# EXPERIMENTS

AND

# OBSERVATIONS

RELATIVE TO THE

# INFLUENCE LATELY DISCOVERED BY M. GALVANI,

AND COMMONLY CALLED

# ANIMAL ELECTRICITY.


By RICHARD FOWLER.



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# 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. }

1

## SECTION I.

### *Are the Phenomena, exhibited by the Application of certain different Metals to 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 [2] 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 [3] 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 [4] 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 [5] contraction. In estimating the comparative powers of different metals as excitors, I found zinc[2] by 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 [6] 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 [7] 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 [8] 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 9
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]. 10

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 11
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 12
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 13
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 14
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, 15
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 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 16
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. 17

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. 18

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 19 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 20 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 21 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 22 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 23 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 24
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 25
skin of these animals is abundantly supplied with nerves, whose trunks communicate, at different places, 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; 26
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 27
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 28
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 29
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 30
appeared to Dr Valli a circumstance so essential to the production of Galvani's phenomena, that (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 31
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 32
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, 33
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 34
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 35
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 36
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 37
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 38
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 39 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 40 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 41 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.

Should it, therefore, be ever proved, that the phenomena discovered by Galvani are effects of the 42 action of electricity, I cannot think Dr Valli’s hypothesis will be deemed a satisfactory account of the manner in which it produces them.

Strong, however, as is the analogy, which, in many particulars this influence bears to electricity, considerable doubts must, I think, still remain as to their identity.

The grounds of these doubts would best appear in an accurate and full statement of the several points, both of resemblance and of difference between this influence, electricity, and that power which distinguishes the torpedo, the gymnotus, and the silurus; but, I can here promise no more than a very imperfect and desultory sketch of these.

In order to accumulate artificial electricity, if I may be allowed the use of such an expression, it seems necessary, that there should be motion between two substances, an electric and a conductor. 43 But, neither motion nor electrics have any share in the production of that influence which occasions the phenomena in question. The motion, here, is the effect, and not the cause of the accumulation: and instead of one conducting substance of any kind whatever, two metallic substances seem indispensably requisite[9].

That influence, whatever it be, which is possessed by the torpedo, &c. seems to depend entirely 44 upon the will of these animals, both for its production, and management, as appears not only from the retraction of their eyes within their sockets, whenever they mean to give a shock, but, likewise, from each shock being increased, diminished, or withheld, as they are irritated or aware of some obstacle to its transmission. But the will of an animal has no share in the production of the phenomena discovered by Galvani.

In the scale of conductors of electricity, charcoal holds a higher place than the fluids of animal bodies, and ice than the metallic salts. But of the influence in question, I have found animal fluids, and metallic salts, excellent conductors, at the same time that I have never observed it pass through charcoal, or even dried wood. I have, likewise, reason to believe that it does not pass through ice. Ice, indeed, is but a very imperfect conductor of electricity, when free from air 45 bubbles, and when the experiments with it are made in a very low degree of temperature. Yet we are told by Mr Achard, that it will conduct electricity, even when Reaumur’s Thermometer stands at 6 degrees below 0.

But the temperature of the room, in which I made my experiments, was at least 55 degrees above 0, by Fahrenheit’s scale. I may likewise remind the reader of the experiment, in which the abdomen of a frog was filled with mercury, and a rod of silver passed through it to the sciatic nerves. A piece of zinc, touching both mercury and silver, excited no contractions; whereas most vigorous ones were excited when water was substituted for the mercury. A proof, as I take it, that water is a much better conductor of this influence than mercury: but of electricity, mercury is deemed a better conductor than water.

We are told by Mr Cavendish, that Mr Walsh found the shock of the torpedo would not pass through 46
a small brass chain: but the influence discovered by Galvani, passes, without sensible diminution of its effects, through a small brass chain of several inches in length, when it is drawn so tight as to bring its links into close contact with each other: and it passes through a gold chain when held between two persons, and suffered to hang with a considerable bend. Yet, if we may be allowed to judge of the comparative strength of the two influences, by the effects which they produce upon animals, that of the torpedo must certainly be allowed to be the strongest; and I see no other way of accounting for its finding an insuperable obstacle to its transmission, where the other finds scarcely any, except by supposing that they are in reality different in their nature.

Dr Valli tells us, that he observed the hairs of a mouse, attached to the nerves of frogs by the tin- 47
foil, with which he surrounded them, alternately attracted, and repelled by each other, whenever another metal was so applied as to excite contractions in the frogs.

This experiment I have many times repeated, both in the manner described by the Dr, and with every variation in the disposition of the hairs which I could devise: but whether they were placed upon the metals, the nerves, or the muscles, or upon all at the same time, neither I, nor my friends who assisted me, have in any instance been able to observe them agitated in the slightest degree.

I have made similar experiments upon a dog, and upon a large and lively skate, by disposing, in the same way that I did the hairs of a mouse, flakes of the finest flax, swansdown, and gold leaf; but
although the contractions produced in the skate, by the contact of the metals, were so strong as to 48
make the animal bound from the table, not the least appearance of electricity was indicated.

I next suspended, from a stick of glass fixed in the ceiling of a close room, some threads, five feet in length, of the flax which I used in the former experiment; and approached some frogs, recently killed, and insulated upon glass as near to them as was possible, without touching: but the threads were in nowise affected by the contractions produced in the frogs.

In this respect, therefore, this influence agrees with that of the torpedo, &c. So far as I know, M. Volta’s instrument for collecting, condensing, and rendering sensible, very small degree of electricity has not been employed in the examination of either.

And indeed I am not sure, if, in examining the newly discovered influence, by such a test, a
sufficient quantity of electricity might not be produced merely by the motion of the animals, 49
subjected to the experiment, to occasion some fallacy in the result. Certain, however, it is, that although this influence did not affect the electrometer in these experiments, it produces infinitely stronger effects upon an animal, than any which can be produced by a quantity of electricity sufficient to affect an electrometer to a very high degree. I have frequently detached the crural nerves of frogs for some length; and having supported them upon a rod of silver, have applied an excited piece of glass, or sealing wax, to the whole length of this rod. The coarsest electrometers have been effected by it, at considerable distances: but I have never, in this way, been able to excite contractions, unless by laying the rod upon the excited cylender of a powerful electrical machine.

This new influence likewise resembles that of the torpedo, in producing its effects almost equally
well, when both it and the subject upon which it acts are insulated from surrounding conductors. 50
But an experiment similar to that, which I have related, of insulating, and positively electrifying, both the frog and the metals applied to it, has never (so far as I am acquainted,) been tried with the torpedo.

Both these influences agree too, in not producing so strong an effect, when the subject, upon which they act, is immersed in water, as when it is in the open air. When the separated leg of a frog was held under water, and formed part of the circuit through which this, to influence, had to pass, in order to excite another leg; it never contracted, although it did, and strongly, when held above the surface, as I have already had occasion to notice. And we are told by Mr Walsh, that the shock of the torpedo was four times stronger in air, than when given under water.

This influence differs, both from that of the torpedo, &c. and from electricity, in producing no 51
sensation (in man at least,) at all similar to that from an electrical shock.

With respect to the single instance related by M. Cotugno, it is probable that both he himself, and all who have repeated experiments of this nature, must have been long ago convinced, that he was deceived into the belief of a shock, from the sensation produced by the struggles of the animal he dissected.

That some kind of disagreeable sensation is occasioned by it, even in frogs, independent of that which must necessarily arise from irritation, and the contractions of their muscles, is evident from their restlessness, and expressions of uneasiness. In other animals, as I shall afterwards have occasion to shew, these expressions are still less equivocal: and, in man, we can ascertain both their
degree and their kind. That they differ considerably from such as are produced by electricity, will 52
be proved when I come to speak of the effects of this influence upon our senses.

But the most important, and characteristic difference, which I have yet been able to discover, between this new influence and electricity, consists in their effects upon the contractile power of animals and of plants. The contractions of animals excited by electricity have a tendency to destroy that power upon which contractions depend. But the contractions excited, by the application of the metals, have, in all my experiments, had the directly opposite effect. The more frequently contractions have been, in this way, excited, the longer do they continue excitable: and the longer are the parts, upon which such experiments are made, preserved from putridity. An influence, capable of exciting contractions, without occasioning exhaustion, was a thing I so little expected to
find, and so contrary to the character which had been given of this, both by Galvani and by Dr Valli, 53

that I, at first, distrusted my own observation of the fact: but the number of comparative experiments, which I had afterwards occasion to make, though with views different from that of ascertaining the point in question, convinced me that this influence, so far from destroying the contractility of muscles, has a tendency to preserve it. Oxygene is, so far as I know, the only stimulus in nature, whose effects are at all analogous.

When a frog has been long dead, I have been sometimes more than a quarter of an hour without being able to excite a single contraction by the application of the metals: but after this, without at all varying the means employed, contractions have appeared, and have become gradually more and more vigorous.

It is said, (for I have never had an opportunity of making the experiment,) that a stream of 54 electricity passed through a sensitive plant produces an almost immediate collapse of its leaves. But the influence, discovered by Galvani, produced no such effect in the following experiment. Having separated the leg of a frog from its body, I freed its crural nerve from surrounding parts, and with one hand held it supported upon the end of a probe. An assistant placed a piece of silver under its foot, and held the zinc with which it was to be touched. A sensitive plant formed the medium of communication between us. He held the bottom of its stem between his fingers, while I held the top: so that when the silver was touched by the zinc, the influence passed up the plant, and through the whole of its stem. The frog’s leg instantly contracted, and repeated its contractions every time the silver and zinc were in contact: but the leaves of the plant did not collapse; neither did they when any of its branches formed part of the circuit.

I must, however, confess that the plant, upon which this experiment was made, had been kept 55 through the winter. With a young one the result might possibly be different; but such an one I have not yet had it in my power to procure.

The torpedo does not appear at all affected by the influence which itself produces. Animals, in which Galvani’s phenomena are produced, are strongly affected. From this circumstance, and still more from the presence of metals being absolutely requisite to their production, some may be induced to believe, that the influence, which causes them, is something external to animals; and that it arises from the mutual contact of the metals only. I must confess I was, for some time, inclined to entertain this opinion; and its probability appeared to be not a little increased by observing that its effects differed with the metals employed, and were strongest when their surfaces were extended, and applied horizontally to each other. I began, therefore, to suspect that 56 it might be some hitherto undiscovered property of metals; for that it was not an electrical phenomenon, seemed still further proved by the circumstance above related. It has been demonstrated, by the very interesting discoveries of M. Volta, that, ‘wherever the capacity of holding electricity is greater, there the intensity of electricity is less’:—‘and that the capacity of a conductor is increased, when, instead of remaining quite insulated, the conductor is presented to another conductor not insulated; and this increase is more conspicuous, according as the surfaces of those conductors are larger, and come nearer to each other[10].’

When, therefore, a plate of silver, communicating with the leg of a frog, was laid upon glass, and a plate of zinc was lowered horizontally upon it, the capacities of both, for any electricity which they might have contained, must have been so much increased, that no one will suspect the contractions 57 of the frog’s leg, to have been occasioned by any discharge of the electrical fluid from them.

As little are we authorised to suppose, that the contractions were produced in consequence of the metals attracting the electrical fluid from the leg: for, since the leg was insulated, it is impossible that it should have received a new supply of electricity, after having been deprived by the metals of what it naturally possessed; and consequently, after once or twice contracting, no further contractions should have taken place: but this is contrary to the fact.

I have before shewn, that flakes of gold leaf, placed between the metals, were not affected by their approach to each other; and that, besides, a quantity of electricity, sufficient strongly to affect an electrometer, was far too weak to excite contractions in the muscles of a frog.

That this influence, however, whatever it be, is not derived from the metals alone, but that animals 58 at least contribute to its production, as well as indicate its presence, is, I think, rendered highly probable, by what I have already urged, relative to the necessity of a communication between the metals, and the muscles, as well as between the metals and the nerves.

I may likewise observe, that animals appear to have a much more complete controul over its effects, than one would expect them to have over an influence wholely external to them.

When living and entire frogs are placed upon a plate of zinc, or tin-foil, and a piece of silver, or of gold, is passed over different parts of their legs, and thighs, till it come into contact with the plate; contractions are very seldom produced, and scarcely ever, if the frogs be healthy and upon their guard. But the instant their sciatic nerves are divided, the contractions produced are as free and 59 vigorous, as if the legs had been completely separated from the body. This difference is not owing to the silver coming in contact with the wound, necessarily made in order to divide the nerve; for I have always taken care that it should not, and indeed when it did, no contractions were produced, unless the nerve had been divided.

Taking off the head of an animal, or intercepting, in any way, the influence of its will upon the muscles of the part excited, has precisely the same effect. But the will is not able to controul the effects of electricity, when the electricity is otherwise sufficiently strong to excite muscles to contraction. I have repeatedly found that even by the strongest voluntary contractions of the muscles of my arm, I have not been able altogether to counteract the involuntary ones, produced by electrical sparks, nor have I found that frogs could ever counteract them.

On attending carefully to the state of the muscles of the legs of living frogs, at the instant the metals were applied, I could perceive by the touch, that, in many frogs, though by no means in all, their muscles were perfectly soft and relaxed: a proof that they have other means of counteracting the involuntary contractions, which the metals have a tendency to produce, besides keeping their muscles in a state of permanent and voluntary contraction.

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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 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.

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!

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.

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.

9. Since what I had before written upon this subject went to the press, I have been informed by a friend, that Dr Lind of Windsor has found, that contractions may be excited in a frog by touching it with iron alone. In a frog very recently killed, I have myself, sometimes, excited contractions, by touching its nerves with iron and steel in conjunction. But I can by no means consider this as a satisfactory proof, that contractions may be excited by the contact of one metal alone; since I have never been able to excite contractions with a piece of iron, of the same quality throughout, applied to a frog which had been so long dead as to leave no suspicion that the contractions were occasioned by mechanical irritation. In Dr Valli's

experiment, with scissars of bad steel, upon a frog recently killed, these circumstances do not appear to have been sufficiently attended to.

10. Phil. Trans. vol. 72. part i. Appen.

## SECTION II.

### *Has Magnetism any concern in the Phenomena discovered by Galvani?*

In answer to this question I have little to say, as the experiments which it suggested, and which I had an opportunity of making, have been but few.

I have repeatedly excited contractions, both with the natural and the artificial loadstone, but I could never observe any difference between them, and such as were excited by unmagnetised iron, or an ore containing an equal quantity of iron with the natural loadstone.

When the separated leg of a frog was laid upon a plate of iron, and a loadstone was brought in 62
contact both with its nerve and the plate, no contraction was excited. I have often brought frogs, in every state of preparation, as nearly as possible to a very sensible magnetic needle, but no variation in its direction was in any case produced by the contractions of the frogs excited by the metals.

## SECTION III.

*What are the relations which subsist between the influence discovered by Galvani, and the muscles, the nervous, and the vascular systems, of animals?*

In proposing to myself a question of this very extensive nature, it will hardly be imputed to me, that I ever entertained, for a moment, the idle expectation of being able completely to solve it. It is prefixed to the following experiments as the most commodious general head under which I could arrange, not only what I had further to say, upon the influence discovered by Galvani, but likewise upon the several physiological subjects, in the examination of which this influence was employed merely as a test. 64

# OF THE MUSCLES.

As I am acquainted with no criterion by which we can assure ourselves of the complete separation of muscular fibres from nerves, without rendering them objects too minute for accurate experiment; it can never be in our power, so far as I am able to judge, to satisfy ourselves, if this new influence can act immediately upon the muscular fibre. A doubt must always remain, whether nerve has not been present; and from this doubt will arise another still more difficult to solve, whether the influence produced or excited by the metals have passed through the nerve to the muscles? or if it have merely acted as a stimulus to the nerve, serving to rouse that unknown energy, by which nerves are known in certain circumstances to excite muscles to contraction.

The following experiments, made upon animals considered by anatomists, in general, as destitute of 65
nerves, may to some appear decisive of this question, but to myself, I confess, they are by no means so. In by far the greater number of animals, we are precluded from the possibility of discovering nerves by their minuteness; yet the actions of these animals, not merely excited by mechanical irritation, but so obviously directed to the attainment of an end, oblige us to infer their existence even where our senses, aided by the best glasses, do not enable us to detect them.

Having laid some earth worms upon a plate of zinc, I tried to excite contractions in them, by passing a rod of silver over different parts of their length, till it came in contact with the plate; but for a long time without producing any effect. Application of the metals to a part recently divided seemed to produce as little effect. At length, I perceived one of them dart itself forwards, whenever the 66
silver was passed under its belly near to a part which had been divided and rejoined. On repeating the experiment again, and with more care, I found, (as in the frog,) that when the animal was perfectly lively, and upon its guard, no contraction could in this way be excited; but that when a part had been rendered more sensible by previous disease, recent irritation, &c. or when the worm was taken unawares by hanging it over a probe, and lowering both upon the plate at the same instant; a sudden and involuntary motion seemed to dart through a great part of the worm’s length from the part touched towards the head; a direction contrary to that in which it takes place in other animals. I never could produce the same effect upon leeches. On varying the experiment, a most whimsical, but satisfactory phenomenon presented itself. I had laid a leech upon a crown piece of silver, placed in the middle of a large plate of zinc. The animal moved its mouth over the surface of 67
the silver without expressing the least uneasiness; but having stretched beyond it and touched the zinc plate with its mouth, it instantly recoiled, as if in the most acute pain, and continued thus alternately touching and recoiling from the zinc, till it had the appearance of being quite fatigued. When placed wholly upon the zinc, it seemed perfectly at its ease; but, when at any time its mouth came in contact with the silver lying upon the zinc, the same expression of pain was exhibited as before.

With the earth worm, this experiment succeeded still more decisively. The animal sprang from the zinc in writhing convulsions; if, when the worm stretched itself forwards, one of its folds lit upon the zinc, it expressed little uneasiness in comparison of what it shewed when the point of its head touched the zinc.

These extraordinary effects were, however, considerably different from those produced by the 68
metals upon the limbs of frogs, and other animals. They had not so much the appearance of involuntary, instantaneous convulsions, as long continued expressions of pain and disgust; such as are produced by applying zinc and silver to the tongue of a child.

A strong presumptive proof, in my humble opinion, that these animals are endowed with a most exquisite organ of sense, and, consequently, that they are not, as has been supposed, destitute of a nervous system.

Doubtful, therefore, if this influence can ever act upon the muscular fibre, except through the medium of nerves, I shall reserve what I have to say upon particular muscles, till I have related some facts relative to the nerves.

69

# OF THE NERVES.

It appears from every experiment, which has been made in prosecution of Galvani’s discovery, that the nerves are very essentially concerned, in all the phenomena which it exhibits. It becomes, therefore, an object of inquiry, highly interesting, to ascertain if all the nerves of the body are equally subjects of this new influence, or if its effects are confined to those appropriated to muscles of voluntary motion. With this view, I surrounded with tin-foil the parvagum and intercostal nerves of several cows and sheep, while the auricles of their hearts were still contracting, and placed one end of a bent silver rod, at one time upon the heart itself, at another upon adjacent muscles, and 70
sometimes upon the nerves; but all without producing the slightest perceptible variations, in the contractions of the heart, or a renewal of them when they had ceased.

I likewise included the caroted artery in the tin-foil; and, at another time, inserted the foil in longitudinal incisions made in the nerves, that it might be more immediately in contact with their substance; but still no contractions followed. I had as little success when I made similar experiments upon a dog, cats, rabbits, fowls, and frogs; yet, in all these animals, I could in general excite vigorous contractions, by arming the nerves of parts obedient to the will: I say in general, for in rabbits I have sometimes failed altogether; especially when they have been drowned in very cold water. Soon after making these experiments, I perceived from one of Dr Valli’s letters, published in the Journal de Physique, that he had made a similar one upon the heart of a dog, and with the same 71
result. The heart, through the medium of its nerves, is not excitable, therefore, by the same means which are found efficacious in exciting other muscles to contraction. I confess I had not expected this result. It has been asserted indeed, by many physiologists of the first name[11], that the heart can in nowise be affected by the application of a stimulus to its nerves, or to the brain; but many considerations excited my doubts upon this subject, and some experiments which I made at this place, more than a year ago, tended to confirm me in an opposite opinion. That both the frequency, and the strength of the heart’s contractions are affected by passions of the mind, is a fact known to every one; but what is much more to the purpose, since we know so little either of mind or of its 72
mode of influencing the body, we know that many derangements of the brain, such as apoplexy, hydrocephalus, phrenitis, &c. together with all kinds of mechanical injuries, (and what are these, but so many stimuli irritating the brain, and consequently the nerves sent to the heart?) affect the motions of the heart most materially and obviously. The contractions of the heart, so long as the brain remains entire, may be affected by a thousand different substances thrown into the stomach; but it appears from the experiment of Mr Kite, that this is by no means the case, when the functions of the brain are suspended by hanging, or drowning[12]. Dr Whytt’s experiment on this subject is one of the most decisive with which I am acquainted. He found, that opium operates much more slowly in destroying the heart’s motion in frogs, deprived of their brain and spinal marrow, than it does 73
when these animals ate entire. Several of my own experiments, though not made expressly with this view, gave the same result with those of Dr Whytt. M. Fontana tells us, he has discovered the heart of the wheel polypus to be a voluntary muscle. It was probably this discovery which led him to try the effects of his will upon his own heart. For the success of his experiment, we have the testimony of his friend Dr Gerardi, Professor of Anatomy in the University of Parma, who, in a very learned little Dissertation on the Origin of the Intercostal Nerve, published in the Journal de Physique for September last, makes the following short mention of it; ‘Je ne dois point oublier de vous dire que M. Fontana a la faculté d’accélérer, ou de retarder à volonté son pouls, sans aucune contraction sensible des muscles.’

The direct experiments, by which I was first led to adopt the opinion that the heart might be affected by the mechanical irritation of its nerves, were made upon very young cats and rabbits; 74
some with the assistance of my friend Dr Physick, now settled in Philadelphia; others in presence of several other gentlemen studying at this university. It appeared very decidedly from two or three of these experiments, that the contractions of the heart were quickened by irritating the brain at the origin of the spinal marrow. In others again, the result was by no means so clear. But it should be recollected that the evidence of one accurate, and positive experiment, is not in the least invalidated by twenty unsuccessful ones, especially upon animals of warm blood; where the irritability of their muscles is so very fleeting, and the result liable to variation from so many, as yet, unknown causes. The irritability of the arteries, for example, is now completely established, yet Haller’s experiments led him to deny it. And even those of the accurate Verschnir, to whom we are indebted for unquestionably the best series of experiments upon this subject, failed of success (as 75
we are told by Dr Dennison, in an excellent Thesis confirming their truth,) when repeated before some of the Faculty here. Immediately, therefore, on discovering the superior powers of zinc, and molybdena, in exciting contractions, I began again to repeat with these metals the experiments on the nerves passing to the hearts of frogs; but for a long time without satisfying either myself, or others, whether any effect was really produced. At length, however, I was so happy as to succeed completely. On the 18th of March last, in presence of my friends, Mr Hunter and Mr Thomson, having dissected away the pericardium from a frog’s heart, which had an hour before ceased spontaneously to contract, I removed the muscles, and cellular membrane covering its nerves, and large blood vessels. I then placed one end of a rod of pure silver in contact with one side of these nerves, and blood vessels, and one end of a rod of zinc on the other, both of them at about the 76
distance of the third part of an inch from the auricles of the heart. On bringing the opposite ends of these rods in contact with each other, the auricle first, and then the ventricle of the heart immediately contracted, and repeated their contractions as often as the ends of the metal rods were made to touch each other. When a stick of glass, wax, or wood, was made use of in place of one of the metals, no contraction took place. Contractions, however, were excited by irritating the heart itself with the point of a sharp instrument. The contractions were both more vigorous, and more

constant when the metals were placed in contact with the heart itself, than when touching only its blood vessels and nerves. I have several times attempted to trace some of the nerves, which may be seen near the large blood vessels of the heart of a frog, into the heart itself, in order to arm them separated from other parts; but, partly on account of their minuteness, and partly on account of the weak state of my eyes, which does not permit me to look intently at minute objects, I have never 77
been able to succeed.

Since making this last experiment, I have repeated it upwards of twenty times. In order to its complete success, it is necessary that the spontaneous contractions of the heart should nearly, if not altogether, have ceased; and, when in this state, the experiment is rendered still more satisfactory by removing the heart from the body of the frog, and laying it upon a plate of zinc. We are then sure that its contractions cannot have been excited, by any mechanical irritation, arising from the contractions of the muscles of the thorax.

For want of sufficient leisure, and convenient opportunities, I have neglected to make this experiment upon any animals of warm blood, except cats and rabbits. A few days after I had discovered the possibility of exciting the heart to contraction by means of zinc, and silver applied to 78
its nerves, I procured an ordinary sized cat, and drowned it in water, as nearly as possible, of its own temperature. Four minutes after immersion, it was taken out of the water and dryed. Its thorax was immediately laid open, but no contractions were observed in any part of its heart, except in the right auricle, and even these were very slight. A plate of zinc was then placed in contact with the parvagum, and intercostal nerves, on one side of the trachea, and a half crown piece in contact with those of the other; both at the distance of about one third of an inch from the auricles. Every time the zinc and silver were brought into contact, complete contractions of the right auricle, and sometimes slight ones of the left were produced, but none in the ventricles. The contractions were observed to become stronger, in proportion as the metals were approached to the heart, and were strongest when one or both was in contact with the auricle. I think the contractions were fully as 79
strong when molybdena, as when silver was used. No contractions could be excited, by arming any of the nerves of voluntary muscles, in this cat.

The next experiment was made upon a female cat, far gone with young. She was drowned in very cold water, and although her thorax was opened the instant she had ceased to struggle, which was in less than four minutes after immersion, her heart had ceased to contract; nor could its contractions be renewed, either by the application of the metals in the way described, in the last experiment, or by pricking or otherwise irritating its surface: but the diaphragm, the intercostal muscles, the fore legs, and the ears, continued to contract long and vigorously, when the metals were as usual applied to their nerves. On cutting into the uterus, however, and taking out one of the young, I found both auricles and ventricles of its heart, contracting most vigorously, though the 80
mother had now been dead upwards of twenty minutes.

An opportunity, not to be neglected, now presented itself, of trying if it were possible to transmit this influence from the mother to the fœtus, through the medium of the umbilical chord. I therefore applied the two metals in the manner I before described, 1st, to the uterus of the mother, and to the cotyledans; afterwards to several different parts of her; but neither uterus nor fœtus were in any instance affected. As little was the fœtus affected, by arming the chord itself. As the hearts of the kittens continued their spontaneous contractions, for more than an hour after they were taken from the mother, I had repeatedly the pleasure of observing, and pointing out to Mr Thomson, and Mr Simpson, who obligingly lent me their assistance in these experiments, the effects of the metals when in contact with the parvagum, and entercostal nerves, both of quickening the repetition of the 81
hearts contractions, while they continued spontaneous, and of exciting them anew when they had ceased to be so. This experiment, repeated upon a kitten a few days after birth, succeeded, but not quite in so satisfactory a manner as the foregoing, although the heart continued contracting for more than an hour and an half after the thorax was opened. Its contractions were quickened, and rendered vibratory by the slightest mechanical touch of its surface; so that it was difficult to determine the precise share which the application of the metals had in their production.

When these had ceased, I did not find that I could revive them by the application of the metals. In the hearts of some young rabbits, upon which I tried this experiment, the contractions appeared to be still more decidedly, occasioned by the application of the metals, than even in the cats.

Having ascertained this important fact, that one muscle, not subjected to the influence of the will, 82
might be made to contract by the application of zinc and silver to its nerves; I proceeded to examine whether the same were the case with respect to all involuntary muscles. I could not, however, observe that any contractions were produced in the stomach or intestines, by placing the metals near the stomachic flexus and semilunar ganglion in a cat. I next proceeded to examine the effects of the metals upon the different organs of sense.

M. Volta’s discovery of the sensation produced upon the end of the tongue, by coating its upper and under surfaces with different metals, led me to compare this sensation with that produced by electricity. I found a very considerable difference between them. Both, indeed, are subacid, but as unlike to each other, as the taste of vinegar is to that of diluted vitriolic acid. That occasioned by the metals is accompanied with what is familiarly called the metallic taste; and differs according to 83
the metals employed. With the greater number of metals it is scarcely perceptible. With zinc and gold, I think, it is strongest; next so with zinc and silver, or molybdena, and insufferably disagreeable with any of them.

The sensation is most distinct when the tongue is of its ordinary temperature, and when the metals are of the same temperature with the tongue. When either the tongue, or the metals, or both, are heated or cooled, as far as can be borne without inconvenience, scarcely any sensation is produced.

That this difference in the effect is owing to the alteration which has been produced in the state of the tongue, and not to that in the temperature of the metals, is evident from experiments which I have already related; from which it appears that neither the conducting, nor the exciting powers of metals are affected by differences of their temperature. But I have found it the uniform result of 84
many experiments, that both the life and irritability of the most vigorous frogs is completely destroyed in a few minutes, by placing them in water heated to 106 degrees of Fahrenheit's scale.

Cold, however, though it appears to affect the sensibility of the tongue nearly as much as heat, did not, in one or two instances in which I tried it, affect the irritability of the muscles of a frog. Some separated legs contracted equally well after they had lain upon a piece of ice for some hours, as they did before they had been in that situation.

Whatever has a tendency to blunt the sensibility of the tongue, as laudanum, a strong solution of opium in water, distilled spirits, acids, &c. diminishes the effect of the metals. Acids, I think, diminish it least.

On placing different metals in the meatus auditorius externus of both my ears, and establishing an 85
insulated metallic communication between them, I felt, or fancied that I felt, a disagreeable jirk of my head. The metals used were a silver probe, a roll of tin-foil, and a common brass conductor belonging to an electrical machine. On withdrawing them from my ears, I experienced a feeling similar to that which one has after emerging from under water. I was not sensible of having hurt my ears by the experiment, nor had I any uneasy sensation after it; but, on getting out of bed next morning, I perceived both my pillow and my face stained with blood; and, on examining, found that it had come from one of my ears. An hæmorrhagy from this part had never happened to me before. From whatever cause this accident happened, (and it is highly probable that it arose from some hurt unperceived at the time), I need not say, that I have never repeated the experiment, and that I certainly never shall.

I never could perceive, that the senses, either of touch or of smell, were in the least affected by the 86
metals; but the effect which they produce upon the eye is very remarkable. Having laid a piece of tin-foil upon the point of my tongue, I placed the rounded end of a silver pencil-case, against the ball of my eye, in the inner canthus, and suffered them to remain in these situations till the parts were so far accustomed to them, that I could examine the sensations produced; I then brought the metals into contact with each other, and, to my surprise, perceived a pale flash of light diffuse itself over the whole of my eye. My tongue was at the same time affected with a similar sensation to that produced when both the metals are in contact with it. On darkening the room, the flash became more distinct, and of a stronger colour. This sensation is not the effect of pressure upon the eye, as in Sir Isaac Newton's experiment; for no pressure should be used. All that is required, is, that the silver lie between the lids of the eye, and in contact with any part of the ball. If the experiment be 87
made with zinc and gold, instead of tin-foil and silver, the flash is incomparably more vivid. I had the disagreeable opportunity of trying this experiment upon one of my eyes, in a state of inflammation; and, in this case, found the flash much more strong than it was in the uninflamed eye. I tried it likewise upon a patient, affected with amaurosis; but the man was so stupid that I could not satisfy myself as to the precise result.

Recollecting that fine nervous twigs pass from the ciliary or ophthalmic ganglion, through the sclerotic coat of the eye, to the choroid coat, and to the uvea; and that this ganglion is in great part formed from a twig of the nasal branch, of the fifth pair of nerves, in conjunction with a branch of the third, I proceeded to try if, by insinuating a rod of silver, as far as possible, up my nose, and thus arming this nasal branch, I could, by bringing the silver into contact with a piece of zinc, 88
placed upon my tongue, pass this new influence up the course of the nerve, and thus produce the flash in the eye. The experiment answered my most sanguine expectation. The flash, in this way produced, is, I think, if any thing, stronger than when the ball of the eye itself is armed. I now thought I had discovered a certain method, by which I could ascertain the effect of Galvani's influence, upon a very important, involuntary muscle, the human iris. It occurred to me that the ingenious physiologist Dr Whytt, had been able, through the medium of the nasal branch of the fifth pair of nerves, to produce, at pleasure, dilatations of the contracted pupil of a boy, in the last stage of hydrocephalus, by applying aq. ammonia to his nostrils; and this instance of the affection, of an involuntary muscle, through the medium of its nerves, had, previously to making any experiments upon the subject, always operated with me as a strong presumptive argument, that the contractions 89
of the heart might be influenced in a similar manner.

I therefore desired some of my friends to observe my pupil, while I repeated the experiment, which I have above described. When the external light was strong, they found some difficulty in determining, whether the pupil contracted or not; but when no more light was admitted, than what was just sufficient for discerning the pupil, they perceived a very distinct contraction, every time the metals were brought into contact with each other. This experiment requires some attention, in order that it may succeed satisfactorily; but although I have repeated it a great number of times upon the eyes of others, it has seldom failed, when made in a steady light, and when the silver has been passed far enough up the nose.

The dilatation of the pupil, instead of its contraction, on the application of a stimulus to its nerves, as in the case related by Dr Whytt, is, I apprehend, not so uncommon a circumstance, as it may at 90
first be supposed. I have myself seen three instances of it in diseases of the head. One of these was in an epileptic patient, whose pupils, during the intervals of his fits, became suddenly dilated whenever his eyes were exposed to a strong light.

My friend, Mr George Hunter of York, while one day amusing himself with repeating some of these experiments, discovered that by placing one of the metals as high up as possible between the gums

and the upper lip, and the other in a similar situation with respect to the under lip, a flash was produced as vivid as that occasioned by passing one of the metals up the nose, and placing the other upon the tongue. It differs, however, from the flash produced in any other way, in the singular circumstance of not being confined to the eye alone, but appearing diffused over the whole of the
face. On attending to the concomitant sensations produced by this disposition of the metals, I 91
perceived that a sense of warmth, at the instant they were brought into contact, diffused itself over the whole upper surface of the tongue, proceeding from its root to the point. Dr Rutherford, to whom Mr Hunter had communicated this experiment, remarked, on repeating it, that a flash is produced not only at the instant the metals are brought into contact, but likewise at the instant of their separation. While they remain in contact, no flash is observed.

This fact is precisely analogous to one already mentioned of contractions being produced in the leg of a frog, at the instant one of the metals in contact with the other metal is withdrawn from the leg.

After this full detail of these curious phenomena, I hardly need remark, that they demonstrate the
free communication, which subsists between the several branches of the fifth pair of nerves, and 92
consequently give strong support, if not absolute confirmation, to the well known doctrine of nervous sympathy, or of the reciprocal influence, which different parts exert upon each other, through the medium of nerves.

If I might be allowed to hazard a conjecture, where we cannot have recourse to demonstration, I should say that the flash, observed in the above experiments, was the effect of contractions excited in involuntary muscles by the application of a stimulus to their nerves; or, in other words, that the effects of the application of the metals to the nasal branch of the first division of the fifth pair of nerves, had been propagated through the ciliary ganglion, along the ciliary nerves, and to the choroid coat, whose vessels it had excited into instantaneous action; and that their action again (as in the case of action excited by pressure, or a blow upon the eye,) had by stimulating the retina occasioned the sense of light.

This supposition is, I think, rendered probable by several considerations. I have already shewn that 93
this influence can excite contractions in involuntary muscles, through the medium of their nerves. And certainly no reason can be assigned, *a priori*, why it should not act equally upon every description of involuntary muscles; upon those which make a part of the minutest vessels in the body, as well as upon the heart, or upon the iris.

That it excites to increased action the arteries of the tongue in the experiment, in which a sense of warmth is produced along its surface by the application of the metals to the lips, seems to be almost demonstrated; for it would be difficult to point out the presence of another cause competent to occasion the evolution of the heat, in this case, besides the increased action of the arteries: and that
this cause is competent to the effect we know from numberless experiments, too familiar to need 94
being particularized here.

Whether the metals, however, do or do not affect the action of the blood vessels, is a question which admits of solution by experiment. The following, I confess, was not quite satisfactory, and I have not yet found leisure and opportunity to repeat it with all the attention it requires.

I inspected the foot of a living frog with a microscope of very high powers. In fixing the foot so as to keep the web expanded, a considerable degree of inflammation was excited, notwithstanding every precaution to avoid it. The current of blood was seen distinctly in several vessels, now flowing rapidly, now slowly, and now in a direction contrary to that in which it was first observed, but with equal rapidity. A thin plate of zinc was introduced between the fleshy part of the foot and its
supporter, and a silver probe was used as an excitor. To me, the circulation appeared very 95
decidedly to be quickened several times when the metals were made to touch each other: but the gentlemen who assisted me could observe no change. To prevent the contractions in the muscles of the leg from producing any fallacy, the crural artery should be laid bare, and insulated from surrounding parts, by passing a thin plate of glass, or sealing wax, between it and them.

That the flash is the effect of such an increased action of the vessels, composing the choroid coat, might be somewhat more difficult to prove. It is however known to every one, that a blow, and that pressure upon the eye, are capable, as I have before observed, of producing a similar effect. And the following case, which Bonetus quotes from Hermannus Cummius, if it may be credited, affords an almost positive proof, that vision depends upon the stimulus given to the retina by the activity of
blood vessels in some part of the eye. ‘Quando theologus, plaga dolorifica, a rupta instrumenti 96
musici chorda accepta, nocte subsequenti jam adulta, e somno evigilans, cuncta clare, ac si de die esset, vidit, adeo, ut minimos picturarum et tapetum tractus observare, characteresque ex libro legere posset. Oculo vero læso clauso, tenebras densissimas adesse ille percepit, eodemque iterum aperto, conclave illustratum visum est, lucem tamen candelæ allatae solisque splendorem de die, ægre tulit oculus affectus, quod per aliquot dies duravit, tandemque sensim remisit.’

Haller speaks of such cases as by no means uncommon, and quotes the names of several authors, who have related similar ones.

The direction of this influence, when suffered to pursue its natural course, appears to be the same with that of most other stimuli, i. e. from the place at which it first affects a nerve, onwards to the
part, in which that nerve terminates. I have repeatedly caused electrical sparks to be passed into 97
my own ulnar nerve at its passage over the inner condyle of the humerus, but both the sensations and the contractions produced by them have been entirely confined to the hand and fore arm.

It appears too, both from the experiments of Dr Monro, and of Dr William Alexander of Halifax in Yorkshire, that when no communication is left between the trunk and posterior extremities of a frog, except by its sciatic nerves, a strong solution of opium, injected under the skin of its posterior extremities, deprives them both of their sensibility and of their contractile power; but that it does not in the least affect the trunk of the body. If, on the contrary, it be applied to the trunk, it exhausts both the trunk and the extremities.

M. Galvani is said to have observed the effects of the influence, which he discovered, diffused over
the whole body of a frog, when the metals were applied to a nerve merely laid bare, without being 98
either divided or separated from surrounding parts. If we are allowed to infer this diffusion of the influence from the restlessness expressed by the animal, M. Galvani's observation may be just. If from the contractions produced, I suspect it is by no means so; since, in every experiment which I have made upon the subject, the contractions have been confined to those parts to which the nerve touched by the metals was distributed.

That this influence, however, may pass in a direction contrary to the course of nerves, is evident from some of the experiments which I have related relative to its effects upon the senses, but is still more clearly demonstrated by the following.

If, after having divided at the pelvis a frog recently killed, the sciatic nerves be freed from cellular
membrane up to their origin from the spine, and all the parts below this, except themselves, be cut 99
away, the muscles on each side of the spine, for some little way up, may be brought into contraction by touching the nerves alone with the two metals in contact. This experiment has not always succeeded with me, and never unless the frog had been recently killed. So long as the hind legs remain undivided from the nerves, it never succeeded; the only contractions produced being in the legs.

100

# OF THE BLOOD VESSELS.

We are told by Dr Valli, that no contractions are excited by arming the blood vessels; but as he has not told us whether his experiments were made upon them while the blood still continued to flow through them, or after they had been deprived of their blood, I determined to make the following experiment.

Having laid bare, and separated from surrounding parts and from each other, the crural artery, and nerve, in the thigh of a full grown frog, I cut out the whole of the nerve between the pelvis and the knee. I then insinuated beneath the artery a thin plate of sealing wax, spread upon paper, and 101
broad enough to keep a large portion of the artery completely apart from the rest of the thigh. The blood still continued to flow, through the whole course of the artery, in an undiminished stream. The artery, thus partially insulated, was touched with silver and zinc, which were then brought into contact with each other; but no contraction whatever was produced, in any muscle of the limb. This experiment was frequently repeated upon several different frogs, both in whom the nerve was, and in whom it was not, divided. The result was uniformly the same. But vivid contractions were produced in the whole limb, when an electrical spark, or even a full stream of the aura, was passed into the artery.

It, however, by no means follows from this experiment, that the sanguiferous system of animals bears no relation whatever to the influence discovered by Galvani. I have already shewn, that the heart may be affected by it, and have given reason to believe, that the smallest arteries of the body 102
are not exempted from its action. Should it ever be proved to be an exclusive property of animals, it is not impossible but that even its origin may be traced to their sanguiferous system.

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11. I have not been at the pains to inform myself, who first was the author of this doctrine; but its adoption by Caldani, by Haller, and by Fontana, and by all upon the faith of experiment, was certainly sufficient to give it currency, in opposition to that of Willis, Lower, Kaau, Boerhaave, Laghi, and even of the ingenious Whytt.

12. Mem. Med. Soc. Lond. vol. iii.

## SECTION IV.

*An attempt to investigate the Source from which the respective Powers of Nerves, and of Muscles, are derived.*

As yet, the question whence the nerves and muscles of animals derive their respective properties, remains in a state of doubt. By many, the brain has been considered as the source not only of the several energies exerted by nerves, whether appropriated to sensation, to the excitement of muscles subservient to the will, or distributed to organs exempted from its influence; but likewise of that unascertained power, by which muscles contract on the application of a stimulus. 104

By others again, these several properties are supposed to be derived from the arteries, which may either supply the materials and construction of that exquisite and peculiar organization, which fits nerves and muscles for performing their respective functions, or may furnish, from the blood, some subtile principle, such as that believed by M. Fontana, to exist there, or such as that we are now examining, which differently modified in different parts, may be the latent cause of all the phenomena exhibited by animals.

The advocates for the first opinion observe, that whenever the brain is considerably injured, or its free communication, by means of nerves, with moving parts is interrupted, a deprivation both of sense and motion is the uniform consequence: and, further, that the several organs, both of sense and of motion, appear to suffer detriment from the over strained exertions of the brain in thinking, 105
equal to that which they experience from their own exertions.

The second opinion is countenanced by facts and observations not less important. From experiments of Haller; some which are recorded in one of the early volumes of the Philosophical Transactions, and others, it appears that a paralysis of the posterior extremities of animals was induced by tying their aorta.

Both Dr Monro and Dr Alexander of Halifax have remarked, that when all the blood vessels, supplying the posterior extremities of frogs, had been divided, and a solution of opium injected under the skin of these extremities, they became, in less than half an hour, both motionless and insensible; whereas, the fore part of the body was not observably affected six hours afterwards; and, in Dr Monro’s experiments, the frogs lived till the day following. Hence Dr Monro concludes, 106
‘that concomitant arteries, somehow or other, tune the nerves, so as to fit them to convey impression[13].’

On the other hand, where it is intended that nerves shall convey impressions with great accuracy, as in all the senses, and very remarkably in the part which some have amused themselves by considering as a sixth organ of sense, the distribution of blood vessels is more profuse than in almost any other equal part. It is likewise universally true, that increase of vascular action in a part is always attended with a proportional increase of sensibility there.

From the valuable experiments made by Mr Cruikshanks, and which have since received the fullest confirmation from those repeated by M. Fontana and others, it appears, that whatever may be the relation between brain and nerves, the latter may certainly be regenerated after excision, and have 107
their functions fully restored. Now, in what manner this can be accomplished, unless by the agency of arteries, would, I imagine, be no easy task to point out.

The influence discovered by Galvani appeared to me an admirable test, by which something decisive might be ascertained relative to these important points in the physiology of animals, and as such I have employed it in the following experiments.

Considering, therefore, the brain on the one hand, and the sanguiferous system on the other, as the possible sources from which nerves and muscles might derive their power, I began by comparing the effects which result from interrupting their communication, first with the brain, and then with the arteries. This mode of procedure seemed to afford the best prospect of information with respect to every object which I had in view, but particularly with regard to the relations which this 108
influence may bear to the several parts examined.

Before relating the experiments, I must observe that the comparison was instituted between the effects of only partially interrupted communication; since it must be obvious that a complete interruption, either of nervous or of arterious communication between any part of an animal, and the rest of its body, could not have been effected without so far injuring the animal, as to render the result fallacious.

109

## *Experiments in which the Sciatic Nerves of Frogs were divided.*

### EXPERIMENT I.

I divided the sciatic nerve, on one side only, in four large frogs. The division was made at the very top of their thighs, and before the nerve had given off the first large branch to the muscles of the thigh. This nerve lies immediately underneath the large crural artery, to which it is closely attached by a sheath of fine but very strong cellular membrane. A small nerve, which supplies some of the muscles on the under side of the thigh, was suffered to remain undivided. The legs, whose nerves had been divided, became completely paralytic below the knee, and very nearly so above it. These 110
legs too, immediately after the division of their nerves, contracted vigorously when laid upon zinc, and excited by passing a rod of silver in contact with the under part of the knee till it touched the zinc; but the other legs which were suffered to remain in their natural state, in order that the contractility of one leg might all along be compared with that of the other, did not contract when the metals were similarly applied to them.

These frogs were all killed by cutting off their heads; the first, at the end of two days after dividing the nerve; the second, at the end of five days; the third, at the end of seven; and the fourth, at the end of nine. Their legs were carefully examined, in the manner I have described, four or five times every day after their heads had been taken off, so long as any contractions could be excited; but I could not perceive, in any one of these instances, that the contractile power continued either longer 111
or more vigorous in the legs, in which the nerves were not divided than it did in those in which they were.

Both in these experiments, and in all my others, where a comparison was instituted between the two legs of the same frog, I divided equal portions of skin on both thighs, that there might be no unequal exposure of the muscles to the water, which would have occasioned a fallacy in the result.

### EXPERIMENT II. 112

On the 31st of March last, I divided, in two, a frog, in one of whose legs I had four months before excited inflammation, by laying bare the crural artery and nerve. The inflammation had been so violent and general, that the frog lost its cuticle in consequence of it, and, when compared with a healthy frog, its resperation was observed to be remarkably frequent. Three weeks after this, when the wound in its thigh had perfectly skinned over, I laid it open again, and divided the sciatic nerve. No general inflammation this time took place, nor did the wound again skin over; but for about a month before it was killed, a large ulcer had formed immediately over the division of the nerve, but 113
had not proceeded down to it. The limb, at the time I killed the frog, was as destitute both of motion and of sensation, as at the first instant the nerve was divided, but contractions were excited in it, by touching the ulcer with zinc and silver. When the frog was dead, however, the contractions were found much more feeble in this than in the other leg.

The metals were now applied to the sciatic nerves within the abdomen. Vigorous contractions were excited in the sound leg, but none in that whose nerve had been divided. Hence it was plain, that no actual regeneration had taken place. On examining the nerve accurately at the part divided, I found the divided ends, which had receded considerably from each other, connected by a transparent gelatinous substance. From the upper end, which appeared elongated into a conical form, several red streaks projected into the interposed substance. The lower end was opaque, thickened, and 114
rounded. No appearance of spiral bands could be detected, either in the interposed substance, or in the part of the nerve below the division, when these parts were examined with the assistance of a microscope. This substance had attained sufficient consistence to support the under part of the nerve, when the upper was raised with a pair of forceps. The leg, in which the nerve had been divided, continued to contract as long as the other, though much less vigorously, and the part, from which I could longest excite contractions, was the ulcer.

### EXPERIMENT III. 115

On the 14th of April last, I killed two other frogs, by dividing their hind extremities from their bodies. In one, the right sciatic nerve had been divided more than six weeks previous to its death. In the other, one of the sciatic nerves had been divided between three weeks and a month.

The legs of these frogs, examined by the metals both before and after their separation from the body, were found in a state very different from those before spoken of. The contractions were scarcely perceptible. The incisions made through the skin, in order to get at their nerves, had closed completely in less than a week after they had been made.

The appearance of the muscles in the legs, whose nerves had been divided, was found to be 116
precisely the same as in those where nothing had been done; but, notwithstanding this circumstance, even strong electrical sparks excited but very feeble contractions. On examining the nerves, the ends of that which had been longest divided were found connected by a substance not at all resembling nerve, but similar to that found in the former experiment, and evidently proceeding from the upper division. In the nerve which had not been so long divided, this circumstance was still more apparent, as the substance had not extended quite to the lower division. The cellular membrane surrounding these upper divisions had the appearance of innumerable vessels finely injected, and some red streaks were seen projecting, as if from the nerve itself, into the gelatinous production. In the sound nerves, the obliquely transverse lines of alternate

117

opacity and transparency, or, as Fontana has called them, the white spiral bands of nerves, were seen distinctly at the first glance of the eye, and without the assistance of a glass; but no appearance of these could be found in the parts of the divided nerves below the division; these were uniformly opaque. Their bulk, however, was not in the least diminished. The organization of nerves long divided, therefore, undergoes a very evident alteration, although it is by no means so clear that the same change happens in the muscles, to which these nerves are distributed. Yet their susceptibility to the action of electricity, as well as to that of this new influence, was nearly lost. Some may consider this as an additional argument, that stimuli act upon muscles only through the medium of nerves.

I have before observed that muscles of frogs, from whom the skin has been stripped, become in a short time hard when exposed to the action of water. Wishing, therefore, to see if there would be any difference between these legs, whose nerves had been divided, and others, in this respect, I laid 118
them in water, and examined them every ten minutes, but both became hard nearly at the same time. Mr Allen, a gentleman well versed in physiological pursuits, was with me when I examined the alteration which had taken place in one of these nerves, in consequence of its having remained long divided, and I had afterwards an opportunity of shewing it to Dr Rutherford. In all the frogs, whose nerves I have divided, I have observed that the divided extremities, though placed in most exact contact from each other, had after a time receded at least 1/12 of an inch from each other.

119

## *Experiments in which the Crural Arteries of Frogs were tied as near to the Trunks of their Bodies, as where the Nerves had been divided in the former Experiments.*

### EXPERIMENT I.

Both crural arteries of a full grown frog having been laid bare, one of them was tied. The leg, in which this was done, became instantly weaker than the other, and rather dragged when the animal was put into water. The frog, however, could still jump about with great agility. Four hours after this operation, it was killed by crushing its brain. It continued to move its legs spontaneously, when touched, during more than two days after this, and contractions were excitable by the application of the metals for two days longer. Sometimes it appeared rather doubtful, which leg 120
contracted most vigorously, but, in general, the leg in which the artery remained free did so, and contractions could be excited in it, more than an hour after every means to excite them in the other leg had failed.

### EXPERIMENT II. 121

Ligatures were passed round the crural arteries of two other frogs, and one of them was suffered to live thirty six hours afterwards, before its head was crushed: the other four days. In these, the disproportion between the vigour and continuance of the contractions in the compared legs, was so much greater than in the preceding experiment, as to leave no doubt of the effects produced by tying an artery. The leg, whose artery had remained tied four days, never contracted near so strongly as its fellow, and contractions had ceased to be excitable in it, upwards of twenty hours before they had ceased in the leg, whose artery had not been tied.

From these experiments, it appears decidedly, that a much greater detriment to that condition of a 122
limb, upon which contraction depends, is induced by interrupting its circulation, than by intercepting its communication with the brain.

But still, as the effects arising from the interception of the influence of the brain, and of the circulation, were not compared with each other in the same but in different animals, whose age, relative strength, &c. might possibly differ, I thought proper to repeat the comparison, in the following manner.

123

## *Experiments in which the Sciatic Nerve was divided on one side, and the Crural Artery tied on the other.*

### EXPERIMENT I.

I divided the sciatic nerve of one leg, and tied the crural artery of the other, in a large frog. Scarcely any blood was lost in doing either. Two days after this, I strangled it. During the first 24 hours, the leg, in which the nerve had been divided, appeared to contract with most vigour; after this period, the difference between them became more doubtful; but the contractions were at no time stronger in the leg, whose artery was tied, than in that whose nerve was divided.

### EXPERIMENT II. 124

The same operations were performed upon a large female frog full of spawn. Four hours afterwards, she was observed covered by a male, who had been treated in a similar manner. I mention this circumstance, as it tends to prove that the pain occasioned by the operation was probably not so great as to produce much fallacy.

On the day following, she had spawned, and on the sixth day from the operations, she was strangled. When laid upon a plate of zinc, and excited by means of a rod of silver, the contractions were found extremely feeble in the leg whose artery had been tied, and ceased altogether in about twenty-two hours after her death. In the leg, whose nerve had been divided, they appeared as 125
vigorous as they usually are in legs to which no injury has been previously done, and continued excitable upwards of two days after they had ceased to be so in the other.

### EXPERIMENT III. 126

Having tied the crural artery on one side, and divided the sciatic nerve on the other, on three full grown male frogs, I strangled them all on the sixth day following. My motive for killing the frogs, subjected to such experiments, either in this manner or by crushing their heads, will be obvious. It was of consequence to preserve their circulation as entire as possible, and, at the same time, avoid the continuance of pain, which by exhausting all the parts of the body, whose communication with the brain was not interrupted, might considerably have affected the result of the experiments.

The contractions excited by means of the metals, were, in all these instances, likewise as much 127
more strong and durable in the legs, whose nerves had been divided, than what they were in the legs, whose arteries had been tied, as what I had found them to be in the preceding experiment.

Having thus found, that a diminution of the circulation of a part, was accompanied with a proportionable diminution of the respective powers of nerves and muscles in that part, I next proceeded to examine if an increased circulation would be attended with a proportionable increase of these powers. That this is actually the case, with respect to the nerves, the few facts which I have related of the eye, in a state of inflammation, have a tendency to prove; and we all know how much the sensibility of every part of the body is increased, by an increase of vascular action. That a similar relation subsists between an increased action of the arteries, and the contractile power of muscles, is, I think, proved by the following experiment.

128

## *Experiments made with a view of ascertaining some of the Effects of Inflammation.*

### EXPERIMENT I.

I have before said that if a living and entire frog be set upon a plate of zinc, contractions can very seldom be produced in any part of its body by passing a rod of silver over it, so that the silver, the frog, and the zinc, may be all in contact with each other. But, I have found in upwards of twenty experiments, that when inflammation had been excited in one of the hind legs of a frog, by irritating it with a brush, contractions uniformly took place in that leg when the metals were applied to it, although none had been produced in it before it was inflamed, nor could still be produced in the other leg which remained in its natural state.

### EXPERIMENT II. 129

Having previously excited inflammation, by means of a brush, in the foot and leg of a healthy and large frog, I cut off its head. The contractions excited by the metals in the inflamed leg were in vigorous and instantaneous jirks; those in the sound leg more languid and difficultly excited. Spontaneous motions continued at this time nearly the same in both. Till the end of the second day, after this frog’s head had been taken off, the contractions excited in the inflamed leg continued uniformly, and beyond all comparison more vigorous than what I could by any means excite in the sound leg. But, after this time, the inflamed leg became hard as a piece of wood; probably in 130
consequence of the effusion to which the inflammation had given rise.

The event of five similar experiments was so nearly the same, that I should be thought unnecessarily minute, were I to relate them in detail.

We are now perhaps prepared to account for the deficiency of contractile power in those legs, whose sciatic nerves had been divided, the one, between three weeks and a month, the other, six weeks, compared with its continuance in the leg, whose nerve had been divided upwards of three months. It appears, from the circumstances of those experiments, that some of the arteries, appropriated to the supply of the sciatic nerves of frogs, have the same course with the nerves themselves; since the deposition of new matter could in all be traced from the upper division of the nerves. It is obvious, therefore, that the part of the nerves below the division, must have been 131
deprived of so considerable a portion of their usual arterial supply, as in time would occasion some alteration in their structure, and consequently in their powers. We accordingly find that such alteration of structure, and such deficiency of power, had actually taken place. It is further probable, that, in proportion as the supply from the arteries was restored, the powers of that nerve, which had been three months divided, had been likewise restored. This supposition is countenanced by every instance in which nerves are reproduced; as we find the functions of the parts in which they had been divided, are not immediately, but gradually restored.

M. Fontana seems too hastily to have adopted the opinion, that the sciatic nerves, when divided, are probably never reunited by truly nervous structure, because no reunion took place during the very short period which he suffered to elapse between their division, and their subsequent examination. 132
In the experiments, which I have related, the progress towards reunion seems to have borne a very exact proportion to the time the nerves had remained divided; and, in an experiment related by Dr Monro, where the sciatic nerve of a frog had been divided a year previous to the death of the animal, the reproduction was advanced so far as to have the appearance of being perfect. Nor can I doubt, that both the sensibility and the motion of the limb would have been restored, had the animal been permitted to live a sufficient length of time. The following fact renders the supposition at least extremely probable.

In the first volume of the Edinburgh Medical Essays, the case of a Captain of a man of war is related, who entirely lost the use of his right arm, in consequence of a gun-shot wound received in his neck. The circumstances of the case are such as leave no reason to doubt, that the loss of the power of motion, in this gentleman’s arm, was owing to the division of the cervical nerves 133
proceeding to the arm: yet both the full use, and strength of this arm, were restored, after a period of about two years and a half. A proof perfectly satisfactory that an actual regeneration of nerves had, in this case, taken place; and if in this, one sees no reason why it should not equally take place in any other part of the body.

It might be difficult to assign a satisfactory reason for the very speedy reproduction of the intercostal, parvagum, and recurrent nerves, when compared with the great length of time required for the reproduction of others. May it not be owing to the very profuse manner in which they are supplied with arteries, probably both in an ascending, and in a descending direction; from above, by the superior, and from below, by the inferior laryngeal arteries?

It appears upon the whole, therefore, tolerably certain, that the sanguiferous system contributes 134
more immediately than the brain to the support of that condition of muscles and of nerves, upon which the phenomena of contraction depend; since that condition is much more injured by intercepting the influence of the former than of the latter.

Every experiment and observation, which has been made upon the subject of nutrition, and of the reproduction of parts, clearly demonstrates that nerves and muscles, in common with every other part of the body, derive their structure from the arteries; and it is evident, that upon this structure their several properties must in some measure depend. But M. Galvani’s discovery of a subtile influence, which may be transmitted apparently from one part of an animal to another through

foreign media, may reasonably give rise to a conjecture that the phenomena exhibited by nerves
and by muscles may perhaps depend more immediately upon some such influence; and reasons 135
exist, which might induce some to suspect that even this is derived from the blood.

136

## *Experiments suggested by some opinions of M. Fontana.*

From the greatest number of experiments, perhaps, ever made by one physiologist, M. Fontana has been led to conclude, that the venom of the viper, opium, and several other poisons, which he examined, produce no effects whatever, when applied immediately to nerves and muscles alone, but that they destroy life, by exerting their influence upon some subtile principle existing in the blood.

Independent of the experiments, published by M. Fontana, on this subject, his opinion respecting the existence of such a principle may be thought to receive no inconsiderable countenance, from 137
the opinions of Harvey and of Mr Hunter, concerning the life of the blood, and from those experiments, by which Mr Hewson has demonstrated, that changes are instantaneously produced upon the coagulability of the blood, by passions of the mind, and whatever else affects the action of the heart and arteries. An experiment made by Dr Alexander of Halifax, and published at this place in the year 1790, in his excellent Thesis, 'De partibus corporis quae viribus opii parent,' may at first appear a sufficient refutation of M. Fontana's opinion.

He found that thirty three drops of a strong solution of opium in water, injected into the jugular vein of a large rabbit, destroyed it, as in M. Fontana's experiments, in four minutes and a half; whereas, the same quantity injected into the crural vein in each leg of another rabbit, with an interval of twenty six minutes between the two injections, although it rendered the animal sleepy 138
and stupid for a few hours, did it no material or permanent injury. Hence, Dr Alexander concludes, that the opium, injected into the jugular vein, did not destroy the animal by acting upon the blood alone, since if it had, the same effect, should have been produced, by introducing an equal quantity into any other vein of the body; but a quantity double of that, which had occasioned death when introduced into the jugular vein, failed to occasion it when introduced into the crurals.

It is not, however, by one experiment, formidable as it must be allowed to be, that the innumerable hosts brought to the contest by M. Fontana ought to be combated. Besides, it might be objected even to this one, that the opium was introduced into veins, from which it must have been so much longer in passing to the arterial blood, than from the jugular vein, and consequently so much more diluted, and perhaps too altered in its nature before it got there, as might be sufficient to account 139
for the difference of result in the two cases compared.

The opportunity afforded by M. Galvani's discovery, of putting the truth of the opinion held by M. Fontana more fully to the test, and the possibility which presented itself, that if any such principle, as he supposes in the blood, should really be found to exist there, it might prove to be identically the same with that discovered by M. Galvani, induced me to make the following experiments.

### EXPERIMENT I. 140

Having selected two frogs as nearly as possible of the same size and vigour, I deprived one of its blood by opening, first, one of its crural veins, then, a crural artery, and last of all, the heart. To assure myself of the complete evacuation of its blood, I next injected water into its heart, and immediately afterwards forty drops of a strong aqueous solution of opium[14].

I then removed the sternum of the other frog, and having made an opening into the ventricle of its 141
heart, injected into it likewise forty drops of the solution. Less blood was effused in doing this, than one would at first expect; for the ventricle contracts so strongly, immediately after the incision, as to prevent much blood from passing out, unless the incision be made, as it was in the other frog, purposely large.

The moment, at which each injection was made, was accurately noted, and the time expended in evacuating the blood from the first frog, was allowed for. The frog, from which the blood had been withdrawn, ceased to contract, when irritated, very nearly an hour before the other, even calculating not from the time of injection, but from the moment I began to bleed it; nor could I by means of the metals excite contractions in it, for upwards of a day before they had ceased to be excitable in the other frog.

### EXPERIMENT II. 142

As evacuating the blood from a living animal is rather a severe operation, and might have occasioned some fallacy in the last experiment, by subjecting the frog, in which this was done, to a greater degree of pain, and consequently of exhaustion, than what the other was subjected to, I crushed the brains of two other frogs before I proceeded, as in the former experiment, to withdraw the blood from one of them. Instead of forty, I injected no more than thirteen drops of the strong solution of opium, into each of the hearts of these frogs. The instant the injection had entered, both hearts became white, and ceased from contracting. Forty eight hours after the injection of the 143
opium, the contractions excited by the metals in the frog, deprived of its blood, had become very slight, particularly in the limb whose vein and artery had been opened. The other frog still continued to contract with so much vigour, as to raise its body from the plate of zinc, upon which it was laid. Seventy two hours after the injection, no contractions could be excited in the frog, from which the blood had been withdrawn, except some very slight ones in the leg, whose artery and vein had not been opened. The contractions in the legs of the other frog, continued still so vigorous as to raise its body from the plate, and some were produced even by mechanical irritation.

Ninety six hours after the opium had been injected, (both the frogs having lain out of water all night,) that without blood was found quite putrid. In the other, the contractions, produced by

exciting the legs, were sufficiently strong to move the feet: as the body, however, had become 144
putrid and offensive, it was thrown away.

### EXPERIMENT III.

The heads of two other full grown and lively frogs, having been crushed, their hearts were laid bare, and the blood was evacuated from one of them, as in the former experiments. A small portion of the skull of each then being removed, eight drops of the strong solution of opium was injected upon their brains. At least half the quantity seemed to return from the wound. Both frogs became instantaneously motionless after the injection, but, in about an hour, were considerably recovered.

Spontaneous motions continued during more than fifty hours, in the legs of that from which the 145
blood had not been drawn, and contractions were excitable by the metals, upwards of 24 hours after they had ceased to be so, in that from which the blood had been drawn.

The following experiments may be deemed still more satisfactory, than the preceding, from the circumstance of the comparison having been instituted, between the effects of opium, upon different, but similar parts of the same frog, differently circumstanced.

### EXPERIMENT IV. 146

One of the crural arteries of a frog having been included in a tight ligature, as near as possible to the body, I suffered four days to elapse, and then injected through a perforation in its skull, eight drops of the strong solution upon its brain, and in a direction towards its spinal marrow. This frog continued most violently convulsed for more than an hour, and, in two, was to all appearance dead. When laid upon zinc, and excited with silver, the contractions were not at first perceptibly stronger in one leg than in the other. After eight hours, however, they were evidently most strong in
the leg whose artery remained free. After 21 hours, this difference became still more decided. At 147
the end of 34 hours, scarcely any contractions could be excited in the leg whose artery had been tied; though they continued vigorous in the other; and, at the end of 46 hours, they had ceased altogether to be excitable, in the leg whose artery was tied. In the other, they continued during several hours afterwards.

### EXPERIMENT V.

Having tied one of the crural arteries of another frog, I filled its stomach, immediately afterwards, with a saturated solution of opium in water. The difference between the strength, and the continuance of the contractions, excited by the metals, in the two legs of this frog, was not so great as in the former; yet still the difference was considerable in favour of that leg in which the artery remained free.

### EXPERIMENT VI. 148

In two other frogs, in each of which a crural artery had been tied, and the solution of opium (without regard being paid to quantity), repeatedly injected underneath their skulls immediately after; the contractions appeared to be very little weaker in the legs, whose arteries were tied, than what they were in the legs in which they were not tied, and they continued excitable during an equal length of time in both.

### EXPERIMENT VII. 149

Having tied the crural artery of another frog, I immediately filled both its stomach and abdomen with a strong solution of opium. In an hour after this, it was to appearance quite dead. At the end of eight hours, the contractions, excited by the metals, had become very feeble in the leg whose artery was tied, in comparison of what they were in the other leg; and, at the end of twelve hours, no contractions could be excited in any part of the frog, except in the leg whose artery remained free. In this they continued excitable about an hour longer.

As it was possible, that the more speedy exhaustion of the legs, in which the arteries were tied, 150
might have been owing in some measure to the pain, occasioned by that operation, I repeated the experiment with the following variation.

### EXPERIMENT VIII.

I first divided the sciatic nerves, in both legs of two frogs, and then tied the crural artery in one leg of each. Eight drops of the solution of opium were immediately afterwards injected upon their brains. But the event of this experiment was precisely the same with the majority of those before related. The contractions excited by the metals, in the legs whose arteries were tied, were uniformly more feeble, and of shorter duration, than those excited in the other legs: yet it is
evident, that, in all these experiments, the very reverse of this ought to have taken place, had it 151
been true, as M. Fontana has asserted, that opium has no effect upon any part of the body, except through the medium of the blood.

The experiments however, which I am now to relate, may perhaps appear still more satisfactory.

### EXPERIMENT IX.

Having laid equally bare both the sciatic nerves of a frog, at the upper part of its thighs, I passed a ligature round one of them, and drew it as tight as it was well possible, without dividing the nerve. I then removed a portion of its skull, and with a small brush, kept it constantly wet with laudanum during several hours. The frog soon became convulsed; and, during ten or twelve hours, continued in that state of exquisite sensibility, which opium never fails to produce in these animals. 152
It may here be worth remarking, that, while they are in this state, the slightest touch of a feather, or even breathing upon them, excites instantaneous convulsions. The leg whose nerve was tied, remained paralytic during this time, but when it was laid upon zinc and excited with silver, it contracted as strongly as the other. After forty three hours, the contractions were very feeble in the leg whose nerve was not tied, but still vigorous in the other. After fifty three hours, no contractions could be excited in any part of the frog, except in the leg whose nerve was tied. In this they were sufficiently strong to move the foot, and continued so for more than an hour longer.

## EXPERIMENT X. 153

One of the crural nerves of another frog having been tied in a similar manner, eight drops of the strong solution of opium were injected upon its brain. The animal instantly became motionless, but, in less than an hour afterwards, was considerably recovered.

The contractions, excited by the metals, in the leg whose nerve was free, soon became more feeble than those excited in the leg, whose nerve had been tied. This disproportion, between them, continued increasing during ninety six hours, after the opium had been injected, when contractions could no longer be excited in the leg whose nerve remained free. In that, in which the nerve had 154
been tied, they continued upwards of 4 hours afterwards.

## EXPERIMENT XI.

Immediately after having divided the sciatic nerve, in one thigh only, of three other frogs, I injected as much of the strong solution of opium underneath their skulls, as could possibly be retained. The legs, in which the nerves had been divided, continued contractile several hours after the others had ceased to be so.

Hence, then, we see no reason for suspecting that the more speedy cessation of contractions in those legs, in which the crural arteries were tied, than in those on which no operation was performed, was owing to the pain occasioned by such operation, since even the more painful 155
operations of tying or dividing the sciatic nerves, were attended with no such effect.

Upon the whole, therefore, it appears, that the conclusion which M. Fontana draws from his numerous experiments with opium, ‘That the circulation of the blood and humours in the animal machine, is the vehicle for opium, and that, without this circulation, it would have no action on the living body,’ is the very reverse of that which I am warranted to draw from the experiments I have just related; since the parts, most affected by the action of opium, were not those in which the circulation remained most entire, but those in which it had been almost altogether interrupted; and since in two parts where the circulation remained equal, and entire, the action of opium was rendered unequal, by interrupting the communication of one of them, by means of nerves, with the parts to which the opium was applied.

The existence, consequently, of any such principle in the blood, as that supposed by M. Fontana to 156
exist there, is rendered far too problematical, even to allow me to expect that it can ever be proved: far less that it may turn out to be the same with that discovered by M. Galvani, or with that, whatever it may be, upon which the phenomena of nerves and of muscles may depend.

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13. Essays Physical and Literary.

14. This solution, which is the same that I employed in all my subsequent experiments, was of the same strength with that used by Dr Alexander in the greater number of his, viz. an ounce of crude opium mixed in a mortar with two ounces and a half of water, and filtered through paper, after having remained twelve hours, in a close corked bottle, near a chamber fire.

# APPENDIX.

I was unwilling to interrupt the narration of the preceding experiments, by the mention of the following facts, which they afforded me an opportunity of observing, as they were not immediately connected with the objects on account of which the experiments were instituted; and I have yet some few to relate, which, from the haste with which these sheets were prepared for the press, I had omitted to insert in their proper places.

1. In one of my first experiments, in which I had occasion to suffer a frog to remain tolerably entire, so long as contractions could be excited in any part of its body, I was surprised to find, on removing its sternum, that its heart had ceased to contract, nor could be roused by the application of any [158] stimulus whatever, notwithstanding the contractions in its hind legs, excited by the metals, were still vigorous, and continued so for several hours afterwards. On paying particular attention to this circumstance in another frog, upon whose brain opium had been injected, I found that its legs continued excitable, upwards of forty hours longer than its heart. This discovery of the continuance of the contractile power, in the muscles of the posterior extremities, so long after its disappearance in the heart, is so contradictory to the opinion generally received upon this subject, and long established among physiologists, that I can scarcely expect it should be credited, by those who may not themselves have opportunities of observing it. It is a fact, however, of which, in the course of these experiments, I have had the most satisfactory and uniform proofs, both in such frogs as have, and in such as have not, been under the influence of opium. If a different opinion has hitherto been [159] held by experimentalists upon this subject, it should be recollected, that, till the discovery made by Galvani, we had no means of ascertaining the presence of the contractile power of muscles, which had not, at the same time that they indicated its continuance, a tendency to destroy it, and consequently to render it impossible for us to trace its natural progress to extinction.

I have, more than once, observed the same circumstance in both cats and rabbits.

2. Dr Alexander, in his excellent Thesis already quoted, tells us, that the contractility of all the voluntary muscles of frogs was destroyed in the course of a very few minutes, by injecting eight drops of a strong solution of opium in water, (similar to that which I employed) upon the surfaces of their brains. But that the contractions of their hearts did not appear to be much, if at all, affected by this treatment. In all the similar experiments, which I have made, the event has been very different. [160] I have not found it possible by any quantity, either of aqueous or of spirituous solution of opium, injected upon the brains of frogs, to produce that rapid extinction of the contractility of their voluntary muscles, of which Dr Alexander speaks. They commonly recovered in less than an hour, from the complete insensibility and paralysis, first occasioned by the injection of the opium, and after that time, their spontaneous motions almost always continued for several hours longer, and, by the application of the metals, contractions were excitable even for days. Their hearts, as I have already said, uniformly lost their susceptibility of the action of stimuli, long before their posterior extremities.

3. The arguments against the antiquated doctrine of transudation, through parts of a living body, are already so numerous and satisfactory, that it may be thought unnecessary to notice in this [161] place, a decisive one so far as relates to the skin of frogs, at least, which may be deduced from the fact already mentioned; that so long as the skin was suffered to remain upon the limbs of frogs, placing them in water, very evidently preserved the contractility of their muscles, whereas when the skin was taken off, the muscles became hard, and incapable of contracting, in a very few hours. Had there been a possibility of water soaking through the skin, this difference could not possibly have had place.

4. In speaking of some of the relations, which subsist between the influence discovered by Galvani and the nerves, I omitted mentioning the following facts.

A very different effect is produced by applying the metals to the brain or spinal marrow of frogs, from what is produced by applying them to their nerves. In the latter case, I have observed, that every muscle, to which a nerve below the part touched is distributed, is brought into instant [162] contraction. But no muscles are brought into contraction, when the metals are applied to the brain or spinal marrow, except such as derive their nerves from the part immediately in contact with the metals. The influence does not stimulate or pass along the spinal marrow, as it would along the trunk of a nerve, to affect all other nerves branching off from it.

I first became acquainted with this fact, while making the following experiment. Having laid bare the brain of a living frog, and put a stop to its spontaneous motions, by gently pressing upon the brain, I introduced a long slip of tin-foil doubled underneath a part of the skull, which had not been removed, and placed a silver probe upon its tongue. The only muscles which contracted, when the tin-foil was bent over the nose of the frog, so as to come in contact with the probe, were those which move the eyes, and the transparent membrane which defends them, those of the tongue and [163] of the throat. When the tin-foil was twisted into a thin roll, and passed a little way down the spine, the muscles of the upper extremities and of the thorax contracted; when a little further, those of the back and of the abdomen contracted; and when, introduced still further, to where the sciatic nerves leave the spine, the posterior extremities were, for the first time, strongly convulsed. I have repeated this experiment very frequently; and have always found, that, as soon as the spontaneous motions of frogs had ceased, the contractions, excited by the metals, were uniformly progressive from the head downwards, corresponding exactly to the progress of the metals down the spine. The experiment sometimes succeeds when neither the brain nor the spinal marrow have been laid bare, and when even the skin has not been divided, but, when the frog is placed upon a plate of zinc, and one of the ends of a bent silver wire is placed upon any part of its spine, while the other is made to [164] touch the plate.

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5. As it has not been till very lately, that I have been able to procure an electropherus, I have as yet made but few experiments with it; their result, however, is such as tends still more to confirm me in the opinion, that the influence, discovered by Galvani, has no relation whatever to electricity.

Having, first, so far freed the instrument, from the small quantity of electricity collected, by wiping it, that none was indicated by a very sensible electrometer of linen-yarn, suspended from the wooden part of its handle; I placed it within a few inches of a glass stand, upon which I had laid a plate of zinc, supporting a frog recently killed, and with its sciatic nerves within the abdomen laid bare. A bar of zinc formed the communication between the frog and the metal plate of the electropherus. Contractions were then excited in the frog, by placing one end of a bent silver wire, 165 insulated in sealing wax, upon the nerves of the frog, and the other end upon the bar of zinc. After strong contractions had, in this way, been kept up for about half a minute, I carefully removed the bar of zinc, by means of a stick of wax, that there might be no possibility of the electricity escaping, if any should have been collected. The metal plate was then raised from the varnished surface. The electrometer attached to its handle was very slightly affected; but a fine thread, presented to the plate, was perceptibly attracted by it.

I had a strong suspicion, that the electricity, thus collected, had been excited solely by the friction of the frog's legs during contraction, against the insulated plate of zinc upon which it lay; and I soon found that my conjecture was just; for an equal quantity of electricity was obtained from another frog similarly disposed, when contractions were excited in it, by merely mechanical irritation. 166

The result was the same when these frogs were laid successively upon the metal plate of the electropherus itself, and excited, the one in M. Galvani's method, the other by mechanical irritation only.

These experiments were very frequently repeated, but the quantity of electricity collected was always greater where the contractions, or, in other words, the friction had been most considerable, and did not, in any instance, appear to depend on the means employed to excite the contractions.

What still further proves, that the electricity, in this way collected, had no dependence whatever, for its production, upon the application of the metals to the frog, but had been merely the portion of electricity, naturally possessed by the frog, in common with other conducting substances, is, that 167 when the electricity, which was collected from its first contractions, had been drawn off from the plate, no more could afterwards be collected, although the contractions, excited by the metals, still continued as vigorous as ever.

6. When the electropherus was charged with electricity, as highly as it was possible to charge it by friction, the contractions produced by the insulated metals in a prepared frog, laid upon the metal plate of the electropherus, were not at all affected by raising it from the varnished surface. A proof that the phenomena in question are not affected, either by the condensation or rarefaction of the electricity, in either the animal or the metals, by which they are exhibited.

I have not found, that any quantity of electricity, which I could accumulate in the metal plate of the electropherus, did ever, when discharged into the nerve of a frog, excite contractions nearly so 168 strong as what are excited by the application of zinc and silver; nor could I, at any time, collect a sufficient quantity of electricity, from five insulated frogs, sufficient to excite contractions in a single leg of a frog, though recently separated from its body, and consequently excitable by stimuli of very weak powers.

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The politeness of the very learned Mr Robison, Professor of Natural Philosophy in this University, enables me to lay before the public the following communication; which, independent of its intrinsic merit, affords an additional gratification, by evincing the great interest taken in the subject, by one whose abilities and extensive knowledge so well qualify him for giving it a full investigation.

To Mr Fowler, 169

Edin. *May 28. 1793.*

SIR,

About a fortnight ago, my son told me of a curious experiment, with a piece of zinc and a piece of silver applied to the tongue, by which a strong irritation, resembling taste, was produced, and that a luminous flash was excited, by applying one of them to the eye. I immediately repeated them according to his directions, and my curiosity was greatly excited to prosecute them in a variety of circumstances. I understand, that these experiments have originated from the curious discoveries made some time ago in Italy, of which I was informed last winter. But I have been so much out of the world for some years past, that I have had no opportunity of knowing what was going forward.

Being informed, that you have been long engaged in experiments on this subject, and are 170 about to favour the public with an account of them, I have taken the liberty of communicating to you, a few facts which have occurred to me, some of which, perhaps, may be new to you.

1. I find, that if a piece of zinc be applied to the tongue, and be in contact with a piece of silver, which touches any part of the lining of the mouth, nostrils, ear, urethra, or anus, the sensation resembling taste is felt on the tongue. If the experiment be inverted, by applying the

silver to the tongue, the irritation produced by the zinc is not sensible, except in the mouth and the urethra, and is very slight. I find the irritation by the zinc strongest when the contact is very slight, and confined to a narrow space, and when the contact of the silver is very extensive, as when the tongue is applied to the cavity of a silver spoon. When the zinc touches in an extensive surface, the irritation produced by a narrow contact of the silver is very distinct, especially on the upper side of the tongue, and along its margin. This irritation seems 171
to be mere pungency, without any resemblance to taste, and it leaves a lasting impression, like that made by caustic alcali.

2. If the zinc (finely polished) be applied to the ball of the eye, the brightness of the flash seems to correspond with the surface of contact, of the silver with the tongue, palate, fauces or cheek. The same thing happens when the silver is applied to the eye.

3. When a rod of zinc, and one of silver are applied to the roof of the mouth, as far back as possible, the irritations produced, by bringing their outer ends into contact, are very strong, and that by the zinc resembles taste, in the same manner as when applied to the tongue.

4. I had been paring my toe nails with scizzars, and had cut off a considerable portion of the thick skin, so that the blood began to *ooze* through, in the middle of the wound. I applied the 172
zinc there, and an extensive surface of silver to the tongue. Every time I brought the metals into contact, I felt a very smart irritation by the zinc at the wound.

5. I made a piece of zinc having a sharp point, projecting laterally from its end. I applied this point to a hole in a tooth, which has sometimes ached a little, and applied the silver in an extensive surface to the inside of the cheek. When the metals were brought into contact, I felt a very smart and painful twitch in the tooth, perfectly resembling a twitch of the toothack. I thought this twitch double, and that one of them happened before the metals came into absolute contact. I am now almost convinced, that this is the case, for when I make the silver rest on a dry tooth, without touching the tongue or fauces, I have no twitch on bringing the outer ends of the metals together: showing that there is not a proper communication through 173
a dry tooth. If, while the outer ends remain in contact, I touch the silver with the tip of the tongue, still no twitch is felt in the tooth. If I now separate the outer ends of the metals, keeping the tongue applied to the silver, a slight twitch is felt in the moment of separation, and a strong double twitch when they are again brought into contact. N. B. This twitch is prevented, by allowing the tongue or lip to touch any part of the zinc.

6. I had a number of pieces of zinc made of the size of a shilling, and made them up into a rouleau, with as many shillings. I find that this alternation, in some circumstances, increases considerably the irritation, and expect, on some such principle, to produce a still greater increase. If the side of the rouleau be applied to the tongue, so that all the pieces are touched by it, the irritation is very strong and disagreeable. This explains what I have often observed, the strong taste of soldered seams of metal. I can now perceive seams in brass and copper 174
vessels by the tongue, which the eye cannot discover, and can distinguish the base mixtures which abound in gold and silver trinkets.

If any of the above facts can add to the stock of knowledge you have acquired on this subject, it will give me great satisfaction, and I shall not fail to communicate any thing which may afterwards occur. My indisposition hinders me from taking an active part in the researches, to which this wonderful and important discovery incites; but it is both my duty and my earnest wish, to contribute my feeble assistance to every gentleman engaged in this interesting pursuit.

I find that common silver thread makes a very good conductor, and this to any distance.

Since writing the above, I have found a very easy way of producing very sensible convulsions, (I think muscular) and corroborating my opinion, that the communication (of this part of the 175
whole effect) takes place before contact.

Put a plate of zinc into one cheek, and a plate of silver, (a crown piece) into the other, at a little distance from each. Apply the cheeks to them as extensively as possible. Thrust in a rod of zinc between the zinc and the cheek, and a rod of silver between the silver and the other cheek. Bring their outer ends slowly into contact, and a smart convulsive twitch will be felt in the parts of the gums situated between them, accompanied by bright flashes in the eyes. And these will be distinctly perceived before contact, and a second time on separating the ends of the rods, or when they have again attained what may be called the striking distance. If the rods be alternated, no effect whatever is produced.

Care must be taken, not to press the pieces hard to the gums; this either hinders us from 176
perceiving the convulsion, or prevents it. I find too, that one rod, whether zinc or silver, is sufficient for the communication, and even bringing the two pieces together, will do as well, or perhaps better. But the rods are easier in the management.

Asking pardon for the liberty I have taken, without having the honour of your acquaintance, I am,

With great regard,

SIR,

Your most obedient

Humble servant,

JOHN ROBISON.

FINIS.

Transcriber’s notes:

Obvious typographical errors were repaired. This included one word duplicated across page boundaries.

Non-obvious “misspellings” were left alone.

Possible old spellings inside quotations were left alone.

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