XI. *Observations on the Influence, which incites the Muscles of Animals to contract in Mr.* Galvani's *Experiments. By* William Charles Wells, *M. D. F. R. S.*

Read March 19, 1795.

MR. VOLTA, in his letters to Mr. CAVALLO, which have been read to this Society, not only has shewn that the conclusions, which Mr. GALVANI drew from his experiments on the application of metals to the nerves and muscles of animals, are in various respects erroneous, but has also made known several important facts, in addition to those which had been discovered by that author. As he appears, however, from these letters, to have fallen into some mistakes himself, and has certainly not exhausted the subject which he has treated in them, I shall venture to communicate to this learned body a few observations I have made respecting it, which may contribute both to correct his errors, and to increase our knowledge of the cause of those motions, which have been attributed by Mr. GALVANI and others to an animal electricity. These observations will be so arranged, as to furnish answers, more or less satisfactory, to the following questions: Does the incitement of the influence which, in Mr. GALVANI's experiments, occasions the muscles of animals to contract, either wholly, or in part, depend upon any peculiar property of living bodies? What are the conditions necessary for the excitement of this influence? Is it electrical?

When a muscle contracts upon a connection being formed, by means of one or more metals, between its external surface and the nerve which penetrates it, Mr. GALVANI contends that, previously to this effect, the inner and outer parts of the muscle contain different quantities of the electric fluid; that the nerve is consequently in the same state, with respect to that fluid, as the internal substance of the muscle; and that upon the application of one or more metals between its outer surface and the nerve, an electrical discharge takes place, which is the cause of the contraction of the muscle. In short, he supposes a complete similarity to exist between a muscle, in a proper condition to exhibit this appearance, and a charged Leyden phial; the nerve of the former answering, as far as his experiments are concerned, the same purpose as the wire, which is connected with the internal surface of the latter.

Now, if this were just, such a muscle ought to contract, whenever a communication is formed between its internal surface and the nerve, by means of *any* conductor of electricity; and accordingly Mr. VOLTA, who to a certain extent adopts Mr. GALVANI's theory, asserts this to be the case, as often as the experiment is made upon an animal which has been newly killed. But I am inclined to believe that he rests this assertion upon some general principle, which he thinks established, and not upon particular facts; for he gives none in proof of it, and I have often held a nerve of an animal newly killed in one hand, while with the other I touched the muscle to which the nerve belonged, but never saw contractions by this means excited. I have also frequently taken hold of a nerve of an animal, which was recently killed, with a non-conductor of electricity, and have in this way applied its loose end to the external surface of the muscle which it entered, without ever observing motion to follow. I think, therefore, I am entitled to conclude, not only that the theory advanced by Mr. GALVANI, respecting the cause of the muscular motions in his experiments, is erroneous; but also, that the influence, whatever its nature may be, by which they are excited, does not exist in a disengaged state in the muscles and nerves, previously to the application of metals. Should it be urged against this conclusion, that, since metals are much better conductors of electricity than moist substances, the charge of a muscle may be too weak to force its way through the latter, though it may be able to pass along the former; my answer is, that, in all Mr. GALVANI'S experiments, the nerve makes a part of the connecting medium between the two surfaces of the muscle, and that the power of no compound conductor can be greater than that of the worst conducting substance, which constitutes a part of it.

It may be said, however, that, although there is no proof that any influence naturally resides in the nerves or muscles, capable of producing the effects mentioned by Mr. GALVANI, these substances may still, by some power independent of the properties they possess in common with dead matter, *contribute* to the excitement of the influence, which is so well known to exist in them, after a certain application of metals. Before I enter upon the discussion of this supposition, I must observe, that there are two cases of such an application of metals; the first is, when we employ only one metal; the second, when we employ two or more. With respect to the first case, a late author, Dr. FOWLER, who seems to have made many experiments relative to this point, positively asserts, that he never saw a fair instance of motion being produced by the mere application of a single metal to a muscle and its nerve. I shall, therefore, defer treating this case, till I speak of the conditions which are necessary for the excitement of the influence. Nor will the present subject suffer from this delay; for if it be shewn, as I expect it will, that, when two or more metals are used, the muscle and its nerve do not furnish any thing but what every other moist substance is equally capable of doing, it will, I think, be readily granted, that they can give nothing more, when only one metal is applied to them.

In regard to the second case, Mr. VOLTA has affirmed, or has said at least, what I regard as equivalent to affirming, that, when two metals are employed, the influence in question is excited by their action upon the mere moisture of the parts which they touch. The proofs, however, of this assertion were reserved for some future communication. But as more than two years have now elapsed since they were promised, and none have been given to this Society, or have appeared, as far as I can learn, in any other way, I hope I shall not be thought precipitate, if, at this distance of time, I offer one of the same point, which seems to me both plain and decisive.

It is known, that, if a muscle and its nerve be covered with two pieces of the same metal, no motion will take place upon connecting those pieces, by means of one or more different metals. After making this experiment one day, I accidentally applied the metal I had used as the connector, and which I still held in one hand, to the coating of the muscle only, while with the other hand I touched the similar coating of the nerve, and was surprised to find that the muscle was immediately thrown into contraction. Having produced motions in this way sufficiently often to place the fact beyond doubt, I next began to consider its relations to other facts formerly known. I very soon perceived, that the immediate exciting cause of these motions could not be derived from the action of the metals upon the muscle and nerve, to which they were applied; otherwise it must have been admitted, that my body and a metal formed together a better conductor of the exciting influence than a metal alone, the contrary of which I had known, from many experiments, to be the case. The only source, therefore, to which it could possibly be referred, was the action of the metals upon my own body. It then occurred to me, that a proper opportunity now offered itself of determining, whether animals contribute to the production of this influence by means of any other property than their moisture. With this view, I employed various moist substances, in which there could be no suspicion of life, to constitute, with one or more metals, different from that of the coatings of the muscle and nerve, a connecting medium between those coatings, and found that they produced the same effect as my body. A single drop of water was even sufficient for this purpose; though, in general, the greater the quantity of the moisture which was used, the more readily and powerfully were contractions of the muscle excited. But, if the mutual operation of metals and moisture be fully adequate to the excitement of an influence capable of occasioning muscles to contract, it follows, as an immediate consequence, that animals act by their moisture alone in giving origin to the same influence in Mr. GALVANI's experiments, unless we are to admit more causes of an effect than what are sufficient for its production.

Before I dismiss this part of my subject I may mention, that, being in possession of a method to determine what substances are capable, along with metals, of exciting the influence, I made several experiments for the purpose of ascertaining this point. I found, in consequence, that all fluid bodies, except mercury, that are good conductors of electricity, all those at least which I tried, can with the aid of metals produce it. The bodies I tried, beside water, were alcohol, vinegar, and the mineral acids; the last both in their concentrated states, and when diluted with various portions of water. Alcohol, however, operated feebly. On the other hand, no fluid, which is a non-conductor of electricity, would assist in its production: those upon which the experiment was made were the fat and essential oils. Ether, from its similarity to alcohol, I expected would also have concurred in the excitement of the influence, but it did not; neither would it conduct the influence when excited by any other means. I may remark, however, that the ether I employed had been prepared with great care; other ether, therefore, less accurately made, may possibly be found to contribute to the excitement of the influence, either from the undecomposed alcohol, or naked acid, it may contain.

Having thus given an answer to the first question, I proceed to the discussion of the second.

It has hitherto been maintained by every author, whose works I have read upon the subject of Mr. GALVANI's experiments, and by every person with whom I have conversed respecting it, that metals are the only substances capable, by their application to parts of animals, of exciting the influence, which in those experiments occasions the muscles to contract. But it appears rather extraordinary, that none of those, who contend for the identity of this influence and the electric fluid, have ever suspected, that the only very good *dry* conductor of the latter which we know, beside the metals, possesses like them the property of exciting the former. I confess, however, that it was not this consideration, but accident, which led me to discover that charcoal is endowed with this property, and in such a degree that, along with zinc, it excites at least as strongly as gold with zinc, the most powerful combination, I believe, which can in this way be formed of the metals. But to prevent disappointments I must mention, that all charcoal is not equally fit for this purpose, and that long keeping seems to diminish its power.

It being shewn that charcoal is also to be ranked among the exciters of this influence, I shall now speak of the circumstances, in which both it and the metals must be placed, to fit them for the exercise of their power. With respect to metals, Mr. VOLTA maintains, that to this end it is only necessary, that two different species be applied to any other body which is a good conductor of electricity, and that a communication be established between the two metallic coatings. But charcoal is a much better conductor of electricity than water, and yet metals in contact with it alone will not excite. Again, Mr. VOLTA says, that the simple application of two metals to two parts of an animal disturbs the equilibrium of the electric fluid, and disposes it to pass from one of the parts to the other, which passage actually takes place, as soon as a conductor is applied between the metals. But what should prevent the passage of the fluid *before* the application of a new conductor, since the metals were already connected by means of the moisture of the animal? Further, a consequence of this opinion is, that, if the under surfaces of two different metals be placed in moisture, and their upper surfaces be afterwards connected by means of a nerve, still attached to its muscle, contractions ought then to be produced; since the whole quantity of the electric fluid necessary to restore the equilibrium, which has been disturbed by the action of the metals, must pass through the nerve. This experiment I have made, and as I did not find the muscle to contract, I must hold Mr. VOLTA's opinion on this point to be likewise ill founded. The fact is, that as far as the contraction of muscles is a test, whether the influence exists or not, and we have no other, it is never excited, when two metals, or one metal and charcoal are necessary for this purpose, unless these substances touch each other, and are also in contact with some of the fluids formerly mentioned.

But there is still another requisite for the excitement of the influence, which is a communication, by means of some good conductor of electricity, between the two quantities of fluid, to which the dry exciters are applied, beside that which takes place between the same quantities of fluid, when the dry exciters are brought into contact with each other. As from this last circumstance, a complete circle of connection is formed among the different substances employed, it has been imagined by many, that the individual quantity of the influence excited goes the whole round, each time contraction is produced. There is an experiment however, first, I believe, made by Dr. FOWLER, which appears to contradict this opinion. He brought two different metals into contact with each other in water, at the distance of about an inch from the divided end of a nerve, placed in the same water, and found that the muscles, which depended upon it, were from this procedure thrown into contractions. Now, in this experiment, there was surely room enough for the influence to pass through both metals, and the moisture immediately touching them, without going near to the nerve. I think it, therefore, probable, that motions are in no case produced by any thing passing from the dry exciters through the muscles and nerve, but that they are occasioned by some influence, naturally contained in those bodies as moist substances, being suddenly put in motion when the two dry exciters are made to touch both them, and each other; in like manner as persons, it is said, have been killed by the motion of their proper quantity of the electric fluid. But to return from conjecture to facts, I shall now examine, whether it be always necessary to employ two dry exciters, that is, two metals, or one metal and charcoal, in order to occasion contractions.

Gold and zinc, the first the most perfect of the metals, the other an imperfect one, operate together very powerfully in producing contractions; while gold, and the next most perfect metal, silver, operate very feebly. It would seem, therefore, that the more similar the metals are, which are thus used, the less is the power arising from their combination. Two pieces of the same metal, but with different portions of alloy, are still more feeble than gold and silver; and the power of such pieces becomes less and less, in proportion as they approach each other in point of purity. From these facts it has been inferred, that, if any two pieces of the same metal were to possess precisely the same degree of purity, they would if used together be entirely inert, in regard to the excitement of muscular contractions; in confirmation of which, many persons have asserted, that they have never observed muscles to move from the employment of two such pieces of metal, or of one piece of metal having the same fineness through its whole extent. Others, however, upon the authority of their observations, have maintained the contrary; and to the testimony of these I must add my own, as I have frequently seen muscular motions produced not only by a single metal, but likewise by charcoal alone. Nor will credit be denied me on this head, after I have pointed out certain practices, by which any one of those substances may at pleasure be made to produce contractions. The most proper way of mentioning these practices will, perhaps, be to relate in what manner they came to my knowledge.

I one day placed a piece of silver, and another of tin-foil, at a small distance from each other upon the crural nerve of a frog, and then applied a bent silver probe between them, with the view of ascertaining, whether contractions would arise, agreeably to Mr. Volta's declaration, from the influence passing through a portion of the nerve without entering the muscles. Having finished this experiment, I immediately after applied the same probe between the silver coating of the nerve and the naked muscles, and was surprised to see these contract. A second and third application were followed by the same effects, but further applications were of no avail. It then occurred to me that motions might re-appear, if I again touched the two coatings with the probe, and the event proved the conjecture to have been fortunate; for after every application of the probe to the two coatings, contractions were several times excited by it. The fact being thus established, that under certain circumstances contractions could be produced by silver alone, it next became a subject of inquiry, whether this was owing to any disposition of the muscles and nerve, which had been induced upon them by Mr. VOLTA'S experiment, or whether, the condition of the muscles and nerve being unaltered by that experiment, the silver had gained some new property by coming into contact with the tin-foil. The point in doubt was soon determined, by applying the probe to a piece of tin-foil, which had no connection with any part of the animal; for, when this was done, it was again enabled to produce contractions. As these experiments, however, frequently did not succeed when made upon other frogs, I afterwards varied the metals, and found in consequence, that zinc, particularly if moistened, communicated an exciting power pretty constantly to silver, gold, and iron. If any of these metals were slightly rubbed on the zinc, they almost always acquired such a power.

It will, perhaps, be thought from the last-mentioned circumstance, that, in every instance of motion being in this way produced, it was in truth owing to some part of one of the metals having been abraded by the other; so that, under the appearance of one metal, two were in reality applied. But it can scarcely be supposed, that, from touching the polished surface of tin-foil in the gentlest manner with the smooth round end of a silver probe, any part of the former metal was carried away by the latter; and even when friction was used, as the zinc was much harder than the gold and silver, it is not probable that it was in the least abraded by them. Besides, moisture, as I have already said, increases this effect of friction, though it lessens friction itself.

The most powerful argument, however, in favour of my opinion, is another fact I discovered in pursuing this subject; which is, that an exciting power may be given to a metal by rubbing it on many substances beside another metal, such as silk, woollen, leather, fish-skin, the palm of the human hand, sealing-wax, marble, and wood. Other substances will, doubtless, be hereafter added to this list.

As the metals while they were rubbed were held in my hand, which, from the dryness of its scarf-skin, might have afforded some resistance to the passage of small quantities of the electric fluid; and as the substances, upon which the friction was made, were either electrics, or imperfect conductors of electricity; I once thought it possible, that the metal subjected to the friction had acquired by means of it an electrical charge, which, though very slight, was still sufficient to act as a stimulus upon the nerves to which it was communicated. But that this was not the case was afterwards made evident, by the following experiments and considerations.

1. A metal, rendered capable by friction of exciting contractions, produced no change upon Mr. BENNET's gold-leaf electrometer.

2. The interposition of moisture does not, in any instance I know of, increase the effect of friction in exciting the electric fluid. In some instances it certainly lessens this effect. But moistened substances, when rubbed by a metal, communicate to it the capacity of producing contractions, much more readily than the same substances do when dry.

3. If my hand, from being an imperfect conductor, had occasioned an accumulation of electricity in the metal which was rubbed, a greater effect of the same kind ought certainly

to have been produced by insulating the metal completely; which is contrary to fact.

4. I placed a limb of a frog, properly prepared, upon the floor of my chamber; if a severe frost had not prevailed when I made this experiment, I should have laid it upon the moistened surface of the earth. I then raised from the muscles, by means of an electric, the loose end of the nerve, and touched it with the rubbed part of a piece of metal; but no contractions followed. To be convinced that this was not owing to any want of virtue in the metal, I kept the same part of it still in contact with the nerve, while I applied another part to the muscles; immediately upon which contractions were excited.

5. Admitting now the limb of an animal to be in such an experiment completely insulated, and that the metal actually becomes electrical from the friction it undergoes, surely a very few applications can only be required to place them both in the same state with respect to the electric fluid; and when this happens, all motions depending on the transflux of that fluid must necessarily cease. I have found, however, that a piece of metal which has been rubbed will excite contractions, after it has been *many* times applied to the limb. In one instance, vigorous contractions were occasioned by the 200th application; and if I had chosen to push the experiment further, I might certainly have produced many more. I may mention also, as connected with this fact, that I have frequently observed a piece of metal to excite motions, an entire day after it had been rubbed.

What I have said will, probably, be thought more than sufficient to prove, that metals, after being rubbed, do not produce muscular contractions by means of any disengaged electricity they contain. If my opinion were now asked, respecting the mode in which friction communicates such a power to them, I should say, that the part which has been rubbed is so far altered, in some condition or property, as to be affected differently, by the fluid exciters, from a part which has not been rubbed; in short, that the rubbed part becomes, as it were, a different metal. There are two facts, beside those already mentioned, which support this conjecture. The first is, that when I have endeavoured to give an equal degree of friction to the two parts of the metal which I applied to the muscle and its nerve, little or no motion was excited by it; so that it is reasonable to suppose, that, if precisely the same degree of friction were given to both the parts, no contractions would ever be produced by them, when used in this way. The second is, that, although only one part of the metal be rubbed, still, if both the muscle and nerve be coated with some other metal, the application of the rubbed metal between these similar coatings will not be followed by motions; which, however, will immediately be produced, by touching the naked muscle and nerve with the same piece of metal. But, whether any part of my reasoning upon this head be admitted as just or not, it must yet be granted, as I think I cannot be mistaken respecting the facts which have been mentioned, that very slight accidents may give the power of exciting contractions to a single metal, which had it not before; and that we may hence easily account for the discordant testimonies of authors upon this point.

Hitherto I have spoken only of the effects of friction upon metals. But to conclude this part of my subject, I must now remark, that charcoal, though from its friability not very fit for the experiment, may yet be rendered capable by the same means of producing contractions, without the assistance of any of the metals.

My next and last object is to inquire, whether the influence, which in all these experiments immediately excites the muscles to act, be electrical or not.

The points of difference between any two species of natural bodies, even those which, from the similarity of some of their most obvious qualities, have once been thought the same, are found, upon accurate examination, greatly to exceed in number those of their agreement. When, therefore, two substances are known to have many properties in common, while their differences are few, and none of these absolutely contradict such a conclusion, we infer with considerable confidence, that they are the same, though we may not be immediately able to explain why their resemblance is not complete. After Mr. WALSH, for instance, had discovered, that the influence of the torpedo was transmitted by all the various bodies which are good conductors of the electric fluid, philosophers made little hesitation in admitting them to be one and the same substance, though some of their apparent differences could not then be accounted for. In like manner, the inquirers into the nature of the influence, the effects of which are so evident in Mr. GALVANI's experiments, have very generally, and in my opinion justly, allowed it to be electrical, on the ground that its conductors and those of electricity are altogether the same. To this, however, an objection has been made by Dr. FOWLER, which, if well founded, would certainly prove them to be different substances; for he has asserted that charcoal, which is so good a conductor of electricity, refuses to transmit the influence, upon which the motions in Mr. GALVANI's experiments depend. In reply I shall only say, that Dr. FOWLER must have been unfortunate with respect to the charcoal he employed; since all the pieces I ever tried, and I have tried many, were found to conduct this influence.

Other arguments have likewise been urged against the identity of the two influences; all of which, however, excepting one, I shall decline discussing, as they either are of little importance, or have not been stated with sufficient precision. The objection I mean is, that in none of the experiments with animals, prepared after the manner of Mr. GALVANI, are those appearances of attraction and repulsion to be observed, which are held to be the tests of the presence of electricity. My answer to it is, that no such appearances can occur in Mr. GALVANI's experiments, consistently with the known requisites for their success, and the established laws of electricity. For, as it has been proved that there is naturally no disengaged electric fluid in the nerves and muscles of animals, I except the torpedo and a few others, no signs of attraction and repulsion can be looked for in those substances, before the application of metals or charcoal; and after these have been applied, the equilibrium of the influence, agreeably to what has been already shewn, is never disturbed, unless means for its restoration be at the same time afforded. Neither then ought signs of attraction and repulsion to be in this case presented, on the supposition that the influence is electrical; since it is necessary for the exhibition of such appearances, that bodies, after becoming electrical, should remain so during some sensible portion of time: it being well known, for example, that the passage of the charge of a Leyden phial, from one of its surfaces to the other, does not affect the most delicate electrometer, suspended from a wire or other substance, which forms the communication between them.

Such are the observations I mean at present to submit to the consideration of this Society, respecting the influence which incites the muscles of animals to contract, in Mr. GALVANI's experiments.