FOWLER RICHARD

EXPERIMENTS AND OBSERVATIONS

Richard Fowler

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Содержание

Richard Fowler
Experiments and Observations / Relative to the Influence Lately Discovered / by M. Galvani and Commonly Called Animal Electricity

PREFACE

The subject of the following experiments, has excited such general curiosity, that every new fact respecting it, may afford some gratification; and although the few which I have to offer, have not led me to what many may think very important conclusions, they will not I hope be found wholly undeserving of attention. The experiments were begun, with the view of ascertaining if the influence discovered by M. Galvani, be referrible to any known law of nature, or if it be itself a new law.

Finding that it indicated, with tolerable accuracy, the presence of very small degrees of the contractile power of muscles, without appearing in the least to diminish that power, as electricity and most other stimuli never fail to do; I thought it might be used with advantage, as a test, in the investigation of some important subjects in physiology; and I have accordingly employed it as such.

Every circumstance, observed in the course of these experiments, was carefully noted down, at the instant it occurred, and the greater number of these was made in the presence of gentlemen, whose accuracy I had reason to hope would detect any fallacy, which might have escaped myself. I have a particular pleasure, in expressing my obligation to Mr George Hunter of York, for the very friendly assistance which he afforded me, in almost every experiment, which rendered such assistance necessary.

Edin. May 28. }
1793. }

SECTION I.
Are the Phenomena, exhibited by the Applicationof certain different Metalsto Animals, referrible to Electricity?

The whole train of circumstances, which preceded this discovery, had a tendency to occasion the belief of its relation to electricity.

Some accidental appearances, certainly electrical, excited, by their novelty, the attention of the Professor of Anatomy at Bologna, to the investigation of the possible, but unknown, dependencies of the motions of animals upon electricity; and the astonishing effects of that influence upon the human body, particularly in paralytic diseases, whether owing to derangements of the nerves, or of the muscles; the experiments, which prove that the fluids of animals are better conductors of electricity, than water is; and that, "if an electric shock pass through a given part of a living animal, the same shock, after the animal is dead, will be visibly transmitted over the surface of the part, but not through it[1]:" the recollection, too, of that singular power, which some animals possess, as the *torpedo*, the *gymnotus electricus*, and the *silurus electricus*, of collecting and discharging at pleasure the electrical fluid; but, above all, the wonderful, but solitary, instance of an electrical shock received from a mouse, under dissection, recently related by his countryman Cotugno; were circumstances, which seem to have rendered the expectations of the Professor not a little sanguine as to his success.

At length, after many ingenious and interesting experiments, illustrative of the relations which subsist between artificial electricity and the involuntary motions of animals, a happy accident discovered to him the phenomena, which have since been the objects of so much curious research, and which have given to immortality the name of *Galvani*.

He one day observed, that some frogs, hooked by the spine of the back, and suspended from the iron palisades, which surrounded his garden, contracted frequently and involuntarily. Examining minutely into the cause of these contractions, he found that he could produce them at pleasure, by touching the animals with two different metals, at the same time in contact with each other.

To a mind prepared by such observations and experiments as those which had previously occupied M. Galvani, the resemblance which this new discovery bore to the facts he had before observed, must have produced conviction of the identity of their cause; and the experiments, which have since been made both by himself, and Dr Valli, have given no small degree of plausibility to the opinion. A repetition of some of these experiments excited my doubts as to the legitimacy of the conclusions which had been drawn from them, and induced me at length to proceed in the following investigation.

My first object was to ascertain as well the various circumstances, which are essentially requisite to the production of these new phenomena, as those in which they can be rendered most obvious. After a great variety of experiments, of which it would be unnecessary here to relate more than the result, I found that I could not excite in an animal the appearances described by Galvani with any substances whatever, whether solid or fluid, except the metals: and that the mutual contact of two different metals with each other, so far as I was able to determine, was in every case necessary to the effect.

When metals are either calcined, or combined with acids, they are no longer capable of exciting contraction. In estimating the comparative powers of different metals as excitors, I found zinc[2] by

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[1] Cavallo.

[2] On this metal Cronstedt has the following very curious remark: "It seems to become electrical by friction, and then its smaller particles are attracted by the loadstone; which effects are not yet properly investigated." Zinc is an ingredient of the best amalgam for smearing the rubbers of electrical machines: But I have not been able to render a bar of zinc electrical by friction, nor to find that its

far the most efficacious, especially when in contact with gold, silver, molybdena, steel, or copper, although these latter excite but feeble contractions when in contact only with each other. Next to zinc, tin foil, and lead appear to be the best excitors. But with zinc, and gold, silver, or molybdena, I have frequently succeeded in exciting contractions in the foot of a frog, upwards of a day after they had ceased to be excited, by arming the nerve with tin foil, and using some other metal as a conductor, in the way the experiment is commonly performed.

When the bulk of the metals is large, and the quantity of surface, of an animal with which they are in contact, is considerable, I think, the contractions are both stronger and more readily excited, than when the reverse of this is the case. Thus I have almost always been able to make a limb contract, by laying it upon a broad plate of zinc, and employing a half crown piece for an excitor, long after a small piece of zinc, and a silver probe, had failed to produce any effect.

I have said, that, in order to excite contractions, I believe it necessary that two different metals, communicating with the part to be excited, should be in contact with each other. Some few instances have been observed, which seem to prove the contrary. In a lecture, so long ago as October last, in which Dr Baillie of London mentioned Galvani's discovery, I think I remember him saying, that he had twice or thrice produced contractions by the application of one metal only: and Dr Valli, in his 9th letter upon this subject, speaks of his having done the same with a pair of scissars, made of bad steel, and in a frog recently killed. I think it not impossible that there may have been some unnoticed fallacy in these instances. I happened one day to touch the crural nerve of a frog, with a small gold tooth-pick slid from a silver case, and the leg instantly contracted; I again touched it, and it again contracted. At another time I observed contractions from touching a nerve, with a silver cannula, and at another from placing one in the folds of a silver chain. All these appeared at the time to be so many decisive instances of contractions from the application of one metal, till the following experiment seemed to afford a different explanation. Having placed, one end of a silver probe upon the sciatic nerve of a frog, lying in water some inches below the surface, I observed that no contractions followed, neither did they, when I touched the part of the probe above the surface with a piece of zinc. But when I touched it at the surface, so that both the zinc and the silver were in contact with the water, although the zinc was at the same time many inches removed from the frog, contractions were produced equally vigorous, as if both the metals had been in immediate contact with the frog.

I was now no longer at a loss to account for the contractions produced by the gold tooth pick, since the circumstances both of that case, and of the experiment related, were probably the same, two metals in contact with each other. The gold in immediate contact with a nerve; and the silver case communicating with it, and the muscles through the medium of the moisture, with which the whole was perhaps surrounded. This led me to examine the chain, and the cannula. I found both the links of the one, and the sides of the other, soldered with a different metal[3]. So that, in these cases, two metals had at the same time been in contact with a nerve, with moisture, and with each other.

However this may be with respect to the necessity of mutual contact, between two different metals in order to excite contractions, I have long ago found, that contractions may be excited in an animal, when no more than one metal is in contact with it[4].

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smaller particles were in any state attracted by the loadstone, unless they had been scraped off by means of an instrument of iron. But, in this way, the dust of any metal is rendered susceptible of the influence of the loadstone.

[3] If further experiments should establish decidedly, that the mutual contact of two different metals is absolutely necessary for the productions of Galvani's phenomena, may not this circumstance afford an useful test of the purity of the precious metals? For instance, contractions in an animal produced by the contact of a piece of gold or silver, whose purity we wish to ascertain, with a piece of the same metal known to be pure, would then prove incontestably the presence of alloy.

[4] In an able lecture, which Dr Monro lately delivered, chiefly upon this subject, he demonstrated the possibility of exciting contractions in the limb of a frog, without either of the metals he employed being in contact with it; or having any other communication with it than through the medium of some moist substance. In varying this experiment, I find, that if a frog be divided in two parts, just above the origin of the sciatic nerves, and put into a bason of water, the hind legs may be thrown into strong contractions, by bringing zinc, or tin-foil, and silver, in contact with each other, at the distance of at least an inch from the divided spine, so long as they are kept nearly in a right line with it. Water, in this case, is the only communication between the metals and the origin of the nerves.

At the time I first observed this fact, I was making experiments to ascertain whether it was possible to transmit the influence, which had excited contractions in one leg, into another, removed to some distance from it, and communicating with it, only by means of a single conducting substance, (such as silver, for example). For this purpose, I had separated from the trunk, and from each other, the hind legs of a frog recently killed, and had detached their nerves as far as the knee. I then laid them at some distance from each other, upon a plate of glass, and included the nerve of one leg, and the foot of the other, in the folded ends of a silver chain. With one hand I now raised from the muscles, upon the end of a silver probe, the nerve of the leg, whose foot was folded in the chain; and with a piece of zinc, in the other hand, touched, at the same time, the nerve and the probe. This leg was thrown into strong contractions; but none were excited in the other. I then touched the chain, and nerve of the other, and, to my surprise, both legs instantly contracted. I had observed, in the beginning of November last, that it was not necessary for the metals to be in contact with any thing but nerve, in order to excite contractions in the muscles, to which it was distributed; and had mentioned this fact immediately afterwards to the Medical Society of this place, as a sufficient refutation of the theory, which Dr Valli had formed of Galvani's discovery. It would not, therefore, have surpassed my expectations, had the influence, excited by the mutual contact of a piece of zinc, and probe, with the nerve, passed through the medium of the chain, from the leg, in which it first excited contractions, and produced contractions in the distant leg. But, I now thought that I had not only passed the influence from one leg to the other, but in one of the legs in a direction contrary to the course of its nerves. The removal of the leg, whose nerve communicated with the chain, convinced me of my error: but, at the same time, discovered to me a fact of much greater importance, than any with which I had hitherto been acquainted. For now, upon touching the chain alone with the zinc, I found that the leg, whose foot it still included, and whose nerve I held suspended upon a probe, contracted as strongly as before. The influence of the two metals, in contact with the nerve of the other leg, had not, therefore, passed into, and excited this.

It had from the first been known, that contractions could be excited by placing two different metals in contact, one with the nerve, the other with the muscles, and making a communication between them: but, in this experiment, the only metal in contact, either with the nerve, or muscle, was silver. Neither had the influence passed through the chain, and up the leg against the course of the nerve, in consequence of a communication by means of moisture subsisting between the zinc, and the foot, as well as between the silver chain, and the foot; for the experiment succeeded equally well when the chain was removed, and the foot laid upon a silver plate made perfectly dry. But when either the zinc, or probe was held by another person not communicating with me; or when either of them was insulated in a stick of sealing wax; no contraction whatever took place. Neither, indeed, were contractions excited in any part of the leg, except the foot, when the probe was withdrawn from the nerve; and the foot, and silver, were both touched with the zinc. It is then clear, that the influence, which, in the former case, excited the whole leg to contraction, must have passed through the medium of my body. It is not necessary that the silver should be laid under the foot; all that is required, is, that it should communicate with it by means of moisture; it may then be laid at almost any distance from it[5].

The course of this influence, however, was still undetermined: it might be from the muscles to the nerve: it might be from the nerve to the muscles. To ascertain this, and to prove that the influence, which had excited one limb to contraction, might pass on, through a foreign medium, and excite contractions in another, I made the following experiment.

The leg of a frog was disposed as in the former experiment. The probe, suspending the nerve, was held by myself; the zinc excitor by another person; and the leg of another frog formed the

[5] The contractions produced seemed to be strong in proportion to the extent of the surfaces of the metals in contact, strongest when a large plate of zinc is laid horizontally upon a large plate of silver or gold. If the zinc be suffered to remain in contact with the silver, for a little time, the contractions of the leg cease. The zinc may then be slid over the silver, till it even touch the leg without renewing the contractions: but, in withdrawing the silver, the leg contracts at the instant the silver parts from it!

communication betwixt us. So long as I had hold of the nerve, and the person assisting me held the foot of this interposed leg, no contractions were excited in it, by the influence, which passed through it and excited the other leg. But when the person holding the zinc, held the nerve of the interposed leg; and I held the foot, both legs contracted with equal strength. From this experiment it is evident, that Galvani's influence had passed either from the muscles, or the zinc and silver; and in the direct course of the nerves of both legs.

I was now in possession of an easy method of ascertaining the different substances, which do, or which do not, afford a passage to this new influence.

All the metals when pure appear to be excellent conductors; not quite so good when in the ore; and, I think, least so when combined with acids, forming metallic salts. They are however, in this state, by no means bad conductors, even when so carefully dried, as to leave no suspicion of the slightest degree of moisture adhering to their surface. But, when the metals are calcined, their capacity as conductors is quite destroyed: at least this was the case with the calces of zinc, of bismuth, of iron, and of mercury; the only ones, with which I have had an opportunity of making the experiment. I could not observe that any contractions were excited through the medium of stones, nor ever through barytes.

The different non-conductors of electricity are likewise, I find non-conductors of this influence: even wood, charcoal, and linen, do not conduct except when moist. But all the living vegetables I could procure afforded it a ready passage: probably from the fluids which they contain. While I held the probe which supported the nerve, I touched the shoe of a gentleman, who applied the zinc to the silver under the foot of the frog. Strong contractions were excited, but when he took off his shoe, and we held it between our hands, no contractions could be excited. In the first case, the influence had to pass through no more than the thickness of the shoe: in the second, through its whole length, which might not be all equally moist. This gentleman had on thread stockings. When I touched the foot of another, who had on cotton stockings, no contractions were excited. Cotton is a non-conductor of electricity.

Oils of all kinds are so far from conducting, that if the fingers of the person holding either the probe, or the zinc, have perspired much, even this operates as a complete obstruction to the passage of the influence: the instant the perspired matter has been wiped away, and the fingers have been dipped in water, it again passes, and excites contractions. When the intestines of a frog are removed, and its abdomen is filled with oil, no contraction can be excited by placing one metal upon its sciatic nerves, and bringing another in contact with it, either above or below the surface of the oil.

There is something singular in this respect, with regard to mercury. If the abdomen of a frog be filled with it, a piece of zinc passed through it, so as to touch the sciatic nerves, excites contractions. But a piece of silver, passed to them, excites none. Neither are any excited by touching the silver, beneath the surface of the mercury, with a piece of zinc. But I have before shewn, that, when water is used instead of mercury, contractions may be in this way excited; yet mercury is reckoned a much better conductor of electricity than water. I have repeatedly passed this influence through a great length of thin brass wire, and through the bodies of five persons communicating with each other, by dipping their fingers in basins of water placed between them; yet it did not appear to have lost any of its force, in this long and diffused passage: for the contractions excited in the frog's leg were equally strong, as when it had passed only through one person. Vitriolic acid, and alcohol appear still better conductors than water.

Wishing to ascertain whether it passed over the surface, or through the substance of metals, I coated several rods of different metals with sealing wax, leaving nothing but their ends, by which they were held, uncovered. Contractions were excited as readily through the media of these, as if they had not been coated. It seems to meet with no obstruction in passing from link to link, of several chains, even when no pressure, except that of their own weight, is used to bring them into contact. I was led from this to hope, that I should be able to make it pass through a very thin plate of air. I, therefore, coated a stick of sealing wax, with a plate of tin-foil, and then made an almost imperceptible division a-cross it with a sharp pen-knife. But even this interruption of continuity in the conductor was sufficient effectually to bar its passage.

The chains, through which it passed most readily, were of gold and silver. It did not pass through a very long and fine brass chain, unless as much force as could be used, without breaking the chain, was employed to bring its links into close contact.

I next proceeded to examine if the capacity of different substances, as conductors, or non-conductors, was at all affected by differences of their temperature. But this was not the case with zinc, iron, water, coal, or a common crucible, the only substances with which I tried the experiment. A red hot iron, and boiling water, conducted equally as well as iron and water that had not been heated: and neither the crucible, nor the coal, became conductors from any addition of heat.

I at first thought that ice conducted; but as, on some trials, no contractions were excited through its medium; and as it appeared uniformly to conduct ill in proportion to the dryness of its surface; I suspect that, if perfectly dry, it would not conduct at all. The instant a part of its surface had been dissolved by the heat of the room, contractions were excited with as much ease, as they usually are through a bason of water. It would appear, therefore, that neither very hot, nor very cold water disperse this influence, as has been asserted by Dr Valli, nor do they seem in the least degree to diminish its power of producing contractions[6].

It appears upon the whole to be necessary, that this influence should pass to a part in a very condensed state, in order to excite contractions: although there are some facts, which, without reflecting, might lead one to suppose, that, passing even in a diffused state, it would excite them. In making that experiment, in which the piece of zinc under the foot of a frog is touched with zinc, while its crural nerve is supported by a silver probe; no contraction takes place, if the probe be either lowered, so as to come in contact with the muscles of the thigh, or if it be made to touch the silver under the foot.

If again, two persons, one of whom holds the probe, the other the zinc, communicate with each other by dipping their unemployed hands in a bason of water; and the person using the zinc holds another leg of a frog, suspended between his fingers by its nerve beneath the surface of the water; no contraction will take place in this leg, when the silver under the other is touched with zinc, at the same time that strong ones are excited in that other. But, if its nerve be raised above the surface of the water, it then contracts as vividly as the other. It appears that in the last of these instances, at least the greater part of the influence had diffused itself through the water, instead of passing directly through the nerve, from the fingers of the person holding it, and that in both it had passed into the legs, in too diffused a state to excite them to contraction.

I have often likewise observed, that when the nerve of a nearly exhausted leg of a frog had been laid upon a piece of zinc, and both were touched with silver, the contractions excited were very distinct: but when the zinc was placed in contact with the muscle, as well as with the nerve, either no contractions could be excited, or such feeble ones that they were scarcely perceptible.

Contractions, however, certainly may be excited in different parts of a frog, without making any division of its skin, by laying the part of the frog to be excited upon a plate of zinc, or tin-foil, and passing a piece of silver over it, till all three are in contact with each other[7]. Yet even here the influence does not pass into the part in so diffused a state as it may at first appear to do. For the skin of these animals is abundantly supplied with nerves, whose trunks communicate, at different places,

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[6] 'L'eau trop échauffée, ou qui est en éboullition, disperse l'électricité, de manière à en détruire les phenomènes.''L'excès du froid prive l'eau même de la propriété de conduire le fluide en question.' —*Dr Valli, Lettre 9me.*

[7] It was in this way, indeed, that I have always excited contractions, when I have employed this new mode of influencing animals, as a test of remaining life in any part of them.They were constantly kept in fresh water, as the situation most natural to them, during the whole of the time they were under experiment; and their skins were suffered to remain as entire as possible, since I found their muscles lost their contractile power, in a few hours, and became rigid when exposed, deprived of their skins, to the action of the water.

with those which supply the muscles. And the contractions are always strongest, and most readily excited, when the silver is passed over the course of any of the nerves, which go to the muscles.

From the fact, which I have before mentioned, that a limb may be made to contract, when the metals have apparently no communication with any part of it except its nerve; it might reasonably be doubted, whether, in any case, a communication between the muscles, as well as the nerve, and the metals, were necessary, in order that contractions may be excited.

Several considerations, however, induce me to believe, that such communication is absolutely requisite. If the contact of two different metals were alone sufficient to excite contractions; contractions should always take place, whenever a good conductor is interposed between the metals, and the nerve alone. But I have, in no instance, observed this to be the case. In the experiment, where the crural nerve must be supported upon a silver probe, it is necessary that the piece of silver, with which the zinc is brought in contact, should communicate either immediately, or through some good conducting medium, with the muscles of the foot, or leg, before any contraction takes place. And even in the experiment, where water forms the only communication between the metals, and the origin of the sciatic nerves, that same water, it must be observed, forms likewise a communication between the metals and the muscles, to which these nerves are distributed. But the fact, which appears to me most decisive of this question, is the following: When a nerve, which for some time has been detached from surrounding parts, is either carefully wiped quite dry with a piece of fine muslin, or (lest this should be thought to injure its structure,) suffered to remain suspended till its moisture has evaporated; no contractions can be excited in the muscles, to which it is distributed, by touching it alone with any two metals in contact with each other. But, if it be again moistened with a few drops of water, contractions instantly take place: and, in this way, by alternately drying and moistening the nerve, contractions may, at pleasure, be alternately suspended and renewed for a considerable time. It may, indeed, be contended, that the moisture softened, and thus restored electricity and free expansion to the dried cellular membrane surrounding the fibres, of which the trunk of a nerve is composed; and thus, by removing constraint, gave free play to their organization[8].

But from observing, that, in every other instance, where contractions are produced by the mutual contact of the metals, a conducting substance is interposed between them and the muscles, as well as between them and the nerve; I think it would be unphilosophical not to allow, that, in the instance in question, the moisture, adhering to the surface of the nerve, formed that requisite communication between the metals and the muscles.

I relate the following fact, in this place, because at the same time that it gives further confirmation to the above opinion, it affords an instance in which insulation diminished the effect of the metals. I had one day laid the nearly exhausted leg of a frog upon my hand, with a piece of zinc in contact with its nerve only; and, when I touched these with a silver probe, tolerably strong contractions were excited, even when the nerve appeared dry: but when both the leg and the metals, thus disposed, were insulated by means of glass and sealing wax, the contractions were scarcely perceptible. My hand, it would appear, had, in these instances, supplied the place of the moisture in the other; and been the conducting medium between the muscles and the metals.

This communication of the muscles with the nerve, through the medium of the metals, had appeared to Dr Valli a circumstance so essential to the production of Galvani's phenomena, that

[8] M. Fontana, in the first volume of his work on Poisons, mentions some facts, which may, to some, appear to give considerable countenance to this explanation. The microscopical eels found in dry and smutty wheat; the seta equina or gordius of Linnaeus; and the wheal polypus, all, when dry, become apparently dead: but again recover motion and life when moistened with water. One of the latter was put, by M. Fontana, upon a bit of glass, and exposed, during a whole summer, to the noon-day sun. It became so dry that it was like a piece of hardened glue. A few drops of water, however, did not fail to restore it to life. Another was, in this way, recovered after a similar exposure of a year and a half. Father Gumillo, a Jesuit, and the Indians of Peru, are quoted by the same author, on the authority of Bonguer, as speaking of 'a large and venemous snake, which being dead and dried in the open air, or in the smoke of a chimney, has the property of coming again to life, on its being exposed, for some days, to the sun, in a stagnant and corrupted water.'But it would almost require the credulity of an Indian to credit the testimony of the Jesuit.

(taking it for granted they were occasioned by the action of the electrical fluid), it seems to have suggested the hypothesis, which he has offered in order to account for them.

Aware that no electrical phenomenon can possibly have place, except between the opposite states of positive and negative electricity, or, in other words, where there is a breach of equilibrium in the distribution of the electrical fluid; he supposes it to be one office of the nerves, to produce this breach of equilibrium, by continually pumping (to use his own expression) the electrical fluid from the internal parts of muscles, and in this way rendering them negative, with respect to the external surface. The brain, he makes the common receptacle for this fluid. The metals, he seems to consider in the light of a conductor, interposed between the outside of muscles and their nerves. And the rapid transmission of the fluid to restore the equilibrium, as the cause of the contractions.

He presumes his hypothesis proved from the following considerations:

I. The interval which commonly takes place between the contractions; which interval, according to him, is necessary for the restoration of the breach of equilibrium.

II. From observing, that fishermen, in order to preserve their fish from putridity, crush their brains; and thus, by interrupting the medium between the external and internal surfaces of muscles, prevent these repeated discharges of the electrical fluid, which, according to Dr Valli, hastens their putridity.

III. From finding that in general, when the sciatic nerve on one side of a living frog was divided, the other being left entire, communicating with the brain, both armed and equally excited, the limb, in which the nerve had been divided, preserved its power of contracting longer than the other. From this well devised experiment, he concludes, likewise, that animal electricity is the principle of life. That, on the side where the nerve remained entire, it was withdrawn from the muscles, and deposited in the brain. That, from the impossibility of this taking place on the other side, where the nerve was divided, it had continued in the limb, and enabled it to contract.

If it were indisputably true, as I once believed, that contractions could be excited in a limb without the metals having any communication with it, except through the medium of a nerve; this circumstance would alone be a sufficient refutation of Dr Valli's hypothesis: but, as I have already shewn, that contractions were not in this way produced in any experiment, which I have made, when no moisture, forming a communication between the metals and the muscles, had been left adhering to the surface of the nerve, it becomes necessary to have recourse to less dubious arguments.

The Dr should have recollected that, in cases of a breach of equilibrium in the distribution of the electrical fluid, all that is required, in order to restore equality of distribution, is, the interposition of a single conducting substance between the place in which it abounds, and that in which there is a deficiency. Whereas, in the phenomena, which he attempts to explain, two conducting substances are necessary to the effect.

When a separated limb is placed under water, one would naturally imagine, that from the perfect communication, which is then formed between the external surfaces of muscles and their nerves, no breach of equilibrium could possibly have place: yet we find Galvani's phenomena even more readily produced in this situation, than when both muscles and nerves are free from surrounding moisture.

The following experiment was made with a view of rendering the equilibrium of the electrical fluid, in different parts of frogs, as perfect as possible.

The head of a frog having been separated from its body, the latter was laid upon a plate of zinc, held by a person sitting in an insulated chair, which communicated with the prime conductor of an electrical machine. The machine was put in action, and both the person and the frog were electrified positively. In these circumstances, no sparks could be drawn from the frog, by the person holding it: nor could any other electrical appearance take place between them. But, when a piece of silver was passed over different parts of the frog, and, at the same time, brought into contact with the zinc plate, contractions were uniformly excited, differing not in the least, either in strength or frequency, from those which are excited when no artificial electricity is present. The result was precisely the same, when the frog and the person holding it were negatively electrified. This experiment was often repeated. The following experiment was made, in order to see if the effect produced upon a frog, by the passage of artificial electricity from any part of its body, would be increased by employing two different metals as conductors.

A frog was laid, successively, upon a number of different metals, insulated upon glass, and positively electrified by communicating with the prime conductor of an electrical machine. The contractions produced in the frog, thus disposed, by drawing sparks from it, with metals different from those on which it was placed, were not in the least stronger, than those occasioned by drawing similar sparks from it, with conductors of the same metal.

In establishing a communication between two opposite electricities, as, for example, between the two sides of a charged phial, it is matter of indifference to which the conductor is first applied. But it is by no means so, in the case of muscles and armed nerves. For, if one branch of a conductor be applied to the tin-foil arming a nerve, before the other branch has been applied to the muscles, it frequently fails to excite contractions. If first applied to the muscles, this is very seldom the case.

As for the intervals of rest which alternate with the contractions, and which the Dr considers as employed by the nerves, in restoring the breach of equilibrium between the internal surfaces of muscles, and their external; these may possibly admit of a different explanation.

We find them alternating with contractions however excited. It is difficult to conceive, that violent contractions should not derange in some degree, however slight, the intimate organization of muscular fibres: and some time must necessarily elapse before their elasticity can have restored the organized particles, of which they are composed, to that relative situation with respect to each other, which will fit them for again contracting.

This explanation is drawn from observing the following facts. Hearts, taken from the living thorax, and exposed to the action of a strong stimulus, contract vividly for a time, and then cease to be effected by any further application. If they be then removed from the stimulus, and placed for a time either in cold water or in open air, they are observed to regain their susceptibility of the action of stimuli, and again contract. Mr Coleman, in his excellent dissertation on Suspended Respiration, makes an observation, which I have often had opportunity of verifying: that hearts distended with blood, and in which no contraction can be produced, by scratching their surface with a pointed instrument, contract spontaneously, if one of the large vessels, at some distance from them, be cut so as to evacuate some of the blood.

The organization, in this case, is suffered to recover by the removal of the stimulus, (distention) which had deranged it. Even, in the living and entire animal, the heart does not renew its contractions, on the first influx of blood. Some time must elapse, while it recovers from the derangement occasioned by the preceding contraction.

I have repeatedly excited, by means of zinc and silver, contractions in the leg of a frog, whose head had been divided from its body, upwards of three days before. The receptacle, for the electrical fluid, was in these cases removed. Now, either the nerves continued extracting it from the internal parts of muscles, or they did not. If they did, having no longer a receptacle, in which they could deposite their electricity, they must have remained positively electrified; and thus, being in the same state with the outer surface of the muscles, no contraction should, according to the hypothesis, have been excited by the application of the metals. But this is contrary to the fact.

If it be contended, on the other hand, that their pumping power had ceased; then the first application of the metals, which produced a contraction, having restored the equilibrium, which could not afterwards be broken, must have precluded the possibility of further contractions. But this too is contrary to fact.

This argument appears, to me, to do away all support, which the hypothesis may seem to derive from the experiment, before quoted, of applying the metals equally to both sciatic nerves, after one of them had been divided; I may however remark, that the pain necessarily excited by arming a nerve, whose communication with the brain was not interrupted, would fully account for the more rapid exhaustion of the muscles, to which it belonged, compared with such as had not been acted upon by so strong an additional stimulus. As fact, however, is always more satisfactory than argument, I shall relate the following accidental experiment, in proof of the relevancy of the foregoing observation.

Four days after I had divided the crural nerve of a female frog, full of spawn, I found her dead; she had been observed alive the night before. The application of the metals to the leg, whose nerve had not been divided, did not excite the slightest contractions, but on applying them to the leg, in which the nerve had been divided, tolerably strong contractions were excitable, for more than twelve hours after she was found. The spawning season had closed, upwards of a week before this happened, and, as this frog had long been without a male to assist her, it is probable, that her death had been occasioned by the retention of her spawn, as it was found in a very dissolved state. The pain, necessarily preceding such a death, could affect the different parts of the animal, only through the medium of its nerves; and hence the exemption of that part from its effects, to which the communication, by nerve, had been interrupted.

The same observation will apply to that argument, which Dr Valli has drawn, in support of his hypothesis, from the practice of fishermen. By destroying the brain, they take away all sense of pain, and, consequently, preclude that exhaustion which is so notorious for disposing to putridity.

Конец ознакомительного фрагмента.