injury to various parts of the cranio-spinal axis which have specially to do with the co-ordination of movement—to certain parts, that is, of the pons varolii, and the middle and posterior cerebellar peduncles, to the restiform bodies, and to the posterior columns of the spinal cord. Some inhibitory influence is evidently withdrawn which, while present, served to keep the muscles steady. There has been, as it would seem, a lessening of cerebellar action in particular—a change perhaps, by which the cerebellum is made to take the lower level, so to speak, of the rhythmic centres which have to do with cardiac and respiratory movement. In the case of trembling it seems as if the electromotive action of the cerebellum, like that of the cardiac and

respiratory centres, ran down in the interval between the moments in which fresh arterial blood is supplied to the centre, that the sinking and rising movements in electromotive action, so caused, lead to the development of instantaneous currents of high tension, and that the muscular contractions belonging to the state of trembling are due to the action of these currents. So read, indeed, the case of trembling would only seem to be that of the heart or chest upon a wider scale, the difference between the two cases depending only upon this, that the cerebellum in its disorder influences a far wider muscular region than that which is governed naturally by the cardiac and respiratory centres. Nor is there anything in the fact that trembling is notoriously a consequence of fear to invalidate the view here taken, for this fact (which may be supposed to imply a lowering of nervous action in all nerve-centres, in the cerebral hemispheres more especially) may, after all, only show that other great nerve-centres, besides those which are concerned in the co-ordination of movement, co-operate with the cerebellum in the production of trembling, by participating in the same state of electromotive unsteadiness.

7. Nor is it otherwise with the convulsion caused by injury to certain parts of the nervous system. Turning or rolling in this direction or that, or a backward or forward rushing movement, is the consequence of injury to the hindermost parts of the thalami optici, to the corpora quadrigemina, to certain parts of the superior cerebellar peduncles, to the pons varolii and middle cerebellar peduncles, to the lateral columns of the medulla oblongata, to the parts in the immediate neighbourhood of the insertion of the auditory and facial nerves (as well as to the expansion of the former nerve in the ear

and to the trunk low down), and also, in a greater or lesser degree, to the upper ends of the anterior columns of the spinal cord, and to the lateral columns also except at their upper ends. Convulsion of an epileptiform character is also the consequence of certain injuries to the medulla oblongata, and in this case the parts injured seem to be more or less closely contiguous to the insertions of the nerves which together go to make up the eighth pair—the glossopharyngeal, the vagus, and the spinal accessory; and these results are not altogether unintelligible. The injury to the medulla oblongata which issues in general convulsion may oppose a grave impediment in the way of proper respiration, or it may put a stop to it altogether, by interfering with or interrupting the respiratory movements, and by thus lessening or arresting the supply of arterial blood to this as well as to other nerve-centres, it may lessen or arrest electromotive action in these centres, making an action which was previously constant and steady, inconstant and unsteady, and so bringing about convulsion through the instrumentality of the instantaneous currents of high tension, which are of necessity developed concurrently with any falling or rising movement in electromotive action. The case, indeed, is plain enough. A given supply of arterial blood to an ordinary non-rhythmic nerve-centre is necessary to keep up electromotive action in that centre for a given time. Ordinarily, this supply of arterial blood to the centre is sufficient to keep up electromotive action in the centre all through the intervals which lie between the two moments when the centre receives fresh supplies of arterial blood, and hence it is that the electromotive action of this centre is practically constant, and, because constant, unattended by the development of the instantaneous currents which give rise to the convulsion. But not so when the supply of arterial blood to the centre is insufficient to keep up electromotive action in the centre throughout the intervals between the moments when the centre receives fresh supplies of arterial blood, for then it is evident that the electromotive action will be inconstant, and, because inconstant, attended by the development of the instantaneous currents of high tension which may give rise to convulsion. In fact, the effect of the deficient supply of arterial blood to certain great nerve-centres, which are ordinarily non-rhythmic in their mode of action, may be to make them rhythmic—to reduce them, as I have said before, to the lower level which belongs naturally to the special centres which have to do with cardiac and respiratory movement. Nor is it really otherwise with the convulsive movements which take the form of turning or rolling, or of rushing backwards or forwards, or sideways, though here it is only possible to speak conjecturally. This, however, may be allowed, that certain parts of the great nerve-centres are connected in an especial manner with certain muscles, perhaps with certain combinations of movements, and that the effect of injury to any one of these parts is for this reason localized very definitively. This has long been known to be true of certain parts in the cranio-spinal axis, and to a lesser degree in the cerebellum also, and in all probability the recent investigation of Dr. Ferrier only bring out the old facts in a new light by showing, not that the convolutions of the brain are mapped out into districts like certain underlying portions of the automatic cranio-spinal axis, but that these latter portions may be reached from a distance through the brain. And this, too, is more or less plain, if the premises are tenable, that the injury which leads to turning or any other of its companion forms of partial convulsive movement, may have acted upon the electromotive activity of that part by changing a state of constancy into a state of inconstancy as in the former instances, and so leading on to partial convulsive movement through the action of the instantaneous currents of high tension, which are developed coincidentally with every falling and rising movement in electromotive action there. In a word the case of partial convulsion and the case of general convulsion, so far as the electrical part of their history is concerned, may only differ in this, that the nerve-centres concerned in the movement are affected partially in the one and generally in the other.

8. And even when dealing with the action of the nervous system in voluntary movement it is possible to pass on in the same direction without being brought to a sudden stop. It is certain that this movement is in no way peculiar so far as its electrical history is concerned. The contraction brought about by the will is accompanied by a disappearance of the electricity always present in the quiescent muscle, and in the quiescent nerve belonging to it, and therefore there is no difficulty in believing that electricity, the slave of the will in this case, may have been ordered out of the way, so to speak, and the molecular attractive force of the muscle so left to its own devices. The will, in short, may simply make the natural electricity and the natural molecular attractive force of the muscle do its work in this matter in their own way, without calling upon any vital force to render any assistance; and most assuredly the view, which is to my mind most satisfactory, is not the current view of voluntary movement, but this and only this.

9. And thus the work of the nervous system in

muscular motion is plainly that in which electricity may have everything to do, and 'nervous influence' nothing. There is, as it seems, nothing which may not be done by electricity. There is, as it seems, nothing which can be done by 'nervous influence.' In a word, the whole story of the work of the nervous system in muscular motion, as I read it, is precisely what is *not* to be explained on the hypothesis, that a force called 'nervous influence,' generated in the nerve-centres in direct proportion to the supply of blood to these centres—in direct proportion, that is, to the functional activity of these centres—is expended as a stimulus to a vital property of contractility in the production of contraction, the contraction being always proportionate to the supply of force. And it is well that there is no need to depend upon this hypothesis of 'nervous influence,' for if there were it would be necessary to explain away all the facts set forth in the last chapter, and in particular that in which it appears that contraction is in excess when the supply of arterial blood to the great nerve-centres is, not at a maximum, but at a mininum.

CHAPTER VIII.

ON THE REMOVAL OF CERTAIN OBJECTIONS TO THE VIEW HERE TAKEN OF MUSCULAR MOTION.

1. After what has been done hitherto the little that remains to be done presents no real difficulty. Indeed, all that it is proposed to do in the present chapter is to inquire how it is,—that muscle lengthens and shortens without undergoing any change of volume,—that the force of the contraction lessens as the muscle shortens,—that dead muscle tears more readily than living muscle,—that muscle wastes in proportion to the number of its contractions,—that a tired-out muscle needs to be refreshed by rest before it can act again,—and that muscular contraction is brought about by the action of certain so-called "stimuli,"—to explain, that is, certain facts which have been thought to show that living muscle must have some special endowment by which it is enabled to act as no lifeless body can act.

2. In order to account for the fact that muscle lengthens and shortens without undergoing any change of volume, it is enough to remember that muscle is an elastic body, for it is certainly true, not only that muscle is an elastic body, but also that a band of india-rubber, or any other elastic substance, lengthens and shortens without any change of volume. And thus, instead of pointing to a vital property of contractility as a special endowment of muscle, the fact in question may only

supply an additional proof of the soundness of the view which sees no more than the operation of simple elasticity in muscular contraction.

Here, also, is a fitting place for making mention of two experiments by the late Dr. Joule,* which, taken together, go to show very clearly that a bar of iron lengthens and shortens as it gains and loses magnetism without undergoing the very least change of volume.

In one of these experiments the bar is connected with a system of levers by which any changes in its length are multiplied exceedingly ; in the other it is enclosed in a sufficiently large glass tube filled with water, and having a glass capillary prolongation in which any changes in the level of the water are readily perceptible ; and in both the iron is made magnetic and non-magnetic in the ordinary way, that is, by placing it within a coil of insulated wire, and by making and breaking the connexions between the coil and a voltaic battery. And this is what is noticed on making and breaking these connexions. On making them, there is, in the one experiment, a movement of the levers showing lengthening of the bar, and, in the other, no change of level in the water contained in the capillary tube. On breaking them, there is a movement of the levers showing shortening of the bar, in the first experiment, and no change in the level of the water contained in the capillary tube, in the second. Under these circumstances, that is to say, the bar is seen to agree with muscle and other elastic bodies, in that it lengthens and shortens without undergoing any change of volume, for if the lengthening and shortening to which the action of the levers bears witness had been attended by any change of volume

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

there must of necessity have been unequivocal risings and sinkings in the level of the water in the capillary tube.

Under the action of varying degrees of heat, there are also changes more or less parallel to these in rhombs of calc-spar, and in certain other crystals—changes of angles, that is, which show that what is gained in one direction is precisely compensated by what is lost in another; but it is not necessary to lay stress upon these or any other facts which are at all doubtful in their character, for after what has been said about elastic bodies, it is obvious that muscle may lengthen and shorten without undergoing any change of volume, for the simple reason that it happens to be an elastic body.

3. In passing from the out-stretched to the un-stretched state, the force of attraction displayed in a band of india-rubber is found to be inversely related to the degree of the shortening. In this respect, indeed, the case is precisely that of the muscle; and therefore the fact that the force of the contraction lessens as the muscle shortens may be nothing more than the necessary consequence of the muscle being an elastic body. At all events, it is possible to go far enough in this direction to be able to see clearly that there is nothing in the fact in question which need point to the working of a vital property of contractility.

4. Nor is it necessary to entertain the hypothesis that a living power of contractility has been destroyed by death in order to account for the diminished cohesiveness of dead muscle. Dead muscle may tear more readily than living muscle because the fluid more or less analogous to gastric juice—the juice of flesh—which is contained in muscular tissue, is beginning to dissolve the muscle, or because the muscular molecules them-

selves have begun to be resolved into their constituent elements. Dead muscle may also tear more easily than living muscle because the tearing may set up in the latter a state of contraction which it does not set up in the former—a state in which, by the constituent molecules being brought more closely together, molecular attraction is enabled to tell more forcibly. In a word, there is more than one way of accounting for the fact in question without being driven to the conclusion that dead muscle has lost a principle of strength which belonged to living muscle.

5. It is no doubt a fact that muscular action is attended by waste of the muscle, and that this waste is proportionate to the action; but it does not follow that contraction is for this reason the sign of functional activity in a vital property of contractility. On the contrary, the waste may have been incurred in restoring the state of *relaxation*, and, after what has been said, this is no unnatural conclusion. After what has been said, indeed, it may be supposed that the muscle cannot return to the relaxed state unless its natural electrical condition be re-established, and that this re-establishment involves some corresponding chemical change, which change is of the nature of waste in the tissues concerned.

6. Again: It is possible that a muscle may cease to contract after a time, not because a vital property of contractility is too much fatigued to allow of further contraction until it has been *revived* by rest, but simply because the electromotive apparatus of the nerves and muscles has got out of gear by being kept in action too long. The case, indeed, may be no other than that of any ordinary battery in which the electromotive action has run down. Nor does it follow as a consequence of

this view that the muscles should remain contracted when they are utterly tired-out, for it may be supposed that it is the capacity for nervous action rather than the capacity for muscular action which is affected in this case at this time, and that until rigor mortis sets in there is always enough electricity left in the muscle to counteract contraction. Indeed, if living muscle be left to itself, its electrical condition is naturally one which necessitates, not contraction, but relaxation,—which prevents or inhibits contraction, in short.

7. Nor is there anything in the history of the action of the several so-called "stimuli" in vital motion to make it necessary to fall back upon a different mode of interpretation.

There is no occasion to think that the prick of a pin or the pinch of a pair of forceps does its work by "stimulating" a vital property of irritability, for this action may easily be made to resolve itself into the mechanical disturbance of the electric arrangements of the parts, the pin or forceps accidentally closing electro-motive circles which were previously open, and so leading to the development of those instantaneous currents of high tension (extra-currents, and induced-currents) which have been seen to have so much to do in setting up the state of action in nerve or muscle.

It may be doubted, also, whether the contraction of a hollow viscus is ever rightly ascribable to "irritation" on the part of the contents of the viscus. In the case of the bladder, for example, the urine accumulates, and the contraction is deferred until the distension is an occasion of discomfort. Up to this point the "stimulus" of the urine has seemed to favour dilatation rather than contraction in the bladder, and at this point it is not a whit more necessary to suppose that the contraction is due to the urine having acted as a stimulus is supposed to act. When the bladder is full, there is a feeling of uneasiness which shows that the distension is acting upon the afferent nerves and producing a change in them, which, reflected through the efferent nerves belonging to the same nervous arcs, may act upon the bladder; but it does not follow from this feeling of uneasiness that the vitality of the afferent and efferent nerves concerned in this process is stimulated into action. For, in fact, the former electro-physical explanation may still hold good, the uncomfortable sensation implying the presence of that change in the electricity of the afferent nerve which leads to action through the development of the instantaneous currents of high tension, and the electrical change in the afferent nerves in turn setting up corresponding changes in the efferent nerves and in the muscles belonging to the same reflex nervous arc, because the different parts of this arc are so bound up together as to make it difficult to imagine a change in one part which does not extend to the others also. And this is all that need be said on the subject, except this—that there are of necessity the same reasons for concluding that the hypothesis of "irritation" on the part of the contents of the viscus is unnecessary in order to explain the contractions of any other hollow viscus.

Indeed, the same electrophysical law is manifestly applicable in every case in which muscle or nerve is made to act by any so-called stimulus, be this mechanical, chemical, galenical, electrical, thermal, or other. For the broad fact is that such action, however produced, is always attended by "negative variation" of the nerve-current and of the muscle-current, one or both, so that in each case there is the very same electrophysical basis for action. Nay, the action of the will itself upon muscle must be brought within the same category, for voluntary muscular action has nothing to distinguish it from other forms of muscular action so far as its electrical history is concerned. And certainly the dignity of the will as a vital power is enhanced rather than compromised by this view, for one practical consequence of its adoption must be this,—that instead of having to do its own work in muscular action, by being, as it were, infused into the acting nerve and muscle, electricity becomes the slave, and is made to do the work, of the will.

8. If the history of muscular action be that which is here set forth elasticity has a very important part to play in the matter. It has to play the part which has been wrongly assigned to a visionary vital property of irritability or tonicity; it may have to play the part which has been assigned to one or other form of physical attractive force. In particular, it may not be necessary to look upon the contractile force of muscle as a product of the transformation of the natural electricity of the muscle. This view, no doubt, finds no little support in the doctrine which suggests it—the doctrine of the "correlation of the physical forces," but the view which would resolve the contractile force of muscle into elasticity is, to my mind, more simple in itself, and more in accordance with the facts. Elasticity must operate in muscular contraction; and, this being the case, 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 ever actually transformed into the contractile force of muscle.

9. In point of fact, electricity and elasticity would seem to be everything in vital motion, and vitality

nothing. In saying this about electricity, however, I have no wish to elevate that which is physical at the expense of that which is vital. On the contrary, I firmly believe—and with this remark I bring to a close what I have to say upon vital motion in its physiological relations—that what is called electricity is only a one-sided manifestation of the workings of a single, central, cosmical law, which, when fully revealed, will be found to rule living and lifeless bodies alike, not by entombing spirit in matter, but by transfiguring and spiritualizing matter—a law which, without confusion of substance, binds all things together in the very closest communion—a law which makes the old belief of multieity in unity, and unity in multieity, a sober fact.

THE ARGUMENT:

II.

VITAL MOTION

REGARDED PATHOLOGICALLY.

CHAPTER I.

ON VITAL MOTION AS EXHIBITED IN EPILEPSY AND OTHER CONVULSIVE DISORDERS.

I.—ON THE HISTORY OF EPILEPSY AND OTHER CONVULSIVE DISORDERS AS SET FORTH IN THE VASCULAR SYSTEM.

I.

1. The immediate precursor of the epileptic paroxysm is a sign which is somewhat difficult to catch—corpse-like pallor of the countenance. M. Delasiauve* 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," says he,† "qui se produit au moment de la chûte, qui n'est imitable pour personne, c'est la pâleur très prononcée cadaverique, qui couvre pour un instant la face epileptique. Nous ne la voyons pas, parceque nous arrivons trop tard, alors que la face est dejá d'une rouge très prononcée." In fact the general form of the epileptic or epileptiform paroxysm begins in the same way as the partial form to which the name of *petit mal* is often given, for cadaverous pallor of the countenance has certainly a very conspicuous position among the initial symptoms in this latter case.

2. A habit of sighing, as if the proper balance of breathing could only be maintained by now and then taking breath more deeply than usual, is also observable

* "Traité de l'Epilepsie." 8vo. Paris, 1855.

† "L'Union Médicale." 28th Apr., 1855.

in many epileptics, and not unfrequently, especially when the fit happens during sleep, the breathing may come to a standstill so complete and so prolonged as to make a bystander fear that the patient is actually dead. More than once I have felt this fear myself; and again and again my attention has been struck by the sighings to which I have referred ; and, in short, there is, as I believe, good reason for believing that there is some radical fault in the breathing in many epileptics, and that the fit itself is often ushered in by a complete cessation of all proper respiratory movements.

3. In the actual epileptic or epileptiform paroxysm, the staring, squinting, out-starting eyes, the black and bloated countenance, the guttural sounds of strangling, and the spasm-bound chest, show plainly enough what is happening. The state which accompanies the convulsion is evidently that of suffocation. It is as if some invisible fiend had tightened his strong fingers around the throat of the unhappy sufferer. Nor is it really otherwise in these varieties of the disorder, partial or general, in which the face remains pale and shrunken from the beginning to the end of the paroxysm, for here 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.

4. 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 is at once hard and full and frequent. How then is this ? What is the true meaning of this state of seeming vascular over-action ? The common belief on the subject is that an increased quantity of *red* blood is injected into the arteries during

the paroxysm, and that this increased quantity of red blood produces the convulsion by provoking a state of increased functional activity in one or other of the great nerve-centres, and but lately Schroeder van de Kolk* has given distinct expression to this belief. In reality, however, there is reason to know that the pulse acquires power during the paroxysm, 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 fails rapidly 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 arteries fill at the expense of the veins, and the pulse in the arteries becomes stronger and fuller as the colour of the blood in the arteries changes from red to black. Evidence to this effect is supplied in the experiments of Reid and Draper of which an account is to be found on a former page (p. 145). It is, indeed, certain that the strong and full pulse of the epileptic or epileptiform paroxysm may be nothing more than the natural pulse belonging to 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 is available, for with the respiration completely, or all but completely, at a standstill, as it is in fact, it is simply impossible that there can be increased injection of *red* blood into the arteries during the paroxysm. Moreover, a right view in this matter explains many apparent anomalies in the pulse. It explains, for example, how it is that blood drawn from the temporal artery in a fit is often black

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

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 at fault, the pulse may beat with seemingly contradictory power in the very last moments of life. It shows in fact that the pulse may derive a fictitious value from the admission of black blood into the arteries when the respiration is insufficient, and that mere power of pulse, apart from the condition of the respiration, is a very unsafe criterion of true vascular vigour.

5. Over-activity of the circulation, indeed, forms no part of the history of epileptic or epileptiform disorder, either in the fit or in the interparoxysmal period. Instead of predisposing to these disorders all febrile excitement would seem to have a contrary effect, for it often happens that fits of daily recurrence are suspended for days by fever, as in the case where severe symptomatic fever follows a burn or other injury received during a fit, or in that where sharp idiopathic fever happens to be set up, and that they occur as frequently as before when the fever has passed off. And certainly nothing to the contrary is to be gathered from the state of the circulation in the interparoxysmal period, for here if anything at all out of the common is noticeable it is sure to be some indication of defective vascular vigour—a pulse easily flagging, hands and feet readily becoming cold and clammy, and the like.

6. In short, the history of epilepsy and epileptiform disorder, as set forth in the vascular system, is quite in keeping with all that has been said upon the working of the blood in vital motion, in that the convulsion is always seen to be connected with a state which is as far as possible removed from vascular over-action.

II.

1. In the convulsive disorders associated with hysteria and chorea 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 sense of breathlessness almost amounting to actual stifling; and there is nothing in this state of the circulation which can be looked upon as pointing to over-activity of any kind. In hysteria the difficulty is to keep the hands or feet warm, or to avoid chilblains, when the weather is at all cold; and frequent palpitations show how often the heart is called upon to make up for work which is left undone by the "capillary force." There is, in fact, a sort of radical weakness in the vascular system which seems to be not remotely akin to that which is met with in hybernating animals. Nor is it really otherwise in chorea. Here perhaps, there is a disposition to rheumatic fever—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 along with the febrile symptoms, but before them, or after them—often long before or long after them; and the inevitable inference from the facts 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 integuments, anæmic vascular murmurs, and the like. Indeed, the very predisposition to rheumatic fever may itself be taken as a reason for thinking that the circula-

tion in chorea is radically weak. Moreover, it not unfrequently happens that the symptoms of chorea are suspended by the accidental development of scarlet fever, or other true febrile disorder, and that they return again when this state of feverishness is at an end.

2. The true place of the convulsion in connection with any form of febrile disorder, moreover, is in the cold stage before the hot stage, or in the stage of collapse after the hot stage, and not in the hot stage. It seems as if there were something uncongenial or even incompatible between the convulsion and an excited state 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; 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 the consequence of the convulsion, than the convulsion which is the consequence of the fever. And certainly it is difficult to connect with anything like fever the convulsion which may happen at a later period in the state of collapse which is too often the immediate precursor of death.

3. Nor can a different place be assigned to the convulsion connected with teething, or worms, or any other of the many manifestations of the state to which the name of "morbid irritability" has been given; for here, most assuredly, in the few instances where the history of the convulsion is at all complicated with fever, the place of the convulsion is, not side by side with the fever, but before the fever or after it, precisely as it was in the cases referred to a moment ago.

4. And surely nothing contradictory to these con-

clusions is to be found in the sad case in which convulsion takes its most terrible form—the case of convulsion from loss of blood in childbed: for here any doubt as to the connection between the hæmorrhage and the convulsion, if that were possible, must be set aside by the fact that transfusion of blood must be reckoned as the very chiefest means of arresting the convulsion, and saving life.

5. In the convulsion which may happen in uræmia and other forms of blood-poisoning, the condition of the vascular system may be somewhat obscure, but there is no reason to think that it is essentially altered. Possibly the great deficiency of blood corpuscles, which is a marked feature of Bright's disease in its advanced stage, is 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 in which the blood is at last reduced in albuminuria may have something to do in bringing about the stupor and coma which generally 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 accept this view, 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 very imperfectly: and also this—that the whole drift of the argument hitherto is contradictory to the notion that the convulsion is brought about by the urea in the blood (or rather the carbonate of ammonia into

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which the urea is resolved) having acted as a *stimulus* to a vital property of irritability in the nervous system or elsewhere. And what may be said of the condition of the vascular system in the convulsion connected with uræmic poisoning is also more or less applicable to the cases where convulsion happens in persons whose blood is loaded with bile or other matters which ought to be eliminated.

6. And thus, convulsion in all its forms, epileptic and epileptiform, hysterical, choreic, and the rest, is found to be associated with a state of the vascular system which is as far as possible removed from hyperæmic or feverish activity—a state in which the supply of *red* blood to the system is either arrested altogether or greatly interfered with—a state in which it is easy to believe that the blood must have acted in preventing rather than in provoking convulsion, the case in all particulars being one which is in perfect accordance with the conclusions arrived at, in the remarks upon the work of the blood in vital motion, in the physiological part of the argument.

II.—ON THE HISTORY OF EPILEPSY AND OTHER CONVULSIVE DISORDERS AS SET FORTH IN THE NERVOUS SYSTEM.

I.

1. In epilepsy all consciousness is suddenly suspended, and memory keeps no record of what happened in the fit. Fire is not felt at this time nor yet the knife, and the patient may wake to find himself terribly mutilated without knowing why. And in the minor form of the disorder (petit mal), where the screaming, falling, and struggling of the major form (haut mal) are want-

ing, and where even walking and talking may go on automatically without interruption, the mind is altogether blank, perceiving nothing, remembering nothing. For the time being, in point of fact, the state during the actual attack of epilepsy, partial and general alike, is nothing less than one of mental annihilation.

2. A mind weak in every way, even to the degree of idiotcy or fatuousness, is closely associated with the worst forms of epilepsy, and, without doubt, the tendency in these cases is to become worse and worse mentally as the convulsive disorder gains ground. And what is true of the worst forms of the disorder may also be affirmed as true of the general run of cases, though in these latter there are undoubtedly very many in which it is very difficult to detect any mental flaw, or to say that matters change for the worse mentally as time wears on. Indeed, there is much, very much, in the mental history of the epileptic in the interparoxysmal period to justify the statement that the disposition to epilepsy is inversely related to the degree of mental power.

3. The morbid appearances after death from epilepsy are not yet set down with all the clearness which is desirable. 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 referable to insanity rather than to epilepsy, and for this reason—that they are as common, or more common, in insanity uncomplicated by epilepsy. In other cases there are signs of degeneration of the brain, such as

pallor of its grey substance, softening, granular induration, atrophy, superabundance of serum,—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 in the demented state there is certainly a marked liability, not only to epilepsy, but also to convulsive disorder in other forms—palsied shaking, cramp, spasm, and the like. 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 may be ossified ; but there are in the brain proper, or in its membranes, no changes of which the connexion with epilepsy is to be looked upon as constant—not even that change in the pituitary body to which Wenzel attached so much importance ;* for, writing of it, 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, indeed, in the medulla oblongata alone 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 very evident 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,

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

† "Manual of Path. Anat." Tr. (for the Sydenham Society), by Dr. C. H. Moore. Vol. iii, p. 424.

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, the blood vessels of this part are found to be considerably dilated, and with their walls much thickened—in the course of the hypoglossal nerve and corpus olivare, in epileptics who were in the habit of biting their tongues in a fit,—in the course of the roots of the vagus in those who were not in this habit.* In a word, the appearances detected by Schroeder van der Kolk in the medulla oblongata of epileptics, are such as to make it certain, not only that this part of the nervous system is especially affected in epileptics, but also that it is acting less perfectly than it ought to do; for how is anything but very imperfect action compatible with a state in which natural nerve-tissue is to so very considerable a degree displaced by the products of fatty and other forms of degeneration, as well as by the pressure arising from the presence of the dilated and thickened vessels?

4. In epilepsy, therefore, the nervous system would seem to be at fault in a way which cannot well be mistaken. There is manifest inaction of those parts which have in an especial manner to do the work of the mind. There is manifest inaction in those parts which have to do with respiratory movement. Convulsion and suffocation evidently go hand in hand. The case is one which is altogether inconsistent with the notion that convulsion has to do with exalted functional activity in any part of the nervous system, for the failure in the supply of arterial blood to the part must carry with it an equivalent failure of functional activity in the part. The case is one which is altogether consistent with the notion

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

that the failure in the supply of arterial blood may carry with it an equivalent failure of electromotive activity in the nervous system generally, beginning, perhaps, in the tract which has specially to do with respiration, and that the disturbance in electric equilibrium thus brought about may issue in convulsion in the very same way as that which has been indicated with sufficient distinctness in the physiological portion of the argument (p. 172). In all respects it is as it should be if the premises are unassailable.

II.

1. In hysteria the patient is at most barely conscious during the paroxsym, and in a bad bout of choreic agitation there is no very great difference in this respect. In both cases the will is altogether in abeyance. And certainly the mental state which is most characteristic of the inter-paroxsymal period in both hysteria and chorea is one which is the reverse of strong-mindedness—a state of extreme nerveless unrest.

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

In half the cases, perhaps, traces of inflammation, more or less vague, and always of very 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 certain 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, more especially, perhaps, the thalami optici and the corpora striata, with the grey matter of the convolutions nearest to them (the island of Reil), instead of being inflamed, are actually starved for want of blood, by the plugging of the minute vessels, arising either, as Dr. Kirkes pointed out, from the passage into the arterial system of minute warty vegetations that had become detached from the cardiac valves (and hearts with valves covered with such vegetations are so common in chorea as to have got the name of choreic hearts), or else, as Dr. Bastian supposes, 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 undoubtedly met with in some of these cases. On the contrary, it is only necessary to suppose that they are the effect of the disease, and not the cause—that, in cases where they are present, the disease has been prolonged until—in consequence of the vaso-motor nerves being at length exhausted or paralyzed by the continuance of the state of over-action beyond a certain limit—the time has arrived in which the vessels of the part pass out of the state of contraction which belongs to the stage of "irritation" into that of congestion and inflammation. Nay, it is quite conceivable 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—and there is more to the same effect further on—goes to show that the change to be expected under these circumstances is this, and no other.

And, as with chorea, so also with hysteria or any other disorder in which convulsion of a non-epileptic character is a symptom, there is really nothing in any appearance after death to make it necessary to modify this conclusion in any particular.

3. Convulsion is not a common symptom of inflammation of the brain or its membranes. Now and then, in children especially, it may happen at the onset of this 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 during the time when general febrile reaction, with determination of blood to the brain, is fully established.

4. Neither does convulsion find a place among the symptoms of acute mania. Acute mania may be a consequence of convulsion, and convulsion may return when the maniacal excitement has subsided, but the convulsion and the acute mania are not really concurrent phenomena. Indeed, the simple fact appears to be that here also the convulsion is incompatible with anything like active determination of blood to the brain.

5. And in the case where it may happen to be associated with cerebral apoplexy convulsion may have to be referred, not to "active determination of blood to the brain," nor yet to engorgement of the cerebral veins, but to the pressure or other damage caused by the extravasated blood. It may well be that convulsion is thus brought about, if only the apopletic effusion be on or in some part of the brain which has to do with automatic

or reflex movement; it can scarcely be that it has any real connection either with "active determination of blood to the brain," or with engorgement of the cerebral veins. A sufficient contradiction to this notion that it is connected with "active determination of blood to the brain" is supplied in the brief comments on inflammation of the brain which have just been made: and a contradiction to the other notion, that it is connected with engorgement of the cerebral veins, is not far to seek. In whooping-cough, where these veins are often congested to a very high degree during the paroxysm, when convulsion happens it is, not at this time in particular, but rather when the face is pale and the patient spent, and either sleepy or else asleep. In extreme congestion of the lungs, also, where these veins are greatly gorged with dark blood, the most likely consequences are dreamy sleepiness, stupor, coma it may be, seldom convulsion. And in cases where extreme venous congestion of the brain is brought about by straining, or in some similar way, the symptoms are coma simply, not coma and convulsion. In these cases, indeed, it is as it would seem to be in certain recent experiments in which the external and internal jugulars of rabbits were tied by Drs. Kussmaul and Tenner, for here the operation resulted, not in convulsion, but in stupefaction, with, perhaps, some slight grinding of the teeth.

6. Nor do I find anything in the relation of convulsion to the state called "exalted or morbid irritability" which is at all calculated to invalidate these conclusions. For what is this state? 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 is

irritable, easily over-balancing on the side of excitement, or on that of depression, as the case may be—a state in which exhaustion is very readily brought about, and for which *nervous exhaustion* would seem to be as good a name as any—a state, in fact, which is more readily accounted for on the supposition that certain nerve-centres are starved for want of blood, than upon the contrary supposition that they are 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 at variance with the view here taken of epilepsy and convulsive disorders generally.

Looking back at the history of epilepsy and other convulsive disorders, as thus delineated, it is not difficult to trace everywhere a continuous path along which, with few windings, it is possible to pass directly to the same conclusion as that already arrived at in the physiological part of the argument. In no one place, so far as I can see, is there anything to justify the belief in the current doctrine of vital motion; and I can only wonder that such a doctrine should ever have been seriously applied to the interpretation of convulsion. The simple truth appears to be that epilepsy and convulsive disorder generally are connected, not with a state of fever or inflammation, but with a state of "irritation" which may issue in fever or inflammation,—not with hyperæmia, but with anæmia,—not with a state of circulation which may carry with it a condition of exalted vitality, but with one which is altogether contradictory to this notion. It would seem as if there were something altogether uncongenial and incompatible between convulsion and anything approaching to vital over-activity. It would seem, indeed, as if the only master-key to the facts is that which is offered in the physiological portion of the argument, namely this, that in epilepsy and convulsive disorders generally, there is, for want of arterial blood, failure of electromotive action in the great nerve-centres—in those which have to do with respiration primarily—and that the disturbance of electric equilibrium, so caused, leads to convulsion in the way about which enough has been said already in the physiological portion of the argument (p. 172).

CHAPTER II.

ON VITAL MOTION AS EXHIBITED IN TETANUS AND OTHER SPASMODIC DISORDERS.

I.—ON THE HISTORY OF TETANUS AND OTHER SPASMODIC DISORDERS AS SET FORTH IN THE VASCULAR SYSTEM.

I.

1. In tetanus, not unfrequently, the pulse may be hard and quick, but when this is the case the breathing is so far embarrassed by the spasm as to make it certain that imperfectly aërated blood is then finding its way into the arteries, and that the pulse is none other than that of partial suffocation (p. 144). Indeed, the constant rule appears to be for the spasms to become more and more masterful as the pulse points more and more unequivocally to the feverless exhaustion of coming dissolution.

2. During the fits of spasm, too, and in a lesser degree in the interval between the fits, the skin may be very hot, the heat rising in some cases as high as 110·75°; but, as in the case of the hard and firm pulse, so in this, it is certain that this fact does not point to anything like real over-activity in the circulation, for as death approaches, the temperature, instead of falling as it might be expected to do, rises higher and higher, and what is still more strange, may go on still rising for some time after actual death. The cases put on record by Drs. Wunderlich,* Erb,† Sidney Reiger,‡ Weber,§ Sanderson,§ and others, make it very evident that the temperature does actually rise in this way, not in tetanus

* "Archiv. d. Heilkunde." Bd. x, 2, 3, and 5. (1861, 1862, and 1865.)

† "Deutches Archiv. f. klin. Med." Vol. i. 1866.

‡ "Med. Times and Gaz." Vol. ii. 1867.

§ "Clinical Soc. Transactions." Vol. i. 1868.

only, but in other cases also, and a few of these cases may not be out of place here, for the fact which they serve to establish, and which is of vital importance in the present instance, is not so familiar as it ought to be.

The patient in one of Dr. Wunderlich's 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 consequent upon 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 here and there was injected and considerably disorganized. The temperature of the ward at the time of death was 77° Fahrenheit; the notes of the temperature of the body at different times before and after death run thus :—

Before death,	July 24, 1861	...	... 102° Fahr.
	,, 25, ,,	...	... 102°
	,, 26, ,,	9 a.m.	... 104·45°
		6 p.m.	... 103·55°
		9.20 p.m.	... 110·1°
Death at		9.35 ,,	... 112·55°
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°

Another case, by the same careful observer, was one of traumatic tetanus, fatal on the tenth day, in a man aged 20. Up to twenty-four hours before death the spasms were well marked, and the mind was quite clear; afterwards, and especially during the last six hours of life, unrest, talkativeness, jactitation, and slight delirium, were the most prominent symptoms. The appearances after death agreed very closely with those noticed in the last case, and these are the notes of the temperatures :—

	Temp. 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°

A third case, also recorded by Dr. Wunderlich, was one of idiopathic or rheumatic tetanus, 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 appearance after death pointing to the pulmonary disorder. In this case the notes of the temperature are :—

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

A fourth case, reported 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 ranging from 44 to 60 in the minute, and an uncountable pulse, being the more prominent symptoms during the last 24 hours of life. Here the notes of the temperatures are these :—

24 hours *before* death	... ...	102·65° Fahr.
At death	... ...	104·9° ,,
13 minutes *after* death	... ...	105·12° ,,
15 ,, ,, ,,	... ...	104·67° ,,
55 ,, ,, ,,	... ...	104° ,,

A fifth case, also put on record by Dr. Erb, was one of purulent meningitis, the patient being a woman, aged 22, six months gone in pregnancy, the more conspicuous 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 of the temperatures run thus ;—

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

The sixth and last case, which came under my own notice in the summer of 1870, 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, the notes of the temperature before and after death being these :—

12 hours *before* death ..	... ...	103·25° Fahr.
3 ,, ,, ,,	... ...	104° ,,
At death	... ...	Not ascertained.
hours *after* death	... ...	105·5° Fahr.

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 before death, and remain very hot for some time after death, in cholera, in scarlet fever, and in other cases also, which cases 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 anything like over-activity of the circulation—with anything which can be properly spoken of as fever. The temperature continues to rise, not only in the moribund state, but after actual death ; and thus it would seem that the rising in question must be connected, not with over-activity of the circulation, not with anything like true fever, but with an exactly contrary state of things. Nor is it more easy to connect this increased heat with the spasms. In part, perhaps, the increment may be accounted for in this manner, but only very partially. Indeed, the simple fact that in more than one of the cases to which reference has just been made, the mercury continued to rise coincidently with a decided abatement in the severity of the spasms, and that in all cases the rise continued after death, when all spasm was at an end, is in itself a conclusive proof that the explanation of the increased heat of tetanus is not to be found in the excessive muscular action of tetanus. Moreover, the fact that the temperature continues to rise in the same way before and after death in cases where neither convulsion nor spasm had place among the symptoms during life must of necessity lead to the same conclusion. How to explain the phenomena in question is another matter. Increased heat is a consequence of mechanical injury to the cord or medulla oblongata. Increased heat, as is shown in some of the cases which have been cited, is an accompaniment of certain diseases which annihilate more of less completely cerebral action, without causing convulsion. It seems as if the heightened temperature here might be owing to the removal or paralysis of some regulating power belonging to the brain, and beyond this it is difficult to see further, except it be that this removal or paralysis, reaching to the vaso-motor nerves, may allow the minute vessels to dilate and receive more blood, and that the state of congestion, thus brought about, even though the blood be stagnant, as it is after death, may lead to increased molecular changes, of which the increased heat may be the sign and measure. What is now intended, however, is not to discuss the cause of the increased heat in tetanus, but simply to insist upon the fact that this phenomenon does not imply over-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 seem to me, is the only inference which can be drawn legitimately from the facts which have been cited in evidence.

II.

1. In tetanus, in the severe bouts of spasm more especially, the patient, as a rule, is unnerved, alarmed, agitated, absorbed in his sufferings, absent and hopeless. The patient is only half alive mentally, and the muscles seem to be in a hurry to pass into the state of rigor mortis. The whole case is one in which everything goes to show that the spasm has to do, not with cerebral excitement in any of its many forms, but with a state diametrically opposed to it. Upon this point there can

be no doubt, for it is true, not only that the state is as far as possible removed from cerebral excitement in the ordinary run of cases of tetanus, but also that spasm is a less conspicuous symptom in the exceptional cases in which there may be such excitement.

2. It is a common impression that tetanic 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 the back had been injured by a blow or a wound, 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 to the injury as to the tetanus. And in no one of the many cases in which the cords of animals killed by strychnia were examined by Majendie, Orfila, and Ollivier, was there any perceptible organic lesion, inflammatory or other. 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.

Dr. 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 "other exudations," but pus corpuscles are not mentioned. "In the walls of the blood-vessels," Dr. 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 he 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 Dr. Clarke finds in six cases is substantially the same as that found by Dr. Dickinson in the one case recorded by him,† for

* "Med. Chir. Trans.," vol. xlviii, 1865.
† "Med. Chir. Trans.," vol. li, 1868.

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, not to inflammation, but to serous effusion. 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 the wound 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 lower bowel; 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 limbs 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, for that reason, exquisitely tender, serous membrane, and its character is certainly more like that of pleurisy than that 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, as it seems to me, 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 indiquant 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 make a mistake in diagnosis somewhat difficult. 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 mastication, causing lock-jaw, and to extend from them 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 the muscles 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, of which the effect is to keep the legs widely apart and rigidly extended, and to stretch out the arms stiffly and clench the fingers. 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 makes it evident 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 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 being now and then violent enough to crack the teeth, or to grind them out of their sockets, or to break the thigh bones, or to tear across great muscles like the psoas and rectus femoralis. In acute spinal meningitis, on the other hand, the jaw, if it be set at all, is set 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; no particular change in the 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—these are 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 meningits, 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 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 does happen. 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 concomitants. Nor is a different conclusion to be drawn from the occasional presence in the paralysed muscles of a state which is analogous to, if 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 months continuously, even during sleep, or it may have 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 in tetany 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 with 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 taken in order to put 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 so questiioning. 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 sympton 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 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 is 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 as 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, fancifulness, a habit of romancing, undue lowness of spirits or the contrary, and other symptoms whose name is legion. Nay, not only is the name of these symptoms legion, but there is ever going on a process of mutual metamophosis 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 structual change, the prolonged muscular contraction, which has to do with spinal irritation, is associated.

A like inference is also to be drawn from the fact that the traces of inflammation after death which may be met with in cases of tetanus are, not in the spinal cord exclusively, but in various parts of the brain, in the nerves, and in other parts of the body as well. There is, in truth, a vagueness in the seat of these traces which makes it certain that inflammation in any one nerve-centre cannot be looked upon as essential to the spasm, and whch suggests the notion—which, as will be seen presently, arises more particularly in connection with the history of the spasm in hydrophobia—that the inflammation which has left these traces may have had to do with some secondary, and, it may be, depurative process by which the system has striven to rid itself of some virus, rather than with the spasm.

3. What, then, is the particular conclusion respecting tetanus which would seem to be necessitated by the actual state of the vascular and nervous system in tetanus? It is that there is in some part of the nervous system, in some great centre of innervation, a state, not of inflammation, but of "irritation,"—a state which, as regards vitality, is marked by the sign *minus* rather than by the sign *plus*,—a state which is certainly associated with defective activity in the circulation and respiration. In a word, it may be that the part affected is that which includes the corpora olivaria and the tracts above and below these bodies in which the most conspicuous effect of injury is persistent spasm (p. 169); and that the way in which the spasm is brought about is simply that which is pointed out in the chapter on the

work of the nervous system in vital motion, and about which I have nothing new to say now.

II.—ON THE HISTORY OF TETANUS AND OTHER SPASMODIC DISORDERS AS WRITTEN IN THE NERVOUS SYSTEM.

I.

1. As in tetanus, so in the other forms of spasmodic disorder, there is reason to believe that the spasm is associated with a condition of the vascular system which is the very reverse of anything like over-activity, and that the increased temperature met with in some cases must have the same significance as in tetanus. During the attack of catalepsy the appearance of the patient is so corpse-like that it may even be necessary to put the ear to the chest to know of a certainty that the heart has not ceased to beat. In cholera the cramps are coincident with a state closely approaching to pulselessness, and any increase of temperature before or after death is evidently of the same nature as the analogous phenomenon in tetanus. In hydrohobia the circulation flags from the very beginning; and in spasmodic ergotism there is no evidence of vascular excitement throughout the whole course of the disorder. And, certainly, no contrary conclusion 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 occur so frequently in old people, and in those in whom the nerveless and marrowless period of old age is anticipated by chronic softening of the brain.

2. In hydrophobia, as in tetanus, it appears to be the rule for the spasm to gain ground as the pulse loses in power, for on analysing the histories of a considerable

number of cases, I find that there was less agitation, less convulsion, less spasm, when the state was less feverish than in the ordinary run of cases. Nor is a different conclusion to be drawn from the history of spasm as 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 state has passed off; that it disappears if pneumonia, bronchitis, or any other inflammatory disorder 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 connexion with what has gone before, and with what has yet to come, it helps not a little to strengthen the conviction that spasm, like convulsion, is antagonized rather than favoured by an excited condition of the circulation.

3. As in tetanus, too, the vagueness in the seat of the inflammation which may be developed in the course of various other spasmodic disorders would 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. Thus, in 46 cases of hydrophobia, of which the histories were carefully analysed by my brother, Mr. J. Netten Radcliffe,* "the morbid appearances were in the dura mater in 8, in the arachnoid membrane in 10, in the pia mater in 16, in the valum interpositern in 2, in the choroid plexus in 12, in the cerebral hemispheres in 28, in the spinal cord and membranes in 18, in the pons varolii and medulla oblongata 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 æsophagus in 16, in the stomach in 20, in the

* *Lancet*, Sept., 1856.

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 in every gradation of injection of the blood-vessels, from the slightest blush to the most vivid red or dark black congestion; of alteration in 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 structural changes occurring in asthenic conditions of the system." Now, this vagueness in the seat of these inflammatory and other structural changes is a very curious and significant fact—a fact which, perhaps, more clearly than any other single fact, is calculated to show the true relations of inflammation to spasm. It is calculated to show that inflammation of any 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 obscure febrile condition which may be developed in the course of the disorder—that, in fact, it is little more than accident which fixes the seat of the inflammation in one part of the nervous system rather than in another, or in one part of the body rather than in another. In the case of hydrophobia, indeed, it is calculated to put any inflammation which may be developed in the course of the malady in the position of a depurative process—a process which, as in the inflammation developed in connexion with the fever of small-pox, is intended to rid the system of a morbid virus. And thus it may be that spasm is connected not with a state of inflammation in any part of the nervous system or elsewhere, 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 hyperœmia, but by contraction of vessels and anœmia: and, so far as I know, there is nothing in the history of any form of spasmodic disorder other than tetanus which is in any way calculated to exhibit the relations of inflammation to spasm in a different light.

II.

1. Nor is it only in tetanus that there is reason to know that the spasm is associated with a very depressed condition of brain-power. In catalepsy the mind is either in a deep sleep, or else wrapt in some dreamy vision. The cramps of cholera are attended by a state of hopeless indifference, than which is no surer sign of mental prostration. In hydrophobia the mind is in a state which may be described as an exaggeration of that which is met with in delirium tremens. In spasmodic ergotism the state of the brain is one bordering 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 symptom in general or partial dementia; and thus again, spasm in the stomach and bowels is not unfrequently the immediate result of sudden mental depression. In a word, the little that has to be said under this head is simply a repetition of what was said under the same head when speaking of tetanus; for, as has been already seen, the traces of inflammation which may be found in various parts of the nervous system

after death from hydrophobia, supply no reason for supposing that this inflammation had any direct connexion with the spasmodic symptoms belonging to this disorder.

And thus, there is nothing in the history of tetanus or other spasmodic disorder which does not strictly harmonize with the history of epilepsy or other convulsive disorder, and with the physiological premises. It is evident that spasm is in every case connected, not with a state of fever or inflammation, but with that anæmic state which may issue in fever and inflammation, and to which the name of "irritation" is given. It is evident that the action of inflammation of the spinal cord is to counteract spasm rather than to cause it, and that the same rule holds good for inflammation in any other nerve-centre. In point of fact, the only conclusion which appears to be at all warranted by the evidence is this—that there has been, for want of blood in the part, a failure of electromotive action in the olivary bodies, and in the tracts above and below these bodies which in like manner counteract spasm by keeping up a state of muscular tension, and that the disturbance of electric equilibrium, so caused, gives rise to spasm in the way indicated in the remarks upon the work of the nervous system in vital motion (p. 169).

CHAPTER III.

ON VITAL MOTION AS EXHIBITED IN VARIOUS FORMS OF TREMOR.

I.—ON THE HISTORY OF TREMOR AS SET FORTH IN THE VASCULAR SYSTEM.

1. In the case where a man shivers in the cold, and ceases to shiver when he becomes warm again, the shivering is evidently connected with a depressed state of the circulation. There can be but one opinion upon this point. In an attack of common trembling the circulation is in very much the same case as that in which it is in shivering, and the pulse does not recover itself until the paroxysm is over. In the ordinary forms of paralysis agitans, as well as in that exceptional form of the disorder in which the tremulous agitation shows itself only or chiefly when an attempt is made to stand or move about on the feet, and in which the spinal cord after death is found to be in that state of degeneration to which the name of disseminated spinal sclerosis has been given, there may be occasional bouts of obscure feverishness, in which, perhaps, the muscles may be a little more quiet than usual, but the habitual state of the circulation can only be spoken of as one of feebleness and prostration. In delirium tremens the cold sweats, 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 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 steadying themselves.

2. 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 certain 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 is to 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 altogether prostrate and powerless. In a word, there are many facts which appear to show that there is something altogether uncongenial between tremor and an excited condition of the circulation.

I.—ON THE HISTORY OF TREMOR AS SET FORTH IN THE NERVOUS SYSTEM.

1. The persons most likely to tremble are *timid* at all times, and most of all when trembling. Their right to be regarded as strong-minded is as small as it well can be. And so too in other cases. In tremulous old age, and in paralysis agitans, the system is lowered mentally and in every way; 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 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 dim 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 at the time of trembling.

2. In the different forms of tremor, the condition of the nervous system, as reflected in the state of the mind, is, in short, 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 circulation is one which must necessitate a state of very imperfect activity, not in one nerve-centre only, but in all nerve-centres indifferently.

As in the last two chapters, so in this, all the evidence points to the same conclusion. Everything is calculated to contradict the notion that tremor has to do with an excessive development of nervous influence resulting from an over-abundant supply of blood to some part of the nervous system. Everything is calculated to support the notion that it may have to do with failure of electromotive action, consequent upon insufficient supply of arterial blood, in one or other part of the nervous system, and that this failure tells in the production of tremor in the way which has been pointed out in the chapter on the work of the nervous system in vital motion (p. 171). In this, as in the former cases, the pathology is perfectly in accordance with the physiology, and nothing remains to be noticed which is in any measure peculiar or exceptional.

CHAPTER IV.

ON VITAL MOTION AS EXHIBITED IN VARIOUS FORMS OF NEURALGIA.

I.—ON THE HISTORY OF NEURALGIC DISORDERS AS SET FORTH IN THE VASCULAR SYSTEM.

1. In the neuralgia so frequently met with in agueish districts the state of the pulse and of the circulation generally during the paroxysm is not unlike that which belongs to the cold stage of ague, and these resemblances are further borne out in this—that rigors often go along with the pain in close companionship, and that as often the pain begins and ends with strict periodicity at a given time, and is followed by an obscure hot fit. It would seem as if the neuralgia and the shivering had both of them to do with the state of the circulation, to which, in ague, the name of cold stage is given—a state, that is, not of over-activity, but of under-activity. And what holds good in this particular case is also true in all cases of neuralgia, for in all these cases there is, together with a state of the circulation, which, to say the least, is not unlike that of the cold stage of ague, the same disposition to periodicity, and the same accompaniments of shivering and shuddering.

2. In rheumatic fever the rule, I believe, will be found to be this—that the pains, which had been torturing the patient for weeks or months previously, come to an end when the feverish reaction and local inflamma-

tion of the fully formed disorder make their appearance. After this, the joints are *tender* enough, but if the patient keep still, as he is very likely to do under the circumstances, he is comparatively at peace so far as the 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 inflammatory symptoms—an event which happens more frequently in this disorder than is commonly supposed.

3. 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 fulness 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 in comparison 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 shiverings, 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 happens occasionally, and the inflammation runs higher than usual, *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 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

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

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.

4. 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.

A few years ago, I had a patient in the Westminster Hospital, who, when I saw him first, complained of violent pains all over the body, in the back and loins especially, and also of chills and shiverings. Shortly afterwards he was hot and feverish, and the pains and chills and shiverings had all taken their departure. The case was one of small-pox ; and the lesson it impressed upon my mind was that the pains and rigors were symptoms which ought to be classed together, and regarded 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 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 the same 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 of tender parts 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 essental accompaniment of the inflammation—a consequence, as I have just said, of the inflamed and tender tissues being kept on the stretch, 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 with the opposite state of febrile or inflammatory excitement.

II.—ON THE HISTORY OF NEURALGIC DISORDER AS SET FORTH IN THE NERVOUS SYSTEM.

1. Pain is no very conspicuous symptom in the common form of cerebral meningitis—in, that is, the tubercular form; and in simple meningitis there is reason to believe that any severe pain in the head is precursory to, rather than 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 felt cold to the touch, the pupils were dilated, the pulse was contracted and feeble, and what was complained of chiefly was agonising pain in the head, with frequent chills and shiverings. 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 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.

In the same hospital, about the same time, there was also under my care 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, with shivering and retention of urine, the patient beginning to suffer in this way shortly after sleeping for some time flat on his back upon 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 was not of a neuralgic character, but the result of tenderness, and that pain of a neuralgic character in this case was antagonized rather than favoured by the inflammation.

2. 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 im-

possible 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 tenderness takes the place of these pains when the eruption is fully formed.

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 some slight feverish reaction, without chills and shiverings, and without neuralgia, the true neuralgia for the most part coming to an end coincidently with the establishment of the local inflammation.

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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.

And so it is that the real history of neuralgic disorder is none other than that of convulsion, and spasm, and tremor. There is always a state of vascular inactivity which is altogether inconsistent with the current notion that this form of pain is connected with a state of exalted vitality in the parts of the nervous system which have to do with sensation. The case is the very opposite of what it is commonly supposed to be. The case is one for which no other key is wanted than that of which use has been made all along—failure of electromotive action in a nerve-centre consequent upon insufficient supply of arterial blood to the centre, if only it be supposed that the scene of the failure be shifted from the parts which have to do with motion to those which have to do with sensation. The pathological evidence, indeed, is in keeping with itself everywhere, and with the physiological premises. The physiology explains the pathology, and the pathology establishes the physiology. From first to last it is one and the same story throughout.

EPILOGUE

A GLANCE RETROSPECTIVELY.

The sum of all that has been said amounts to no less than this—that a complete revolution is necessary in the doctrine of vital motion.

Every word, as it would seem, is opposed to the notion that vital motion is a manifestation of life, provoked by the action of certain so-called 'stimuli,' in a vital property of irritability, and that the disposition to such motion is directly proportionate to the supply of blood to the seat of irritability. When, for example, vital motion is exaggerated as it is in convulsion, in spasm, or in any other case of the kind, it is held that the irritability of this or that part of the nervous system is in a morbid state of exaltation because the supply of blood to the part is over-abundant. In fact, however, the actual case is 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, therefore, there is no ground whatever for the notion that the exaggeration of vital motion in any one of these cases is due to a state of exalted vitality, consequent upon hyperæmia, in a vital property of irritability. 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, as in all others, telling one and the same story.

As it would seem, also, every word is in harmony with the notion that vital motion is merely a mode of physical motion, for which the only key needed is that which is supplied in the natural workings of electricity and elasticity.

There is reason to believe that amœboid movements are the simple result of the action in amœboid bodies of the electricity belonging to these bodies in common with all terrestrial bodies.

The argument is sufficiently simple. Electrically the surface of the earth everywhere is in the state, not of zero, but of ever-changing *charge.* The action of this charge is to cause expansion in the charged bodies by keeping their molecules in a state of mutual repulsion; and, as the charge is ever varying, the expansion is ever varying also. Moreover, the action of this charge tells more expansively in some bodies, or in some parts of these bodies, than in others; more in bodies, or in parts of these bodies, which are less solid than in those which are more so, and, perhaps, only *perceptibly* in the bodies, or in the parts of these bodies, which are fluid rather than solid. Hence it may be that the changes in expansion, consequent upon changes in the amount of charge, may be perceptible in the parts of amœboid bodies which remain in the hyaline state of nascent protoplasm, and not in the parts which have become granular; for it is in amœboid bodies which are in the main hyaline, and not in those which have become more decidedly granular, that amœboid movements are really perceptible. And because these hyaline portions are distributed irregularly it may be that the variations of expansion, consequent upon changes in the amount of charge, may appear in the guise of that irregular protrusion and retraction of processes which is characteristic of amœboid movement. This is all. There is no occasion to call in the aid of a vital property of irritability. Indeed, it is difficult to see how this property could act so as to bring about the double movement of protrusion and retraction which has to be accounted for in this particular form of vital motion.

Passing on from the electrical history of amœboid movement to that of muscular movement, it is still seen that the elongation and contraction of the muscular fibres agrees with the protrusion and retraction of amœboid processes in this—that a charge which was present during the state of elongation is greatly diminished when this state passes into that of contraction; but the charge at work is not the same in the two cases. In the case of amœboid movement, this charge is simply that which belongs to amœboid bodies in common with all terrestrial bodies; in the case of muscular movement, the charge is peculiar to the living muscle and to the living nerves related to it. In the case of living muscle during the time of rest, it seems, indeed, as if each fibre and cell had to be regarded as an electromotive element in the state of open circuit,—as if the peculiar charge in question had its source in the statical action of these elements. It is held that the coats and contents of these fibres and cells are sufficiently heterogeneous in composition to give rise to electromotive action by simple apposition in conjunction with the action of the oxygen of the blood or atmosphere upon one of them, the coats and contents forming, in fact, a voltaic pair, and the arterial blood or fresh air being the fluid material by which electromotive action is set up in it; and also this—that the absence of electromotive action in the simple protoplasmic bodies in which amœboid movements are manifested is to be accounted for by the simple absence of these fibres and cells, or of any other sufficiently marked differentiation of composition. During the time of rest, it is held—that the electric condition of these electromotive elements is, not fluent but statical—a state of charge,—that this charge tells in causing expansion in the contents of the cells and fibres rather than in the coats, because these contents are less removed from the condition of nascent protoplasm than the coats, the case being really not unlike that of hyaline amœboid bodies in which amœboid movements are present, and of granular amœboid bodies in which such movements are absent,—and that the expansion in the contents of the fibres and cells, thus brought about by the charge connected with statical electromotive action, keeps the muscular fibres in the state of elongation, the expansion operating in the direction of the length of the fibres rather than in all directions equally, because the comparatively inexpansible coats of the fibres act upon the expansible contents in the same way as the tube of the thermometer acts upon the enclosed column of mercury. This is all. The case is simply one in which molecular attraction is for the time over-balanced by electrical repulsion. It is the very opposite of that which is presented at the time when the state of rest passes into that of contraction, and when electrical repulsion is overbalanced by molecular attraction. During the time of rest, the statical electromotive action which keeps the muscular fibres in the state of elongation is steady or constant; at the time when the state of rest changes into that of action, there is a very marked diminution of electromotive action, followed by as marked a state of electromotive inconstancy or unsteadiness. At the time when the state of rest changes into that of action, there is, that is, a change in the electric condition of the muscle which necessitates the development of instantaneous currents of high tension (induced currents and extra-currents), for such currents must spring into existence whenever the electromotive action passes from the state of constancy into that of inconstancy. Under these circumstances, indeed, muscular contraction must follow upon muscular elongation, not because the instantaneous currents of high tension have excited a state of action in a vital property of irritability, but simply because they have removed for a moment the charge which previously counteracted the action of the attractive force or forces inherent in the physical constitution of the muscular molecules—have removed this charge, it may be, by making a way by which the electromotive elements are able to pass from the state of statical action into that of fluent or current action. Nor is it to be objected that the electricity of the muscle is too feeble to produce these results, for it may be that the electromotive action of muscle is proportionate to the number of electromotive elements in the muscle, and that both charge and discharge are masked, the one by being expended in the production of muscular elongation, the other by being short-circuited within the body. Nay, it is even possible that the instantaneous currents of high tension which produce contraction would prove to be as powerful as those which constitute the discharge of the torpedo if they were not so short-circuited. And all that is said of the action of the natural electricity of the muscle in muscular movement is more than borne out by what is said of the action of artificial electricity in these movements, for in the latter case it is seen, not only that charge acts in the same way in causing muscular elongation, but also that the elongation is proportionate to the charge, and that the extra-contraction in electrotonus is only the simple result of the muscle having in this particular case to return from a state of extra-elongation consequent upon a state of extra-charge.

And so also in rigor mortis: for here the charge which counteracted the state of contraction in the living muscle is no longer present, and, here also, the contents of the muscular fibres and cells have become more or less altered in the direction of hardening.

Again. There is reason to believe that the case of the nerve electrically is not different from that of the muscle —that there is the same state of charge during the time of rest, produced in the same way, and the same disappearance of the charge, together with the same development of instantaneous currents of high tension at the time when the state ol rest changes into that of action, as well as the same intensification of charge and discharge arising from the interaction of innumerable electromotive elements. Nor is a different conclusion to be drawn from the fact that nerve differs from muscle in the absence of contraction at the time when the state of rest changes into that of action, for this absence may mean nothing more than this—that the contents of the nerve-fibres and nerve-cells disagree with those of muscle in being more elaborated, and that they are, for this reason, in the case of the granular bits of elderly protoplasm in which amœboid movements are no longer present. Indeed, there is reason to believe that the electrical histories of nerve and muscle are identical in this— that the electromotive elements in nerve and muscle interact reciprocally, the action of the nervous elements intensifying that of the muscular elements, and *vice versâ*.

Again: there is reason to believe that the work of the nervous system and of the blood in vital motion is carried out by electricity.

It would seem that the due electromotive action of the nerve-centres, and of all other parts of the nervous system, is dependent upon a due supply of arterial blood. It would seem that in the ordinary nerve-centres, when these centres are left to themselves, this action is constant, because there is not time for it to run down perceptibly between any two successive supplies of fresh arterial blood. It would seem that in the "rhythmic nerve-centres" the electromotive action is not constant, because (owing to some peculiar instability in the constitution of the electromotive elements) it runs down in the interval between any two fresh supplies of arterial blood, and that, for this reason, the working of these centres upon the muscles shows itself in rest alternating with action, the cause of the action being the instantaneous currents of high tension which are developed coincidentally with every falling or rising movement in electromotive action. And further, it would seem that in the case of convulsion, or spasm, or tremor, or neuralgia, or any other analogous form of exaggerated vital motion, this electromotive action changes from the state of constancy into that of inconstancy in certain nerve-centres in which this action is naturally constant, because the supply of arterial blood to these centres is at the time insufficient to keep it up to the natural pitch of constancy. *In each of these cases it is certain that the supply of arterial blood to these centres is invariably insufficient.* The case in this respect is the very opposite of what it is believed to be. And this being the fact, it follows that there may be a change from a state of constancy into a state of inconstancy in the electromotive action of these centres—a change meaning that this action is not kept up throughout the whole of the interval between any two successive supplies of fresh arterial blood, and, in this way, leading to the development of instantaneous currents of high tension —a change by which, so to speak, the electromotive condition of ordinary nerve-centres becomes reduced to the lower level of "rhythmic nerve-centres"— a change which expresses itself in exaggerated vital motion, convulsion, spasm, and the like, because the failure of electromotive action, thus brought about, happens to be in this or that part of the nervous system. Different localities are affected in these different cases of exaggerated vital motion, but the essential nature of the disorder is the same in all. And, in short, there is every reason to believe that the actual agency by which the work of the nervous system and of the blood in vital motion is carried out is, not a vital property of irritability, but electricity, or rather, electricity in conjunction with elasticity.

This, broadly stated, is the conclusion to which I am compelled to come. Everything, as it seems to me, is in flat contradiction to the current doctrine of vital motion; everything, as it seems to me, tends to bring phenomena which have been regarded as exclusively vital under the dominion of physical law—to transmute vital motion into what proves to be nothing more than a mere mode of physical motion.

THE END.