No. 1.

EXPERIMENTS

AND

OBSERVATIONS

SEE SLIP

RELATIVE TO THE

INFLUENCE LATELY DISCOVERED BY
M. GALVANI,

AND COMMONLY CALLED

ANIMAL ELECTRICITY.

By RICHARD FOWLER.

EDINBURGH:
PRINTED FOR T. DUNCAN, P. HILL, ROBERTSON &
BERRY, AND G. MUDIE; AND J. JOHNSON
ST PAUL'S CHURCH-YARD, LONDON.

1793.

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

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.

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.

LEITH, May 28.
1793.

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 the mufcles; the experiments, which prove that the fluids of animals are better conductors of electricity, than water is; and that, "if an electric "fhock pafs through a given part of a living animal, the fame fhock, after the animal is dead, will be vifibly tranfmitted "over the furface of the part, but not through "it *:" the recollection, too, of that fingular power, which fome animals poffefs, as the *torpedo*, the *gymnotus electricus*, and the *filurus electricus*, of collecting and difcharging at pleafure the electrical fluid; but, above all, the wonderful, but folitary, inftance of an electrical fhock received from a moufe, under diffection, recently related by his countryman Cotugno; were circumftances, which feem to have rendered the expectations of the Profeffor not a little fanguine as to his fuccefs.

At length, after many ingenious and interefting experiments, illuftrative of the relations

* Cavallo.

lations which fubfift between artificial electricity and the involuntary motions of animals, a happy accident difcovered to him the phenomena, which have fince been the objects of fo much curious refearch, and which have given to immortality the name of Galvani.

He one day obferved, that fome frogs, hooked by the fpine of the back, and fufpended from the iron palifades, which furrounded his garden, contracted frequently and involuntarily. Examining minutely into the caufe of thefe contractions, he found that he could produce them at pleafure, by touching the animals with two different metals, at the fame time in contact with each other.

To a mind prepared by fuch obfervations and experiments as thofe which had previoufly occupied M. Galvani, the refemblance which this new difcovery bore to the facts he had before obferved, muft have produced

conviction

conviction of the identity of their cause; and the experiments, which have since been made both by himself, and Dr Valli, have given no small degree of plausibility to the opinion. A repetition of some of these experiments excited my doubts as to the legitimacy of the conclusions which had been drawn from them, and induced me at length to proceed in the following investigation.

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

When

When metals are either calcined, or combined with acids, they are no longer capable of exciting contraction. In eftimating the comparative powers of different metals as excitors, I found zinc* by far the moft efficacious, efpecially when in contact with gold, filver, molybdena, fteel, or copper, although thefe latter excite but feeble contractions when in contact only with each other. Next to zinc, tin foil, and lead appear to be the beft excitors. But with zinc, and gold, filver, or molybdena, I have frequently fucceeded in exciting contractions in the foot

* On this metal Cronftedt has the following very curious remark: "It feems to become electrical by friction, and then its fmaller particles are attracted by the loadftone; which effects are not yet properly inveftigated." Zinc is an ingredient of the beft amalgam for fmearing 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 fmaller particles were in any ftate attracted by the loadftone, unlefs they had been fcraped off by means of an inftrument of iron. But, in this way, the duft of any metal is rendered fufceptible of the influence of the loadftone.

foot of a frog, upwards of a day after they had ceafed to be excited, by arming the nerve with tin foil, and ufing fome 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 furface, of an animal with which they are in contact, is confiderable, I think, the contractions are both ftronger and more readily excited, than when the reverfe of this is the cafe. Thus I have almoft 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 fmall piece of zinc, and a filver probe, had failed to produce any effect.

I have faid, that, in order to excite contractions, I believe it neceffary that two different metals, communicating with the part to be excited, fhould be in contact with each other. Some few inftances have been obferved, obſerved, which ſeem to prove the contrary. In a lecture, ſo long ago as October laſt, in which Dr Baillie of London mentioned Galvani's diſcovery, I think I remember him ſaying, that he had twice or thrice produced contractions by the application of one metal only: and Dr Valli, in his 9th letter upon this ſubject, ſpeaks of his having done the ſame with a pair of ſciſſars, made of bad ſteel, and in a frog recently killed. I think it not impoſſible that there may have been ſome unnoticed fallacy in theſe inſtances. I happened one day to touch the crural nerve of a frog, with a ſmall gold tooth-pick ſlid from a ſilver caſe, and the leg inſtantly contracted; I again touched it, and it again contracted. At another time I obſerved contractions from touching a nerve, with a ſilver cannula, and at another from placing one in the folds of a ſilver chain. All theſe appeared at the time to be ſo many deciſive inſtances of contractions from the application of one metal, till the following experiment

ment ſeemed to afford a different explanation. Having placed, one end of a ſilver probe upon the ſciatic nerve of a frog, lying in water ſome inches below the ſurface, I obſerved that no contractions followed, neither did they, when I touched the part of the probe above the ſurface with a piece of zinc. But when I touched it at the ſurface, ſo that both the zinc and the ſilver were in contact with the water, although the zinc was at the ſame time many inches removed from the frog, contractions were produced equally vigorous, as if both the metals had been in immediate contact with the frog.

I was now no longer at a loſs to account for the contractions produced by the gold tooth pick, ſince the circumſtances both of that caſe, and of the experiment related, were probably the ſame, two metals in contact with each other. The gold in immediate contact with a nerve; and the ſilver caſe communicating with it, and the muſcles

through

through the medium of the moiſture, with which the whole was perhaps ſurrounded. This led me to examine the chain, and the cannula. I found both the links of the one, and the ſides of the other, ſoldered with a different metal *. 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

* If further experiments ſhould eſtabliſh decidedly, that the mutual contact of two different metals is abſolutely neceſſary for the productions of Galvani's pheno- [illegible] 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.

when no more than one metal is in contact with it *.

At the time I firft obferved this fact, I was making experiments to afcertain whether it was poffible to tranfmit the influence, which had excited contractions in one leg, into another, removed to fome diftance from it, and communicating with it, only by means of a fingle conducting fubftance, (fuch as filver, for example). For this purpofe,

* In an able lecture, which Dr Monro lately delivered, chiefly upon this fubject, he demonftrated the poffibility 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 fome moift fubftance. In varying this experiment, I find, that if a frog be divided in two parts, juft above the origin of the fciatic nerves, and put into a bafon of water, the hind legs may be thrown into ftrong contractions, by bringing zinc, or tin-foil, and filver, in contact with each other, at the diftance of at leaft an inch from the divided fpine, fo long as they are kept nearly in a right line with it. Water, in this cafe, is the only communication between the metals and the origin of the nerves.

purpofe, I had feparated from the trunk, and from each other, the hind legs of a frog recently killed, and had detached their nerves as far as the knee. I then laid them at fome diftance from each other, upon a plate of glafs, and included the nerve of one leg, and the foot of the other, in the folded ends of a filver chain. With one hand I now raifed from the mufcles, upon the end of a filver probe, the nerve of the leg, whofe foot was folded in the chain; and with a piece of zinc, in the other hand, touched, at the fame time, the nerve and the probe. This leg was thrown into ftrong contractions; but none were excited in the other. I then touched the chain, and nerve of the other, and, to my furprife, both legs inftantly contracted. I had obferved, in the beginning of November laft, that it was not neceffary for the metals to be in contact with any thing but nerve, in order to excite contractions in the mufcles, to which it was diftributed; and had mentioned this fact immediately afterwards to the

the Medical Society of this place, as a ſufficient refutation of the theory, which Dr Valli had formed of Galvani's diſcovery. It would not, therefore, have ſurpaſſed my expectations, had the influence, excited by the mutual contact of a piece of zinc, and probe, with the nerve, paſſed through the medium of the chain, from the leg, in which it firſt excited contractions, and produced contractions in the diſtant leg. But, I now thought that I had not only paſſed the influence from one leg to the other, but in one of the legs in a direction contrary to the courſe of its nerves. The removal of the leg, whoſe nerve communicated with the chain, convinced me of my error: but, at the ſame time, diſcovered to me a fact of much greater importance, than any with which I had hitherto been acquainted. For now, upon touching the chain alone with the zinc, I found that the leg, whoſe foot it ſtill included, and whoſe nerve I held ſuſpended upon a probe, contracted as ſtrongly as before. The influence of the two metals, in in contact with the nerve of the other leg, had not, therefore, paſſed into, and excited this.

It had from the firſt been known, that contractions could be excited by placing two different metals in contact, one with the nerve, the other with the muſcles, and making a communication between them: but, in this experiment, the only metal in contact, either with the nerve, or muſcle, was ſilver. Neither had the influence paſſed through the chain, and up the leg againſt the courſe of the nerve, in conſequence of a communication by means of moiſture ſubſiſting between the zinc, and the foot, as well as between the ſilver chain, and the foot; for the experiment ſucceeded equally well when the chain was removed, and the foot laid upon a ſilver plate made perfectly dry. But when either the zinc, or probe was held by another perſon not communicating with me; or when either of them was inſulated in a ſtick of ſealing wax; no contraction whatever took place.

Neither,

Neither, indeed, were contractions excited in any part of the leg, except the foot, when the probe was withdrawn from the nerve; and the foot, and ſilver, were both touched with the zinc. It is then clear, that the influence, which, in the former caſe, excited the whole leg to contraction, muſt have paſſed through the medium of my body. It is not neceſſary that the ſilver ſhould be laid under the foot; all that is required, is, that it ſhould communicate with it by means of moiſture; it may then be laid at almoſt any diſtance from it *.

The courſe of this influence, however, was ſtill undetermined: it might be from the muſcles to the nerve: it might be from the

* 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!

the nerve to the muſcles. To aſcertain this, and to prove that the influence, which had excited one limb to contraction, might paſs on, through a foreign medium, and excite contractions in another, I made the following experiment.

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

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

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

The different non-conductors of electri-

city

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

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

There is ſomething ſingular in this reſpect, with regard to mercury. If the abdomen of a frog be filled with it, a piece of zinc paſſed through it, ſo as to touch the ſciatic nerves, excites contractions. But a piece of ſilver, paſſed to them, excites none. Neither are any excited by touching the ſilver, beneath the ſurface of the mercury, with a piece of zinc. But I have before ſhewn, that, when water is uſed inſtead of

mercury,

mercury, contractions may be in this way excited; yet mercury is reckoned a much better conductor of electricity than water. I have repeatedly paſſed this influence through a great length of thin braſs wire, and through the bodies of five perſons communicating with each other, by dipping their fingers in baſons 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 alchol 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

with no obſtruction in paſſing from link to link, of ſeveral chains, even when no preſſure, except that of their own weight, is uſed to bring them into contact. I was led from this to hope, that I ſhould be able to make it paſs through a very thin plate of air. I, therefore, coated a ſtick of ſealing wax, with a plate of tin-foil, and then made an almoſt imperceptible diviſion a-croſs it with a ſharp pen-knife. But even this interruption of continuity in the conductor was ſufficient effectually to bar its paſſage.

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

I next proceeded to examine if the capacity of different ſubſtances, as conductors, or non-conductors, was at all affected by differences of their temperature. But this

was

was not the caſe with zinc, iron, water, coal, or a common crucible, the only ſubſtances with which I tried the experiment. A red hot iron, and boiling water, conducted equally as well as iron and water that had not been heated: and neither the crucible, nor the coal, became conductors from any addition of heat.

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

gree to diminifh its power of producing contractions *.

It appears upon the whole to be necef-fary, that this influence fhould pafs to a part in a very condenfed ftate, in order to excite contractions: although there are fome facts, which, without reflecting, might lead one to fuppofe, that, paffing even in a dif-fufed ftate, it would excite them. In mak-ing that experiment, in which the piece of zinc under the foot of a frog is touched with zinc, while its crural nerve is fupported by a filver probe; no contraction takes place, if the probe be either lowered, fo as to come in contact with the mufcles of the thigh, or if it be made to touch the filver under the foot.

If

* 'L'eau trop échauffée, ou qui eft en éboullition, dif-perfe l'électricité, de manière à en détruire les pheno-mènes.'

'L'excès du froid prive l'eau même de la propriété de conduire le fluide en queftion.'—*Dr Valli, Lettre 9me.*

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

I have often likewiſe obſerved, that when the nerve of a nearly exhauſted leg of a frog had been laid upon a piece of zinc, and both

were

were touched with ſilver, the contractions excited were very diſtinct: but when the zinc was placed in contact with the muſcle, as well as with the nerve, either no contractions could be excited, or ſuch feeble ones that they were ſcarcely perceptible.

Contractions, however, certainly may be excited in different parts of a frog, without making any diviſion of its ſkin, by laying the part of the frog to be excited upon a plate of zinc, or tin-foil, and paſſing a piece of ſilver over it, till all three are in contact with each other*. Yet even here the influence

* 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.

fluence does not paſs into the part in ſo diffuſed a ſtate as it may at firſt appear to do. For the ſkin of theſe animals is abundantly ſupplied with nerves, whoſe trunks communicate, at different places, with thoſe which ſupply the muſcles. And the contractions are always ſtrongeſt, and moſt readily excited, when the ſilver is paſſed over the courſe of any of the nerves, which go to the muſcles.

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

Several conſiderations, however, induce me to believe, that ſuch communication is abſolutely requiſite. If the contact of two different metals were alone ſufficient to excite contractions; contractions ſhould always take place, whenever a good conductor is interpoſed between the metals, and the nerve alone. But I have, in no inſtance, obſerved this to be the caſe. In the experiment, where the crural nerve muſt be ſupported upon a ſilver probe, it is neceſſary that the piece of ſilver, with which the zinc is brought in contact, ſhould communicate either immediately, or through ſome good conducting medium, with the muſcles of the foot, or leg, before any contraction takes place. And even in the experiment, where water forms the only communication between the metals, and the origin of the ſciatic nerves, that ſame water, it muſt be obſerved, forms likewiſe a communication between the metals and the muſcles, to which theſe nerves are diſtributed. But the fact, which appears to me moſt deciſive of this queſtion, is the following: When a nerve, which for ſome time has been detached from ſurrounding parts, is either

carefully

carefully wiped quite dry with a piece of fine muslin, or (lest this should be thought to injure its structure,) suffered to remain suspended till its moisture has evaporated; no contractions can be excited in the muscles, to which it is distributed, by touching it alone with any two metals in contact with each other. But, if it be again moistened with a few drops of water, contractions instantly take place: and, in this way, by alternately drying and moistening the nerve, contractions may, at pleasure, be alternately suspended and renewed for a considerable time. It may, indeed, be contended, that the moisture softened, and thus restored electricity and free expansion to the dried cellular membrane surrounding the fibres, of which the trunk of a nerve is composed; and thus, by removing constraint, gave free play to their organization *.

But

* M. Fontana, in the first volume of his work on Poisons, mentions some facts, which may, to some, appear

But from obſerving, that, in every other inſtance, where contractions are produced by the mutual contact of the metals, a conducting ſubſtance is interpoſed between them and the muſcles, as well as between them and the nerve ; I think it would be unphiloſophical

pear 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 wheel 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. Fontána, 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.

unphilofophical not to allow, that, in the inftance in queftion, the moifture, adhering to the furface of the nerve, formed that requifite communication between the metals and the mufcles.

I relate the following fact, in this place, becaufe at the fame time that it gives further confirmation to the above opinion, it affords an inftance in which infulation diminifhed the effect of the metals. I had one day laid the nearly exhaufted leg of a frog upon my hand, with a piece of zinc in contact with its nerve only; and, when I touched thefe with a filver probe, tolerably ftrong contractions were excited, even when the nerve appeared dry: but when both the leg and the metals, thus difpofed, were infulated by means of glafs and fealing wax, the contractions were fcarcely perceptible. My hand, it would appear, had, in thefe inftances, fupplied the place of the moifture in the other; and been the conducting medium between the mufcles and the metals.

This communication of the mufcles with the nerve, through the medium of the metals, had appeared to Dr Valli a circumftance fo effential to the production of Galvani's phenomena, that (taking it for granted they were occafioned by the action of the electrical fluid), it feems to have fuggefted the hypothefis, which he has offered in order to account for them.

Aware that no electrical phenomenon can poffibly have place, except between the oppofite ftates of pofitive and negative electricity, or, in other words, where there is a breach of equilibrium in the diftribution of the electrical fluid; he fuppofes it to be one office of the nerves, to produce this breach of equilibrium, by continually pumping (to ufe his own expreffion) the electrical fluid from the internal parts of mufcles, and in this way rendering them negative, with refpect to the external furface. The brain, he makes the common receptacle for this fluid. The metals, he feems to confider in the light

of

of a conductor, interpoſed between the out-ſide of muſcles and their nerves. And the rapid tranſmiſſion of the fluid to reſtore the equilibrium, as the cauſe of the contractions.

He preſumes his hypotheſis proved from the following conſiderations:

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

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

III. From finding that in general, when the ſciatic nerve on one ſide of a living frog

was

was divided, the other being left entire, communicating with the brain, both armed and equally excited, the limb, in which the nerve had been divided, preferved its power of contracting longer than the other. From this well devifed experiment, he concludes, likewife, that animal electricity is the principle of life. That, on the fide where the nerve remained entire, it was withdrawn from the mufcles, and depofited in the brain. That, from the impoffibility of this taking place on the other fide, where the nerve was divided, it had continued in the limb, and enabled it to contract.

If it were indifputably 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 circumftance would alone be a fufficient refutation of Dr Valli's hypothefis: but, as I have already fhewn, that contractions were not in this way produced in any experiment, which I have

have made, when no moiſture, forming a communication between the metals and the muſcles, had been left adhering to the ſurface of the nerve, it becomes neceſſary to have recourſe to leſs dubious arguments.

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

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

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

The head of a frog having been separated from its body, the latter was laid upon a plate of zinc, held by a person sitting in an insulated chair, which communicated with the prime conductor of an electrical machine. The machine was put in action, and both the person and the frog were electrified positively. In these circumstances, no sparks could be drawn from the frog, by the person holding it: nor could any other electrical appearance take place between them. But, when a piece of silver was passed over different parts of the frog, and, at the same time, brought into contact with

with the zinc plate, contractions were uniformly excited, differing not in the leaſt, either in ſtrength or frequency, from thoſe which are excited when no artificial electricity is preſent. The reſult was preciſely the ſame, when the frog and the perſon holding it were negatively electrified. This experiment was often repeated. The following experiment was made, in order to ſee if the effect produced upon a frog, by the paſſage of artificial electricity from any part of its body, would be increaſed by employing two different metals as conductors.

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

ſioned

ſioned by drawing ſimilar ſparks from it, with conductors of the ſame metal.

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

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

We

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

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

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

I have repeatedly excited, by means of zinc and ſilver, contractions in the leg of a frog, whoſe head had been divided from its body, upwards of three days before. The receptacle, for the electrical fluid, was in theſe caſes removed. Now, either the nerves

continued

continued extracting it from the internal parts of muſcles, or they did not. If they did, having no longer a receptacle, in which they could depoſite their electricity, they muſt have remained poſitively electrified; and thus, being in the ſame ſtate with the outer ſurface of the muſcles, no contraction ſhould, according to the hypotheſis, have been excited by the application of the metals. But this is contrary to the fact.

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

This argument appears, to me, to do away all ſupport, which the hypotheſis may ſeem to derive from the experiment, before quoted, of applying the metals equally to both

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

Four days after I had divided the crural nerve of a female frog, full of ſpawn, I found her dead; ſhe had been obſerved alive the night before. The application of the metals to the leg, whoſe nerve had not been divided, did not excite the ſlighteſt contractions, but on applying them to the leg, in which the nerve had been divided, tolerably ſtrong contractions were excitable, for more than twelve hours after ſhe was

found.

found. The ſpawning ſeaſon had cloſed, upwards of a week before this happened, and, as this frog had long been without a male to aſſiſt her, it is probable, that her death had been occaſioned by the retention of her ſpawn, as it was found in a very diſſolved ſtate. The pain, neceſſarily preceding ſuch a death, could affect the different parts of the animal, only through the medium of its nerves; and hence the exemption of that part from its effects, to which the communication, by nerve, had been interrupted.

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

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

Strong, however, as is the analogy, which, in many particulars this influence bears to electricity, confiderable doubts muft, I think, ftill remain as to their identity.

The grounds of thefe doubts would beft appear in an accurate and full ftatement of the feveral points, both of refemblance and of difference between this influence, electricity, and that power which diftinguifhes the torpedo, the gymnotus, and the filurus: but, I can here promife no more than a very imperfect and defultory fketch of thefe.

In order to accumulate artificial electricity, if I may be allowed the ufe of fuch an expreffion, it feems neceffary, that there fhould be motion between two fubftances,

an

an electric and a conductor. 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 *.

That

* 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.

That influence, whatever it be, which is poſſeſſed by the torpedo, &c. ſeems to depend entirely 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 im-

perfect

perfect conductor of electricity, when free from air 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 o.

But the temperature of the room, in which I made my experiments, was at leaſt 55 degrees above o, 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

We are told by Mr Cavendifh, that Mr Walfh found the fhock of the torpedo would not pafs through a fmall brafs chain: but the influence difcovered by Galvani, paffes, without fenfible diminution of its effects, through a fmall brafs chain of feveral inches in length, when it is drawn fo tight as to bring its links into clofe contact with each other: and it paffes through a gold chain when held between two perfons, and fuffered to hang with a confiderable bend. Yet, if we may be allowed to judge of the comparative ftrength of the two influences, by the effects which they produce upon animals, that of the torpedo muft certainly be allowed to be the ftrongeft; and I fee no other way of accounting for its finding an infuperable obftacle to its tranfmiffion, where the other finds fcarcely any, except by fuppofing that they are in reality different in their nature.

Dr Valli tells us, that he obferved the hairs of a moufe, attached to the nerves of frogs

frogs by the tin-foil, with which he furrounded them, alternately attracted, and repelled by each other, whenever another metal was fo applied as to excite contractions in the frogs.

This experiment I have many times repeated, both in the manner defcribed by the Dr, and with every variation in the difpofition of the hairs which I could devife: but whether they were placed upon the metals, the nerves, or the mufcles, or upon all at the fame time, neither I, nor my friends who affifted me, have in any inftance been able to obferve them agitated in the flighteft degree.

I have made fimilar experiments upon a dog, and upon a large and lively fkate, by difpofing, in the fame way that I did the hairs of a moufe, flakes of the fineft flax, fwanfdown, and gold leaf; but although the contractions produced in the fkate, by the contact of the metals, were fo ftrong as to make

make the animal bound from the table, not the leaft appearance of electricity was indicated.

I next fufpended, from a ftick of glafs fixed in the ceiling of a clofe room, fome threads, five feet in length, of the flax which I ufed in the former experiment; and approached fome frogs, recently killed, and infulated upon glafs as near to them as was poffible, without touching: but the threads were in nowife affected by the contractions produced in the frogs.

In this refpect, therefore, this influence agrees with that of the torpedo, &c. So far as I know, M. Votta's inftrument for collecting, condenfing, and rendering fenfible, very fmall degree of electricity has not been employed in the examination of either.

And indeed I am not fure, if, in examining the newly difcovered influence, by fuch a teft, a fufficient quantity of electricity might

not

might not be produced merely by the motion of the animals, 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. 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

the torpedo, &c. and from electricity, in producing no 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 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, 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 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 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

it

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*.'

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 of

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

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 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 divid-

ed,

ed, the contractions produced are as free and 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

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.

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

When the ſeparated leg of a frog was laid upon a plate of iron, and a loadſtone was brought in 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.

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

The following experiments, made upon animals confidered by anatomifts, in general, as deftitute of nerves, may to fome appear decifive of this queftion, but to myfelf, I confefs, they are by no means fo. In by far the greater number of animals, we are precluded from the poffibility of difcovering nerves by their minutenefs; yet the actions of thefe animals, not merely excited by mechanical irritation, but fo obvioufly directed to the attainment of an end, oblige us to infer their exiftence even where our fenfes, aided by the beft glaffes, do not enable us to detect them.

Having laid fome earth worms upon a plate of zinc, I tried to excite contractions in them, by paffing a rod of filver 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 feemed to produce as little effect. At length, I perceived one of them dart itfelf forwards, whenever the 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

face of 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, confiderably different from thofe produced by the metals upon the limbs of frogs, and other animals. They had not fo much the appearance of involuntary, inftantaneous convulfions, as long continued expreffions of pain and difguft; fuch as are produced by applying zinc and filver to the tongue of a child.

A ftrong prefumptive proof, in my humble opinion, that thefe animals are endowed with a moft exquifite organ of fenfe, and, confequently, that they are not, as has been fuppofed, deftitute of a nervous fyftem.

Doubtful, therefore, if this influence can ever act upon the mufcular fibre, except through the medium of nerves, I fhall referve what I have to fay upon particular mufcles, till I have related fome facts relative to the nerves.

OF

OF THE NERVES.

IT appears from every experiment, which has been made in profecution of Galvani's difcovery, that the nerves are very effentially concerned, in all the phenomena which it exhibits. It becomes, therefore, an object of inquiry, highly interefting, to afcertain if all the nerves of the body are equally fubjects of this new influence, or if its effects are confined to thofe appropriated to mufcles of voluntary motion. With this view, I furrounded with tin-foil the par-vagum and intercoftal nerves of feveral cows and fheep, while the auricles of their hearts were ftill contracting, and placed one end of a bent filver rod, at one time upon the heart itfelf, at another upon adjacent muf-

cles,

cles, and 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

with the ſame 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*, 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

* 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.

we know ſo little either of mind or of its 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*. 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

* Mem. Med. Soc. Lond. vol. iii.

than it does when theſe animals are 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; 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

upon this ſubject, failed of ſucceſs (as 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 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,

eyes, which does not permit me to look intently at minute objects, I have never been able to fucceed.

Since making this laft experiment, I have repeated it upwards of twenty times. In order to its complete fuccefs, it is neceffary that the fpontaneous contractions of the heart fhould nearly, if not altogether, have ceafed; and, when in this ftate, the experiment is rendered ftill more fatisfactory by removing the heart from the body of the frog, and laying it upon a plate of zinc. We are then fure that its contractions cannot have been excited, by any mechanical irritation, arifing from the contractions of the mufcles of the thorax.

For want of fufficient leifure, 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 difcovered the poffibility of exciting the heart to contraction by

means

means of zinc, and ſilver applied to 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

were fully as ftrong when molybdena, as when filver was ufed. No contractions could be excited, by arming any of the nerves of voluntary mufcles, 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 inftant fhe had ceafed to ftruggle, which was in lefs than four minutes after immerfion, her heart had ceafed to contract; nor could its contractions be renewed, either by the application of the metals in the way defcribed, in the laft experiment, or by pricking or otherwife irritating its furface: but the diaphragm, the intercoftal mufcles, the fore legs, and the ears, continued to contract long and vigoroufly, when the metals were as ufual 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 moft vigoroufly, gorouſly, though the 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

tion of the 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

that one mufcle, not fubjected to the influence of the will, might be made to contract by the application of zinc and filver to its nerves; I proceeded to examine whether the fame were the cafe with refpect to all involuntary mufcles. I could not, however, obferve that any contractions were produced in the ftomach or inteftines, by placing the metals near the ftomachic flexus and femilunar ganglion in a cat. I next proceeded to examine the effects of the metals upon the different organs of fenfe.

M. Volta's difcovery of the fenfation produced upon the end of the tongue, by coating its upper and under furfaces with different metals, led me to compare this fenfation with that produced by electricity. I found a very confiderable difference between them. Both, indeed, are fubacid, but as unlike to each other, as the tafte of vinegar is to that of diluted vitriolic acid. That occafioned by the metals is accompanied

panied with what is familiarly called the metallic tafte; and differs according to the metals employed. With the greater number of metals it is fcarcely perceptible. With zinc and gold, I think, it is ftrongeft; next fo with zinc and filver, or molybdena, and infufferably difagreeable with any of them.

The fenfation is moft diftinct when the tongue is of its ordinary temperature, and when the metals are of the fame 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, fcarcely any fenfation is produced. That this difference in the effect is owing to the alteration which has been produced in the ftate 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 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

On placing different metals in the meatus auditorius externus of both my ears, and eſtabliſhing an 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 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

the eye, and in contact with any part of the ball. If the experiment be made with zinc and gold, inftead of tin-foil and filver, the flafh is incomparably more vivid. I had the difagreeable opportunity of trying this experiment upon one of my eyes, in a ftate of inflammation; and, in this cafe, found the flafh much more ftrong than it was in the uninflamed eye. I tried it likewife upon a patient, affected with amaurofis; but the man was fo ftupid that I could not fatisfy myfelf as to the precife refult.

Recollecting that fine nervous twigs pafs from the ciliary or ophthalmic ganglion, through the fclerotic 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 nafal branch, of the fifth pair of nerves, in conjunction with a branch of the third, I proceeded to try if, by infinuating a rod of filver, as far as poffible, up my nofe, and thus arming this nafal branch, I could, by bringing the filver into contact with a

piece

piece of zinc, 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,

gument, that the contractions of the heart might be influenced in a fimilar manner.

I therefore defired fome of my friends to obferve my pupil, while I repeated the experiment, which I have above defcribed. When the external light was ftrong, they found fome difficulty in determining, whether the pupil contracted or not; but when no more light was admitted, than what was juft fufficient for difcerning the pupil, they perceived a very diftinct contraction, every time the metals were brought into contact with each other. This experiment requires fome attention, in order that it may fucceed fatisfactorily; but although I have repeated it a great number of times upon the eyes of others, it has feldom failed, when made in a fteady light, and when the filver has been paffed far enough up the nofe.

The dilatation of the pupil, inftead of its contraction, on the application of a ftimulus to its nerves, as in the cafe related by Dr

Whytt, is, I apprehend, not fo uncommon a circumftance, as it may at firft be fuppofed. I have myfelf feen three inftances of it in difeafes of the head. One of thefe was in an epileptic patient, whofe pupils, during the intervals of his fits, became fuddenly dilated whenever his eyes were expofed to a ftrong light.

My friend, Mr George Hunter of York, while one day amufing himfelf with repeating fome of thefe experiments, difcovered that by placing one of the metals as high up as poffible between the gums and the upper lip, and the other in a fimilar fituation with refpect to the under lip, a flafh was produced as vivid as that occafioned by paffing one of the metals up the nofe, and placing the other upon the tongue. It differs, however, from the flafh produced in any other way, in the fingular circumftance of not being confined to the eye alone, but appearing diffufed over the whole of the face. On attending to the concomitant

mitant ſenſations produced by this diſpoſition of the metals, I 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

ſubſiſts between the ſeveral branches of the fifth pair of nerves, and 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 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,

periments, too familiar to need 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,

me, the circulation appeared very 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 theolo-

' gus,

'gus, plaga dolorifica, a rupta inſtrumenti 'muſici chorda accepta, nocte ſubſequenti 'jam adulta, e ſomno evigilans, cuncta cla-'re, ac ſi de die eſſet, vidit, adeo, ut mini-'mos picturarum et tapetum tractus obſer-'vare, characteresque ex libro legere poſſet. 'Oculo vero læſo clauſo, tenebras denſiſſi-'mas adeſſe ille percepit, eodemque iterum 'aperto, conclave illuſtratum viſum eſt, lu-'cem tamen candelæ allatae ſoliſque ſplen-'dorem de die, ægre tulit oculus affec-'tus, quod per aliquot dies duravit, tan-'demque 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 ſti-muli, i. e. from the place at which it firſt af-fects a nerve, onwards to the part, in which

that

that nerve terminates. I have repeatedly caufed electrical fparks to be paffed into my own ulnar nerve at its paffage over the inner condyle of the humerus, but both the fenfations 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 Yorkfhire, that when no communication is left between the trunk and pofterior extremities of a frog, except by its fciatic nerves, a ftrong folution of opium, injected under the fkin of its pofterior extremities, deprives them both of their fenfibility and of their contractile power; but that it does not in the leaft affect the trunk of the body. If, on the contrary, it be applied to the trunk, it exhaufts both the trunk and the extremities.

M. Galvani is faid to have obferved the effects of the influence, which he difcovered, diffufed over the whole body of a frog, when the metals were applied to a nerve merely laid bare, without being either divided or feparated from furrounding parts. If we are allowed to infer this diffufion of the influence from the reftleffnefs expreffed by the animal, M. Galvani's obfervation may be juft. If from the contractions produced, I fufpect it is by no means fo; fince, in every experiment which I have made upon the fubject, the contractions have been confined to thofe parts to which the nerve touched by the metals was diftributed.

That this influence, however, may pafs in a direction contrary to the courfe of nerves, is evident from fome of the experiments which I have related relative to its effects upon the fenfes, but is ftill more clearly demonftrated by the following.

If, after having divided at the pelvis a frog recently killed, the fciatic nerves be freed from cellular membrane up to their origin from the fpine, and all the parts be-

low

low this, except themſelves, be cut 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.

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 broad

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,

lieve, that the ſmalleſt arteries of the body 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.

SEC-

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,

power, by which mufcles contract on the application of a ftimulus.

By others again, thefe feveral properties are fuppofed to be derived from the arteries, which may either fupply the materials and conftruction of that exquifite and peculiar organization, which fits nerves and mufcles for performing their refpective functions, or may furnifh, from the blood, fome fubtile principle, fuch as that believed by M. Fontana, to exift there, or fuch as that we are now examining, which differently modified in different parts, may be the latent caufe of all the phenomena exhibited by animals.

The advocates for the firft opinion obferve, that whenever the brain is confiderably injured, or its free communication, by means of nerves, with moving parts is interrupted, a deprivation both of fenfe and motion is the uniform confequence: and, further, that the feveral organs, both of fenfe and of motion, appear to fuffer detriment

from

from the over ſtrained exertions of the brain in thinking, 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, 'that concomitant arte-
'ries, somehow or other, tune the nerves,
'so as to fit them to convey impression *.'

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

* Essays Physical and Literary.

have their functions fully reftored. Now, in what manner this can be accomplifhed, unlefs by the agency of arteries, would, I imagine, be no eafy tafk to point out.

The influence difcovered by Galvani appeared to me an admirable teft, by which fomething decifive might be afcertained relative to thefe important points in the phyfiology of animals, and as fuch I have employed it in the following experiments.

Confidering, therefore, the brain on the one hand, and the fanguiferous fyftem on the other, as the poffible fources from which nerves and mufcles might derive their power, I began by comparing the effects which refult from interrupting their communication, firft with the brain, and then with the arteries. This mode of procedure feemed to afford the beft profpect of information with refpect to every object which I had in view, but particularly with regard

to

to the relations which this 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.

Experiments

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 para-

lytic

lytic below the knee, and very nearly ſo above it. Theſe 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

longer 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.

EXPE-

EXPERIMENT II.

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 ſkined 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,

nerve, but had not proceeded down to it. The limb, at the time I killed the frog, was as deftitute both of motion and of fenfation, as at the firft inftant the nerve was divided, but contractions were excited in it, by touching the ulcer with zinc and filver. 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 fciatic nerves within the abdomen. Vigorous contractions were excited in the found leg, but none in that whofe 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 confiderably from each other, connected by a tranfparent gelatinous fubftance. From the upper end, which appeared elongated into a conical form, feveral red ftreaks projected into the interpofed fubftance. The lower end was opaque, thickened, and

P rounded,

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.

EXPE-

EXPERIMENT III.

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

The appearance of the mufcles in the legs, whofe nerves had been divided, was found to be precifely the fame as in thofe where nothing had been done; but, notwithftanding this circumftance, even ftrong electrical fparks excited but very feeble contractions. On examining the nerves, the ends of that which had been longeft divided were found connected by a fubftance not at all refembling nerve, but fimilar to that found in the former experiment, and evidently proceeding from the upper divifion. In the nerve which had not been fo long divided, this circumftance was ftill more apparent, as the fubftance had not extended quite to the lower divifion. The cellular membrane furrounding thefe upper divifions had the appearance of innumerable veffels finely injected, and fome red ftreaks were feen projecting, as if from the nerve itfelf, into the gelatinous production. In the found nerves, the obliquely tranfverfe lines of alternate opacity and tranfparency, or, as Fontana has called them, the white

fpiral

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

ference between theſe legs, whoſe nerves had been divided, and others, in this reſpect, I laid 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 $\frac{1}{12}$ of an inch from each other.

Experiments

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 inftantly weaker than the other, and rather dragged when the animal was put into water. The frog, however, could ftill jump about with great agility. Four hours after this operation, it was killed by crufhing its brain. It continued to move its legs fpontaneoufly, when touched, during more than two days after this, and contractions were excitable by the applica-

tion

tion of the metals for two days longer. Sometimes it appeared rather doubtful, which leg 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.

EXPE-

EXPERIMENT II.

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

But ftill, as the effects arifing from the interception of the influence of the brain, and of the circulation, were not compared with each other in the fame but in different animals, whofe age, relative ftrength, &c. might poffibly differ, I thought proper to repeat the comparifon, in the following manner.

Experiments

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.

EXPE-

EXPERIMENT II.

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 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.

HAVING tied the crural artery on one fide, and divided the fciatic nerve on the other, on three full grown male frogs, I ftrangled them all on the fixth day following. My motive for killing the frogs, fubjected to fuch experiments, either in this manner or by crufhing their heads, will be obvious. It was of confequence to preferve their circulation as entire as poffible, and, at the fame time, avoid the continuance of pain, which by exhaufting all the parts of the body, whofe communication with the brain was not interrupted, might confiderably have affected the refult of the experiments.

The contractions excited by means of the metals,

metals, were, in all theſe inſtances, likewiſe as much 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.

Experiments

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

EXPERIMENT I.

I HAVE before faid that if a living and entire frog be fet upon a plate of zinc, contractions can very feldom be produced in any part of its body by paffing a rod of filver over it, fo that the filver, 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 brufh, 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 ftill be produced in the other leg which remained in its natural ftate.

EXPE-

EXPERIMENT II.

HAVING previoufly excited inflammation, by means of a brufh, 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 inftantaneous jirks; thofe in the found leg more languid and difficultly excited. Spontaneous motions continued at this time nearly the fame in both. Till the end of the fecond day, after this frog's head had been taken off, the contractions excited in the inflamed leg continued uniformly, and beyond all comparifon more vigorous than what I could by any means excite in the found leg. But, after this time, the inflamed leg became hard as a piece of wood; probably in confequence of the effufion to which the inflammation had given rife.

The event of five fimilar experiments was fo nearly the fame, that I fhould be thought unneceffarily minute, were I to relate them in detail.

We are now perhaps prepared to account for the deficiency of contractile power in thofe legs, whofe fciatic nerves had been divided, the one, between three weeks and a month, the other, fix weeks, compared with its continuance in the leg, whofe nerve had been divided upwards of three months. It appears, from the circumftances of thofe experiments, that fome of the arteries, appropriated to the fupply of the fciatic nerves of frogs, have the fame courfe with the nerves themfelves; fince the depofition of new matter could in all be traced from the upper divifion of the nerves. It is obvious, therefore, that the part of the nerves below

below the diviſion, muſt have been depriv-ed of ſo conſiderable a portion of their uſu-al arterial ſupply, as in time would occaſion ſome alteration in their ſtructure, and conſe-quently 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 pro-portion 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 coun-tenanced 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 re-union took place during the very ſhort pe-riod which he ſuffered to elapſe between their diviſion, and their ſubſequent exami-

nation.

nation. 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

was owing to the divifion of the cervical nerves proceeding to the arm: yet both the full ufe, and ftrength of this arm, were reftored, after a period of about two years and a half. A proof perfectly fatisfactory that an actual regeneration of nerves had, in this cafe, taken place; and if in this, one fees no reafon why it fhould not equally take place in any other part of the body.

It might be difficult to affign a fatisfactory reafon for the very fpeedy reproduction of the intercoftal, 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 profufe manner in which they are fupplied with arteries, probably both in an afcending, and in a defcending direction; from above, by the fuperior, and from below, by the inferior laryngeal arteries?

It appears upon the whole, therefore, tolerably ferably certain, that the fanguiferous fyftem contributes more immediately than the brain to the fupport of that condition of mufcles and of nerves, upon which the phenomena of contraction depend; fince that condition is much more injured by intercepting the influence of the former than of the latter.

Every experiment and obfervation, which has been made upon the fubject of nutrition, and of the reproduction of parts, clearly demonftrates that nerves and mufcles, in common with every other part of the body, derive their ftructure from the arteries; and it is evident, that upon this ftructure their feveral properties muft in fome meafure depend. But M. Galvani's difcovery of a fubtile influence, which may be tranfmitted apparently from one part of an animal to another through foreign media, may reafonably give rife to a conjecture that the phenomena exhibited by nerves and by mufcles may perhaps depend more imme-

diately

diately upon ſome ſuch influence ; and rea-
ſons exiſt, which might induce ſome to
ſuſpect that even this is derived from the
blood.

Experiments

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.

Independant 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

ſiderable countenance, from 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 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

might be ſufficient to account 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.

EXPE-

EXPERIMENT I.

HAVING felected two frogs as nearly as poffible of the fame fize and vigour, I deprived one of its blood by opening, firft, one of its crural veins, then, a crural artery, and laft of all, the heart. To affure myfelf of the complete evacuation of its blood, I next injected water into its heart, and immediately afterwards forty drops of a ftrong aqueous folution of opium*.

I

* This folution, which is the fame that I employed in all my fubfequent experiments, was of the fame ftrength with that ufed 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 clofe corked bottle, near a chamber fire.

I then removed the ſternum of the other frog, and having made an opening into the ventricle of its 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.

EXPE-

EXPERIMENT II.

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

tion of the 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 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

more than fifty hours, in the legs of that from which the 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.

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 differ-

ence

ence became ftill more decided. At the end of 34 hours, fcarcely any contractions could be excited in the leg whofe artery had been tied; though they continued vigorous in the other; and, at the end of 46 hours, they had ceafed altogether to be excitable, in the leg whofe artery was tied. In the other, they continued during feveral hours afterwards.

EXPERIMENT V.

HAVING tied one of the crural arteries of another frog, I filled its ftomach, immediately afterwards, with a faturated folution of opium in water. The difference between the ftrength, and the continuance of the contractions, excited by the metals, in the two legs of this frog, was not fo great as in the former; yet ftill the difference was confiderable in favour of that leg in which the artery remained free.

EXPE-

EXPERIMENT VI.

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.

EXPE-

EXPERIMENT VII.

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

exhauſtion of the legs, in which the arteries were tied, 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 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

quiſite ſenſibility, which opium never fails to produce in theſe animals. 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.

EXPE-

EXPERIMENT X.

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 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

even the more painful 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 hu-‘ mours in the animal machine, is the ve-‘ hicle 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 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.

APPEN-

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 obferving, as they were not immediately connected with the objects on account of which the experiments were inftituted; and I have yet fome few to relate, which, from the hafte with which thefe fheets were prepared for the prefs, I had omitted to infert in their proper places.

1. In one of my firft experiments, in which I had occafion to fuffer a frog to remain tolerably entire, fo long as contractions could be excited in any part of its body, I was furprifed to find, on removing its fternum, that

that its heart had ceafed to contract, nor could be roufed by the application of any ftimulus whatever, notwithftanding the contractions in its hind legs, excited by the metals, were ftill vigorous, and continued fo for feveral hours afterwards. On paying particular attention to this circumftance in another frog, upon whofe brain opium had been injected, I found that its legs continued excitable, upwards of forty hours longer than its heart. This difcovery of the continuance of the contractile power, in the mufcles of the pofterior extremities, fo long after its difappearance in the heart, is fo contradictory to the opinion generally received upon this fubject, and long eftablifhed among phyfiologifts, that I can fcarcely expect it fhould be credited, by thofe who may not themfelves have opportunities of obferving it. It is a fact, however, of which, in the courfe of thefe experiments, I have had the moft fatisfactory and uniform proofs, both in fuch frogs as have, and in fuch as have not, been under the influence of opium.

If

If a different opinion has hitherto been 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

milar experiments, which I have made, the event has been very different. 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

ceſſary to notice in this 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 diftributed, is brought into inftant contraction. But no mufcles are brought into contraction, when the metals are applied to the brain or fpinal marrow, except fuch as derive their nerves from the part immediately in contact with the metals. The influence does not ftimulate or pafs along the fpinal marrow, as it would along the trunk of a nerve, to affect all other nerves branching off from it.

I firft became acquainted with this fact, while making the following experiment. Having laid bare the brain of a living frog, and put a ftop to its fpontaneous motions, by gently preffing upon the brain, I introduced a long flip of tin-foil doubled underneath a part of the fkull, which had not been removed, and placed a filver probe upon its tongue. The only mufcles which contracted, when the tin-foil was bent over the nofe of the frog, fo as to come in contact with the probe, were thofe which move the

the eyes, and the tranſparent membrane which defends them, thoſe of the tongue and 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

placed upon any part of its ſpine, while the other is made to touch the plate.

---

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. Contrac-

tions

tions were then excited in the frog, by placing one end of a bent ſilver wire, 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.

The refult was the fame when thefe frogs were laid fucceffively upon the metal plate of the electropherus itfelf, and excited, the one in M. Galvani's method, the other by mechanical irritation only.

Thefe experiments were very frequently repeated, but the quantity of electricity collected was always greateft where the contractions, or, in other words, the friction had been moft confiderable, and did not, in any inftance, appear to depend on the means employed to excite the contractions.

What ftill 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 poffeffed by the frog, in common with other conducting fubftances,

is,

is, that 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 electrophorus, 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,

frog, excite contractions nearly so 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.

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,

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 about to favour the public with an account of them, I have taken the liberty of cummunicating 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

along its margin. This irritation ſeems 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 be-

gan

gan to *ooze* through, in the middle of the wound. I applied the 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

communication through 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

ceive ſeams in braſs and copper 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

borating my opinion, that the communication (of this part of the 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. Thurſt 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 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.

F I N I S.