EXPERIMENTS

ON

ANIMAL ELECTRICITY,

WITH THEIR

APPLICATION

TO

PHYSIOLOGY.

AND SOME

PATHOLOGICAL AND MEDICAL OBSERVATIONS.

By EUSEBIUS VALLI, M. D.

CORRESPONDING MEMBER OF THE ROYAL ACADEMY OF SCIENCES OF TURIN.

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M DCC XCIII.

TO

SAMUEL FOART SIMMONS,

M. D. F. R. S.

THESE

EXPERIMENTS

ON

ANIMAL ELECTRICITY,

ARE INSCRIBED,

WITH VERY SINCERE ESTEEM,

BY HIS MOST HUMBLE SERVANT,

E. VALLI.

TO

Mr. MOORCROFT,

VETERINARIAN.

SIR,

MY Work comes from your Pen in a much better State than it originally came from mine. A thorough Conviction of your Talents and Science led me to expect it, and I am not disappointed by the Result. This is the Language of my Soul, which is a Stranger to Dissimulation and Flattery.

Permit me then to offer you this public Mark of my Esteem, whilst my Heart shall ever continue in secret, to pay you the still dearer Tribute of Gratitude.

I am, Sir,

Your very obedient Servant and Friend,

EUSEBIUS VALLI.

PREFACE.

THE discovery of M. Galvani concerning animal electricity, has opened a new field of enquiry for physiologists and physicians. At a very early period I entered upon it, and communicated to the public every step I took, with an impatience arising from motives too obvious to mention. But on taking a retrospect*

* *My first experiments were published at Pavia, in April* 1792. *The impatience which I expressed in my letter upon this subject, left reason to suspect that I had precipitated the experiments. They were however made with all necessary time, and with the assistance of Doctor Masini, a very active, and well-informed young man. Many of them were new, at least no one had undertaken them after the discovery of M. Galvani. I did not draw any conclusions from the results I obtained. This note is intended by way of answer to what M. Volta has had the complaisance*

ſpect of the reſults of my different Eſſays, it ſtruck me, that if they had been properly arranged and commented upon, they would have been of much greater utility. This reflection induced me to prepare the preſent Work, of which the following is the plan.

I firſt give an outline of the hiſtory of Electricity down to the diſcovery alluded to, on which I dwell a little both to explain the circumſtances which gave riſe to it, and the nature of the diſcovery itſelf.

complaiſance to inſert alluſive to me, in one of his letters upon Animal Electricity. Theſe letters of M. Volta contain ſome experiments between muſcle and muſcle of prepared frogs and in live frogs, which I publiſhed as my own. I cannot do leſs in juſtice to myſelf, than obſerve, that I made theſe experiments at Turin, in the month of May 1792, *in the preſence of many ſpectators, among whom I ſhall only mention the Count de St. Martin, the Abbé Eandi, and Doctors Giulio and Boniva, Profeſſors of the Univerſity. By this I do not mean to ſay that M. Volta has taken his experiments from me, but to prove that I did not borrow them from him. This gentleman publiſhed them as belonging to him, without knowing that I was the author of them, juſt in the ſame way as he publiſhed as his own the Diſcovery of the Inflammable Air of Stagnant Water, which had been before made by Franklin.*

Then

Then follow my Experiments, many of which are analogous to those of the Professor of Bologna, and a great number entirely new. Many of these experiments are not in the Journal de Physique, and the others, which are only announced in this Journal, are here treated in detail. In the relation of these facts, I have often had occasion to enter into physiological and pathological reflections.

After that, I have endeavoured to demonstrate the identity of the nervous fluid with the matter of electricity, and have supported this doctrine by the experiments of others by my own, and by analogy.

In order that electricity be in a condition for action, it must exist in two contrary states. This circumstance presents a strong difficulty, viz.—How is it possible that electricity can be condensed in a body, all the parts of which have the property of conducting this fluid? In reply to this I observed, that the animal is furnished with particular powers

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and resources; but what is still more to the point, there are direct proofs of this condensation. The gymnotus, torpedo, &c. have electrical batteries, from which they dart destruction upon fishes they mean to feed upon. These animals possess organs of a singular construction.—But what are to be found in these organs except an arrangement of fibres, membranes, vessels, and nerves?

If we were to judge of their functions in consequence of an anatomical inspection of their organization, we should never be led to conclude, that they were adapted for the accumulation of electricity, because all these parts are alike conducting substances. Thus, if this accumulation does take place, as is most certain, we must acknowledge that there exists in the animal, a secret power, which performs*

* *The nervous system possess, probably, this power. "Mr. Hunter has well observed, says Sir J. Pringle," and I think he is the first who has made the observation, "that the magnitude and number of the nerves bestowed on*

performs this operation. This power is common to all animals in whom the phenomena of electricity are apparent. I believed to have discovered in the muscles which are subject to the empire of the will, the plan which nature has proposed in the construction of the

on those electric organs, in proportion to their size, must appear as extraordinary as their effects; and that, if we except the important organs of our senses, there is no part even of the most perfect animal, which, for its size, is more liberally supplied with nerves than the torpedo; nor yet do these nerves of the electric organs seem necessary for any sensation than can belong to them; and with respect to action, Mr. Hunter observes, that there is no part of any animal, however strong and constant its action may be, which enjoys so large a portion of them. If then it be probable, that these nerves are unnecessary for the purpose, either of sensasation or action, may we not conclude, that they are subservient to the formation, collection, and management of the electric fluid? especially as it appears, from Mr. Walsh's experiments, that the will of the animal commands the electric powers of its organ? If these reflections are just, we may with some probability foretell, that no discovery of consequence will ever be made by future physiologists, concerning the nature of the nervous fluid, without acknowledging the lights they have borrowed from the experiments of Mr. Walsh upon the living torpedo, and the dissection of the dead animal, by Mr. Hunter." (Sir J. Pringle's Discourses.)

organs

organs of the fishes just mentioned, and for this reason I consider muscles as electrical machines.

Their action arises from the circulation of their electricity, which is effected by the medium of the nerves.

The motions of other muscular parts, independent of the will, are executed by a simple afflux of nervous electricity, determined to these parts by specific stimuli.

I have often coated the nerves of muscles independent of the will, and have established a communication between the coating and the naked nerves, or the muscles themselves, by means of a conductor; but I could never succeed in producing contractions. If this circumstance had not happened, instead of having recourse to the afflux of nervous electricity, I should have said, that the mechanism of these muscles was the same as that of those which obey the will. For a considerable time I entertained this idea, as appearing most conformable

formable to the simplicity of nature; and it is with regret that I have been obliged to abandon it.

The sensations depend upon a change in the state of the electricity of the nerves.

The coats of nerves being bad conductors with respect to their interior substance, as is proved by my experiments, prevent this fluid being dispersed among the surrounding parts.

Having advanced my opinion with regard to the influence of electricity on the motions and sensations, I wished to establish it upon a more solid basis; and with this view, I examined both these functions along with those which are connected with them, viz. the secretions and nutrition, as well in the state of health as in that of disease.

In the explanation of the mechanism of the muscular motions which arise from the action of the mind, I have by no means followed the theory of M. Galvani.

Electricity (in my opinion) does not act as a stimulus, nor does it ever equilibrate. The contractions and relaxations of muscles derive only by a change in the state of this fluid. An examination of the changes observable in muscles during their action; the rapidity with which these changes succeed each other; the property which the electrical matter possesses of increasing the cohesion of bodies, and of preserving itself under certain circumstances in the contrary states after the discharge; and lastly, the property of retaining electricity, which certain substances enjoy, render my theory sufficiently probable.

With regard to the other functions, as they are executed in a very obscure manner, I was obliged to grope my way in the dark, and imagine hypotheses, which are perhaps no better than dreams. But our ignorance, with respect to the means which nature employs for the management of the electrical fluid, is not an argument against the existence of animal electricity.

It is ſufficient for us to know, that this fluid exiſts in animals, that it can be there in a ſtate fit for action, and that muſcular motion, ſenſation, and nutrition, are not better explained than admitting the identity of the nervous fluid with electrical matter.

If any one can convince me of the non-neceſſity of a fluid in the nerves, and at the ſame time will explain, without the concurrence of this agent, all the phenomena and functions of which I have treated in the courſe of this work, I will candidly renounce my opinions; and without being aſhamed, avow that I have been miſtaken.

I ſhall, I doubt not, be charged with cruelty by many of my readers; but thoſe who are thoroughly acquainted with my character, and know that I have ſacrificed every comfort and convenience of life, with all the pleaſures of ſociety, for the purpoſe of viſiting different countries in ſearch of information, that I have experienced every poſſible hardſhip and fa-

tigue

tigue, and solely with a view of offering the fruits of my labour to the public, they, I say, will not do me this injustice, but rather stand forwards in my defence.

EXPERIMENTS

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ON

ANIMAL ELECTRICITY.

SECTION I.

THE ſcience of electricity, which is now in ſo advanced a ſtate, was entirely unknown to the ancients.

The firſt diſcoveries relative to it, were rather the effects of accident, than of any exertions of human ingenuity. Accident pointed out to Gray, the difference between conducting and non-conducting ſubſtances. Accident diſcovered to Du Fay the *vitreous* and *reſinous* electricities, and it was by chance likewiſe, that Van

Kleiſt diſcovered the means of accumulating electricity, ſo as to produce the ſhock. Soon after this diſcovery, which, in point of importance, far ſurpaſſed the preceeding ones; the Italian phyſicians endeavoured to impoſe upon the world with their *medicated tubes*.

Whilſt this was going fowards, philoſophers who were engaged in examining the effects of this new agent, fancied they diſcovered an affinity betwixt them and thoſe of thunder. Dr. Franklin, who had already diſcovered the power of points in attracting the electric fluid, was the firſt who pointed out the means of aſcertaining the fact. Experiment put the matter beyond diſpute. Then for the firſt time, thunder was ſubjected to the power of man.

As ſoon as the Leyden phial became known, philoſophers applied themſelves with great eagerneſs to the conſtruction of inſtruments, capable of producing electrical phenomena, in a manner at once commodious and ſtriking. At the ſame time, they examined different electrical ſubſtances from which they obtained different reſults, equally curious and intereſting.

It was very natural to reaſon upon, and attempt to explain theſe phenomena, and very ſoon different hypotheſes were advanced; of which, thoſe of the *unctuous effluvia*, of the *vitreous* and *reſinous*, or oppoſite electricities, and of the *poſitive* and *negative* electricity, were the principal. Franklin, who may be conſidered as the author of the laſt-mentioned theory, though it was originally propoſed by Watſon, explained the electrical phenomena, by its application, in a manner much more ſatisfactory than any others had done. This great philoſopher carried his doctrine to a very conſiderable extent, and his followers, as well as thoſe who diſſented from him in opinion, inſtituted a great variety of experiments and enquiries. By means of theſe, this branch of phyſics became enriched with new facts, from which new principles and doctrines might be deduced.—Volta effected this—He eſtabliſhed the laws of the atmoſpheric electricity, the exiſtence of which was already known to Franklin and others, and took his idea of the electrophorus from what Beccaria had written upon vindictive electricity.—*Elettricita vindice.*

This was a gigantic ſtep towards improvement in the ſcience of electricity, which was ſoon followed by others, which have enabled us, by nice and accurate meaſures, to calculate the ſmalleſt powers of this principle, and to diſcover thoſe proceſſes, which nature appears to have concealed with ſolicitude from the inquiſitive reſearches of human curioſity.

The electrical fluid being conſidered as the

the ſoul of the univerſe, phyſiologiſts imagined they could exhibit it as a principal agent in the animal economy; it was accordingly ſubſtituted by them for the *animal ſpirits*, concerning which, till this period, nothing ſatisfactory had been advanced. The velocity of the electrical fluid correſponded with that of the nervous fluid, and this analogy afforded the greateſt weight to the new theory.

The hiſtory of the torpedo, gymnotus, and other electrical fiſhes, furniſhed facts which might have completed the triumph of this doctrine, had it not unfortunately remained in obſcurity. Some, 'tis true, endeavoured to ſupport and extend it, but their efforts did not make many converts to their opinions. Phyſiologiſts either denied the exiſtence of *animal ſpirits* altogether, or, if they admitted them, inſiſted upon aſcribing them to ſome other ſource than that of electricity. At preſent, it ſhould ſeem that this queſtion is decided. Profeſſor Galvani, of Bologna, has diſcovered in animals an electricity which is peculiar to them, and which performs the office of the nervous fluid. For this beautiful diſcovery, he was indebted to a fortunate accident. Whilſt diſſecting a frog in a room where ſome of his friends were amuſing themſelves with an electrical machine, one of them drew a ſpark from the conductor, at the ſame time that the profeſſor touched one of the nerves of the animal. In an inſtant, the whole body of the frog was ſhook by a violent convulſion. The profeſſor was aſtoniſhed at the phenomenon, and believed it owing to his having wounded the nerve; to aſſure himſelf whether this was really the caſe, he pricked it with the point of his knife, without any motion of the body being produced; he now touched the nerve with the inſtrument as

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at firſt, and ordered a ſpark to be taken from the machine, on which the contractions were renewed. The experiment was repeated a third time, but the animal remained motionleſs; however, on perceiving he held his ſcalpel by the handle, which was of ivory (a bad conductor), he changed it for a metallic one, and re-excited the movements, which he conſtantly failed of doing whilſt uſing an electric ſubſtance.

After having made a great number of experiments with the electrical machine, he reſolved to proſecute the ſubject with atmoſpheric electricity. To this end, he raiſed a conductor upon the roof of his houſe, from which he brought an iron wire into his room, and to this attached metal conductors, connected with the nerves of the animals deſtined to be the ſubjects of his experiments, and to their legs he tied wires, which reached the floor. Confiderable movements were obferved in the animals, whether of cold or warm blood, whenever it lightened. These preceded thunder, and correfponded with its intenfity and repetition, and even when it did not lighten, the movements took place when any ftormy cloud paffed over the apparatus.

Profeffor Galvani one day fufpended fome frogs, perhaps with fimilar views, on metal hooks, fixed in the fpine of the back, upon the iron railing of his garden; feveral times he remarked that thefe animals contracted, and appeared to receive fhocks; at firft he conceived the movements were owing to changes in the atmofphere, but a more fcrupulous examination undeceived him. Having placed a prepared frog upon an iron plate in his room, and happening with his diffecting forceps to prefs it againft the plate, he obferved the movements to take place.

This experiment ſucceeded with all metallic bodies, but more particularly well with ſilver; non-conducting ſubſtances were not proper for it. From this period, our author began to ſuſpect the animal poſſeſſed an electricity of its own, and in this ſuſpicion he was farther confirmed by the following circumſtance:—he held a prepared frog by a hook with one hand, ſo as to let its feet reſt upon the bottom of a ſmall ſilver cup, which he happened unintentionally to ſtrike with the other; at the inſtant, the body of the animal fell into violent convulſions. If one perſon held the prepared frog, and another touched the cup, no movements were excited. The Profeſſor being now aware of the neceſſity of a communication, undertook a ſeries of experiments for the farther inveſtigation of this ſubject. He firſt placed a prepared frog upon a non-conducting ſurface, and brought one end of a conductor in contact with the hook which ſecured the animal, and with the other touched its feet, on which the contractions took place. When the conducting arch was interrupted by a non-conducting ſubſtance, the frog remained motionleſs.

Having made many experiments of the ſame nature, Profeſſor Galvani publiſhed his doctrine, which we ſhall hereafter examine.

I was not a little ſtruck with this diſcovery; and as it appeared to me of conſiderable importance, immediately prepared to pay it the attention it deſerved. My firſt employment was to repeat the experiments of Profeſſor Galvani, after which I ſet on foot others of a different nature. Theſe experiments, with their application to the animal economy, will conſtitute the ſubject of this work.

Experi-

Experiment I.

My firſt experiment was made on a frog, in the following manner: I opened the abdomen in order to lay bare the ſpine of the back, and diſcover the crural nerves which iſſue from it; a few lines above this point, I cut the animal in two, and by paſſing my ſciſſars immediately under the origin of theſe nerves, removed the remaining portion of the vertebræ column, ſo as only to leave the vertebral which united the bundle of nerves. Having enveloped this portion of the vertebræ with a piece of ſheet lead, with one end of a metal conductor I touched the coated part, and with the other the ſurface of the thighs, which had been previouſly ſtripped of the ſkin. The movements were violent, and continued for a long time. By coating the nerves diſtributed on the fore legs, I procured ſome commotions,

motions, but which were by no means ſo ſtrong as in the poſterior extremities. By my experiments, I likewiſe found that ſilver was the beſt conductor.

Experiment II.

The movements produced in the legs of lizards, prepared in the above-mentioned manner, were feeble and tranſitory; it muſt be obſerved, however, that I am now ſpeaking of ſmall ones, for larger ones were violently agitated, and preſerved their vitality much longer; when the medulla ſpinalis of the tail was coated, the movements were ſtronger, and of longer duration.

Experiment III.

An eel was cut acroſs into two equal portions, and the medulla ſpinalis of the divided ends prepared in the uſual manner. On exciting the tail-piece, it moved as if it had been in its own ele-

ment,

ment, and on continuing to touch it, rolled over from ſide to ſide; it however became gradually exhauſted, and died in about three quarters of an hour. The vital principle did not ſhew itſelf in the ſame degree in the head-piece; that is to ſay, the movements were not ſo ſtrong, but they laſted about five minutes longer.

Experiment IV.

The wing of a lark, prepared as before, experienced ſlight tremblings for three minutes, but the legs did not move in the leaſt.

Experiment V.

The fore legs of a new-born kitten were taken off, and on the nerves being coated, they moved for ſeven or eight minutes. No motion was obſerved in the muſcles of the larynx, or tongue, though they were put in a ſtate of excitation.

Experiment VI.

A dog, which had been killed by a blow on the head, on being prepared, exhibited very conſiderable ſhocks and movements, particularly in one of his fore-paws, which bent five or ſix times, as if in the action of walking. The hyo and genio gloſſi muſcles trembled ſeveral times; thoſe of the larynx, the nerves of which had been likewiſe coated, experienced ſlight tremblings; the heart did not beat, although M. Maſini coated the eighth pair of nerves, whilſt that viſcus was reeking and hot. All the appearances ceaſed within an hour.

In four dogs I endeavoured to excite movements in the heart, but my endeavours were fruitleſs. The diaphragm afforded very conſiderable ones on arming the phrenic nerve, and eſtabliſhing a communication between it and the coating.

Experiment VII.

My friend, Mr. Moorcroft, Veterinarian Surgeon, invited me to attempt the experiment upon a horſe.

The heart remained motionleſs, although it was prepared as ſoon as the animal was killed, which was effected almoſt inſtantaneouſly by the diviſion of the medulla ſpinalis of the neck. We prepared the phrenic, common intercoſtal, and eighth pair of nerves, without producing any appearance. The brachial plexus was laid bare, and wrapped in tin foil without being divided; on touching the coating and the neighbouring muſcles with a ſilver ſpoon, the leg was not convulſed, but oſcillations and tremblings took place in the muſcles of the ſhoulder. On eſtabliſhing a communication between the coating and the nerve, the movements were exceſſively ſtrong. The leg,

ſhoulder,

ſhoulder, thorax, abdominal muſcles, panniculus, carnoſus, and ſkin of that ſide were violently agitated. A ſhilling produced as much excitement as the ſpoon; and a guinea nearly as much as either. The experiment laſted nearly an hour.

Experiment VIII.

A mouſe was ſcarcely dead when I opened it, and having armed the fore legs, touched both the coating and the muſcles. In conſequence of the account given by M. Cotunnio, I expected to ſee violent convulſions, but not one however took place. In this experiment, I obſerved a ſtill more ſingular circumſtance; the hair of the ſkin, when I brought the conductor near to the animal, ſtood an end, and moved as if agitated by a gentle current of air, which muſt certainly be conſidered as the effect of the *aura electrica*. But

perhaps

perhaps it might have been occaſioned by ſome unobſerved friction upon the ſurface.

Experiment VIII.

Another mouſe faſtened alive upon a table by means of pins, was ſtrongly convulſed, not only in the part which I had prepared, but all over the body, and particularly in the tail. It ſoon died, but the movements continued for three quarters of an hour.

Experiment IX.

A rat did not exhibit any movement whatever, nor could I perceive any alteration in its hair.

Experiment X.

I prepared all the feet of a tortoiſe; they all moved forcibly, but ſlowly, ſimilar to the natural motion of that animal; this motion continued for two hours, but at the laſt, I was obliged to make intervals of a few minutes, to allow the limbs to recover a little, in order to procure fresh signs of electricity.

Experiment XI.

I laid bare the nerves of a fowl's wing, without killing it; my sciffars paffed underneath them ferved as a coating, and a French crown piece as a conductor; the movements were very fmart. During thefe difcharges, the animal appeared perfectly tranquil. For fome moments, notwithftanding my exciter, the wing remained motionlefs; I then had recourfe to a leaden coating and a copper conductor: this change did not anfwer my expectation, for the wing ftill continued without motion. To afcertain whether this was owing to a ftate of infenfibility, or want of energy, or to the mufcular fibres being fatigued, I pricked and irritated the nerves, the animal fcreamed violently,

and

and ſhook its wing four or five times with conſiderable force; having irritated them in this way, I again tried a ſilver conductor, but without effect. In the mean time, I armed ſome other branches which were diſtributed upon the wing, and from them obtained the movements by the common means. Theſe obſtacles appeared to me ſo much the more ſingular, inaſmuch as the animal occaſionally moved its wing, although it would not obey the power of the conductor, and yet again movements might be induced by means of mechanical ſtimuli.

The ſtate of repoſe and inertneſs which I have been ſpeaking of, is not continual; for my conductor produced effects, ſometimes in a quarter and ſometimes in half an hour.

The experiment with the wing ſucceeds, although it is detached from the body of the animal. It is neceſſary to warn againſt the nerve being divided near the muſcle, as then either the movements do not take place at all, or if they do, are ſcarcely perceptible, and only for a moment.

Sometimes I have wounded the nerve of the wing, whilſt it remained attached to the body to ſee what would happen, and it has conſtantly proved inimical to the experiment. Sometimes the wing has remained entirely motionleſs ; in this caſe, the nerve loſes the power of conducting the electric fluid. But why does it loſe it? To anſwer this queſtion, one ſhould be acquainted with the ultimate ſtructure of the nerves, their ſprings and mechaniſm ; but we are far from being arrived at this point, and perhaps ſhall never attain it.

In frogs, the caſe is widely different. Whether the nerves are left attached to the ſpine, or ſeparated from it, the motions conſtantly occur ; and one cannot perceive any difference, either with regard to their intenſeneſs or duration. On the contrary it appears, that the limb of a frog, the nerve of which has not any communication with the reſt of the ſyſtem, preſerves its vitality longer than a limb under oppoſite circumſtances.

EXPERIMENT XII.

I took a live frog, opened its belly to diſcover the crural nerves, one of which I divided, and left the other untouched. I then ſtripped the ſkin off the muſcles of both thighs, armed each nerve, and with the exciter produced a diſcharge, ſometimes in the one, and ſometimes in the other limb. The member, the nerve of which was divided, gave marks of life much longer than the other. This effect, however, is not uniform.

By repeating this experiment ſeveral times, I had an opportunity of remarking ſome phenomena, which merit attention.

On touching the uncut nerve with a conductor at the point where it was coated, and at the ſame time the naked part of it, or the muſcles of the correſponding leg, the ſmalleſt ſhock has not taken place, yet the animal from time to time moved this leg, although it would not obey the force I exerciſed upon it; at other times, the frog did not ſhew any ſpontaneous movement, whilſt I could excite violent ones in it. Theſe accidents are not common. It ſometimes, though rarely happens, that on approaching the exciter to the coating and the leg at the ſame time, the limb remains motionleſs, and the animal by its difficult reſpiration and a plaintive cry, plainly ſhews that he experiences pain.

Of all the animals upon which I have made experiments, frogs preſerve their vitality the longeſt. Formerly I attempted to eſtabliſh a meaſure for this principle, but my calculations were imperfect, for the reaſon I am about to adduce.

I was in the habit of preparing frogs, and leaving them in this ſituation for ſeven, nine, and ſometimes twelve hours. When I wiſhed to examine them, I armed the nerves, and eſtabliſhed afterwards the ordinary communication with a metallic arch; ſometimes they ſhewed ſlight ſhocks, at others, none at all. To know if any portion of life ſtill exiſted, it was neceſſary to ſhift the coating lower down, that is nearer the extremity of the limb, and it is only within a ſhort time that I have been aware of the importance of this precaution, and ſince have made ſeveral experiments to aſſure myſelf of the reality of the fact. The reſults have been different, and this was to be expected from the difference in the conſtitution of the animals, and in the ſtate of the atmoſphere. I have ſeen three remarkable inſtances of this occurrence.

Experiment XIII.

A prepared frog was fatigued by excitation, from ten at night till one in the morning; at a quarter paſt nine of the ſame morning, I found it ſhewed ſigns of life.

Experiment XIV.

A ſecond frog was expoſed to the action of an inconſiderable portion of artificial electricity for an hour. Twenty hours afterwards, it appeared that the vital principle was not entirely extinguiſhed.

Experiment XV.

A third, which had been excited for half an hour, lived a whole day after. In the laſt, when I ſeparated the muſcles, at the expiration of two hours, in order to lay bare and coat the nerve, the leg contracted with an extraordinary degree of force.

force. It is proper to obſerve, that I could not obtain from any of theſe three frogs the ſmalleſt mark of electricity, when I touched the coating of the ſpine, or any part of the nerves, anterior to their inſertion into the muſcles; but by putting the coating lower down, the movements took place in a manner ſufficiently evident. I have not met with any that have retained this power longer than twenty-four hours, nor any in which it has diſappeared in leſs than four. If prepared frogs be left in water, their muſcles become impregnated with it, and loſe their irritability in leſs than four hours.

Animals with warm blood are not proper for this kind of experiment. I have often left the wings of fowls prepared for a quarter of an hour, and they have only given a few ſlight tremblings, and ſometimes none at all. If, however, their electricity be excited immediately after they are prepared,

pared, the movements laſt an hour, and ſometimes longer. Does this excitement ſupport life, inſtead of deſtroying it?

Experiment XVI.

In a dog, whilſt one of the fore legs gave ſtrong ſhocks, the other, hitherto untouched, was prepared, but did not exhibit the leaſt motion.

Experiment XVII.

I prepared a fore leg of two rabbits, and twenty minutes after examined the others, but it was not poſſible to procure the ordinary appearances. The firſt prepared legs, although they had been much excited, gave, notwithſtanding, very ſenſible ſhocks.

In fowls, dogs, and rabbits, I have changed the ſituation of the coating as with the frogs, and, in ſome inſtances, have applied another coating to the muſcles,

but

but without ſucceſs. A ſecond coating has been found to be a very proper means of exciting the vitality of the animal parts, when it is languid and about to diſappear.

It is obvious, that this double coating is a means equally well ſuited to increaſe the muſcular power in prepared animals, ſtill in poſſeſſion of the greateſt part of their vital principle. As I was one day repeat-ing this experiment upon frogs, after having taken off, and re-applied ſeveral times, the coating of the muſcles, to ob-ſerve the difference, I was ſtruck with the idea of removing the coating from the nerves, and leaving that of the muſcles, which happened to be a piece of ſilver coin; I then paſſed one end of a metal conductor under the nerves, and with the other touched the piece of money. As Profeſſor Galvani had aſſerted, that in this caſe, the movements did not occur, or at moſt were feeble and infrequent; I was aſtoniſhed to ſee them appear nearly as ſtrong as in the ordinary experiment. This ſucceſs led me on to new trials, of which, perhaps, I ſhould never elſe have entertained an idea.

The firſt was, to prove whether I could not produce the electrical appearances by eſtabliſhing a communication between muſcle and muſcle, as well as by that betwixt muſcle and nerve.

Experiment XVIII.

With this view I wrapped the foot of a frog in ſheet lead, and laid a piece of ſilver coin under the thigh of the ſame ſide. Upon theſe two coatings I brought the extremities of my conductor; at the moment of contact, the ancle of the foot in particular, and the claws bent and ſhook with vivacity. I have repeated this experiment ſeveral times, and the reſult has been uniformly the ſame.

Experiment XIX.

Having found one day among my frogs, one which appeared particularly lively, with one hand I laid hold of the coated leg, and with a ſhilling touched the coating and muſcular parts; by this means, I likewiſe obtained the movements, and have repeated this a thouſand times ſince.

As ſoon as I was aware that I could in a certain degree render myſelf maſter of this principle, and cauſe it to circulate by means of the coatings, I conceived an idea of trying if it were poſſible to attain the ſame end, without depriving the muſcles of their teguments.

Experiment XX.

With this view, I faſtened a frog upon a table by means of pins. A piece of ſilver paſſed under the thigh, ſerved as a coating; ſliding my ſciſſars very gently

over

over the ſurface of the thigh, I brought them to touch the coating, on which contractions took place in the whole leg; frequently the convulſions were communicated to the other leg, and ſometimes to the whole body. The ſhocks were ſtill ſtronger, when a piece of lead was paſſed round the abdomen of the animal, and the ſilver money placed under the pelvis.

Theſe experiments generally fail of ſucceſs when the frogs have been kept for a conſiderable time, and when attempted in a room where there are many perſons. The ſeaſon, along with the ſtate of the atmoſphere, has likewiſe an influence upon them,

What takes place in frogs, one would preſume ſhould occur equally in other animals, and in man himſelf.—I have accordingly more than once made myſelf the ſubject of experiment, but have never ſucceeded.

Infants

Infants and delicate women of great mobility and ſenſibility, with perſons ſubject to hyſterical and nervous affections, would perhaps be very proper ſubjects for this ſpecies of reſearch.

The movements excited in frogs by the methods juſt mentioned, are not in proportion to the vigour of the animal. Some of them, although very lively, have not always been proper for experiment. The will of the animal has perhaps the power of preventing the paſſage of the electrical fluid from one part to another, or of deſtroying its effects.

The animal frequently ſuffers theſe diſcharges and movements, without appearing to be affected by them; ſometimes one has ſcarcely touched the coating with the conductor, before it becomes as it were ſtupified, and when ſet at liberty, moves forwards very ſlowly, or remains motionleſs, although puſhed and irritated.

This

This derives from a particular ſecret conſtitution, or from what, to make uſe of medical language, is termed idioſyncraſy.

I have not been able to aſcertain that fowls feel the ſhocks excited in their wings, by the means above-mentioned. Can this be attributed to the ſmall ſhare of ſenſibility poſſeſſed by this ſpecies of animal? Very often I have lacerated the fleſh of their thighs, without their ſhewing any ſigns of pain, and they have begun to feed very quietly as ſoon as turned out.

Notwithſtanding, in fowls, the muſcular force is very great, Is the irritability, or in other words, the vis inſita of the muſcular fibre, in an inverſe ratio to its ſenſibility?

After the communication between muſcle and muſcle, it was natural enough to imagine that between nerve and nerve.

Profeſſor Galvani had already obſerved, that if one touched the coating of the ſpine, and the nerves at their going out of it, the movements occurred. I had likewiſe made the ſame remark, but had not dwelt upon it.

The reſult of my experiments upon the muſcles, recalled, however, my attention to this object.

Having proved by a great number of experiments, that the movements do not fail to take place when one touches the coating of the ſpine, or the ſpine itſelf, at the part where it is uncoated, or the origin of the nerves; I wiſhed to know, if by laying bare the nerves along the thigh, and coating them at different heights, the ſame effect could not be produced. This I attempted, and it ſucceeded to my wiſh.

But in order to diſcover better the influence of the nerves in the movements, it appeared adviſable to me to begin the trial with the ſpine of the back. For this purpoſe I ſelected a very vigorous frog, and when prepared, laid it upon an inſulated ſurface. The touch of the coating and the ſpine excited in it very violent convulſions; twenty minutes after the animal ceaſed to move, notwithſtanding the application of the exciter. I let it reſt for a few minutes, but this was not of any uſe; inſtead therefore of touching the ſpine, I made the communication immediately below the coating with the nerves, of which I only touched a ſmall ſurface; the movements continued at intervals, for ſomewhat more than half an hour.

When they ceaſed to manifeſt themſelves, it was neceſſary to place the conductor lower down, when freſh ones were again excited; immobility ſoon ſucceeded the ſhocks, which I re-produced by carrying ſtill lower one of the extremities of the conductor. When obliged to lay bare the nerves along the thigh, I was like-

wiſe

wiſe neceſſitated to move the coating lower, or elſe the circle of electricity was not ſufficient, or did not take place. This experiment took me up five hours. At other times, the vitality was deſtroyed in four hours, and ſometimes again in a ſtill ſhorter time.

It is very evident from theſe facts, that the nerves poſſeſs at every part a vital principle, which periſhes in proportion to the repetition and intenſity of the ſhocks, which may be conſidered as ſo many electrical diſcharges.

This principle alſo gradually periſhes of itſelf, and it is always from the higheſt part that it firſt begins to diſappear. One might ſuſpect, that the nerve dries during the experiment, and that to this cauſe its inertneſs, or want of power to conduct the electricity might be owing. This, however, is not the caſe. It cannot be denied, that a nerve, which has long been ſubjected to experiment becomes flat, and loſes its white colour, which, perhaps, may be owing to part of it being carried off along with the electric fluid, but it ſtill does not ceaſe to conduct; for if we touch the coated nerve and the muſcles with the metal conductor, the movements are excited, notwithſtanding the alteration in its conſtitution. Beſides, in the limbs of frogs, which have been either thrown into water, or expoſed to the action of the atmoſphere for a certain time, I have found the nerves apparently in their natural ſtate. Yet, notwithſtanding this appearance, it is by no means a matter of indifference for the experiment, what part of the nerve is prepared and communicated with.

Experiment XXI.

I had ſeveral frogs in a veſſel filled with water, I mean, always prepared ones. I

took

took one: its nerves appeared in a good ſtate. I coated the ſpine, and eſtabliſhed a communication betwixt it and them. The movements did not take place. I laid bare the nerves of the upper part of the thigh, and coated them. My conductor now produced oſcillations and tremblings. A few moments afterwards, I coated the nerves of the other leg at the ſame height —I eſſayed it—the limb did not move—I paſſed the coating ſome lines lower down—it ſtill remained motionleſs—I followed the nerve, but the movements only took place when the coating was brought to the loweſt part of the thigh.

A great number of frogs have been ſacrificed to my experiments; and I have conſtantly obſerved, that when there was any reſiduum of vitality, by lowering the coating, and trying it at the diſtance of every line, that is to ſay, making the communication between the coating and the nerve, I reached the point which anſwered to the experiment.

From this it appears, that this condition of the nerves, by which they poſſeſs the power of exciting muſcular motion—this life, if I may be allowed the expreſſion, continues longer in their extremities than at their origin.

But is not their origin that which I call their extremities? Let us leave this to be decided by future phyſiologiſts.

Since electrical diſcharges, and the movements of which we have been ſpeaking, take place by means of two metals of a different nature, one might be tempted to imagine, that it is the metal itſelf which affords the electricity.

This remark, or rather objection, has been made by many. In anſwer to it, I firſt obſerved, that at two or three different times I had produced ſhocks by being myſelf the conductor. I likewiſe aſſerted, that

that it had been obſerved, that ſometimes one metal was ſufficient for the experiment. At preſent, I can give numerous proofs of this laſt circumſtance. The facts I am about to relate, are by no means uncommon; on the contrary, one may witneſs them in every frog, provided the experiment be made immediately after the animal is killed. The following is the method which I have obſerved.

Having prepared one of theſe animals, I hold it ſuſpended by the foot with one hand, and with the other bring the ſciſſars gently to the ſpine, or to the nerves. On this contact, the legs ſhake and fly from the inſtrument, and ſometimes theſe movements are effected with conſiderable force and vivacity. I find ſmall frogs afford more movements than large ones, and particularly if the ſpine be touched inſtead of the nerves; in ſome of theſe the contact did not produce any effect. When I hold the frogs ſuſpended by a filken thread, or if the fciffars are in-fulated, no movements take place.

The fmall frogs have likewife prefented me with a very fingular phenomenon. As they enjoy much vitality, their legs are contracted, or drawn upwards with as much force as if they were ftill connected with the body. If, during this ftate of contraction, the fpine or the nerves be touched with the fciffars, the legs almoft conftantly become relaxed, and fall down.

I have likewife feen in two frogs the movements occur at the diftance of half an inch from the fciffars, and which ceafed at the moment I infulated the fciffars.

A ftill better manner of exciting the electricity is, to hold the feet of the frogs between the rings of the fciffars, and incline them in fuch a way as to permit the medulla fpinales to fall gently upon them.

Inftead of two conductors, as in the firft experiment, there is here only one, and

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on that account it is, that the effects are more apparent.

The difcharges excited by thefe two means foon ceafe, and then the coating becomes neceffary.

It conftantly happens in this new circumftance, when the coating is of the fame fort of metal with the conductor, that the electricity of the animal does not make its appearance.

Profeffor Galvani afferts, that he has fometimes obtained fome very weak marks of it, although the plate which the coating touched, the coating itfelf, and the conductor were all of iron. There is, however, reafon to believe, that all the iron was not of the fame quality, or that this circumftance happened at the very beginning of the experiment. My attempts have been made not only with iron, but with other metals, as copper, lead, tin, gold, and zinc. To excite the move-

ments,

ments, one ſhould at leaſt have the conductor of a different metal; I ſay at leaſt, for that is not always ſufficient. Amongſt the different metals, there are ſome more proper for the experiment than others. There are others, likewiſe, that are very improper, when the animal is feeble, and its vitality about to diſappear.

That I may convey a more preciſe idea of this matter, I ſhall make a recital of the ſet of experiments I had the honour to make before the Commiſſaries of the Academy of Sciences at Paris, or rather of thoſe we made together.

Experiment XXII.

The crural nerves of a frog were placed upon a piece of gold, and the thighs put in contact with a piece of ſilver. A copper conductor produced ſlight movements. Two coatings of ſilver did the ſame, by means of the ſame conductor.

When a coating of lead, tin, or copper, was ſubſtituted for that of ſilver, which enveloped the nerves, or that of the muſcles, the movements were very violent. One might obſerve the following gradation in the action of metals.—Lead produced the moſt violent movements, next tin, and after that copper. In proportion as the frog loſt its vitality, the metals likewiſe loſt the faculty of determining the paſſage of the electrical fluid in the animal. Lead, tin, and zinc retained this property the longeſt.

A piece of glazier's lead employed for both coatings, with a conductor of the ſame metal, produced nothing. But when lead of different qualities was made uſe of, as, for inſtance, glaziers lead and that of aſſay, an exciter of one of theſe metals produced remarkable effects. When theſe two different leads ceaſed to produce any appearance of contraction, by ſubſtituting for one of the coatings, another of ſilver,

gold,

gold, bifmuth, antimony, or zinc, very fharp movements were induced, which put the animal in a ftate to experience flight convulfions, when the two different leads were re-applied. When the electrical power of the animal was ftill nearer being exhaufted, different metals excited convulfions, but coatings of glaziers and affay lead were incapable of commanding them.

The electrical action has been found to difappear in the following order; glaziers lead being conftantly ufed for one of the coatings.

1ft, Affay lead has ceafed to induce movement.

2d, Tin,

3d Antimony,

4th, Zinc,

5th, Copper,

6th, Gold,

7th, Silver.

A fimi-

A ſimilar phenomenon occured in a frog which had been ſubjected to experiment for an hour. Zinc ſerved as a coating for the nerves. On placing a leaden exciter upon this and the coating of the muſcles, we were not aware of the ſlighteſt movement; but as ſoon as I took off the exciter, and deſtroyed the communication, the movements took place. This experiment was repeated by many of the Commiſſaries, and found to be conſtant.

The nerves of the fore legs of a rabbit, detached from the body, were enveloped with a piece of tin foil. By placing afterwards a piece of ſilver as exciter upon the neighbouring muſcles, and touching the coating with it at the ſame time, the animal experienced violent convulſions.

When one of the coatings was of glaziers lead, and the other of the ſame, or of aſſay lead, there were not any movements.

Nor

Nor did any occur with

Lead and iron,
Lead and gold,
Lead and copper,
Lead and zinc, or
Lead and antimony;

but with lead and biſmuth ſlight movements were excited.

Since that period, I have had an opportunity of repeating ſeveral of theſe experiments, and particularly that where the coatings were of gold and ſilver, and the conductor of a different metal. Very frequently it has diſappointed me. If the coating is of ſilver, and the conductor of gold, there is very ſeldom any motion excited. I have, 'tis true, procured ſome feeble tranſitory movements two or three times in frogs, and twice in fowls, in a great number of experiments, but never in rabbits or cats.

With regard to the other metals, there

are

are ſo many anomalies as to render it impoſſible to eſtabliſh a ſcale of their affinities. In general, it appears, that lead makes the beſt coating, and ſilver the beſt conductor. It ſhews, likewiſe, that the order of ſucceſſion of the other metals, is in a certain degree proportioned to their analogy with the two firſt.

Metals when covered entirely with ſealing-wax, are not good conductors when the animal begins to be weak.

I have obtained only ſome occaſional feeble ſhocks with them, whilſt with another conductor of the ſame metal, diameter and length, I have had frequent and very obvious ones.

In proportion as the principle of life diſappears, they become ſtill worſe conductors, and are incapable of being ſo at all before it is entirely extinct. The other conductor ſerved me as a criterion and meaſure, and, in fact, ſo did theſe when deprived of their non-conducting covering.

Water and other fluids, which give paſ-ſage to the electrical fluid, are likewiſe capable of conducting animal electricity. Profeſſor Galvani made the experiment with water. On repeating it after him, I remarked ſome circumſtances which I do not find taken notice of in the account of the experiments of that author. (V. Bibliotheque de Turin, An. 1792, Mars. V. 1.)

I obſerved, that if the Operator himſelf formed the circle, and carried the metal he held with the fingers of one hand, to the muſcles immerſed in a veſſel of water, and communicated by means of the other hand with the nerves which overhang the edge of the veſſel, in this caſe, the animal remains motionleſs; this does not happen if the experiment be made in a manner directly contrary.

I have likewiſe obſerved, that if the nerves are placed in another glaſs of water near the firſt, it is always neceſſary in

order

order to produce the electrical phenomena, that the metal ſhould be in contact with the coating.

If the conductor conſiſts only of one metal, it is ſufficient that its two extremities touch the water in the two glaſſes. But in a little time, its conducting faculty becomes ſo much weakened, as not to give any apparent mark of the circulation of the electrical fluid. In ſome frogs even it is found uſeleſs, although employed immediately after their preparation.

But if two different metals be ſubſtituted as conductor, the movements will take place; the power of this compound conductor, has, however, its limits; the electricity not paſſing in a ſufficient quantity to put the muſcular fibres in action.

To produce this effect, it ſhould be carried to the nerves by better conductors than water. Metals are the beſt, and it is by means of them we diſcover, that this active principle, of which the animals appeared exhauſted, ſtill retains conſiderable force, and does not periſh in ſo ſhort a time.

Imagining that heat would increaſe the conducting power of water, I heated a ſmall quantity, in which I immerſed a prepared animal. The heat was not conſiderable, but yet I perceived it acted upon the muſcular parts by the diſtenſion of the legs. I now reſolved to eſtabliſh a communication between the legs of the frog and the water, by means of a coating, one end of which was plunged in it. This frog having been much injured, gave, when excited, only ſome ſlight ſhocks. A ſecond was ſubſtituted, and the effects ſucceeded to my expectation. As the water began to cool, I changed it, but that I poured into the veſſel was much hotter than the firſt. On forming the

commu-

communication, I could ſcarcely perceive the ſigns I looked for; but in proportion as the heat diminiſhed, they became more manifeſt.

Not having a thermometer along with me, nor a ſufficient number of frogs to proſecute my reſearches ſo as to purſue and determine all theſe gradations, I bethought myſelf of an experiment, for which I could obtain, as it were, a fixed point; this point was that of boiling water. When in this ſtate, I plunged in it one end of my conductor, and with the other touched the coating of the nerves of the frog, the legs of which communicated with a wire, which was likewiſe in contact with the water, the electricity of the animal did not ſhew itſelf.

Some have employed ſpirit of wine inſtead of water, and becauſe the animal, whilſt immerſed in this fluid, did not move, have concluded that ſpirit of wine is not a conductor.

Spirit of wine, however, conducts, but not as well as water. To aſcertain this property, the animal ſhould not be expoſed to the action of the ſpirit during its examination.

Heat increaſes the conducting power of ſpirit of wine as well as of water, but this it does only in a certain degree. Exceſs of heat deſtroys it altogether.

I have made the ſame obſervation with regard to cold. When water has been at the freezing point, it has never afforded paſſage to the electricity of my frogs. Thoſe who are acquainted with the original and beautiful experiment of the celebrated Achart, will not be aſtoniſhed at this circumſtance.

Knowing that heat changes non-conducting into conducting ſubſtances, I tried it upon glaſs and ſealing-wax, and in both it ſucceeded. To produce this effect, there is a neceſſity for two coatings; one is paſſed under the thighs, and the other round the nerves. An interval ſhould be left between the two coatings, which ſhould be occupied by a piece of glaſs heated till it becomes red; the inſtant the glaſs completes the circle, one may perceive a momentary ſhock. With regard to the ſealing-wax, the apparatus ſhould be placed on the edge of a table, ſo that with a candle, the wax being placed in contact with and between the two coatings may be heated; in this manner, ſlight tremblings may be excited. It is not the flame, which, in this caſe, conducts the electricity; on the contrary, flame injures the experiment, when it makes a part of the conductor. This experiment I repeated after M. Cavallo.

It may be enquired, If flame be not a conductor? For my own part, I am inclined to think it is. But the action which it exerts upon animal electricity, is entirely deſtructive of its effects. I directed a certain quantity of electricity through flame, before it could poſſibly injure the limbs of the frog, and it did not occaſion any irritation. But the frog was extremely powerful when it had not this obſtacle to ſurmount.

May I treſpaſs upon the patience of my readers, with one word more upon conductors? Amongſt men, there are ſome individuals who are good conductors, others who are leſs ſo, and ſome again who appear to be almoſt non-conductors.

I was one day carrying on, with three of my friends, ſome experiments upon frogs. A frog was put in water, and we each by turn eſſayed its power. Two of us excited ſtrong convulſions, the third only feeble ones, and the fourth none at all. This experiment was repeated frequently with the ſame reſult. This is not the only example I could adduce of the reality of this fact, but I do not think it neceſſary to dwell any longer upon it.

SECTION

SECTION II.

BEING convinced that in order to derive every poſſible advantage from the diſcovery which is the occaſion of my writing, it was neceſſary to accumulate facts of different kinds, I undertook the following experiments.

The nerves, the great ſprings by which all the functions of the animal economy are performed, appeared to have the firſt claim to my attention.

As nerves are conductors of a fluid, the properties of which are ſimilar to that of electricity, tying them, it appeared to me, could not prevent its paſſage through them. In conſequence of this reaſoning, I tied the nerves of ſeveral frogs, but not one of them afforded the phenomena I expected. M. Fattori, a young man of information, obſerved to me, that there was a want of uniformity in this reſpect. On repeating it, I remarked that ſome frogs continued to move with vivacity, that others gave very feeble ſhocks, which did not laſt long, and others again remained altogether motionleſs.

I at firſt thought this derived from a difference in the conſtitution of the animals; but as one day I obſerved, that in the ſame frog one leg was completely motionleſs, whilſt the other was agitated by violent convulſions, I entirely gave up this opinion.

Experiment XXIII.

With a view to diſcover the real cauſe of this circumſtance, I made ligatures at different heights in ten frogs; all of them moved except two, in which the ligature was in contact with the muſcles. I now ſtretched the nerve a little, ſo that the ligature was at a very ſmall diſtance from the muſcles without touching them. On having

ing refource to the exciter, the movements took place.

Experiment XXIV.

Although the laft experiment appeared decifive, yet I could not avoid repeating it on feveral more frogs. Whenever the ligature of the nerve was made near its infertion into the mufcles, the electrical fluid did not fhow itfelf; but under oppofite circumftances it was fufficiently evident. By this means, I could at pleafure either fufpend the movements, or call them forth, by approximating or removing the ligature from the mufcles.

Experiment XXV.

I tied the crural nerve of a frog, and armed it above the ligature. The other crural nerve was detached from the fpine, and placed fo as not to touch either the tied nerve or its coating. On applying the exciter to the coating and the ifolated nerve,

nerve, both legs were affected with ſtrong convulſions.

Experiment XXVI.

Several frogs, the nerves of which had been tied, being much weakened, did not give any ſign of vitality, on eſtabliſhing a communication betwixt nerve and nerve, but on forming a communication betwixt muſcle and nerve, ſlight ſhocks were excited.

Experiment XXVII.

On touching with a metal arch the coating and the nerve itſelf above the ligature, the movements have never occurred. It is to be underſtood, I am ſtill ſpeaking of frogs.

Experiment XXVIII.

Ligatures made upon the brachial plexus of dogs, cats, and fowls, although in contact with the muſcles, have not prevented the paſſage of the native electricity.

Experi-

Experiment XXIX.

Having tied and armed the phrenic nerve of ſeveral dogs and cats, I found it poſſible to produce convulſions of the diaphragm, although the extremities of the conductor were applied above the ligature. Towards the laſt, the ligature became prejudicial to the experiment.

From all theſe facts it ſhould ſeem,

1ſt, That when the nerves are tied, the electrical fluid runs off from its direct courſe when it meets with a better conductor.

2d, That when it has no other courſe to take, it follows that of the nerves.

3d, That when weak, it either does not paſs at all, or, if it does, is not in poſſeſſion of ſufficient power to excite the irritability of the muſcular fibre.

The ligature oppoſes to artificial the ſame obſtacles it preſents to animal electricity. When I made my firſt experiment, I tied the nerve in ſuch a manner

as

as to have it in my power eaſily to remove it from, or bring it, near the muſcles.

If the ligature was at a very ſmall diſtance from the muſcles, an extremely minute portion of artificial electricity was ſufficient to put into action the leg of the animal; but if the ligature remained in contact with the muſcles, to obtain the ſame phenomena a quantity was required, which, proportioned to the other, was enormous. This obſervation I communicated to Mr. Nicholſon, whoſe zeal and abilities in philoſophical reſearches are ſo well known to the public.

Along with this gentleman I inſtituted ſeveral experiments, of which the following are the reſult.

Experiment XXXI.

We charged a ſmall Leyden phial, containing three ſquare inches of coated ſurface, with electricity, the wire of which

being

being put in contact with Bennet's electrometer, produced a divergency of half an inch in the gold leaf. We then applied it to a slip of tin foil, which was in contact with the crural nerves of a prepared frog placed upon a glass plate. The electricity, by means of this arrangement, and a similar conductor communicating from the extremity of the feet to the earth, was confined to pass through the limbs. The right nerves were tied by a ligature close to the muscles, and the left by a ligature at some distance from them. The right leg remained motionless, the other exhibited some slight commotions.

Experiment XXXII.

The phial was charged so, that at the distance of three inches from the electrometer, it caused a divergency of half an inch. The experiment being repeated as before, the left leg alone was convulsed several times.

Experiment XXXIII.

The divergency of the gold leaf, at the diſtance of two feet, being as before, the left leg only was convulſed.

Experiment XXXIV.

The phial being at a foot diſtance, and the divergency the ſame, the reſult was as above.

Mr. Nicholſon, wiſhing to have another meaſure of compariſon for the electricity employed in theſe experiments, had recourſe to the electrometers of Lane and Henley. The exploſial ſpark was firſt diſcharged when Lane's electrometer was ſet to $\frac{1}{10}$ of an inch, the intenſity being at the ſame time noted by the electrometer of Henley. The jar was then again charged to this intenſity, and applied to the prepared limbs of the frog as in the foregoing experiments. The left leg only was convulſed.

Experiment XXXV.

He then took a higher charge, the ſpark of which, aſcertained in ſame manner, would have exploded through $\frac{1}{8}$ of an inch. This portion which, threw the left leg into convulſions, did not produce any alteration in the right.

Experiment XXXVI.

As this phial contained only a ſmall quantity, it became neceſſary to ſubſtitute a larger jar; becauſe the firſt diſſipated a portion of the fluid, and by that means prevented our obtaining an exact meaſure. The coated ſurface of this ſubſtituted jar, amounted to 170 ſquare inches. It was charged with a quantity, of which the diſcharging diſtance was by Lane's electrometer $\frac{3}{100}$ of an inch. On directing this fluid through the nerve, which was tied at a diſtance from the muſcles, the correſponding leg was ſmartly agitated.

Experi-

Experiment XXXVII.

A like quantity did not occaſion the ſmalleſt oſcillation when the ligature was placed in contact with the muſcles. The reſiduum excited ſeveral ſhocks when the ligature was ſlipped back to a diſtance from the muſcles.

Experiment XXXVIII.

The diſcharging diſtance being at $\frac{5\frac{1}{2}}{100}$ the movements only took place when the ligature was removed from the muſcles.

Experiment XXXIX.

The diſcharging diſtance being at $\frac{7\frac{1}{2}}{100}$ no movement occurred when the ligature touched the thigh.

Experiment XL.

Although the ligature remained in this ſituation, a very feeble, tranſient trembling

was

was excited when the discharging distance was $\frac{11\frac{1}{2}}{100}$. With a smaller charge, no effect was produced.

Experiment XLI.

A small frog, of which one crural nerve was tied, and the other left in its natural state, was exposed to the action of a portion of electricity from the same jar, the discharging distance of which was $\frac{3}{100}$ of an inch. The leg to which the nerve without ligature was connected was strongly convulsed, whilst the other shewed no signs of motion.

Experiment XLII.

The discharging distance being $\frac{5\frac{1}{2}}{100}$, the movements occurred in both limbs, but in a much less degree in that whose nerve was tied.

Experiment XLIII.

The ſpark being at $\frac{3\frac{1}{2}}{100}$ of an inch, produced no movement in the tied ſide, but conſiderable ones in the other limb.

Experiment XLIV.

That of $\frac{4}{100}$ of an inch, gave riſe to a ſlight oſcillation in the toes of the tied limb, and reiterated ſhocks in the other leg.

Experiment XLV.

A ſpark of $\frac{3\frac{3}{4}}{100}$ of an inch being made uſe of, the leg not tied was ſhaken vigorouſly, the other only gave a ſlight oſcillation, and that weaker than in the preceding experiment.

Experiment XLVI.

A ſpark of $\frac{3\frac{7}{8}}{100}$ of an inch produced an oſcillation in the toes, which was only juſt

juſt perceptible; I mean of the tied ſide, for the other leg continued to be ſtrongly agitated. Here our experiments terminated.

On repeating this kind of experiment by myſelf, I have frequently obſerved, that the legs of which the nerves had been tied at a certain diſtance from the muſcles, did not feel the action of a certain quantity of artificial electricity, although they were violently convulſed by exciting that which was inherent and peculiar to them.

Perhaps this obſervation may ſerve to furniſh us with a criterion, by which we may be enabled to calculate the force of animal electricity. If, for example, five, ſix, ſeven, or eight degrees of artificial electricity are not ſufficient to awaken the muſcular movements, and we can produce them by the native electricity; we ſhall be warranted in concluding, that it is ſtronger than the known quantity of five, ſix, ſeven, or eight degrees of artificial electricity. Might we not by this means eſtabliſh a common meaſure? Let this be ſubmitted to the conſideration of philoſophers.

The impediment which both animal and artificial electricity experience under the circumſtances we have noticed, is owing to the approximation of the coats of the nerves. The coats of the nerves, then, are bad conductors.

There exiſts in nerves a ſubſtance which appears well adapted for conducting electricity, and this is the medullary pulp itſelf. As this pulp is of extreme delicacy, I imagined, that by making it undergo ſome alteration, ſome changes might be produced in its conducting power. This idea determined me to make the following experiments.

EXPERIMENT XLVII.

I applied opium to one of the crural nerves of a frog; it appeared that both this and the other extremity had, in ſome meaſure, ſuffered from its influence. Yet, after a certain time, both recovered their former force.

EXPERIMENT XLVIII.

Opium applied to the cut end of a nerve, did not produce any effect upon its vitality; the movements being very ſtrong, and continuing for a long time.

EXPERIMENT XLIX.

Having enveloped the crural nerve with opium, and let it remain for ten minutes, my exciter could not bring forth any movements. I armed the nerve along the thigh, and the movements occurred. The animality of this leg diſappeared three quarters of an hour before that of the other.

Experiment L.

Opium was ſuffered to remain upon the crural nerve for ten minutes, after which, mechanical ſtimuli did not induce any movement. By our proceſs, however, the leg moved, though in a languid manner. Mechanical ſtiumli produced ſenſible movements in the other leg, and ſtrong ones alſo enſued from the application of the exciter. This leg lived, if I may be allowed the expreſſion, half an hour longer than the other.

Experiment LI.

Four minutes after opium had been applied to the crural nerve, the experiment ſucceeded as well as in the other which had remained untouched.

Experiment LII.

I plunged the crural nerves in a ſolution of opium, but was not aware of any difference betwixt its effects and thoſe of opium uſed in ſubſtance.

Since that time, I have made a great number of experiments with opium, both in ſubſtance and ſolution, the reſults of which I ſhall only notice, as the detail of the whole would be tireſome to the reader.

Opium has ſcarcely ever extinguiſhed the vitality immediately. Sometimes, in about five minutes, it has deprived the portion of nerve enveloped in it of its faculty of conducting electricity, and has almoſt conſtantly accelerated the death of thoſe parts on which it has been allowed to exert its influence for a longer time, as for a quarter of an hour, twenty or thirty minutes.

The ſolution has appeared to me poſſeſſed of much leſs activity than the opium in ſubſtance, and frequently has not done any injury to the parts on which it has been applied.

The life of the nerves being, as we have before obſerved, more particularly inherent in their extremities than in their origin, it became neceſſary to aſcertain what effects opium would produce upon them.

Experiment LIII.

I ſtripped the tibia of its muſcles, ſo as to have it in my power to apply to the large nerves which run along the edge of this bone, a quantity of opium. To the end that it might come into exact contact with them, I pinched it and them a little between my fingers; five minutes afterwards I made uſe of the common exciting proceſs, and to my no ſmall aſtoniſhment, was not able to procure the ſmalleſt movement in the limb.

Experiment LIV.

I repeated the experiment, and had the ſame reſult, the leg on which the opium had not been applied, preſented the ordinary phenomena.

Experiment LV.

In a third frog, I obſerved ſome feeble movements; but on re-applying the opium, the vitality ſoon diſappeared.

Experiment LVI.

Of five frogs employed ſucceſſively, four did not give any movements, but the fifth was ſhocked very forcibly. The opium had been upon the nerves from nine to ten minutes.

Experiment LVII.

The frog (it is always to be underſtood, I am ſpeaking of prepared ones) which had

had not felt the force of the opium, being again ſubmitted to its action for half an hour, was no longer in a ſtate of moving. By many experiments, I have ſince been taught, that frequently half an hour, and in ſome inſtances a longer time was requiſite for the deſtruction of vitality.

Experiment LVIII.

In addition to the above, I made alſo ligatures in the extremities of the nerves, but without any difference enſuing. Neither could I find any when I cut them. It appears, therefore, that opium does not deſtroy all at once the life of the portion of nerve with which it is in contact, but that it affects it in a ſpecific manner, and that this affection extends as far as the ſource of the reſt of the nerves, or to ſpeak with more propriety, as far as the ſpine. This is a moſt intereſting fact, and may become very uſeful in the hands of medical practitioners.

The advantages of the application of bliſters, according to the method propoſed and practiſed by the celebrated Cotunnio, are better explained by the fact under conſideration, than by the hypotheſis of the author.

Experiment LIX.

I have likewiſe enveloped with opium the tibia of live frogs, after having detached the muſcles without deranging the larger nerves. The leg did not become paralytic. On killing the animal and ſubmitting this leg to the experiment, it has experienced more violent contractions than the other, to which opium had not been employed. Ten times I have repeated this experiment, and as often the ſame circumſtance has enſued. How extremely difficult of explanation is this phenomena!

EXPERIMENT LX.

Having opened the abdomen of a live frog and expofed the crural nerves, I paffed under one of them the flat handle of my fcalpel, and applied fome opium upon it. In four minutes the animal loft the power of moving the leg. During this fpace of time, the exciter produced the ordinary effects. This experiment, which I have repeated more than forty times, has not, like many others, prefented anomalies and want of uniformity. At every repetition, the frogs have conftantly loft their power over the leg, and the exciter has produced motion.

In this cafe, it was neceffary to coat either the nerves or the mufcles.

Why opium fhould, under certain circumftances, act upon the nerves and not in others, is what I am altogether ignorant of, and perhaps the beft informed phyfiologifts are not much better inftructed on this head.

Are phyſicians acquainted why thoſe medicines, to which we have improperly given the title of ſedatives or antiſpaſmadies, ſometimes operate in ſuch a manner upon the nervous ſyſtem, as all at once to appeaſe the orgaſm and convulſions? and why at others, the orgaſm and convulſions are exaſperated by their exhibition?

We will now enquire what changes nerves experience when effected by opium. For my part, I believe they become bad conductors, and that in conſequence the electricity, whether animal or artificial, abandons the nerves and diſperſes itſelf as we have before remarked, takes place, when the ligature happens to touch the muſcles.

Let us now examine the effects of opium when applied to the muſcles.

Experiment LXI.

After having held one leg of a frog in an opium bath for ten minutes, in a

quarter

quarter of an hour I ſo fatigued it by excitement, that it appeared to loſe all marks of vitality. I then paſſed to the other, which leaped vigorouſly when touched by the conductor, and ſhewed ſigns of life for at leaſt an hour and a half afterwards.

Experiment LXII.

The muſcles of three frogs which had been immerſed in a ſolution of opium, continued their movements. An hour before, they were forced to ſwallow a ſolution of opium in warm water.

Experiment LXIII.

Having waſhed the adducter muſcles and the triceps of the thigh with the ſame ſolution, their movements, inſtead of being weakened, were evidently increaſed by it.

Experiment LXIV.

A quantity of a ſolution of opium was poured between the ſkin and thigh of two

frogs.

frogs. Notwithſtanding this they ſhewed much vigour, and I was not able to ſuppreſs their movements, although they were immerſed a ſecond, and even a third time in the ſame ſolution.

Experiment LXV.

I repeated this experiment. The ſolution remained between the ſkin and the muſcles for twelve minutes. The movements did not take place.

Experiment LXVI.

In a third frog, the ſolution was ſuffered to remain for fourteen minutes. The exciter only produced ſome ſlight ſhocks.

Experiment LXVII.

Four frogs were all at once made the ſubjects of the ſame experiment. One was ſhocked ſlightly, the others with the greateſt ſmartneſs. The ſolution em-

ployed

ployed was the ſame, ſo that the difference in the reſult could not be attributed to the different quality of the opium, neither could it be imputed to its action, being more or leſs continued upon the parts, as I took care to adopt a common ſpace of time for all of them ; this was a quarter of an hour.

Experiment LXVIII.

Opium was introduced betwixt the fibres of the triceps of the thigh of a frog, the extremities of which were before-hand impregnated with the ſame ſolution. This frog remained motionleſs, notwithſtanding my efforts to excite it.

Experiment LXIX.

Six other frogs preſented different phenomena. Opium in them neither ſuſpended nor weakened the muſcular motions.

Experi-

Experiment LXX.

Opium applied to iſolated muſcles for one time only in twenty trials, extinguiſhed vitality inſtantaneouſly.

Experiment LXXI.

The muſcles of live frogs have become inſenſible to mechanical ſtimuli, after opium has been applied to them or their nerves for ſome time. They have, notwithſtanding, obeyed the power of the conductor as often as I have expoſed them to its influence.

There is reaſon to preſume, that opium, even when applied to the muſcles, acts upon the nerves, and not at all upon the muſcular fibre. Conſequently the immobility of parts ariſing from this cauſe, may be conſidered as the effect of an affection of the nerves, and not of the irritable fibre. We ſhall, however, hereafter have occaſion to remark, that immobility may alſo ariſe from a vitiated ſtate of the muſcular fibres, although the nerves be in a ſtate proper for conducting electricity. And we ſhall likewiſe ſee that the ceſſation of the movements frequently takes place, although both the nerves and the muſcular fibres be in their natural ſtate.

It is true, in this matter, I only hazard conjectures, but there will be found, in the account of the experiments and facts I am about to relate, certain obſervations which render theſe conjectures far from being unreaſonable.

Experiment LXXII.

Two dogs, in which the brain was deeply wounded, died in convulſions. One of them gave ſhocks, but the other remained motionleſs, notwithſtanding different coatings and the beſt known conductors

ductors were employed. The fleſh of the latter dog was of a looſe texture, and he emitted an intolerable ſtench.

Experiment LXXIII.

A cat having received a blow on the head, died in a few minutes, and afforded to the conductor only ſome very feeble ſigns of vitality. There was not any ſenſible change in the muſcles.

Experiment LXXIV.

The brain of a frog being laid bare and irritated, the animal ſoon died convulſed. To aſcertain if its vitality was impaired, I employed the uſual means, without being aware of any difference.

Experiment LXXV.

Of ſeveral frogs, the brain of which had been ſlightly injured, ſome became convulſed, others were rendered paralytic, and others again were exempt from both thefe affections.

Experiment LXXVI.

The brain was wounded in a very confiderable number of frogs. Some of them died immediately, others in a few hours, and fome furvived for fome days.

It has been found, that fome frogs only experience a flow lingering death from the deftruction and laceration of this organ; all of them, however, when expofed to the teft of the exciter, exhibited their ordinary vigour, except three, in which the brain had been lacerated, and which only gave momentary fhocks. The mufcles of thefe frogs had evidently undergone an alteration.

Several frogs killed by a difcharge from a jar, prefented the fame figns with others which had not experienced this commotion.

Experiment LXXVII.

Three fowls deſtroyed by a ſhock from a jar through the brain when laid bare, preſerved ſuch a degree of vitality as rendered them proper for animal electricity.

Experiment LXXVIII.

A fowl killed by a ſtrong diſcharge did not anſwer to the trial, as the electrical fluid had, as it were, diſorganiſed the whole machine.

Experiment LXXIX.

I divided the blood-veſſels of the wing of a pigeon, which was ſcarcely dead by the bleeding, before I prepared the other wing, but it was not poſſible to excite any movements. I likewiſe repeated this upon the legs, without ſucceſs.

EXPERIMENT LXXX.

In two other pigeons I coated the nerves of the wing, having previoufly divided the blood-veffels. From time to time I excited fhocks in one of them, and which, after death, continued to afford fome tremblings. The other, which had been left to itfelf, was not capable after death, of being excited by the ordinary procefs.

EXPERIMENT LXXXI.

Whilft exciting fhocks in the wing of a fowl, juft upon the point of dying from lofs of blood, the convulfions ceafed; but when I omitted ufing the exciter, they again were renewed.

EXPERIMENT LXXXII.

A dog was made to fwallow a quantity of arfenic, which caufed his death. When fubmitted to experiment, it could not be perceived

perceived that the poiſon had weakened his electricity, or vital power.

Experiment LXXXIII.

The cicuta produced the ſame reſult in another dog. I muſt notwithſtanding obſerve, that in the caſe of theſe dogs, after aſſuring myſelf that the movements took place, I did not continue to try them for any long time, ſo that I cannot with propriety make from them any calculation or compariſon. Theſe experiments therefore may be conſidered as deficient in exactneſs.

Experiment LXXXIV.

I made a rabbit ſwallow three grains of corroſive ſublimate; it experienced the effects of the poiſon immediately, and, after two hours paſſed in pain and convulſions, died.

Having prepared a fore-leg, without detaching it from the body, the exciter could not produce any movement in it. The muſcles were tenſe and ſtiff.

Experiment LXXXV.

A ſecond rabbit, on which a like doſe had been forced, languiſhed in a painful and convulſed ſtate for four hours, after which it appeared to become tranquil. Two hours after, however, it died. My attempt to awaken the movements were uſeleſs. The muſcles of its limbs were in a ſtate of relaxation.

Experiment LXXXVI.

Five grains of ſublimate were concealed in a piece of fleſh, and given to a dog. A few minutes after he howled, appeared uneaſy, and vomited ſome bilious matter. His pains increaſed, and in nine hours he died, as it were, ſuffocated by convulſions. I could not obtain any mark of electricity. The fleſh of this animal appeared as if parboiled.

This tenſion, laxity, and maceration of the muſcles, are the moſt common accidents which ſucceed convulſions excited by the cauſes juſt mentioned. Theſe are caſes in which, although the nerves are in a fit ſtate for conducting electricity, yet the experiment does not ſucceed.

In the ſtate of tenſion, the muſcular irritability is in action. In what manner does it ſupport itſelf? Can the electricity remain included between the muſcular fibres, ſo as to maintain them in a ſtate of permanent contraction? Is this phenomenon agreeable to the received laws of electricity? We ſhall endeavour to ſolve theſe queries, when we ſpeak of muſcular motion. There is an example of the circumſtance in queſtion, in the operation called crimping.

The fleſh of fiſh, which are flayed alive, and cut into pieces, falls into contraction, and retains its contracted ſtate even after death.

The

The ſtiffneſs of bodies after acute diſeaſes is not an unuſual occurrence.

In the plague at Smyrna during the year 1784, and which I have deſcribed in a work printed at Lauſanne, the bodies of thoſe who died were in general ſo ſtiff, that one could not bend their arms or legs in any way. Theſe bodies were a long time before they became putrid.

In poiſoned animals, or ſuch as have ſuffered a violent and cruel death, moſt commonly putrefaction ſoon makes its appearance. This indeed might be expected from the very relaxed and almoſt parboiled ſtate, of the muſcles.

Sometimes animals thus circumſtanced emit ſtrong, diſagreeable effluvia, from the ſecretion of a putrid fluid, effected in the midſt of the nervous derangements; this doubtleſs contributes to the diſſolution of the ſolids, and may, on ſome occaſions, be the ſole-deſtroying power.

Let

Let us now return to our experiments.

Experiment LXXXVII.

I plunged a fowl over-head in water, and, when it appeared to be dead, excited its electricity in the wings, which had been previoufly prepared. The animal was reftored to life.

Experiment LXXXVIII.

The fame experiment was repeated upon another fowl, in the prefence of M. de la Metherie, who examined it himfelf, and was convinced that it did not fhew any figns of life. I then employed my exciter upon it, but many difcharges were required to reftore it to its natural ftate.

Experiment LXXXIX.

Encouraged by this refult, I caufed another fowl to be drowned, but what was my ſurpriſe when I ſaw, that inſtead of bringing the animal to life, my exciter could not call forth the ſmalleſt movement.

Experiment XC.

Three more fowls underwent the ſame proceſs, and one only gave ſome very ſlight, and indeed almoſt imperceptible, movements.

Experiment XCI.

Six other fowls, treated as before, were ſtrongly agitated for near an hour.

Experiment XCII.

I laid bare, and coated the brain as well as the wings of other drowned fowls, in order to bring into action parts of greater energy. Although the movement in theſe animals were ſtrong, yet they did not reſuſcitate.

Experiment XCIII.

I likewiſe deſtroyed two rabbits in the ſame way, which experienced the accidents every animal preſents in our experiments. Theſe rabbits were ſmall, and on that account the ſhocks were neither ſtrong nor of long duration.

It muſt not be ſuſpected, that ſometimes the fowls remained motionleſs on the application of the exciter for want of proper precautions. This experiment was always made with the utmoſt diſpatch, and the beſt metals.

It would be more correſpondent with reaſon to ſay, that in theſe fowls the nervous ſyſtem and principle of vitality were affected. Perhaps perſons in aſphyxia, on whom our attempts are ineffectual, are in this fatal ſituation.

The excitement of the native electricity cannot be made uſe of in perſons in the

ſtate

ſtate of aſphyxia, as it requires a barbarous operation, the conſequences of which would be always terrible and diſtreſſing. Artificial electricity, however, may be very applicable under theſe circumſtances, and there is every reaſon to believe its effects would correſpond with our expectations. The ingenious Dr. Abildgaard has by means of this agent been enabled to deprive animals of all ſenſation and motion, and afterwards to recall them to life. His experiments were made upon fowls. On paſſing a violent ſhock through their heads, they fell down apparently dead, but he re-animated them by gentle ſhocks, paſſed through the heart and lungs.

If animals apparently drowned are ſometimes reſtored to life by exciting their own electricity, they are not capable of being ſo reſuſcitated when they happen to periſh by other aſphyxiæ.

Experiment XCIV.

I placed fowls under glaſs veſſels filled ſometimes with inflammable air, and at others with vitiated, or with phlogiſticated air. The ſhocks which I obtained by the ordinary proceſs were extremely weak, and only took place at long intervals from each other, and were not able to bring them to life. In theſe animals there is not any obſervable difference in the muſcles to which one could attribute this want of energy. I have never in theſe inſtances made uſe of artificial electricity, but I believe it would prove ineffectual. This agent is not a remedy for every ſpecies of aſphyxia, on the contrary, in many of them it may prove very dangerous; but without dwelling longer upon this ſubject, let me endeavour to lay before my readers ſome new facts.

Experiment XCV.

I included ſeveral frogs in glaſs veſſels filled with inflammable air. At firſt they appeared not much incommoded by it, but ſome hours after, became reſtleſs, were agitated, and endeavoured to get out. Afterwards they were apparently quiet, but this ſtate of repoſe did not laſt long. Their agonies recommenced, as well as their efforts to eſcape. In the midſt of theſe alternations of agitation and tranquility, their force weakened, and they died.

On cutting them through the body, I found that the motion of the heart was conſtantly kept up.

The fleſh in general retained its natural colour. Sometimes it was rather red: however, one cannot with propriety aſſert, that this appearance is owing to the action of the gas, as I have

ſeen

ſeen many frogs of the ſame colour the inſtant they have been killed.

In theſe caſes, the irritability of the muſcular fibre, and the vital principle appeared to be retained; but I cannot however omit obſerving, that the movements were ſometimes very feeble, although the animal ſubmitted to the experiment, was before well furniſhed with vitality. This fact ſhews, that in a ſtate of pain, the nerves ſuffer under certain circumſtances very eſſential changes.

Experiment XCVI.

Inflammable air, or hydrogena gaz, did not act with increaſed power, or rapidity upon frogs, in which the heart and brain were expoſed.

Experiment XCVII.

The heart when removed from the body and placed in inflammable air, palpitated with the ſame ſwiftneſs and force.

EXPERIMENT XCVIII.

Nitrous air is more noxious to the conſtitution of frogs, than that of which we have been ſpeaking.

They are ſcarcely plunged in it, ſcarcely make one inſpiration before one ſees them ſtriking againſt the veſſel, ſtruggling and tumbling over in the moſt confuſed manner. In the midſt of theſe violent convulſions, they fall as it were into a fainting fit. Some few minutes after, their torments begin again, are ſucceeded by a kind of total deſertion of life. Theſe alterations take place ſeveral times, and at length give way to a tranquil death. In theſe frogs the motion of the heart is commonly deſtroyed, or if it retains ſome palpitations, they are feeble, and at long intervals.

This viſcus is found much diſtended, and filled with very black blood. The

muſcles

muſcles are occaſionally in a ſtate of ſtiff-neſs and tenſion, at which time the move-ments are languid and tranſient. At other times, the movements occur as in the moſt animated frogs.

Four of theſe frogs preſented a ſingular phenomenon. At the firſt contact of the exciter they were much agitated, but be-came motionleſs after three or four ſhocks.

For a few minutes they were left to themſelves. I afterwards endeavoured to excite them, but without ſucceſs. Not-withſtanding in frogs, and in general in all animals, the movements ceaſe gradually by little and little.

It is impoſſible that the nerves ſhould loſe their conducting power all at once without any evident cauſe, and the muſcles their vis inſita.

There exiſts in the animal economy a principle of life which prevents the equi-librium of the electricity to be eſtabliſhed, and it is to the inactivity of this principle, that in all probability the instantaneous immobility is owing.

Experiment XCIX.

The heart when expofed to the contact of nitrous air, continues to beat for fome time.

Experiment C.

Frogs which have their heart expofed, do not perifh more quickly in nitrous air, than others in which it is left under natural circumftances.

I have fometimes feen the movements of the heart ceafe, and notwithftanding the animal continue to exercife its voluntary motions. This fact leads me to prefume, that nitrous air acts more upon the irritability of the heart and lungs, than upon the fentient principle.

Experiment CI.

Muſcles which have been expoſed to the action of the nitrous air, experience a loſs which may be calculated.

Suppoſe we take the poſterior extremities of a frog and ſeparate them from each other—one is placed under a veſſel of nitrous air, and the other under one containing atmoſpheric air. The former leg moves more weakly than the other, and loſes its vitality more quickly; it even ceaſes to give marks of electricity when left too long under the glaſs.

Experiment CII.

Having repeated the ſame experiment with inflammable air, I was aware that it operated upon the muſcular fibre with leſs activity than the nitrous.

EXPERIMENT CIII.

Phlogifticated air, or azotic gaz, is equally noxious to frogs with the nitrous. After death, the heart ftill beats. Their flefh is of a beautiful purple colour, as well as their blood. With regard to the movements, we obferve nearly the fame accidents as occur in the nitrous gas, except that the azota does not produce any change in the mufcles taken off by the animal.

To prove this, I have held the legs of frogs for half an hour, and longer, in contact with this gas, and when I came to compare them with the others, did not find that they had been in the leaft changed.

The flefh of animals as well frugivorous as carnivorous, and even that of fifhes, furnifhes, on chemical analyfis, a confiderable quantity of phlogifticated air.

Phlogifticated air appears to form, as it

were,

were, their baſis, and it ſhould ſeem that this principle gives riſe to the muſcular irritability. This information, which we owe in the firſt place to Mr. Bertholet, and afterwards to Mr. Fourcroy, may perhaps explain why this ſpecies of gas does not produce the ſame effect upon the muſcles as the gaſes before-mentioned.

Will phlogiſticated air be found to be as good an antiſpetic as fixed air, or the acid carbonic gas?

Experiment CIV.

I killed two kittens in phlogiſticated air, and prepared their fore legs, which afforded the ſame movements as in the natural ſtate.

Experiment CV.

Inflammable air deſtroyed the life of a canary bird, but did not affect its electricity, although it is naturally very weak.

Animals may loſe their electricity in like manner as they loſe their heat; I mean to allude to that electricity which animals accumulate, and condenſe in certain organs, for the purpoſe of making uſe of as they find occaſion, and not to that electricity which is found in all bodies, independent of the principle of life.

The action of gas is different from that of poiſon, as is evident from my experiments, as well as thoſe made by other phyſiologiſts, prior to the diſcovery of Galvani. The action of miaſmata is likewiſe different from that of gas and poiſon. The hiſtory of diſeaſes will furniſh us with the moſt convincing proofs of this. In order to obtain ſome facts deduced from experiment, I endeavoured to excite gangrene in the inteſtines of animals, knowing how powerfully the miaſmata ariſing from this ſtate, act upon the vital principle.

Experiment CVI.

I opened the bellies of ſeveral fowls near the anus, laid bare the inteſtines, and paſſed ligatures round them. The reſult was not always the ſame. Sometimes the inflammation took place, and was ſucceeded by gangrene; at other times, the gangrene had ſcarcely begun to make its appearance, before the animal fell a victim. In other caſes again, death did not take place before ſeveral hours had elapſed. When the inflammation was rapid, the gangrene aſſumed a more malignant and deleterious character.

Experiment CVII.

Two fowls died before either inflammation or gangrene had made their appearance.

EXPERIMENT CVIII.

In other fowls, gangrene occurred without appearing to be preceded by inflammation.

In all thoſe inſtances I could not procure any ſigns of electricity. Notwithſtanding, I was in expectation of meeting with it in thoſe fowls, whoſe life was ſo ſuddenly extinguiſhed by the impreſſion of the gangrene miaſmata upon the nervous ſyſtem. We may therefore conclude, that the activity of this matter is ſtill more energetic than that of poiſons and gaz.

EXPERIMENT CIX.

The ſame experiment was made upon three rabbits, which died before gangrene was formed. My attempts to awaken the movements were ſimilar as to their effects, to thoſe with the fowls.

With

With different views, I have formerly famiſhed animals, and obſerved they all died quietly. In theſe caſes, do the animals retain any reſiduum of life? To give a proper ſolution to this query, we muſt have recourſe to experiment, as conjecture only leads to error.

Experiment CX.

Several rabbits were ſhut up in a room without any kind of food. Some of them died in two days, others in three, and ſome in three days and a half.

I was able to examine ſome of them at the moment of their death. They did not afford any ſign of vitality. This appeared to me extraordinary, inaſmuch as the muſcles did not exhibit any perceptible change.

Experiment CXI.

Fowls, although of a very different conſtitution from rabbits, are not capable

of

of ſupporting hunger for a long time. Amongſt ten, the ſubjects of experiment, there was not one that reached the ſixth day. In none of them did I meet with the phenomena I was in ſearch of.

Experiment CXII.

I prepared the wings before the animal was dead, but this precaution did not enſure ſucceſs.

Experiment CXIII.

Three pigeons, of the wings of which I had coated the nerves in the laſt moment of life, on being touched by the exciter, appeared to be enlivened, and gain a degree of vigour; but after ſome ſhocks of the wings, they fell again into debility and agony.

The ſhocks when the vital principle is nearly extinguiſhed are very feeble, and ceaſe altogether two or three minutes before

fore the animal expires. After this period, we endeavour in vain to produce fresh movements.

Experiment CXIV.

In a kitten which had lived nine days without eating or drinking, I had a similar result.

Experiment CXV.

I caused a dog to be killed, which had neither eaten or drank for twenty-three days. Having prepared and excited the legs, I found the movements neither so strong nor so lasting as they ordinarily are.

In the experiments of Redi, dogs lived in a state of the most complete abstinence for thirty-four or thirty-six days; in my own they have exceeded that period.

We meet with similar examples in other classes of animals, and paticularly in the human race. These phenomena begin to

be

be less astonishing in consequence of the information we are now in possession of. We know that the fluid, which has hitherto been called the nervous fluid, by means of which all the movements and functions of the animal economy are executed, is electricity itself, and that animals may receive it from the earth and from the atmosphere. I do not speak of the other means which nature employs for the preservation of these beings, because the subject would be foreign to my present work.

SECTION

SECTION III.

I HAVE aſſerted, that the nervous fluid is the ſame with electricity, and with good reaſon; for

Subſtances which conduct electricity, are conductors likewiſe of the nervous fluid.

Subſtances which are not conductors of electricity, do not conduct the nervous fluid.

Non-conducting bodies, which acquire by heat the property of conducting electricity, preſerve it likewiſe for the nervous fluid.

Cold, at a certain degree, renders water a non-conductor of electricity, as well as of the nervous fluid.

The

The velocity of the nervous fluid is, as far as we can calculate, the ſame with that of electricity.

The obſtacles, which the nerves under certain circumſtances oppoſe to electricity, they preſent likewiſe to the nervous fluid.

Attraction is a property of the electric fluid, and this attraction has been diſcovered in the nervous fluid.

We here ſee the greateſt analogy between theſe fluids; nay, I may even add, the characters of their identity.

As to what regards the attraction, I may perhaps have been deceived in my experiments, or have fancied what did not exiſt.

But though I may miſtruſt my own obſervation on this point, yet the Committee of the Academy of Sciences at Paris, with whom I repeated the experiments upon animal electricity, and who were witneſſes to the attraction in a leſs equivocal manner than I was, could not eaſily be miſtaken. They

They placed a prepared frog in a veſſel, which contained the electrometer of M. Coulomb, charged negatively and poſitively by turns. In both caſes, in exciting the animal in the common way, the ball of the electrometer was attracted. (See Med. Eclairée, ou Journal redigé, par M. Fourcroy, T. 10, n. 11. pour Août 1792.)

If we reflect, at preſent, upon the phenomena preſented by the torpedo and gymnotus; if we conſider that the fluid diſcharged by them is conducted or arreſted by the ſame ſubſtances, which conduct or arreſt the nervous fluid; we cannot avoid being convinced that the ſhock of the torpedo, and the ſhock and ſpark of the gymnotus, are effects of the ſame cauſe, which produces the movements in the frogs, fowls, cats, dogs, and horſes, made the ſubjects of experiment.

And as it would be abſurd to aſſert, that the property of the torpedo is derived from a cauſe different from that of the gymnotus, becauſe the torpedo does not emit ſparks; ſo it would be equally abſurd to maintain, that the fluid of frogs, fowls, cats, dogs, &c. is not the ſame as that of the torpedo and gymnotus, becauſe the former neither gives ſhocks, nor emits ſparks. The principle is the ſame. By means of this principle, all the effects may be explained; conſequently it would be contrary to the laws of philoſophiſing to admit of any other.

If the principle is unique, it muſt be electricity; for if we examine every ſpecies of animal, we ſhall meet with every character of this fluid.

In ſpeaking of the torpedo and gymnotus, we cannot, without being guilty of injuſtice, omit to mention the name of Mr. Walſh, nor refuſe him an extenſive portion of merit.

He was certainly the firſt who explained the

the power of the torpedo by the known laws of electricity, and who knew how to imitate exactly the appearances afforded by this fiſh, with the electrical fluid. Some difficulties ſtill remained, but they were removed by the experiments of Mr. Cavendiſh.

Mr. Walſh likewiſe was the firſt who obtained the ſpark from the gymnotus.

Mr. Williamſon had obſerved, that the fluid of the gymnotus could paſs over a very ſmall interruption of the chain, but was never fortunate enough to ſee the ſpark. Notwithſtanding, he was convinced by his own experiments, that the fluid of the fiſh was electricity itſelf. "As the fluid (ſays he) diſcharged by the eel, affects the ſame parts of the human body, that are affected by the electric fluid; as it excites ſenſations perfectly ſimilar; as it kills or ſtuns animals in the ſame manner; as it is conveyed by the ſame bodies that convey the electric fluid, and refuſes to be conveyed by other bodies that refuſe to convey the electric fluid, it muſt be the true electrical fluid: and the ſhock given by this eel, muſt be the true electrical ſhock." Phil. Tranſ. vol. lxv.

Several naturaliſts, with whom I have converſed upon animal electricity, are of opinion, that this fluid is analogous to the electricity diffuſed through the univerſe, but that it is not of the ſame nature. Electricity, ſay they, cannot act but when its equilibrium does not exiſt: Now it ſhould be ſuppoſed, that it is accumulated in ſome part of the animal; but as the parts of animals are all conductors of electricity, ſuch an accumulation cannot take place, conſequently it is not electricity, which in animals performs the functions of the nervous fluid. In my anſwer to theſe gentlemen, I ſhall obſerve, that animal electricity is in like manner conducted by all the parts of the body : the muſcles, tendons, membranes, veſſels, bones themſelves, and the nerves, are conductors of this fluid. Thus their argument is of no weight, as it does not obviate the difficulty they raiſe.

The following experiments will demonſtrate what I have ſaid reſpecting the conducting power of parts.

Experiment CXVI.

I detached a muſcle with its tendon from a frog. I placed the tendon upon a piece of ſilver coin, and the muſcle upon the thigh of another frog, the crural nerves of which were prepared. This eſtabliſhed a communication between the money and the frog. I then touched the coating and the money with the exciter, and the frog became convulſed. The ſame thing took place equally well when I made uſe of the tendon alone. When the vitality begins to diminiſh, the experiment does not ſucceed, or at leaſt only in an imperfect manner.

Experiment CXVII.

I placed membranes in the ſame manner with the muſcle and tendon, and found they were conductors.

Experiment CXVIII.

I prepared the blood-veſſels of the poſterior extremities of a frog, and managed the coating in ſuch a way as to prevent it touching the nerves which accompanied the veſſels.

When the communication was eſtabliſhed between the veſſels and the muſcles, by means of a conductor, the extremities became convulſed.

Experiment CXIX.

I divided the nerves, and ſeparated them from the veſſels; on eſtabliſhing again the communi-

communication, as in the preceding experiments, the limb remained motionlefs. I then brought the cut ends of the nerves near the veffels, without fuffering them to touch the coating, and the exciter produced very evident movements at feveral different times. This does not occur when the electricity becomes weak.

Experiment CXX.

Having placed the extremity of a bone in contact with a prepared frog, I put the conductor upon the other end and upon the coating. Shocks were excited, which ceafed when the bone was divefted of its periofteum.

Experiment CXXI.

In place of the bone, I fubftituted the nerves of another frog, and the movements ftill occurred.

How can it happen, that parts of the

animal become charged in *plus* or *minus*, in the midſt of circumſtances ſo inimical to it? I am not aware, I confeſs, of the means which nature makes uſe of in this proceſs, but I am perfectly convinced that this proceſs does take place.

Let thoſe who doubt this, conſider for a moment the phenomena of heat in animated beings.

Quadrupeds, inſects, birds, fiſh, in a word, all animals, and even trees, plants, and flowers, have their ſpecific temperature. Each has, if we may ſo expreſs it, its meaſure apart.

Theſe beings even generate their own heat, retain it with tenacity, and periſh before they loſe it entirely. Mr. Hunter has made upon this ſubject ſome very intereſting experiments, from which it appears, (ſays he) " that an animal muſt be deprived of life before it can be frozen—That there is an exertion or expence of animal

powers,

powers, in doing this, in proportion to the perfection of the animal, the natural heat proper to each ſpecies, and to each age.—It may alſo perhaps depend, in ſome degree, on other circumſtances not hitherto obſerved; for, from experiments upon dormice, I found, that in theſe animals, which are of a conſtitution to retain nearly the ſame heat in all temperatures of the air, it required the greateſt cold I could produce to overcome this power, while in other experiments, this power in the toad and ſnail, whoſe natural heat is not always the ſame, but is altered very materially according to the external heat or cold, was exhauſted in a degree of cold not exceeding 10° or 15°, and the ſnail being the moſt imperfect of the two, its power of generating heat was by much the weakeſt.—The imperfect animals will allow of a conſiderable variation in their temperature of heat and cold.—Plants, when in a ſtate

of

of actual vegetation, or even in ſuch a ſtate as to be capable of vegetating under certain circumſtances, muſt be deprived of their principle of vegetation before they can be frozen. Vegetables have a power within themſelves of producing or generating heat, but not always in proportion to the diminution of heat by the application of cold, ſo as to retain at all times an uniform degree of heat; for the internal temperature of vegetables is ſuſceptible of variations to a much greater extent indeed, than that of the moſt imperfect animals, but ſtill within certain limits. Beyond theſe limits, the principle of vegetable, as of animal life, reſiſts any further change. The heat of vegetables varies according to the temperature of the medium in which they are, which we diſcover by varying that temperature, and obſerving the heat of the vegetable. The expence of the vegetating powers in this caſe, is proportioned to the neceſſity, and the whole vegetating powers may be exhauſted in this way.—This power is moſt probably in proportion to the perfection of the plant, the natural heat proper to each ſpecies, and the age of each individual. It may alſo perhaps depend, in ſome degree, on other circumſtances; for in an experiment, the old ſhoot did not loſe its powers, while that which was young, or growing, did; and in two other experiments we found, that the young growing ſhoot of the fir, was with great difficulty frozen at 10°, while a bean-leaf was eaſily frozen at 22; and in another experiment, the young ſhoot of the fir thawed the ice at 28° much faſter than the leaf of the bean. The roots of vegetables are capable of reſiſting cold much more than the ſtem or leaf.

So far, animal and vegetable life appear to be the ſame, yet an animal and vegetable

table differ in one very material circum-ſtance. An animal is equally old in all its parts, excepting where new parts are formed in conſequence of diſeaſes; and we find, that theſe new or young parts in animals, like the young ſhoot of vegetables, are not able to ſupport life equally with the old; but every plant has in it a ſeries of ages. According to its years, it has parts of all the ſucceſſive ages from its firſt formation, each part having powers equal to its age; and each part in this reſpect being ſimilar to animals of ſo many different ages." Philoſ. Tranſ. An. 1775.

The author has likewiſe ſimilar facts in his work on the Animal Economy.

The experiments of Mr. Hunter recall to our recollection thoſe of Dr. Fordyce, which are at once ſurpriſing and deciſive.

The Doctor brought ſome rooms to different temperatures, by gradually in-

creaſing

creasing their heat. The greatest degree of heat the first time was 130°. He exposed himself to it, and was able to remain in it fifteen minutes. The surface of his body was covered with sweat, and the thermometer placed under the tongue stood at 100°.

He increased the heat to 214°, which he was capable of sustaining for a quarter of an hour. The thermometer still did not exceed 100°.

A dog can live in air heated to 260° for a considerable time, and yet retain its own temperature. From other experiments which the author has made, it appears, that in living animals the power of resisting heat does not arise solely from evaporation, and that certain animals, as frogs for instance, receive heat more slowly when living than when dead.

Thus have animals a power of generating caloric, and if we may so say, concentracentrating and preſerving it, although placed in a medium much below their temperature; and they have likewiſe a power of generating cold when the ſurrounding medium is conſiderably above their natural heat.

There is then in bodies, which enjoy life, a principle which regulates their temperature according to the neceſſity of the caſe. And why ſhould there not be a principle, or force, to accumulate and condenſe electricity?

It may be objected, that when an animal is placed in a cold medium, it decompoſes by the proceſs of reſpiration a larger quantity of air, and conſequently a greater quantity of caloric is abſorbed by the blood. That in animals placed in a medium, the temperature of which is above the ſtandard of their natural heat, the evaporation from the ſurface increaſes, and the quantity of inflammable principle

in

in the blood diminiſhes, ſo that there enſues a double loſs of caloric, and it is in this way that the heat of the animal is kept at its level, or ſurpaſſes it but very little. Laſtly, it may be advanced, that every thing ſingular and wonderful obſervable in the heat of animals is capable of being very well explained, without having recourſe to an occult principle of life, and that from theſe facts I cannot deduce any concluſions favorable to my ſyſtem. If one could explain how animated beings which do not breathe, and which are ſo circumſtanced as not to be liable to have evaporation take place with them, have a power of generating heat and cold; if one could prove to demonſtration that the inflammable principle is really increaſed or diminiſhed in proportion to the calls of neceſſity, then I would allow that the powers of generating heat and cold to be known. But philoſophers have not yet

attained

attained this point. Perhaps they will attain it, and then all the myſtery will be revealed. They will perhaps likewiſe be able to diſcover in what manner the condenſation of electricity is performed. But in expectation of this brilliant day, ought we not to confine ourſelves to fact?

The muſcles are the parts which appear to me beſt adapted to effect this condenſation.

Muſcles, it is true, differ from the terrible apparatus of the torpedo and gymnotus as to their diſpoſition and arrangement, but it ſtrikes me as evident, that in both caſes, nature has had the ſame plan and ſame object in view, as appears from what follows.

The electrical organs of the torpedo are two in number, and are placed one on each ſide of the cranium and gills, reaching from thence to the ſemicircular cartilages of each great fin, and extending

longi-

longitudinally from the anterior extremity of the animal to the tranſverſe cartilage, which divides the thorax from the abdomen, and within theſe limits they occupy the whole ſpace between the ſkin of the upper and of the under ſurfaces.

Theſe organs conſiſt of perpendicular columns reaching from the under to the upper ſurface of the body. The length of theſe columns is different in different parts of the body.—Their form is very irregular—the greater part of them is hexagonal, or rather irregularly pentagonal. Their number varies according to the ſize and age of the animal. In a very large torpedo, one electrical organ conſiſted of 1182 columns.

Each column is divided by horizontal partitions placed over each other at very ſmall diſtances, and forming numerous interſtices, which appear to contain a fluid. Theſe partitions conſiſt of a very thin membrane conſiderably tranſparent.

The number of partitions contained in a column of one inch in length of a torpedo, which had been preſerved in proof ſpirit, appeared to be one hundred and fifty.

Strong fibres, which paſs immediately from one column to another, ſerve to connect them. Theſe columns are alſo furniſhed with very thin coats cloſely united, and which are ſtill more united by tendinous fibres, which paſs in an oblique and tranſverſe direction between the columns themſelves.

Theſe organs have likewiſe two thin membranes, or faſciæ, of which the inner appears to be continued into the electric organ by many proceſſes. If we examine the organs with "groſs optics," we may diſcover, according to Mr. Walſh, ſeveral cylinders or hexagonal priſms, the ſurfaces of which taken together form a very conſiderable extent.

The gymnotus has two pair of electrical

organs, a larger and a ſmaller one, which are placed on each ſide, and ſeparated from one another by a membrane. The large pair occupy the whole lower or anterior, and alſo the lateral part of the body, and run from the abdomen to near the end of the tail. It is broadeſt at the end next to the head of the animal, becomes gradually narrower towards the tail, and ends almoſt in a point.

Theſe two organs are ſeparated from one another at the upper part of the muſcles of the back; below that, and towards the middle, they are ſeparated by the middle partition. Their union with the parts to which they are attached is in general by a looſe, but pretty ſtrong, cellular membrane, except at the partition, to which they are joined ſo cloſe as to be almoſt inſeparable.

Theſe organs conſiſt of ſepta and croſs diviſions. The ſepta are thin membranes, which are almoſt in the direction of the longitudinal axis of the body, and their breadth is nearly the ſemi-diameter of the body of the animal. They are of different breadth and length. Their diſtance from each other is not the ſame every where. Thus, at the anterior part, where the breadth is nearly equal, they march pretty parallel to one another; but where the organ becomes narrower, that is to ſay, nearer the tail, if obſerved in ſome places, that two join or unite into one. The termination of this organ is ſo very ſmall, that Mr. Hunter was not able to determine whether it was formed of one or more ſepta.

The diſtance between the ſepta in a fiſh two feet four inches in length was $\frac{1}{22}$ of an inch, and the breadth of the whole organ, at the broadeſt part, about an inch and a quarter, in which ſpace were thirty-four ſepta.

The

The ſmall organ lies along the lower edge of the animal under the muſcles, which move the fin. Its anterior end begins nearly in the ſame line with the large organ, and terminates almoſt inſenſibly near the end of the tail, where the larger organ alſo terminates. Its anterior end is the narroweſt part. In the middle of the organ it is thickeſt, and becomes gradually thinner to the tail, where it is very thin. The two ſmall organs are ſeparated from one another by the middle muſcles, and by the bones.

They have the ſame kind of ſepta as the large pair; in length paſſing from end to end of the organ, and in breadth paſſing quite acroſs: they run ſomewhat ſerpentine, not exactly in ſtraight lines. They differ very much in breadth from one another. They run pretty parallel to one another, but much nearer than thoſe of the large organ, being only about $\frac{1}{56}$

 part

part of an inch aſunder. Their diſtance is greater towards the tail in proportion to the increaſe of breadth of the organ. The organ is about half an inch in breadth, and has fourteen ſepta.

Theſe ſepta in both organs are very tender in conſiſtence. They are interſected tranſverſely by very thin plates or membranes, whoſe breadth is the diſtance between any two ſepta.

Their lengths are equal to the breadths of the ſepta, between which they are ſituated. There is a regular ſeries of them continued from one end of any two ſepta to the other. They appear to be ſo cloſe as even to touch. In an inch in length there are about two hundred and forty, which multiplies the ſurface in the whole to a vaſt extent.

I have dwelt a moment upon the ſtructure of the electrical organs of the two fiſhes, in order to ſhow their difference, although their office be the ſame. But to be perfectly aware of this difference, it is neceſſary to conſult the very complete anatomical account given by Mr. Hunter in the Philoſophical Tranſactions, vol. lxiii. lxv. from whence the preceding extracts have been taken.

If we were acquainted with the electrical organ of the ſilurus, which Mr. Brouſſonet calls "le Trembleur," and that of the fiſh Mr. Paterſon ſpeaks of in a letter to Sir Joſeph Banks, Philoſ. Tranſactions, vol. lxxvi. and of all the hitherto unknown fiſhes, we ſhould certainly ſee a difference of ſtructure, which would render the analogy between theſe organs and the muſcles ſtill more evident and ſtriking.

The muſcles are compoſed of ſeveral ſmall bundles or faſciculi. Theſe faſciculi conſiſt of cylinders or parallel fibres. The ſmalleſt of theſe fibres is diviſible into an infinite number of others, which are ſucceſſively ſmaller, and placed parallel to each other.

The faſciculi are enveloped by cellular membrane, as is alſo each conſtituent fibre. Where the fibres are very ſmall, the cellular membrane is very thin and delicate, and is moiſtened by a ſubtile vapour. In proportion as the parts are larger, the cellular membrane becomes thicker and more obvious, and an oily fluid is ſecreted by the arteries in lieu of vapour.

Betwixt theſe faſciculi are conſtantly found partitions of cellular membrane, which keep them farther removed from each other, and diſpoſe them in a parallel or inclined direction. They are ſurrounded by two denſe membranes, of which the inner is continued from the partitions, and the outer ſerves to ſeparate them from the adjacent fleſh.

The

The abundant cellular membrane, which enters into the compoſition of muſcles, extends beyond them, becomes cloſely compacted, and takes on the form of a round ſlender cord, which is known by the denomination of tendon, the external tunic or covering of which is ſimilar to that of the muſcles. If, inſtead of forming a cord or band, it expands into a broad flat ſurface or membrane, it is called aponeuroſis.

Muſcles, which are not inſerted into any of the bones, have commonly no tendons. Thus there obtain in muſcles, as well as in the electrical organs of the torpedo and gymnotus, cylinders, partitions, a great ſubdiviſion of parts, and an enormous extent of ſurface. But this is not all—the blood-veſſels of the electrical organs are very numerous, follow the courſe of the nerves, and diſtribute their ſmall branches along with them. The quantity of veſſels expended upon the muſcles is alſo prodigious, and they likewiſe are found to accompany the courſe and diſtribution of the nerves.

The ſize and number of the nerves which are beſtowed upon the electrical organs, are truly extraordinary and aſtoniſhing.

The nerves of the muſcles are likewiſe very large, and their number is ſo great, that ſeveral phyſiologiſts have believed, that the muſcular fibre is only compoſed of nervous fibrils.

From theſe conſiderations it appears, that muſcles are ſo many electrical organs, which are more ſingular than the organs of fiſhes, foraſmuch as at the ſame time they ſerve as the inſtruments of motion.

Each muſcle being as it were a battery, the quantity of electrical fluid condenſed by animals muſt be immenſe.

Were it in the power of animals to diſcharge this fluid in a manner ſimilar to the torpedo and gymnotus do with theirs, the effects would prove truly terrible. We may be enabled to form ſome ſlight idea of what would occur by the very extraordinary ſhock which the celebrated Cotunnio experienced, whilſt diſſecting a live mouſe.

The nerves diſtributed upon all the ſurfaces of the cylinders, or ſmall jars of muſcle, are the threads which conduct the fluid in queſtion. They are diſpoſed in ſuch a way as to communicate with each other. The ſprings, or means by which this communication exiſts, have as yet evaded our obſervation. Theſe ſprings are ſubordinate to the direction of the intellectual faculty, which by means of them is capable of producing any diſcharge and motion it will.

The diſcharge implies the exiſtence of the two contrary electricities, poſitive and negative.

negative. But where does the poſitive electricity reſide ? and where the negative ? Profeſſor Galvani, in order to eſtabliſh this point, brought cylinders of glaſs and ſealing-wax near the ſpinal marrow of frogs; the former did not produce any movements, but they were excited by the latter. If the back of the animal was covered by a plate of metal, tin for inſtance, although it might be at the diſtance of four lines or more, yet the ſealing-wax excited muſcular motion. Having brought the animal near the plate of the machine, after having turned it ſeveral times, no motion was produced.

The author then made every poſſible attempt in the ſame manner to excite motion in the muſcles, but without ſucceſs. From hence Galvani concludes, that the poſitive electricity reſides in the nerves or the inner ſurface of the muſcles, and the negative in the outer ſurface, or both

equally

equally in the nerves, and in the mufcles. To fupport this opinion ftill more, he obferves, that ftrong fhocks are produced by touching the coating of the brain, and that if the mufcles are coated, the animal only moves a little, and that even but feldom. He likewife remarks, that the experiment fucceeds on touching the coated nerves when feparated from the mufcles, but that there is not any effect when the mufcles are coated and feparated from the nerves. (Journal de Phyfiq. mois de Juielet 1792, p. 52, &c.)

The author by applying a pofitive or negative electricity to the nerves, believed he could direct it againft a furface charged *plus* or *minus*, but this was a miftake.

The nerves communicate with all the points of the mufcles, and confequently with feveral furfaces charged with oppofite electricities. Thus it is not poffible by this means to judge of the nature of the electricity we may wifh to difcover.

With

With reſpect to the laſt facts the author has brought forward, they are contradicted by the experiments xx. and xxi. in Section I.

Are muſcular motions all effected by the ſame inſtrumentality? The heart, veſſels, ſtomach, inteſtines, in a word, all the muſcular parts which are not in obedience to the will, act, I think, by a ſimple *afflux* of electricity that I ſuppoſe to exiſt in the nerves in two different ſtates.

It is *ſpecific ſtimuli* which give a determination to this *afflux* or diſcharge.

Since there is not any electrical fluid condenſed in theſe viſcera, one cannot by means of a coating and conductor excite ſhocks in them as in muſcles, which perform voluntary motions.

In my firſt letter on animal electricity, publiſhed at Pavia on the 5th of April, 1792, I obſerved, that the heart of a dog, on which I made the experiment, did not palpitate at all. Afterwards I had an opportunity

portunity of making the ſame obſervation on the ſtomach, inteſtines, &c. and others have ſince repeated this experiment after me.

Artificial electricity cannot excite the irritability of theſe parts; at leaſt, I have not yet ſucceeded, either by means of weak or ſtrong charges, or ſparks, or a current, the power of which I regulated either by varying the quantity, or by making uſe of good or bad conductors. Volta made ſeveral of theſe experiments before I did, and with a ſimilar reſult. But from hence we ought not to infer, that electricity is not the cauſe of the motion of the heart, ſtomach, inteſtines, &c.

Let us recall to our recollection, that the ſimple approximation of the coats of the nerves, under certain circumſtances, deſtroys the action both of native and artificial electricity. If the nerves of the organs, of which the operations are ſpontaneous,

are

are difpofed in fuch a manner as to refufe a paffage to this fluid when it is directed upon them, it is certain that the movements will not take place. Nor can electricity when applied to the organ itfelf, however ftrong it may be, produce the leaft effect, becaufe it does not act as a ftimulus, but by another law, as will be fhewn in the fourth Section.

But that the agent, which calls thefe organs into action is electricity, is demonftrable both from analogy and facts. Thefe organs poffefs irritability in common with mufcles. The irritability of the mufcles is moft powerfully excited by the animal electrical fluid. It is therefore very natural to conceive, that thefe organs experience the fame effect. If I am not miftaken, we have inconteftible proofs of this in the hiftory of difeafes of the nerves.

A perfon feized with convulfions, one moment has very terrible fhocks in his whole body; at another in the upper or lower extremities, ſometimes the heart beats with violence, at others there is an involuntary flow of tears, one while the pulſe is regular, at another irregular and in a ſtate of ſpaſmodic contraction, ſometimes a partial pulſation is diſcoverable in ſome particular artery, whilſt the regularity of the pulſe announces that the reſt of the arterial ſyſtem does not partake of this alteration. Laſtly, the muſcles, and other parts poſſeſſing muſcularity, are affected alternately, or at the ſame moment. Can we in theſe phenomena avoid being aware of the exiſtence of a common cauſe?

Willis has made a very curious obſervation, which deſerves to be taken notice of in this place. He ſays, that he has ſeen ſeveral perſons who were obliged to leap, run about, and beat the ground with their hands and feet, to prevent their falling into violent convulſions of the viſcera, which would have come on, had they neg-lected this agitation.

The impreſſions made by objects, whe-ther external or internal, upon the nerves and ~~common~~ brain ~~ſenſory~~, are affected by the ſame cauſe which excites motion.

The force or energy of this fluid is al-ways proportionate to the change which the nerves ſuſtain by the impreſſion of bodies.

That the action of the electricity, in this inſtance, as in all others, is determined invariably to certain parts without being diſperſed or loſt on others, is very eaſy to be conceived in conſequence of my diſ-covery, viz.—That the ſubſtance of the nerves conducts electricity very well, whilſt their coats are very bad conductors.

It now remains for us to conſider elec-tricity under another point of view.

Electricity is not found to reſide exclu-ſively in the muſcles and nerves. It is diffuſed over all the body.

Previous

Previous to the difcovery of Galvani, it was afferted, that animals contained electricity. Mr. Nicholfon, as far as I know, was the firft who fet on foot any experiments to afcertain the quantity a man was capable of containing. "If we fuppofe," fays he, "the bulk of a man to be only "three folid feet, or 5184 folid inches, the "natural electricity of this mafs, will be "equal to the charge of a battery of up-"wards of 15,000 fquare feet."

It is prefumable that this matter exifts in different proportions, in different fpecies of animated beings, and that each has its determined meafure of it, in like manner as each poffeffes its meafure of heat.

This electricity cannot be kept in a ftate of equilibrium. Mufcular action, the fecretions, evacuations, heat, and the emotions of the mind, give rife to conftant changes in the whole conftitution of the animal. The electricity follows thefe changes; thus it is never at reſt, but conſtantly acting; and by making a gentle impetus upon the conſtituent parts of the machine, animates it, and ſuſtains its life.

The exiſtence of the fluid I now ſpeak of, is ſupported by theory, and accords in the moſt perfect manner with the known laws of phyſics. I may go ſtill farther, and give a demonſtration of it, which carries along with it mathematical evidence.

I deduce this demonſtration from a diſcovery made ſome years ago by Mr. Walſh on the gymnotus, and mentioned by M. Cavallo in his "*Complete Treatiſe of Electricity*;" but which neither he, nor any one elſe has, till now, been able to explain. This diſcovery is, that the gymnotus is ſenſible whether the ſubſtances brought near him are proper or not, for receiving the diſcharge.

The

The following is a convincing experiment:—The ends of two wires were put into the water of the veſſel, which contained the animal; theſe wires were of ſome length, ſtretched to their extent, and terminated in two glaſſes of water placed at a conſiderable diſtance from each other. Whilſt the apparatus remained in this ſtate, and the circulation was of courſe interrupted, the animal did not prepare to exerciſe his power, but the inſtant a ſpectator, or any conducting ſubſtance filled the interval, and rendered the circle complete, it inſtantly approached the wires, arranged itſelf, and gave the ſhock.

It is neceſſary to obſerve, that this communication or circle was completed entirely out of the ſight of the animal. If the animal had a knowledge of it, it was undoubtedly by means of a certain ſenſation excited in him, and this ſenſation could not poſſibly be excited but by the circulation of its own electricity. This is the true cause of this phenomenon. Every other explanation I consider in the light of a fallacious hypothesis, or idle dream.

Mr. Partington, known by his cures with medical electricity, assures me, that he has produced movements in flounders, by making use of two coatings, and a conductor of the same metal. I have, in the same fish, endeavoured to ascertain whether it was possible to render the circulation of the electricity apparent by means of only one exciter. In a great number of trials, the experiment has only twice been attended with the desired success. The animal was taken out of water and well wiped; the exciter, of which one end was three times as large as the other, was made to communicate with the belly and back. The tremblings were very slight, and only momentary. The exciter was of tin.

This

This experiment does not ſucceed in frogs, but if the coatings are made uſe of as mentioned in Section I, it never fails of ſucceſs. It ſhould ſeem that the coatings increaſe the want of equilibrium (Sbilancio) of the animal electrical fluid.

This conjecture will be found far from unreaſonable, when we reflect, that electricity is a matter endued with extreme mobility, and that trifling accidents are capable of making it experience modifications and changes, particularly in animated beings, in whom it exiſts in a ſtate of activity.

I do not wiſh this opinion to be taken for granted, as I am myſelf not entirely convinced of its being founded. But I would by no means have it happen that an incapability to explain the phenomena produced by two different kinds of metal, ſhould be a reaſon for renouncing the doctrine of animal electricity.

It is with no ſmall pain I obſerve, that an Italian author, for whom I entertain ſentiments of regard, has adopted this opinion, and has even gone ſo far as to declare himſelf an adverſary of this brilliant doctrine.

Electricity, ſays he, does not perform any function in the bodies of animals, and the doctrine of muſcular motion ſtill remains as obſcure as it was before the diſcovery of the Profeſſor of Bologna.

The effects attributed to animal electricity are, agreeably to his manner of thinking, only owing to common electricity.

This theory is founded upon an imaginary datum, viz. the circulation of the electricity of metals.

The author, with much confidence, promiſes to demonſtrate this at a future period. May we not be allowed to enquire, if it would not have been more adviſeable to have ſet off with this fact,

rather

rather than to have concluded with it? Let us however overlook this considerable defect in the work of the learned author, and pass to the examination of the following positions, upon which he grounds his subsequent reasoning.

1st, Muscular motion takes place in animals without there being a communication between the two surfaces, supposed to be charged in plus and minus.

2d, An extremely small portion of artificial electricity applied to the nerves, is sufficient to excite muscular motion.

3d, This electricity has not any occasion to pass along the nerves in order to produce its effect.

Galvani observed, that by touching the coating and the nerve itself with the exciter, that the contractions and movements took place; and I have remarked, that the same thing happened on establishing a communication betwixt muscle and muscle, provided

provided one of them was furnifhed with a coating. It is upon thefe two facts, that the opponent of animal electricity refts his firft pofition.

Are then the nerves upon which the experiment is made, not diftributed upon all the mufcular fibres? Is there any point of a mufcle deftitute of nerves? Are they not all fo circumftanced as to communicate with each other? Does not the furface of mufcles exhibit an infinite number of nervous fibrils?

If the author can conceive in what manner the nerves, which go to the extremities, or are expended upon the furface of the flefh, can communicate with the conftituent fibres of mufcles, fo as to excite their irritability; how happens it that he will not allow thefe nerves to ferve as conductors of the native electricity?

Certainly this fluid, which has a conftant tendency to maintain its balance, when

when it once finds a paſſage open, purſues it, although under ſomewhat unfavourable circumſtances. Now in order to judge of the influence which the author accords to electricity of metals, it is of conſequence to lay before my readers the following experiments.

Experiment CXXII.

Having prepared ten frogs, I placed them in a circle, in contact with each other, having laid their nerves upon a circular piece of tin, which ſerved as a common coating. I then made a communication by a narrow ſhred of tin a third of an inch in length, between a frog and the metal plate, and they were all ſtrongly convulſed at the ſame moment.

In this experiment, I cannot conceive how the circulation could take place and continue between two metals, which touched each other; and if it did take place,

place, I am at a loſs to imagine how a ſmall quantity of electricity might excite its power upon all the circles.

Experiment CXXIII.

I prepared a frog, coated the crural nerves, and afterwards placed its legs upon the head of another live one which was faſtened to the table. I then touched the coating with one end of the conductor, and the body of the live frog with the other. The prepared frog was inſtantly ſhocked, whilſt the other remained motionleſs.

Experiment CXXIV.

Inſtead of a live frog, I ſubſtituted the legs of another, but could not diſcover any motion except in the animal, whoſe nerves were coated. In this attempt I did not fail to change the direction of the extremities, which formed the chain, or in

other

other words, the continuation of the conductor.

Experiment CXXV.

The triceps muſcle of the thigh being detached from a frog, was put in contact with the thigh of another, the nerves of which were coated.

The application of the conductor to the coating of the triceps excited convulſions and contractions in the frog, but not in the muſcle.

In theſe experiments, the live animal, the extremities, and the iſolated muſcle, ſerved as the chain. The electrical fluid paſſed along them, but they never exhibited the leaſt ſign of motion.

If the electricity did not excite any motion in the parts, which ſerved as conductors, why did it excite movements in prepared frogs furniſhed with coatings?

Can this queſtion be reſolved by the hypotheſis of our antagoniſt? Does not the circulation

circulation of condenſed electricity afford a better ſolution of theſe phenomena?

If it ſhould be objected by any one that the circulation only takes place when two coatings are employed, I would aſk the reaſon, why the movements take place when there is only one coating? and why in this caſe, there ſhould be a neceſſity for a conductor of a different ſort of metal from the coating.

A very ſmall quantity of artificial electricity is ſufficient to excite motion in frogs, when applied to the nerves.

This poſition of the author is certainly true. Five or ſix degrees of electricity meaſured with the condenſer in a very ſenſible electrometer, in his experiments produced evident effects. It ſhould ſeem to be the intention of the author to prove, by means of this fact, that the electricity of the metal, although trifling, is capable of producing the phenomena in queſtion.

This criterion does not however appear

to me ſufficiently accurate, and for this reaſon: the before-mentioned quantity of five or ſix degrees does not call forth muſcular irritability in all the circumſtances under which metals are employed with ſucceſs; and beſides, this meaſure cannot ſerve as a common meaſure for all animals.

Some time ago, I made ſeveral experiments with artificial electricity, and was at a loſs how to determine by this way, the direction of the animal electricity.

I then learnt that the quantity of electrical fluid capable of occaſioning ſtrong contractions in frogs, when applied immediately to the nerves, loſt all its power on making it paſs over a metal conductor of great length, or through water, by forming myſelf a part of the chain. Nevertheleſs, the experiment ſucceeds in theſe animals with a conductor of 200 feet, (Paris meaſure) and even of ſtill greater length. It is the ſame thing, provided

the

the chain is composed of several people. The following is a striking instance:

Twelve of us joined hands; one of the two at the ends of the chain touched the water of the glass in which the thighs of the frogs were placed, whilst the other person touched with a piece of silver coin the coating of the spine, which hung over the edge of the vessel in the opposite side. The animal sprung with force out of the vessel.

The experiment was repeated when the chain was interrupted, but notwithstanding the movements took place, the electricity passing over the floor. One of the persons who had withdrawn their hands in order to destroy the continuity of the chain, seated himself in an insulated chair. In this state, the experiment did not succeed, but he had scarcely touched the floor with his toe, before the animal sprung up and fell into convulsions.

Artificial electricity, when employed

upon

upon other animals, produces effects which do not by any means accord with the ideas of our author.

Experiment CXXVI.

I took a jar, the coated surface of which was thirty square inches. Having charged it with a quantity of electricity, which, at the electrometer of Mr. Cavallo, produced a divergence of two lines, I applied the knob to a piece of iron, which touched the plexus of the wing of a small fowl. The wing remained motionless.

Experiment CXXVII.

The divergence was three lines. No motion.

Experiment CXXVIII.

The divergence of four lines. The wing just stirred.

EXPERIMENT CXXIX.

After ſeven or eight minutes had elapſed, the ſame quantity did not occaſion any motion.

EXPERIMENT CXXX.

The electricity was increaſed ſo as to produce a divergence of ſeven lines. A very feeble contraction was obſerved to take place.

EXPERIMENT CXXXI.

Ten minutes after the wing was detached from the body, and placed upon an iſolated plain. The plexus was coated with a piece of tin-foil. The wing flapped with force in a manner natural to the animal, on the application of a ſilver conductor to the coating and muſcles.

With artificial electricity which diverged the balls nine lines, no effect was produced.

Experiment CXXXII.

With a divergence of ten lines, a motion barely perceptible was occaſioned.

Experiment CXXXIII.

The divergence of eight lines excited ſome motion in the other wing, the nerves of which were juſt laid bare, that is, about ten minutes after the death of the fowl.

Experiment CXXXIV.

A leſs charge failed of producing any effect.

Experiment CXXXV.

I covered the plexus and the coating with oil; notwithſtanding this, the application of the exciter produced tremblings. The communication was made between the coating and the muſcles, it not being poſſible to induce any electrical appearances by touching the coating and nerves. Under theſe circumſtances, the artificial electricity at ſuch a point as to make the cork balls of the electrometer ſtrike the ſides of the glaſs with violence, had no power over the wings.

Experiment CXXXVI.

When the jar was completely charged, it did not produce the ſmalleſt apparent effect.

Experiment CXXXVII.

I paſſed a piece of ſealing-wax under the plexus, in order that the artificial electricity ſhould act with greater energy upon the nerves, by not being diſperſed upon the ſurrounding parts. The movements then took place. But I cannot ſay exactly how much electricity was employed, as the quantity was too great to be meaſured exactly by the electrometer.

Mr. Moorcroft, who aſſiſted me in theſe experiments, very judiciouſly obſerved, that the ſhocks produced by artificial electricity did not imitate the natural muſcular contractions as much as thoſe which were the conſequence of the application of the metalic exciter.

EXPERIMENT CXXXVIII.

I laid bare the brachial plexus of a horſe which was juſt killed. When the jar was charged, and brought within an inch of the plexus, the animal gave a ſtrong ſhock.

EXPERIMENT CXXXIX.

The jar was charged, ſo that at five or ſix inches from the electrometer, the pith-balls diverged four or five lines. On the knob being brought in contact with the nerves, not any motion was obſervable.

Experiment CXL.

I heightened the charge by ſome degrees. The leg then trembled, though faintly.

Experiment CXLI.

I placed the fingers of my right hand under the plexus, which I touched with the knob of the jar held in my left. I experienced myſelf a ſlight ſhock, but the leg remained motionleſs.

Experiment CXLII.

Three quarters of an hour afterwards, by employing the coating and exciter, very perceptible ſhocks were obtained. I failed of ſucceſs when I made uſe of artificial electricity as in the laſt experiment, though in a greater degree.

As the air of the apartment in which I made theſe experiments was loaded with moiſture,

moiſture, and the apartment itſelf was open on one ſide, and conſequently communicated freely with the external air, it was impoſſible for me to procure any very accurate meaſure of the electricity I employed. It notwithſtanding appeared, that a ſmaller quantity than what I at firſt imagined was capable of producing ſenſible phenomena.

Theſe trials deſerve to be repeated, in order that we may determine the meaſures with the preciſion and correctneſs required in phyſical ſubjects.

However, the data of which we are now in poſſeſſion, are ſufficient to convince us, that though five or ſix degrees may be ſufficient for a frog, yet they are not ſufficient for a fowl, a horſe, an ox, an elephant, or a whale. Yet notwithſtanding, a ſmall piece of tin-foil and a ſhilling can produce ſurpriſing effects, as well in large as in ſmall animals.

It ſtill remains for us to make one important obſervation, viz. That the electricity made uſe of in the laſt mentioned experiments was condenſed electricity, the force or intenſity of which cannot be equalled by the electricity of metals in the natural ſtate.

Although it be a well-known fact, that the condenſed electricity of a charged jar poſſeſſes a greater energy than that of a body ſimply electrified, becauſe in like circumſtances the quantity is greater in the former than in the latter caſe, yet it may not be extraneous to introduce a few experiments which tend to illuſtrate it more fully.

Experiment CXLIII.

A frog was prepared, and placed upon a table. A ſtick of ſealing-wax was rubbed until at the diſtance of eighteen inches, from Bennet's electrometer it cauſed a divergence of half an inch. When brought

brought within two or three lines of the nerves, and even in contact with them, it failed of manifesting any power over the animal.

Experiment CXLIV.

The nerves were coated, and the frog insulated. The sealing-wax was so much more excited than in the preceding experiment, that at the distance of three feet a similar divergence took place. The result was the same.

Experiment CXLV.

A glass tube of a considerable diameter was strongly excited, and brought near the nerves of the animal without causing any oscillation in the muscles. The electrometer expressed a divergence of nearly half an inch, when the tube was eight or ten inches from it.

Without stopping to make any further

remarks,

remarks, I ſhall proceed to the laſt propoſition of the author, in which he maintains that the paſſage alone of electricity acroſs the nerves, is ſufficient to throw the muſcles into contraction.

That a ſtimulus exerted upon the nerves is capable of exciting convulſions in animals, and that electricity is the moſt powerful of all ſtimulants, are facts which phyſiologiſts have long been acquainted with.

But there is a limitation to this action, beyond which neither chemical nor mechanical ſtimuli, nor even electricity itſelf, when it only paſſes acroſs the nerves, can produce any effect.

Thus, when in the way pointed out by the author, I diſcharged the machine upon the nerves of animals, which began to grow weak and languid, I could not obtain any effect, whilſt at the ſame time my exciter was employed with ſucceſs.

On the contrary, electricity acts when the exciter ceases to act, provided the fluid takes the course of the nerves, which should seem to prove, that it excites the muscular irritability itself; and the nature of the motions induced by it being, in some measure, different from the natural motions, might be considered as a confirmation of it.

Nevertheless, let us suppose (for the sake of argument) that electricity under every circumstance only serves to stimulate the nerves. The nerves certainly do not propagate this stimulus by tremblings and oscillations, for they are very soft, and exceedingly far from any kind of tension.

There is therefore a fluid which discharges this office.

It is of some consequence to reflect, that this fluid, which we call animal electricity, does not operate, notwithstanding the stimulus exerted upon the nerves, when it cannot follow the direction of the nerves themselves.

Let us recall to mind, that having made a ligature upon a nerve, and eſtabliſhed a communication above this point between the coating and nerve, the frog fell into convulſions; and that when the ligature was placed in contact with the muſcles, all motion ceaſed.

Let us likewiſe recollect, that when the ligature is at the leaſt diſtance from the muſcles, a very ſmall quantity of artificial electricity is enough to put frogs in motion, and that a ſtrong charge is neceſſary to produce the ſame effect when the ligature touches the muſcles.

As the ligature is conſtantly the ſame, and as we do not make any alteration in the conſtitution of the nerves, their manner of feeling muſt remain therefore the ſame, whether the ligature does or does not touch the fleſh.

Thus it is as evident as poſſible, that the difference of the reſult is owing to the

circum-

circumſtance of the electricity not finding the paſſage equally free and open in both caſes.

If it finds the obſtacle oppoſed by the ligature before it meets with a better conductor, that is to ſay, the muſcles, it then overleaps the obſtacle, and having penetrated within the nerves themſelves, finds a road proper for it, and of courſe does not deviate from it. But under oppoſite circumſtances, it abandons the nerves to diſperſe itſelf in the muſcles which afford it an eaſier paſſage.

To this explanation, which I have given in different terms in another place, it may be objected, that the animal electricity is not put into circulation when circumſtances are not convenient for it, and that this is the caſe here.

The ligature, I repeat, does not prevent entirely the paſſage of the animal electricity, and certainly a portion of it reaches

the

the nerves, but not in a quantity ſufficient to render itſelf perceptible. Beſides, the objection cannot by any means be applicable to artificial electricity. We are convinced that this paſſes, and even if we expoſe the frog to a certain current, we ſee ſmall luminous points iſſue from the extremities of the toes, or hear cacklings without the animal moving in the leaſt. Electricity then does not follow the nerves any farther than the ligature, when it touches the muſcles, for otherwiſe the movements would take place either by the irritation exerted upon the nerves, or by its immediate action on the muſcular fibrils.

That muſcles, which we ſuppoſe to be organs charged with electricity, can at the ſame time be likewiſe conductors of this matter appears a paradox. This however may be explained. The electricity which we ſhall call *proper* to the muſcle, is, as it were, ſhut up in, and governed by the nerves.

The nerves are ſo arranged as to appear only to conſtitute one and the ſame body with the fibres.

The nerves alone are capable of receiving their electricity. They alone are conductors of it, and it is by them alone that this circumſtance is effected. now we consider the fibres as electrical organs insulated by the nerves.

But the other parts which compoſe the muſcle, are not ſimilarly circumſtanced with the fibre. They are not electrics, and of courſe the electricity finds an eaſy paſſage through them.

To return to our preſent object. The action of artificial electricity as a ſtimulant of the nerves, does not become an argument againſt the theory of Profeſſor Galvani, for that does not exclude the influence of the native electricity.

After having done away the difficulties propoſed by the ingenious adverſary, I muſt take the liberty of aſking him, how it happens that prepared frogs ſometimes give ſhocks on communicating betwixt a a coated nerve and the legs, when immersed in water, without having recourse to a metallic conductor, but performing one's self the office of a conductor?

And why in these animals have we these phenomena produced constantly at the first moment by means only of a metallic conductor, without the nerves or the muscles being furnished with a coating?

SECTION

SECTION IV.

THE exiſtence of electricity in animals, the power they have of condenſing it, and the particular ſtructure of the nerves, by means of which they are enabled to conduct this fluid without its eſcaping into and being diſperſed among the ſurrounding parts, are three facts which form a ſtable and aſſured baſis for the doctrine of animal electricity.

We ſhall now diſcard from our recollection all the hypotheſes which have been imagined and advanced to this moment, concerning the nature of animal ſpirits, as their whole ſupport is that of authorities, abſtracted from which they remain naked and ſolitary.

I have already endeavoured to explain the influence of the electrical principle upon the animal economy; but as this was done in a curfory manner, I fhall here take up the matter anew, and this examination may be confidered as the final analyfis of our doctrine.

I propofe treating of mufcular motion, the fecretions, fenfations, and nutrition, both in the ftate of nature and that of difeafe.

From amongft the immenfity of objects which this widely-extended field prefents to my confideration, I fhall only felect thofe which appear to me moft relevant to the fubject, and moft ufeful to the art of healing.

Of Muscular Motion.

Phyfiologifts acknowledge the exiftence of three diftinct powers in mufcles, viz.

1ft, Contractility,

2d, Irri-

2d, Irritability, and

3d, Nervous force.

Of Contractility.

Muſcles poſſeſs contractility in common with every other part of the body, if we except the bones. Thus they yield to a certain point, to an extending power, and return upon themſelves ſo as to reſerve their original ſize as ſoon as the extending force is withdrawn.

But when the parts have taken on their original ſtate, this power does not remain inactive, but operates continually by making a gentle uninterrupted effort to bring the elements of the fibres into ſtill more intimate contact.

Thus, if we divide a muſcle in its length, whether in the living or dead ſubject, the divided parts recede from each other ſo as to leave a conſiderable ſpace between them.

If the tendon be cut in this way, the muſcle ſhortens itſelf, and in this contraction is capable of raiſing a weight hung to it more or leſs heavy in proportion to the quality of the muſcle it belongs to.

As this power, which may be called the *tonic force* of the muſcles maintains itſelf a long time after death, Baron Haller has thought proper to term it *dead force.*

Poiſons are capable of exciting it, but punctures, the application of air or fluids, or any other ſtimulus, provided it be not violent, is incapable of calling out its action.

This kind of contraction takes place by a ſlow and continued reciprocal approximation of the fibres.

Of Irritability.

The action which reſults from irritability, is very different from that we have juſt taken notice of.

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The muſcular fibres when irritated oſcillate, contract towards the middle, becoming by this means ſhorter, thicker, hard and wrinkled, and alternately withdraw themſelves from the middle with ſuch a degree of velocity as to elude calculation.

The ſlighteſt irritation, whether mechanical or chemical, is ſufficient to put the irritability into immediate action, which does not ceaſe till after a ſeries of alternate contractions and relaxations. The fibre then becomes as long, ſmooth, and lax, as it was before it received the impulſe.

The muſcles of dead ſubjects, or muſcles extracted from the bodies of live animals, retain this power a much ſhorter time than that of the *dead force*.

As the muſcular fibres in their action ſhorten and become thicker and rougher, it is natural for the muſcles themſelves to acquire the ſame properties.

Different effects will occur in these changes according to the difference of the ſtructure, diſpoſition and connection of theſe organs.

A muſcle in contraction does not preſs upon the blood-veſſels (vide Halleri Phyſiol. tom. iv. lib. x. Mot. Animal), ſo that circulation is not interrupted or altered, and conſequently the temperature of the muſcles remains the ſame.

Neither does their bulk experience any change, the thickneſs of the muſcle increaſing in proportion as its length diminiſhes.

Doctor Blane, in order to verify the latter circumſtance, placed half a live eel in a bottle, the neck of which by means of flame directed againſt it by a blow-pipe, he ſoftened, drew out and reduced to the ſize of the ſtem of a thermometer. He then filled it with water, and by the introduction of a wire, irritated the portion of of the animal, with an intention of throwing it into contraction.

Strong convulsions were excited, but he could not observe any change in the level of the water.

Borelli had before this remarked, that the water of a bath in which a man was placed by his directions, preserved the same height when he put his muscles into very strong contraction.

Swammerdam, Goddard, and Gliffon, made experiments likewise with the same view, the former with the heart of the frog, and the two latter with the arm of a man; but these essays were too imperfect and equivocal to admit of accurate conclusions being drawn from them.

The power of the living contraction, or, in other words, of the muscular irritability, is astonishing and incredible.

It is much more considerable in insects than in large animals, as there are instances among the former, of ſome which have drawn a weight ſeventy or eighty times greater than their own. (Valliſneri)

The degrees of this force increaſe under particular circumſtances, as, for inſtance, when the animal is agitated and put in action by the paſſions and emotions of the mind, as anger, fear, deſire, emulation, &c.

It increaſes in convulſions, ſo that a boy or girl of delicate conſtitution is ſometimes more than equal to the combined ſtrength of ſeveral grown perſons.

Again, it is prodigiouſly increaſed in maniacs and idiots, who have been found capable of breaking, by a ſingle exertion, a chain of iron, which might have reſiſted the united efforts of a couple of horſes.

Our ſurpriſe will not be leſſened if we conſider that the major part of the muſcles have their point of reſiſtance near the hypomochlion: That they are inſerted at very acute angles. That

That in ſeveral of them the fleſhy fibres are not in the ſame direction with the tendon, that others paſs over joints, and that all the muſcles in their action oppoſe a reſiſtance to the bone which ſerves them as a ſupport, and which reſiſtance deſtroys one half of their whole force.

Borelli, who has entirely de~~dica~~voted himself to theſe calculations, has demonſtrated that the abſolute force of a muſcle is the ſmalleſt portion of all the power it employs to produce an effect. I ſhall adduce the deltoide, as affording one of the moſt ſtriking inſtances of this nature.

"Let us ſuppoſe," ſays he, "the weight of the arm to be 4 pounds, and from the ends of the fingers be ſuſpended a weight of 22 pounds. Let us now examine the force by which the deltoide muſcle raiſes the humerus, ſuppoſing it to be raiſed by the action of this muſcle alone. Now, if the whole length of the arm be divided into twenty-ſeven parts, the length of the deltoide, from its origin to its inſertion, will be equal to ſix of theſe parts, and a force not of 28 but of 126 pounds will be required. If the angle of inſertion in the humerus is 10°, and it is really not more conſiderable the force will be as 1,736,482 is to 10,000,000, ſo 126 to 731 pounds. But as this weight ſhould be doubled, it will amount to 1462 pounds. Again, as the fibres of the deltoide unite in the tendon under another angle of 32°, the reſiſtance of the deltoide will be again increaſed, and amount to nearly 1680 pounds." To theſe multiplications the author adds that reſiſtance which ariſes from the different ſtrata of fibres, and finally eſtimates the force of the deltoide to be equal to 61,600 pounds. "Pone eſſe pondus brachii = quatuor libris, et præterea ex digitorum articulatione ultima ſuſpendi libras 24, quæritur vis qua deltoides

deltoides humerum e lavat, ſi ponatur a ſolo deltoide elevari: nempe ſi brachii longitudo fuerit partium 27, eſt fere deltoidi longitudo ab articulatione humeri ad inſertionem partium 6, adeoque vis requiritur non 28 ſed 126lb. Deinde ſi angulus quo inſeritur in humerum fuerit 10 graduum, neque enim major eſt, erit uti 1,736,482 ad 10,000,000 ita 126 ad 731lb. Cum præterea duplum ſumendum ſit hoc pondus, erit 1462lb. Denuo cum fibræ deltoidis ad alium angulum in tendinem coeant, ſit is angulus graduum 30, erit reſiſtentia deltoidis denuo aucta et fere 1680." Vide Boeraahve Praeect. T. iii. Muſc. Actio.

Phyſiologiſts being aſtoniſhed at the immenſity of theſe forces, have made every effort to diſcover the cauſe of them, and have believed they have ſucceeded in the attempt. However, when they have come to ſubmit their opinions to deliberate

exami-

examination, it has been found, either that they were repugnant with the ſtructure of the muſcles, or the laws of mechanics, or that they repoſed upon arbitrary data.

Baron Haller ſetting off from a known fact, that is to ſay, from the approximation which takes place betwixt the elements of the fibres during the moment of muſcular contraction, attributes contraction itſelf to an increaſe of the power of attraction which reſides, as he expreſſes himſelf, in the very nature of the moving fibre.

Dr. Fordyce has taken up the ſame doctrine, and it has likewiſe been embraced by Dr. Blane. This appears alſo to have been the opinion of Sir Iſaac Newton. and we also agree to it.

Of the Nervous Force.

When we make uſe of the term attraction, we only expreſs an effect. This effect

fect has a caufe, and this caufe we affert to be electricity. But in what manner, it may be afked, does electricity operate? Does it irritate the mufcular fibre at the inftant it paffes from one to the other furface of the mufcle, and that its equilibrium is reftored?

For a time I believed this to be the cafe, and it was likewife the opinion of Profeffor Galvani; but on coming to confider it with more attention, it appeared to me to contain infuperable difficulties, viz.

1ft, Each difcharge ought to be fucceeded by a new rupture of the equilibrium, which could not be effected with the rapidity we obferve in the movements, nor perhaps without a conftant lofs of this fluid, and likewife a more complicated architecture.

2d, The attraction would be the effect of a ftimulus, which does not accord with the idea philofophers have affociated with the term.

For

For my own part, I conceive the procefs is carried on in the following manner: The attraction of the fibrils is owing to the different ftate or condition of the electricity in the mufcles, that is to fay, that in the ftate of contraction, the furfaces of the fibrils are differently electrified from what they are in a ftate of relaxation. Agreeably to this hypothefis, the equilibrium never takes place.

That electricity increafes the cohefion of bodies, and that after a difcharge it may be found in two different ftates, are facts demonftrated by experiments fo decifive as to preclude every doubt. I fhall not hefitate to infert them here, as they form the bafis of my theory.

Mr. Symmer was the firft who difcovered the power which electricity poffeffes, of increafing the cohefion of bodies. This gentleman had obferved, that upon putting off his filk ftockings in an evening,

ing, they made a crackling noiſe, and that in the dark he could perceive them emit ſparks of fire.

This phenomenon, which he attributed to electricity, induced him to undertake a ſeries of experiments, which led him to the diſcovery of two ſingular facts, viz.

1ſt, That the electrical appearances took place only, when he made uſe of black and white coloured ſtockings.

2d, That ſtockings electrified in this way, reſiſt the application of a force to diſunite them.

The author made ſome experiments for the purpoſe of calculating the power of the electrical coheſion, and he ſoon found theſe ſtockings capable of raiſing near twenty times their own weight.

But on repeating theſe experiments in a favourable ſeaſon, and with ſtockings which were perfectly new, and the black dipped afreſh, and the white newly

cleaned

cleaned and ſulphured, or that were of a more ſubſtantial make, he found that the power of coheſion was increaſed to a conſiderable degree. Thus, under theſe circumſtances, he has been able to make the black ſtocking and the white, when the rough ſides of each were put together, raiſe (the half gauze) from twenty to forty, and of ſpun ſilk, from forty to ninety times its own weight. Phil. Tranſ. Anno. 1759, vol. li. p. 50.

The author conjectured from this moment, that the ſame effect, and perhaps a greater one might take place between two plates of glaſs, and ſome time afterwards he took an opportunity of putting this idea to the proof.

He took two panes of common window glaſs, and covered one of the ſides of each with thin tin-foil, leaving a ſpace of near an inch from the edges uncovered. He warmed them a little at the fire, and applying the

two

two bare ſides together, laid them upon four wine-glaſſes, which ſupported them at the corners. He then brought down a chain from the prime conductor nearly to touch the coating of the upper plate, and applying a wire which he held in his hand to the coating of the under plate, the machine was put in motion, and the electrification performed, as in the uſe of the common electrical pane.

When the operation was completed, he removed the chain and the wire, and taking hold of two oppoſite corners of the upper glaſs (thoſe correſponding to them in the other having been purpoſely cut away), he lifted it, and found that the under glaſs came up with it. The coheſion appeared to him conſiderably ſtrong, but he had not any proper apparatus to meaſure the ſtrength of it. He laid them down again on the wine-glaſſes, and procured an exploſion, as in the common electrical pane.

He then took hold of the corners of the upper glaſs and lifted up, but found that the coheſion was diſſolved, the under glaſs remaining behind. "Loco citato."

Father Beccaria repeated not only the experiments of Symmer with the plates of glaſs, but made others of his own, which prove that the coheſion far from being diſſolved by the exploſion is increaſed, and becomes more intimate in proportion as the diſcharge is ſtrong. The following are the experiments in the words of the author.

"Laminam vitream lævem A chartâ inauratâ rité indutam faciebam electricam immiſſo in ipſam igne à catenâ. Exutam unam ipſius faciem admovebam faciei ſuperiori laminæ ſimilis B utrinque nudæ, nec ullatenus electricæ, quæ facie ſuâ imâ circello emminebat chartaceo ſeſqui pollicari . . . .

Quum primo disjungerem laminam A à laminâ B jam percipiebam inſolitâ ali-

quâ

quâ vi coherere mutuo ambas: eamque cohesionem experiebar majorem, prout tardius disjungebam laminam A, post plures scilicet attractationes. Si cum primo laminam A imposueram laminæ B, induebam extimam hujus faciem, attractando simul indusia junctorum vitrorum quatiebar, valida repente existebat ab eâ explosione vitrorum cohæsio, eademque validior pro explosione vehementiore.

Duo vitra C et D singula rité induta singulis catenæ ramis objicebam, ut fierent seorsum electrica; atque ut æquilibratas electricitates servarent quales nempe eodem tempore a catenâ eadem immitti potuerant ea cautione, a catenâ ipsâ dimovebam, ut neuter ejus ramus cum solo communicaret, nisi post semota vitra ambo.

Unius superiorem, alterius inferiorem faciem denudabam: hoc illi imponebam: continuo cohæsio obtinebat aliqua: attrectatis extimis indusiis quatiebar, atque sub hac explosione cohæsio invalscebat."

The author having detached the plates A and B before the explofion, found that the plate A was pofitively electrified on both fides, and the plate B negatively electrified on both its furfaces likewife. When he feparated the fame plates after the explofion had taken place, the electricities appeared quite oppofite, that is to fay, the plate A was electrified negatively on both fides, and the plate B pofitively. Phil. Tranf.

In the experiment of Lane, the refult was the fame with regard to the electricity of the plates examined before, and after the explofion, as in the experiment of Beccaria.

But Lane placed the two furfaces which were not coated in contact with each other, and having coated the two outer ones, and charged them by means of the machine as a fingle plate, he placed one of them fo as to touch the prime conductor, whilft he touched the other with his finger.

Mr.

Mr. Henley treating in the ſame manner the Nuremberg glaſs, commonly called Dutch plates, found that each had a poſitive and a negative ſurface when ſeparated after charging, and when they were replaced and a diſcharge made, the electricity of both plates was exchanged for the contrary electricity. Phil. Tranſ. vol. lxvi. An. 1776.

As under theſe circumſtances, the electricity exiſts in two oppoſite ſtates, ſo the electrical appearances are renewed, and laſt until the fluid regains its equilibrium. This retentive power of electricity (to make uſe of the expreſſion of Symmer, who, as we before obſerved, was the firſt who diſcovered this property, and who right ſuſpected it capable of being the cauſe of many ſingular and curious phenomena), is conſtant in the muſcles, and laſts longer even than the life of the animal. The muſcles being conſtantly electrifed, the force of attraction is continually in action within them.

An opinion has been entertained by fome, that irritability is only a tranfitory attraction, but this is a miftake. If we cut the mufcle of an animal we obferve in a moment, the antagonift mufcle take on extraordinary movements, as there does not exift any longer a force capable of oppofing any exertion to it, and preferve a counterpoife.

Let us examine an athletic, nervous man, and in certain attitudes, we fhall obferve the bodies of fome mufcles become turgid and elevated, whilft their antagonifts become more flaccid than even during a ftate of reft.

Thus betwixt mufcles which are in a ftate of contraction, and thofe which are not, the only difference is as to the intenfity of attraction. This difference, however, is very great. Borelli demonftrated,

that

that a muſcle of a dead body might be very eaſily lacerated by a weight of a few pounds, which in the live animal it could have ſupported with the utmoſt eaſe. The experiments of M. Bertier have led him to make the ſame remark, and Dr. Blane has aſſerted the ſame fact.

It is notwithſtanding neceſſary to obſerve, that by this means we cannot obtain any accurate meaſure, as the diſtraction which the muſcle experiences is a cauſe capable of increaſing its living force.

The nerves are the only inſtruments of which nature makes uſe for the purpoſe of changing the ſtate of the electricity in the muſcles, and producing movements in them.

Nerves cannot fail of poſſeſſing an electricity of their own, and it is perhaps by means of this that they put the muſcular electricity in motion.

Thus if we cut, tie, wound, or in any

other way injure a nerve which is diſtributed upon a certain muſcle, this muſcle becomes paralytic and incapable of performing its office.

It has been ſaid, that the ſame accident conſtantly takes place upon tying or cutting the arteries of muſcles, but this aſſertion has been proved to be void of foundation by the obſervations of Kaaw Boerhaave, Langriſh, Pozzi, and ſeveral others. (See Haller, Phyſ. T. iii. lib. xi. ſ. 3.)

If palſy does ſometimes take place, it is not inſtantaneouſly, as is the caſe when the nerve is tied, but increaſes ſlowly, and therefore we may ſay that ſomething is wanting to the integrity of the muſcular fibre, or that the nerves are deprived of this ſubſtance, which is ſeparated by the ſmall arteries, and without which they cannot produce the phenomena of electricity. There are ſome animals which poſſeſs exquiſite irritability, and which, notwithſtanding,

withſtanding, have not any nerves. But from this obſervation are we to conclude, that irritability is independent of the nerves? Certainly not. I ſhould never be brought to ſay that I do not feel by means of the nerves, becauſe there are animals which have not any, and which, notwithſtanding, ſhew unequivocal marks of feeling. If theſe animals are really deſtitute of nerves, they are furniſhed with other organs which perform the ſame office. It may be objected that muſcles preſerve their power of motion when they are ſeparated from the animal, and no longer experience the influence of the brain. But what does this prove? Is it not demonſtrated by innumerable experiments, that the nerves retain their power after their communication with the brain is interrupted and cut off?

It is aſſerted that the nerves are the paſſage which the electricity takes, in order

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to gain the different ſurfaces of the muſcular fibres, and that it is in this way that it occaſions the alternate contractions and relaxations of the muſcles. But as theſe means of communications exiſt continually, the movements ought conſequently to be perpetual. This objection will doubtleſs be made by many, and for my own part I confeſs, I cannot remove it either by direct proofs drawn from the mechaniſm of the muſcles and nerves, or by experiments. Notwithſtanding, I muſt obſerve, that the ſame circumſtance which takes place in the electrical fiſhes, may likewiſe occur here. The electrical organs are charged, the way of communication between the ſurfaces electrified poſitively and negatively exiſt as they do in the muſcles. But theſe ways are only open to the will of the animal. This is a fact, and thoſe who acknowledge and feel the force of analogy, cannot refuſe their con-

currence

currence to this. Beſides, our unacquaintance with the means employed by nature in producing the movements, is not an objection againſt the principal object of our doctrine. If the means are not known, it is not on that account leſs true that electricity is the cauſe of the movements. Let another principle be brought forward which poſſeſſes the power of penetration neceſſary for inſinuating itſelf amongſt the infinite number of muſcular fibrils, which are in play during contraction; a principle which has the power of increaſing in an enormous degree the force of the muſcles, by increaſing the coheſion and attraction of their fibres, and which can produce this increaſe of force and of attraction ſo tranſient and momentary. Laſtly, I ſhould wiſh the quickneſs and rapidity of muſcular action to be explained by any other principle.

What an amazing ſucceſſion of contrac-

tion

tion takes place in an Engliſh race-horſe at full ſpeed, whoſe courſe outſtrips the velocity of the wind? What quickneſs of motion in the muſcles of a rapid ſpeaker, and in birds whilſt flying?

Of the Voluntary Motions.

The nerves are the inſtruments employed by the mind in the exerciſe of its power over the muſcles, which are under its command. The mind, however, is not acquainted either with the nerves or the muſcles, nor with their mechaniſm or force. This induced Leibnitz to ſay, that there exiſted a pre-eſtabliſhed harmony or eternal divine law, by which the body at a thought or inclination of the mind ought to take on motions correſponding with the will of this power.

The mind does not exerciſe any phyſical influence. It willeth, and motion takes place. This power of the mind is undoubted.

undoubted. It is equally certain that its commands are ſucceeded by a change in the origin of the nerves, that this change is extended along their courſe, and that the parts upon which they are diſtributed, are perceptible of its influence in a degree proportioned to the firſt impulſion.

The origin of a nerve is a ſmall organ of a very particular deſign. It is here that the mind holds its empire, though it is impoſſible to demonſtrate how this is effected, and beyond this point its power ceaſes. A proof of this aſſertion is evident, in my opinion, from the following obſervation:—Suppoſe a frog to be divided into two portions by being cut acroſs the body, its poſterior extremities, the nerves of which are ſent from the medulla ſpinalis, move about in the ſame way as if they were ſtill attached to the body of the living animal. The movements become ſtronger when preſſure is

applied

applied to the mutilated parts. Even if they remain quiet and be again handled, they awake as it were, and move as if to avoid being touched. If the body be divided in its whole length, including the head, the fore legs make the ſame motions with the hind ones.

In that caſe, the contact of air with the cut parts, or of any external ſtimulus, produces a ſimilar effect to what the ſentient principle would have cauſed in an animal in a ſtate of integrity.

But if we tie or divide the nerves near their origin, the motions ceaſe as in the living animal, under the ſame circumſtance.

Animals at the moment of their birth perform movements, which require a very elaborate and complex mechaniſm. Beſides, there are ſome ſpecies, which at this period, are as well informed as the individual which gave them birth. Theſe operations are the offspring of inſtinct,

and

and experience has not the ſmalleſt concern with them.

If, however, experience is not the firſt inſtructor of animals, at leaſt it generally contributes in the courſe of time to render them perfect; and man, the fancied lord of the creation, ſtands in greater need of its aid, than any of the ſubordinate animals.

The empire of the mind is extended by experience and practice. By their aſſiſtance it is that many animals attain to a management of their organs of motion, which at once aſtoniſhes and enchants us.

Animals which are not endowed with reaſon, or with a capacity for information equal to ours, when cheriſhed and modelled by the education of man acquire talents of this kind, but ſtill remain infinitely below us in the ſcale of being.

The motions which depend upon the power of the mind, frequently takes place, ~~in oppoſition to its efforts to reſtrain them,~~

when

when an impreſſion is made upon the nervous ſyſtem, whether by moral or by phyſical cauſes.

We frequently laugh involuntarily, and imitate the action of yawning. A perſon afflicted with epilepſy, when he ſees another ſeized with an attack of the ſame complaint, is frequently taken ill himſelf. Convulſions in the fair ſex are likewiſe ſometimes communicated by ſympathy, but more commonly are the effects of emotions of the mind.

We ſee depicted in the countenance of man, the different paſſions by which he is chagrined and controlled, in ſpite of his utmoſt exertions to conceal from us what paſſes within him.

Phyſiognomy informs us what others think both of our words and actions, and it too often happens, to our no ſmall chagrin and mortification, that we diſcover that language does not always correſpond

with

with the real ſentiments of the heart. By the help of phyſiognomy we are rendered capable of analyzing mankind, and aſcertaining without being ſcarcely ever liable to error, their genius and their talents, their virtues and their vices. The practitioner, by the different traits and air of the countenance of his patients, ſeizes the ſtate of the diſeaſe, and what room there may be for hope or deſpair. Hoſpitals and the ſick bed are the only ſchool in which this, I had almoſt ſaid divine, ſcience is attainable, for books only convey to the mind very obſcure and imperfect ideas of it.

The morbific cauſe which changes the features of the face in a thouſand ſtrange and extraordinary ways, by determining the action of the nerves to the muſcles of this part, frequently determines this action to other muſcles deſtined for the performance of voluntary motion, and gives riſe to contractions and convulſive movements, either partial or univerſal.

Epilepſy is a frequent occurence in infants attacked with acute fevers, as are alſo convulſions. At this period of life, theſe accidents are often occaſioned by a painful eruption of the teeth, or by irritation from worms. The preſence of acrid matters and poiſons in the inteſtinal canal excites terrible and frequently fatal convulſions. Certain changes in the conſtitution of the atmoſphere throw maniacs into a ſtate of diſquietude, anger, and tranſports of rage. Some individuals who are ſo organized, as to be ſuſceptible of excitement from the ſlighteſt impulſions, are equally affected by them. Some perſons, even from their peculiar ſenſations, almoſt to a certainty predict fair weather or rain, thunder or ſtorms of wind.

The phaſes of the moon have a decided influence upon nervous affections, and in

particular

particular upon epilepſy. Some author has denied the laſt fact, ſimply becauſe he was incapable of comprehending how it could take place, but this is a very indifferent reaſon.

The influence of the moon upon our globe, or rather the reciprocal influence of the two planets, is manifeſted by the flux and reflux of the waters of the ocean.

If electricity be not the cauſe of this phenomenon, at leaſt it does not take place without this principle being put into action; and how is it poſſible that man can avoid participating in theſe revolutions, who is himſelf an electrical machine, and who receives this matter from the air he breathes and moves in, and from the ground on which he treads?

The diſorders of which we are ſpeaking are not excited every time a cauſe is preſent, and frequently they occur without their cauſe being known.

We are unacquainted with what occurs in the nervous ſyſtem under theſe circumſtances, and conſequently cannot ſhew why ſometimes one affection ſhould exiſt, and at others a different one.

The number of theſe affections is very conſiderable, and their character is various, and often extraordinary.

Thoſe amongſt them which ſtrike me particularly, and arreſt my attention at this moment are, the catalepſy and ſomnambuliſm.

In catalepſy, the perſon who is attacked with it, retains the ſame poſition he was in the moment before the paroxyſm.

He remains as immmoveable as a ſtatue, his eyes almoſt always open. His joints are flexible, and give way to the application of a foreign power, ſo that if one raiſe up the arm of a cataleptic, it remains in this new poſition, and the ſame thing takes place with regard to the other limbs. The

reſpi-

respiration and pulse preserve their natural state, or at least deviate very little from it. The duration of the paroxysm is sometimes of some minutes, at others of some hours, and at others again of some days. It is very seldom that the patient is sensible of what is going on around him, or that he retains the slightest recollection of what happened in the fit.

In this disease, as we have just observed, the muscles remain in the same state of contraction they were put in by the animal, the moment preceding the attack. I have asserted that the relaxation of these organs depends upon a new circuit of electricity, and of a change of the state of this fluid. Thus the contraction will not cease until the will resumes its rights over the body it governs, and causes the circuit to take place, or till an extraneous force is employed instead of it.

From hence we may derive an explanation of the permanent contraction in the flesh of fishes, which are cut in pieces whilst alive, and likewise of the stiffness which is so remarkable in people who have died of the plague, or have been killed by poison.

The circumstances we meet with in the history of noctambulists, is still more extraordinary. When plunged in the most profound sleep they quit their beds, and with their eyes closed, walk about the house or go out, pass without accident along the most dangerous roads, and avoid the obstacles they may chance to meet with. But this is not all; they dress themselves, light fires, get on horseback, correct the animal, handle the reins, write, compose verses, and in a word, exercise many offices of life with much regularity and judgment. They are not easily awakened, and when they are, in general, have not any recollection of what they have been doing.

There

There is a phyfical caufe which determines all thefe different movements, by acting upon the fprings, which are already prepared for this purpofe. The mind has not any concern with it; on the contrary, in general every thing is performed without its being confcious of it.

Cataleptics and Somnanbulifts were very common during the reign of Mefmer. This celebrated impoftor took advantage of the credulity of mankind, and made a moft fhameful and infamous abufe of it.

Of Spontaneous Motion.

There are organs in the animal machine, the operations and movements of which are executed without the concurrence of the will, and on that account are called fpontaneous.

Different ftimuli determine the action of thefe different organs. When I fpeak of ftimulus, I underftand an impreffion made not upon the fibre, but upon the nerves of a given part, which in confequence throw the mufcular fibre into the ftate of contraction.

It is in this fenfe that I call the blood the ftimulant of the heart, the aliment, air, gas and excrement the ftimuli of the inteftenal canal, the urine that of the bladder, light the ftimulus of the Iris, and fo of the reft.

Whenever the nerves feel a ftimulus, the electrical fluid is excited, and then its effects are made manifeft. Under thefe circumftances that takes place, which we have noticed when fpeaking of voluntary motions, that is to fay, the electricity paffing from one ftate to another, and changing its mode of exiftence, produces the contraction. Its equilibrium is never eftablifhed. A new caufe again excites this fluid, and another new effect takes place, which is relaxation. The electricity,

electricity, exiſts again in two oppoſite ſtates; thus if it be again excited, the phenomena ſimilar to the former, will again recur. If, by any accident the fluid finds the equilibrium, the organ in which it occurs becomes paralytic.

I have obſerved in another place, that the muſcles not under the dominion of the will, are not charged negatively and poſitively, but that it is the nerves alone which are diſtributed upon them. This opinion is ſupported by many reaſons:

1ſt, The coating and exciter do not produce any change in theſe muſcles.

2d, Theſe muſcles do not poſſeſs ſo large a quantity of nerves, as is diſtributed upon thoſe which are ſubjected to the command of the will.

3d, The fabric of the former differs from that of the latter.

4th, Their movements are likewiſe different.

5th, The

5th, The electricity difcharged by the nerves, is a caufe fufficient to produce the effect fought for.

6th, Some experiments, as for inftance, that of the learned Cigna with filken ribbons, prove that electricity may exift naturally in fubftances in the two oppofite ftates.

One might eftablifh a theory upon more folid grounds, were one acquainted with the ultimate ftructure of thefe organs, and all the laws of the electrical principle, but being deftitute of thefe advantages, and proceeding with unaffured fteps, it is much to be doubted whether I have feized the truth.

It may be afked, whether an examination of the motions of every organ in particular, would conduct us farther in the difcovery? It appears to me, that it would produce a contrary effect, and embarrafs our imagination ftill more. We

fhould

ſhould ſee machines, which being impatient of the preſence of fluids, are agitated by a perpetual motion; others which generally remain in a ſtate of tranquility, and which only are called into action under certain circumſtances; others which are moſt active in proportion as the neceſſity becomes greater; and we ſhall likewiſe be witneſs to the ſingular and marvellous example of an organ, which at the inſtant it is affected by a particular ſtimulus, voluntarily yields to its impulſe, for no mechanical cauſe can produce any influence upon it, enlarges, and when it attains a certain term, which is not meaſured by its degree of diſtenſion, but by the lapſe of time, contracts upon itſelf, and reſumes its original ſhape and bulk.

Theſe different proceſſes only ſerve to render us ſenſible of the grandeur of the architect, and the ſublime and unbounded plan of his deſigns.

We

We ſhall now deſcend to the conſider-ation of the cauſes, which independent of natural ſtimuli have an influence upon the ſpontaneous motions; but this ſubject is only meant to be touched upon curſorily.

Theſe cauſes are,

1ſt, The animal fluids themſelves, which may have acquired any diſeaſed qualities, either at the inſtant of ſecretion, or after, by the admixture of other fluids, or by ſtagnation.

The bile, for inſtance, the urine, and the ſemen, under the circumſtances juſt mentioned, occaſion diſagreeable accidents, which are not always unattended with danger.

2d, Extraneous matters, either intro-duced or formed within the body.

Thus, poiſons taken into the ſtomach occaſion diſtreſſing vomitings, augment the action of the inteſtines, and invert their natural motion. Air, gaſſes, and other

other fluids, particularly poiſonous ones, injected into the blood-veſſels, or carried into them by the lymphatics, no ſooner touch the heart or even the arteries, than they contract with ſuch violence as to put a ſtop to all circulation, and ſo produce death.

The preſence of a ſtone in the bladder renders this organ very impatient, and throws it into frequent and inconvenient exertion.

3d, Miaſmata—Miaſmata have not an immediate action upon the parts, whoſe functions they derange, and on this account it is, that I believe them to conſtitute a cauſe apart, and diſtinct from thoſe juſt mentioned.

As ſoon as a miaſma is introduced into the maſs of blood, a variety of ſymptoms preſent themſelves, the moſt common of which is an increaſed action of the heart. Vomiting is likewiſe very frequent. Sometimes

times all the nervous action is determined to the ſtomach, without the exiſtence of any febrile ſymptoms.

It is by no means uncommon for all this diſturbance to happen in the inteſtinal, and ſometimes even the bladder is primarily affected by it.

4th, Hyſteric affections.—Theſe affections, or ſecret derangements of the nerves, are ſometimes communicated to one part, and at others to a different one.

Hence the vehement pulſations of the heart, continual vomitings, diarrhoea, cholicky pains, accompanied with throwing up matters of a greeniſh caſt, or of ſome other depraved quality or colour, ſymptoms of a nephritic paroxyſm, ſuppreſſion of urine, and illuſory ſymptoms of ſtone in the bladder, of the copious ſecretion of urine as limpid and tranſparent as water from the pureſt ſpring, hence involuntary floods of tears, and a thouſand

other

other diſorders. (Sydenham Diſſ. Epiſt. de Curat. Variol. nec non de Affect. Hyſter.)

5th, The Emotions of the Mind.—The recollection of our own misfortunes, or of thoſe of others produces tears in man, endued with a large ſhare of ſenſibility and compaſſion.

The heart palpitates under the impreſſions of fear and joy, and when theſe paſſions are very violent and ſudden, this organ being no longer able to reſiſt to the impreſſion ceaſes to act, and ſometimes even for ever.

The ſtomach revolts at the ſight of a corpſe disfigured by putrefaction, and even the bare recollection is capable of exciting nauſea and vomiting. Imagination alone is the ſource of a multiplicity of diſorders of this nature.

6th, The will of an Animal.—There are many perſons who can vomit when they pleaſe, and many inſtances are recorded of others, who have a power of rumination. The *ſnail* and the *tadpole* are able to ſuſpend the motion of the heart. A man of the name of Pené, who lately travelled in Italy, being intereſted in perſuading the public that he was capable, by means of a ſingular ſenſation excited in him by the magnetic fluid, of diſcovering metallic ſubſtances in the bowels of the earth, was much agitated whilſt undergoing the trial, and quickened the action of the pulſe ſo conſiderably as to impoſe upon many ſpectators, and even upon men of information.

The celebrated Italian Philoſopher Fontana, has likewiſe the faculty of exciting a febrile motion in his pulſe. The hiſtory of Colonel Townſhend will doubtleſs be remembered by many of my readers. This gentleman had for many years been afflicted with a nephritic complaint, attended

tended with conſtant vomitings, which rendered his life very uncomfortable. The moſt guarded mode of living, and the uſe of many different remedies did not afford him the leaſt relief, ſo that his diſeaſe was conſidered as incurable. Early one morning he called a conſultation of his phyſicians, and told them he wiſhed to communicate a ſingular circumſtance which had happened to him, viz. That he found he could die or expire whenever he pleaſed, and by a particular effort come back again to life. After having held ſome converſation on this peculiarity, he propoſed making a trial in their preſence, and with this view compoſed himſelf upon his back. In a ſhort time, he was to all appearance dead. The breathing, the pulſation of the heart and arteries were altogether ſuſpended, but remaining in this ſituation for ſome time, the phyſicians began to conclude that he had carried his experiment too far, and that he was abſolutely deprived of life. They were upon the point of leaving him, when they obſerved ſome motion in his body, and upon examination found his pulſe and the motion of his heart gradually returning, he began to breathe gently and ſpeak ſoftly. After this ſingular ſcene, the phyſicians talked with him, and then left him. He died in the evening of the ſame day, calmly and compoſedly. (Cheyne—Engliſh Malady.)

Baron Haller, who relates the hiſtory of Colonel Townſhend, in his Phyſiology, conſiders it as too much ornamented, and thinks that his diſeaſe was a ſimple fainting fit. But it is not wonderful that Haller ſhould put this conſtruction upon the affair, becauſe he would not on any account acknowledge the power of the mind over ſpontaneous or unwilling motions. We never make juſt concluſions if we are pre-occupied with favorite opinions.

It

It is not neceſſary to compoſe a ſeparate article upon the mixed movements; but it will be ſufficient for our purpoſe to remark, that the muſcles deſtined for this office, have a mechaniſm ſimilar to that by which the voluntary motions are performed, and that they are likewiſe ſubject to the influence of cauſes both phyſical and moral, in like manner with all the other muſcles.

Of the Sensations.

The brain, the medulla ſpinalis, and the nerves, are the inſtruments of ſenſation.

The brain is the chef-d'œuvre of the divine architect. It is here that we combine, imagine and create, and from hence emanated ſo many works which have been ſtamped with the ſeal of immortality.

We have only very limited ideas as to the ſtructure of this organ, and perhaps ſhall never attain ſuch a knowledge of its fabric, as to conceive and explain its mechanifm. The quantity of blood which paffes through it is enormous. It has been eftimated at one third, and in the more moderate calculations, at one fourth part of the whole mafs. This blood after having circulated through it, and afterwards defcended by the jugular veins to the heart, is fent back again feveral times to the brain. The artifice, which nature has employed for this purpofe, is amongft the moft fingular we have any knowledge of.

We fhall take fome notice of it in this place, both becaufe it is not fufficiently known, and becaufe it leads us to a refearch which is nearly connected with our fubject.

We are indebted to M. Cotunnio for this beautiful difcovery, and he owes it to accident. Being engaged in fome enquiries concerning the organs of the voice,

he

he removed a large portion of the cranium of a whelp, and expoſed the dura mater, where it covers the upper part of the brain. The longitudinal ſinus was almoſt altogether laid bare, and he imagined that he ſaw a pulſation in it; on applying the point of his finger, he became confident that there really exiſted one, which was obſcure and interrupted. In order to diſcover whether this proceeded from the motion of the brain, or was peculiar and proper to the ſinus, he made an inciſion into the dura mater, and bared the brain without injuring the ſinus.

He was now enabled to perceive the motion of the brain, and at the ſame time to judge by the touch of the motion of the ſinus, and became convinced that theſe motions were diſtinct from, and independent of each other.

To aſſure himſelf ſtill farther of this circumſtance, he divided the ſinus tranſverſely near the ſinciput, and ſaw with ſurpriſe, that when the dog made an inſpiration, the blood flowed ſlowly from it as happens in open veins, and when he expired, it eſcaped in jets as from a wounded artery.

On counting the jets from the moment of the beginning of an expiration to the commencement of a new inſpiration, he found them to be conſtantly three, but when the animal was dying they became ſo frequent, that his eye could no longer follow them.

The ſinus was divided into two portions, one towards the forehead, and the other towards the occiput, but the blood only flowed with force from the occipital part, which ſhewed it was thrown back to the ſinus by the ſuperior vena cava and jugular veins.

The doctor farther obſerved, that the jets of blood correſponded with the ſyſtole

of

of the heart, and that the motion of the jugular veins was ſynchronous to that of the arteries.

Being aſtoniſhed at this unexpected phenemenon, he endeavoured to diſcover by what mechaniſm this reflux took place, and as he enjoys an eminent ſhare of addreſs as well as of judgment, his efforts were crowned with ſucceſs.

Thus he obſerved, that the left ſinus of the heart when turgid with blood, as it always is in the time of expiration by the emptying of the pulmonary into it, puſhes againſt the poſterior paries of the right ſinus, which in conſequence is raiſed up an iſle or dam, which advances ſo as to occupy the paſſage which might remain between the two cavæ, and fills it up in ſuch a manner that it almoſt touches the anterior face of the right ventricle almoſt in its middle, and divides it into two parts, one ſuperior, and the other inferior. The upper current of blood entering into the ſuperior part of the ſinus, and meeting with this barrier or iſle, inſtead of deſcending to the bottom of the ſinus, is directed obliquely (for the iſle here preſents a convex ſurface) againſt the upper part of the auricle, but this cavity being likewiſe muſcular and irritable, at the inſtant the blood ſtrikes againſt it, contracts violently, forces it againſt the ſuperior cava, and makes it mount to the head. This blood is obliged by its gravity to fall again into the auricle, but is again chaſed upwards, ſo that it aſcends and deſcends alternatively, combated by the ſame powers till a new inſpiration takes place.

By the inſpiration, the air enters the thorax, the diaphragm deſcends towards the abdomen from behind forwards, compreſſes the extremity of the inferior cava, and cloſes its extreme orifice which opens in the bottom of the right ſinus. The

euſtachian

euſtachian valve which ſpreads itſelf before this orifice, cloſes all that the diaphragm had left open. At this moment as the left ſinus ceaſes to be turged with blood, the iſle diſappears, and the blood of the ſuperior cava not meeting any further obſtacle, deſcends and paſſes freely into the ventricle.

By this admirable mechaniſm, the two oppoſite currents of the ſuperior and inferior cava are introduced into the ventricle at diff'rent times and ſeparately, ſo that one current does not interrupt the entrance of the other.

The author has farther remarked, that the interior ſurface of the whole auricle is divided into two parts, the limits of which are marked out by a different fabric.

The ſuperior has lacertuli, the arrangement of which is ſuited for forcing the blood from below upwards, and the inferior part is diſpoſed in ſuch a way as to

impel

impel the lower current from right to left in the right ventricle. (Atti della Reale Accademia di Napoli, A. 1788.)

The blood which goes to the brain is, as we have before remarked, very abundant, the reflux of this blood by the jugular veins favours its accumulation, the vainy ſyſtem in this viſcus is ſo conſtructed as to be capable of ſuffering this accumulation. Do theſe circumſtances prove that the brain is the ſecretory organ of the nervous fluid, and that it furniſhes all the nerves with it?

The following reaſons militate againſt this idea.

The brain is not always in proportion to the force of the animal, nor to the ſize of the body, and of the nerves. (Monro's Obſ. on Nerv. Sept.)

There are certain animals which continue to exiſt whole days, and even months, although the brain, or even the head be removed.

removed. The Abbé Fontana, who has repeated theſe experiments in different animals, has obſerved that the body thus mutilated, walks, leaps, ſwims, breathes, turns in different directions, defends itſelf, is frightened, appears to have enjoyment, is irritable, in fine, continues to feel and judge as well as before. One would ſay, that in this ſpecies of animal, the brain is an appendix to the ſpinal marrow, rather than an eſſential organ.

But in anſwer to this it may be obſerved, that there have been likewiſe examples of well-formed fœtus, which, notwithſtanding, were deſtitute of brain.

" In children delivered at the full time, plump and well formed in their trunk and limbs, I have obſerved the ſubſtance which ſupplied the place of the brain not more bulky than a ſmall nut, and inſtead of containing a white medullary ſubſtance, it was of red colour, reſembling a clot of blood;

blood; and ſmall cords occupying the place of the optic nerves, were likewiſe of a red colour. Yet the ſpinal marrow and all the nerves from it, had the ordinary ſize and appearance."

"In a monſtrous kitten with two bodies, and appearance of one head, I found the ſpinal marrow of one of the bodies connected with a brain and cerebellum of the common ſhape and ſize, but the ſpinal marrow of the other body, though equally large, had only a ſmall button of medullary ſubſtance at its upper end, without a ſuitable brain or cerebellum." (Monro's Obſervations on the Nervous Syſtem.)

The ſame ingenious phyſiologiſt has repeatedly cut acroſs the ſpinal marrow or the trunk of the ſciatic nerve in living frogs, and fed the animal for upwards of a year thereafter. In ſome of them the ſciatic nerves were rejoined, but in none

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of the experiments did the nerves under the incifion recover their powers; yet the nerves under the incifion feemed at the end of that period, as large in the limb in which the experiment was made, as they were in the found limb. (Loc. cit.) Monro obferves alfo, that the fubftance of the nerves is not only medullary, but that it is mixed with a cineritious matter, which is furnifhed to them by the pia mater, and that it is from this membrane and from its veffels, that independently of the brain, they derive the energy and the principle of life which they poffefs.

Thus the great quantity of blood which goes to the brain, does not appear deftined for the general fecretion of the nervous fluid, but for the purpofe of furnifhing materials for the prefervation of fo large a mafs as thebrain.

It is however probable that the blood-veffels may have another office, viz. that of feparating the electrical fluid in the brain (by the brain I mean, all the maſs contained within the cranium), as well as in the medulla ſpinalis or nerves.

The brain in particular, at leaſt in animals, in which it forms an eſſential organ, ought to have an abundance of this fluid, as it is by means of this fluid, that it brings into play the ſprings of genius, and that it exerciſes its power and influence upon the different parts of the body. A want of this fluid ought to render a man leſs acute, ſlower, weaker, more ſtupid, and inactive. There are ſome individuals who abound with it, and the imagination of ſuch is fertile, its images beautiful and happy, and they are exquiſite painters of characters, paſſions, and of nature. Children who appear equally to abound with this fire, are very apt to diſcover and mimic whatever they ſee ridiculous in perſon or behaviour. Thoſe people who have moſt vivacity (and this vivacity, cæteris

teris paribus, proceeds from the nervous fluid), are particularly good mimics. Campanella knew how to aſſume the countenance, manner, and geſtures of the people whom he intended to examine, and by this very difficult art was enabled to enter into their thoughts and inclinations as effectually as if he had been changed into the very man.

When the ſecretion of this electrical matter in the brain is increaſed by any cauſe, the functions of this admirable machine becomes more prompt and energetic, and ſometimes even very violent. Wine animates the phlegmatic and indolent, ſhocks invigorates them, gives them wit and humour, and inſpires them with raillery and repartee.

It is particularly common to meet with people in intermittents, who poſſeſs a clearneſs of ideas and facility of expreſſion, which is not uſual to them at other times.

In

In the work of an Engliſh author, we meet with the hiſtory of a perſon to whom a blow upon the head communicated a degree of underſtanding he had not before the accident, and which left him as he recovered. (Robinſon, of the Spleen.) Hyſterical women have, in ſome of their nervous attacks, ſpoken languages they could not ſpeak when in health. (Tiſſot Maladies des Nerfs) Maniacs and hypochondriacs, and in general all thoſe whoſe brain is, if I may be allowed the expreſſion, in a ſtate of orgaſm, are moſt capable of extraordinary bodily exertions, and of ſuffering fatigue, cold, hunger, and watching. Some of thoſe have periodical attacks, which ſeem to ariſe from an accumulation of electricity in the brain. Dr. Simmons who has been in the habit of treating people of deranged underſtanding, informs me, he has obſerved, that after they have been for ſome time in a ſtate of tranquility,

without

without any obvious cauſe, they become uneaſy and reſtleſs, petulant and quarrelſome, and are ſeized with paroxyſms of anger and rage. This tumult ſubſides by degrees, and they reſume their tranquility. Thus, after the horrors of the tempeſt, the air becomes calm, and the ſky recovers its ſerenity.

Is it poſſible in this way to account for the periodical return of diſeaſes?

There is not any inſtant in life in which the brain ceaſes to be in action; conſequently the electricity by which it acts, ought to exiſt conſtantly in it in diſ-equilibrium. (Sbilancio.) I ſuppoſe that it likewiſe exiſts in the ſame way in the ſpinal marrow and nerves, with this difference, that in ſome of the nerves it is not excited, except under certain circumſtances.

Without dwelling upon this theory, I ſhall content myſelf with obſerving, that the brain, ſpinal marrow, and nerves have a ſpecific conſtitution, and that it is upon this that the mode of exiſtence of electricity in them depends. No reaſoning, no experiments will perhaps ever teach us how it happens that this fluid does not maintain its equilibrium in them, or how it comes that it can be found in a ſtate for conſtant action, or for acting only under certain circumſtances.

Iron, ſteel, and nickel, poſſeſs the power of magnetiſm, and they certainly enjoy it in conſequence of the diſpoſition and arrangement of their parts. But who will ever know the nature of this diſpoſition and arrangement of parts? If magnetiſm be an effect of electricity, as many circumſtances would lead us to believe, is it not a farther proof that electricity is capable of producing phenomena in bodies, according to their quality and ſtructure?

But although theſe reflections may appear plauſible, yet in order to eſtabliſh not

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the mode of exiſtence of electricity, but the identity of the principle, we muſt produce arguments of greater weight, and theſe may be furniſhed by the conſideration of ſenſations.

Let us imagine ourſelves placed upon the ſummit of a lofty mountain, which commands the proſpect of an immenſe plain, which preſents us with a world at one view. A thouſand different objects preſent themſelves at the ſame moment, and a thouſand different impreſſions are made upon the brain in the ſame ſpace of time, although we may not have diſtinct ideas of all of them.

A man who has an ear for muſic, hears a variety of tones and voices at the ſame inſtant, yet is capable of detecting the ſmalleſt error either in the meaſure of time, or in the accord of ſound.

Let us now aſk, by what medium the impreſſions made upon the retina, and thoſe upon the membrana tympani, are extended to the brain? And how all the other impreſſions on the nerves are communicated to the ſenſorium?

For the ſolution of this problem, Newton had recourſe to the oſcillations of the æther, which, according to this great philoſopher, was the cauſe of all the motions of the univerſe. But as this æther is not known, and as electricity explains theſe phenomena ſufficiently well, we ſhall ſubſtitute the laſt mentioned agent for the æther of Newton.

Since the medullary ſubſtance of the brain is of a fibrous compoſition, the threads of which are diſpoſed in a parallel direction, as is particularly obvious even to the naked eye in the corpora ſtriata, the thalami of the optic nerves eſpecially of fiſhes, in the fornix, when immerſed in nitrous acid for ſome time; ſince there are nerves in which this compoſition is ſufficiently manifeſt, as in the ſeventh and fifth pair (V. Haller, Prim. Len. Phyſ. cum notis Prof. Wriſberg) we muſt conſider nerves as an aſſemblage of parallel fibres.

Theſe threads or fibres are ſo many electrophori. The electricity of each is excited apart, and each apart impreſſes a ſtroke upon the brain, which is proportioned to the impulſion they receive, and to the excitement of their electrical matter. In this way one excites numberleſs diſtinct impreſſions.

Not only ſeveral impreſſions are made at the ſame time upon, but they are effected with a rapidity which belongs uniquely to the electrical fluid. One may hear three or four ſounds in ſucceſſion very diſtinctly in the ſpace of a quarter of a ſecond. Between the time of touching a body, and the conſciouſneſs of ſuch a touching taking place, there is not any intermediate ſpace which can be calculated by the known meaſures of time. Notwithſtanding, it appears that ſometimes this fluid does not poſſeſs its ordinary velocity, but even common electricity has ſometimes a loitering pace not eaſily reconcilable with its common immeaſurable velocity.

The inſtances of epilepſies being excited by an *aura* which, eſcaping from the extremities or ſome other part of the body, proceed ſlowly to the brain, have nothing to do with the nervous fluid, and do not prove its ſlowneſs. This *aura* (if ſuch be not an illuſion in the patient) is the effect of ſome other ſubtle principle formed in the nervous ſubſtance by ſome accident. It will be eaſy to convince ourſelves of this, if we conſider the matter more cloſely. The patient feels that the *aura* aſcends, there is then another fluid which makes a conſtant impreſſion upon the brain, ſo that he is advertiſed at every inſtant of what

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is taking place within him. He may ſometimes ſtop the progreſs of the *aura* by ligatures, frictions, or violent movements, but it is not ſo eaſy a matter to ſtop the progreſs of electricity. He may alſo ſometimes deſtroy the local diſeaſe by the application of bliſters, or the actual cautery. But theſe means appear only to change the diſeaſed diſpoſition of the nerve, and do not prevent the accumulation of electricity in it, as its power and energy are conſtantly preſerved. I will not deny that in the accounts of obſervations which relate to the nervous ſyſtem, we meet with ſome which are extremely embarraſſing, and appear to militate againſt my theory. But it muſt be obſerved, that nerves are not a dead ſubſtance, but that they poſſeſs properties, by which are capable of modifying their fluid, ſo that it does not apparently obey the general laws of electricity.

Amongſt theſe properties of the

nerves, the greateſt and moſt ſurprizing we know of, is that of feeling. They alone feel. Every other part of the body is deſtitute of this property. The nerves not only enjoy a ſenſibility common to all of them, but each has beſides a particular, diſtinct, ſpecific ſenſe, which belongs to it alone. Moreover, they are ſuſceptible of almoſt innumerable impreſſions and modifications, whence reſult ſo many different ſenſations.

"Quorum ego nunc nequeo cæcas exponere cauſas
Nec reperire figurarum tot nomina quot ſunt
Principiis, unde hæc oritur variantia rerum."

LUCRET.

It is aſtoniſhing with what facility this power of the nerves can be affected. Theſe affections may be reduced to two principal ones, viz.—To a diminution, or an augmentation of their energy.

A per-

A perſon who is abſorbed in profound and ſerious reflections, or who is ſtrongly afflicted with ſorrow, is not aware of what paſſes around him, objects under his eyes do not ſtrike him, and his food has ſcarcely the ſmalleſt taſte.

We ſometimes meet with patients who are not conſcious of any indiſpoſition, though their countenance, pulſe, and other appearances offer a proſpect of danger. Some become inſenſible to every thing they hold dear, regard every object, and hear every converſation with the utmoſt indifference, and even behold their approaching diſſolution without experiencing the leaſt commotion.

Senſibility moſt commonly increaſes.

It appears that the animal itſelf poſſeſſes a power of increaſing it according to neceſſity, by keeping the inſtruments of ſenſation in a very ſtrong tenſion, which happens every time it beſtows its whole attention

tion upon one particular object. It can likewife diminifh it, and mitigate by this means the pains by which it is afflicted, but with much greater difficulty and exertion.

It acquires the fame advantage by exercife and habit. A man born blind, for inftance, who continually exercifes his fenfe of touch for the purpofe of becoming acquainted with the fenfible qualities of bodies, brings it to fuch a degree of perfection, as even to diftinguifh colours by it.

The paffions in general render the fenfes more delicate and exquifite, and in this fituation the flighteft caufes may difconcert both the phyfical and moral parts.

Irritation of certain parts of the body do the fame, particularly of thofe organs endued with much fenfibility, and which fympathize with the reft of the fyftem, as the ftomach and uterus.

When I experience an indigeftion, the

mereſt trifles give me uneaſineſs, and as the mind is equally irritable, I become fretful, petulant, uncivil, and intolerable. The demon which afflicted Saul, probably was a bad ſtate of his ſtomach. A prince, a general, a judge, a phyſician, become very redoubtable and dangerous beings in a moment of indigeſtion. Women in general loſe their good temper during the time of menſtrual evacuation, and if they happen to be hyſterical, the mobility of the ſyſtem is very ſenſibly increaſed. They ſhed tears on the ſlighteſt occaſions, cannot bear to be trifled with, become gloomy and melancholy, are diſguſted with and fly from the pleaſures of ſociety.

Pregnancy renders them ſtill more unquiet and unſocial. In this ſtate, all their caprices muſt be overlooked and excuſed. I was once told by a married woman, that whilſt ſhe was pregnant, her huſband, whom ſhe loved with the utmoſt tenderneſs, nefs, became almoft an object of deteftation, and the innocent amufements of her children, on whom fhe lavifhed extreme affection and care, became tirefome and difgufting to her. Odours affect them ftrongly. Every fudden noife electrifies the whole machine. They lofe all relifh for the moft exquifite viands. They defire what formerly they difliked or detefted, and fometimes thefe defires are prepofterous. In a word, an extraordinary change takes place in their conftitution, which affects both the character and paffions.

The mother of Doctor Mantovani of Pegognago, a country in the Province of Mantua, during pregnancy, conceived an averfion for wine, and retained it even after delivery. She became pregnant a fecond time, and from the inftant of conception, fhe felt a return of defire for wine, and an infuperable averfion to water. This

repugnance

repugnance for water was fuch, and continued fo long, that when I faw her (many years after fhe had left off having children), fhe could not drink any without fuffering an indifpofition from it.

In the difeafes we call nervous, and in acute fevers, the fenfibility is fuch, that a flight current of air, whifpering, a very weak light, and even entire darknefs, irritate and difquiet them, brings on convulfions, phrenzy, and delirium. This happens particularly in hydrophobia and inflammation of the brain. Inflammation frequently increafes the fenfibility of the organ it affects, as in ophthalmia. A man with inflamed eyes could fee clearly in the dark, but on his recovery ~~curement~~ he was deprived of this privilege. (Nicolai Vom. Schmurze.—V. Haller, Phyf. T. iv. L. x. fect. 7.

We know tolerably well the caufes which may di~~ftingu~~minifh or increafe, or

change

change in another way the ſenſibility of the nerves; but we are unacquainted with what happens in the nerves, under theſe different circumſtances. Are the nerves more or leſs charged than commonly? or does the re-action of the brain occur with too much or too little force? Perhaps this laſt is the moſt common cauſe.

We know that the brain does not receive any impreſſion without re-acting upon the nerves. All that I have ſaid upon the effects of different ſtimuli, and of the paſſions upon the voluntary and ſpontaneous motions, afford an inconteſtable proof of this fact.

If this re-action did not happen, then the cauſes of which we have been ſpeaking, viz. the ſtimuli and paſſions would not induce any change in the animal economy.

The cretins of the Vallais who are inſenſible to the ſtimuli, which determine others

others to ſuch a diverſity of actions, and who may be ſaid to vegetate rather than live, ſubſiſting only by the aſſiduous attention of their parents, have the ſprings of the brain ill-arranged and inert.

One cannot perceive, entertain deſire, enjoy the ſweets of life, in ſhort, be capable of ſentiment when this organ does not re-act. It does not re-act, or at leaſt only in a ſmall degree in perſons whoſe figure is unexpreſſive, and theſe are generally ſtupid, or in other words, good ſort of people. Their heart is hardly moved, and only by ſtrong impreſſions, and its motions are merely tranſient.

On the contrary, a ſtrong re-action impreſſes on the front, in the eyes, in the whole countenance, the living characters of the ſoul; and an expreſſive phyſiognomy always indicatesa man ſuſceptible of paſſion. The violence of the paſſions is in proportion to the re-action of which we

are.

are ſpeaking, and of the mobility of the organs in which they are ſeated.

The heart where we feel chagrin, joy, emulation, glory, love, jealouſy, in a word, all the exalted as well as ignoble paſſions, is the machine on which the brain exerciſes moſt its re-action, and where this re-action is moſt durable and conſtant. It frequently renews itſelf, after it has once ceaſed, and this renewal is frequently the cauſe of an inward uneaſy feel, which we are at a loſs to account for, and conſider as portentous of new misfortunes, but which ought rather to be regarded as a repetition of paſt ones.

We have juſt been ſaying, that one property of the brain is that of re-acting (I am not aware of the mechaniſm of this re-action, but ſuch re-action is certain, and ſhould depend in great part on the ſtate of the electricity), that no change occurs when this re-action does not happen, and that the changes are moſt ſenſible when the re-action is moſt ſtrong. May we conclude from theſe facts, that the ſenſations are not formed at the moment the brain receives the impreſſions of objects, but rather during the re-action made upon the different parts in which we feel?

I will not take upon me to decide on ſo difficult and delicate a queſtion, but as it is allowable for every one to advance his opinion, I ſhall not heſitate to give mine, which is, that I am more inclined to believe, that every point of a nerve is a ſenſorium than to limit the ſeat of the ſenſations to the brain. The brain is one of the inſtruments which produce them, and without doubt one of the moſt eſſential, and without which no change can happen of which the animal has a conſciouſneſs. This opinion does not include any idea contrary to the nature of the ſoul. If its empire be extended, it does not follow that it ſhould be ſo too.

Thoſe phyſiologiſts, who for fear of according to the ſoul, the properties of matter have confined it to a point of the brain, have proved themſelves bad philoſophers. It is as abſurd to ſay, that the ſoul reſides in one as in every part of the body.

As the nerves are the only parts of the body which feel, all impreſſions muſt neceſſarily be made upon them, and their affection muſt be the cauſe of the changes which happen in the regulation or government of the different functions of the body. It is upon this principle that modern phyſicians, after combating and renouncing the errors taught in the ſchools of the humoraliſts, have eſtabliſhed the doctrine of diſeaſes.

The doctrine is not novel. Phyſicians of obſervation have for a very long time back obſerved, that the nervous ſyſtem is firſt attacked in diſeaſes.

Morton

Morton particularly entertained this opinion, and no one has given ſo many facts in ſupport of it as he has done.

"All diſeaſes," "ſays he, whether primary or ſecondary, ariſe immediately from a derangement of the ſpirits, as do likewiſe mediately diſeaſes of parts."

"Quod ad morbos attinet primario univerſales, et acutos quorum primum vel inſultum comitantur vehementia ſymptomata: illi certé a ſegni humorum maſſâ quippe particulis conſtat craſſis, et maximâ ex parte motu tantum aliunde communicato præditis, in inſtanti oriri, concipi nequeunt, ſine ſpiritibus eam agitantibus: præſertim ſi ſerio perpendatur, quomodo ſanguis humanus ac alii humores præparari poſtulant antequam aliquam inſignem mutationem ſubire obſerventur. Pertinet huc atque digniſſimum eſt notatu, quod a principio horum morborum tota tragœdia in genere nervoſo agitur, antequam ſanguinis maſſam turbari pulſus indicat, vel aliquam magnam mutationem pati. Quod quidem in variolis, morbillis, et febribus cujus cumque generis quotidie conſpicitur, ubi æger primo momento dolore capitis corripi ſolet, et levi quadam vertigine motu ſpirituum inordinato, et præternaturali fibrillarum cerebri et ejus membranarum diſtentione inde facta; deinde laſſitudine ulceroſa (quale ſymptoma fatigatis accidere ſolet præter inſolitam ſpirituum agitationem, et tranſitum per membra nimis acceleratum, et quaſi exploſivum fibras ultra tonum naturalem diſtendentem) frequenti erum oſcitatione, ſeu motu quodam convulſivo muſculorum maxillam inferiorem deprimentium, ſæpius repetito laborat et tandem frigore ſubito per totos artus diffuſo (quali ſymptomati hyſtericas etiam lipothymicas a mera ſpirituum ἀταξία frequentur obnoxias videre eſt) horrore item, rigore, motu tremulo, et convulſivo

artuum,

artuum, lumbagine a motu elaſtico ſpirituum animalium præter modum agitatorum et a medulla ſpinali in muſculos propinquos exploſorum, comate, vigiliis, deliriis, nauſea, vomitu, ægrotatione univerſali, quam Ventriculus cum tunicis ſuis nerveis, velut commune ſenſorium, præ reliquis partibus ſenſit, aliiſque id genus ſymptomatis quæ ſpirituum officinam, eorumque canales unice afficere deprehenduntur, etc."

The author afterwards remarks (I paſs over in ſilence what he ſays with reſpect to chronic affections, as it is only my buſineſs at preſent to ſpeak of acute diſeaſes), that contagious effluvia could not inſtantly produce any alteration in the blood without the intervention of the nerves, that many diſeaſes ariſe from the paſſions of the mind, and the blood is affected in them by the influence of the ſpirits, and that although there may exiſt caufes capable of altering the mafs of fluids, yet it is always true, that the animal fpirits are the immediate principle which forms the difeafe. He is moreover confirmed in thefe fentiments on finding, that by the new theory he can explain phenomena which were impoffible to be refolved by the obfcure doctrines of the ancients. The ancients, for inftance, explained by fympathy, the vomiting which follows a concuffion of the brain, as well as that which attends nephritic affections; but Morton obferves, that in order to explain thefe accidents, we muft admit a continuity or connection of the whole body with the brain. With the fame principle he explains the metaftafis or tranflation of a difeafe from one part to another, which is obfervable not only in the crifis of fevers, where the difeafe quitting one region all of a fudden attacks the brain, but likewife in *peripneumony*, which frequently

changes

changes into angina; in the unſettled rheumatiſm, and particularly in the ſpurious or nervous, in which the diſeaſe changes every day or every hour, ſometimes even quicker than ſight, and paſſes from one limb to another. In the blood all the fluids are confounded, united, and agitated by a quick uninterrupted motion, and afterwards are equally diſtributed by innumerable veſſels to every part of the machine, ſo that it is not poſſible for any morbific matter to be ſeparated from it, with ſo much rapidity, and be afterwards determined to ſome particular part. This, however, does not prevent the ſpirits when affected by the diſeaſe being carried with a certain impetus, firſt to one part and then to another alternately, and to diſtend, vellicate, throw theſe parts into ſpaſmodic contractions, and produce in them a tranſient diſeaſed affection.

And moreover theſe ſpirits determinate

the mafs of blood and humours which circulate through the body, and imprefs upon them a difeafed character, and it is in this way that metaftafes take place. (De morb. univerf. acut. apparat. curat. morb. univers.)

Morton likewife makes many other judicious reflections in the courfe of the work we have juft quoted, and fubjoins feveral others in his Differtation de Cortice Peruviano atque virtute ejus febrifuga. It is here that he obferves, that poifons act immediately upon the animal fpirits or principle of life, and that at the very inftant the poifon is fwallowed, the patient is attacked with a fever of the malign kind, and other diftreffing complaints. And he likewife obferves, and with propriety, that the action of topical applications throws much light upon his theory. "Quantum enim cerebrum atque genus nervofum afficiuntur ab emplaftro de Gal-

bano

bano umbilicali et a variis odoramentis fœdis aut moschatis ut in hystericis videre est? Quantum a cataplasmate ex allio plantis pedum applicato, paucarum horarum spatio, natura patitur, ut facile cum suo damno quivis in se ipso experiri potest? Quantum denique a suppedaneis, pericarpiis, vesicatoriis, epithematis, cataplasmatis, atque emplastris variis curatio febrium promoveri soleat medicis tyronibus satis notum est. Num autem ab hisce externis massam sanguinis de repente mutatam atque alteratam esse facile quis credat? Multo clarius mehercule! hujus rei ratio reddi potest ab effluviis horum remediorum exterius applicatorum transmissis per poros cutis ad spiritus in fibrillis membranarum hospitantes, unde fermentum in partibus nervosis delitescens immediate, secundario vero massa sanguinis alterari facile potest." (l. c.)

The solidists of the present day rest their theory

theory upon thefe obfervations of Morton, with this difference, that they reject the depravation of the animal fpirits which this phyfician admitted, and inftead of it believe that miafmata affect the nerves alone, and not their fluid. This doctrine is more fimple, more conformable with facts, and with the recent difcovery of the nature of the nervous fluid.

All medical men, however, are far from being convinced that the impreffion of miafmata is only made upon the nerves, and there are many who ftill retain the errors of the humoral pathology. If, notwithftanding, they would take the trouble of examining the hiftory of difeafes, they would foon lay afide their prejudices. It would be the greateft poffible gratification to me to attempt a work with this view, but I muft not flatter myfelf that a phyfician whofe reputation is confined within very narrow limits, would be able to perſuade or make many proſelytes to his opinions. I will, notwithſtanding, dare to preſent a few conſiderations on this head, and recommend it to thoſe who poſſeſs reputation, genius, and a fund of information, to give to the public a detailed and finiſhed ſeries of others. A work of this kind would be particularly uſeful, and give the laſt blow to the pernicious ſect of humoraliſts.

Diſeaſes in their commencement have not any proper and diſtinguiſhing ſymptom, but almoſt all of them exhibit a ſimilar appearance, whence it happens that a phyſician is not always able to judge of their character on their firſt attack.

Certain impreſſions made upon the nerves of different organs, and by different cauſes are frequently followed by the ſame effects.

The powers of the patient are commonly much weakened from the firſt attack, but

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ſometimes on the contrary are increaſed, and again ſometimes are preſerved altogether as in the natural ſtate.

In the ſmall pox it frequently happens that ſome children for three or four days before the appearance of the fever become leſs gay and lively than common, whilſt on the contrary, others of a more lax and phlegmatic temperament acquire from the diſeaſe, a vivacity, cheerfulneſs, and every improvement of complexion beyond what nature had originally beſtowed upon them. (Tiſſot de la petite Verole.) In the epizootic which reigned in the meridional Provinces of France in the year 1774, Vicq d'Azyr obſerved that ſome of the cattle for ſome time previous to their being attacked with this diſeaſe, were more dull and feeble than uſual, whilſt others abandoned themſelves to extraordinary and diſorderly actions, as in galloping, leaping, and ſcratching the ground

with

with their feet. And in the following year, ſeveral, even during the height of the diſorder, preſerved their ſtrength in ſuch a degree as to attempt to attack thoſe who approached them.

Some inhabitants of Nimeg~~a~~uen attacked with the plague, immediately loſt their ſtrength, and became incapable of moving, whilſt others retained their powers to the very moment of diſſolution. (Diemerbroeck de Perte, l. i, cap. 3.)

In this terrible diſeaſe, the infected frequently fall victims to its violence in the midſt of their uſual occupations, and die in the ſtreets, churches, &c.

In the plague which committed ſuch horrible ravages at Bruſſels in the year 1502, people died whilſt eating and drinking as ſuddenly as if they had been ſtruck with lightning. During the plague at Smyrna, I have ſeen unfortunate wretches walking in the ſtreets without being conſcious

ſcious of their being diſeaſed, whilſt a certain paleneſs of the face, diſordered look of the eyes, and other appearances difficult to deſcribe, announced to the by-ſtander the terrible fate which awaited them.

A diſeaſe does not always preſent itſelf under the ſame aſpect, but is capable of aſſuming a vaſt variety of forms. This has been particularly obſerved in periodical fevers, and it is to Morton and Torti that the medical world is indebted for this important truth.

If the miaſmata which cauſe periodical fever, can alſo produce the characters of continued fever, of pleuriſy, peripneumony, conſumption, apoplexy, hemicrania, vomiting, cholera, colic, diarrhoea, general or partial ſpaſms, convulſions, of pains like thoſe of labour, of rheumatiſm, &c. it is ſufficiently evident that the mode of action of the miaſmata

mata upon the nerves is ſufficient to give riſe to a great variety of accidents, or in other words, of different diſeaſes.

We have already obſerved, that the brain conſtantly re-acts, and that a great variety of changes in the animal economy proceeds from this re-action. Hence it is natural to believe, that this organ does every thing in diſeaſes.

A diſeaſe frequently changes its character in the ſame individual, either ſpontaneouſly or by ſome change in the air, or diſorderly conduct of the patient, or ſome miſtake of the phyſician.

Sydenham ſays, that it is difficult to recognize intermittent fevers, particularly autumnal ones, as they appear like continued fevers, and one can ſcarcely diſcover any remiſſion in them, but by degrees they perfectly intermit, and put on a form entirely agreeable to the ſeaſon of the year. And he likewiſe obſerved, that ſometimes fevers after one or two paroxyſms which

which were ſucceeded by a complete apyrexia, became continued as in the epidemic in London An. 1678.

It is particularly in autumn, and during great heat that fevers vary their type and character. This proceeds from a change which the conſtitution of ſome individuals experiences under theſe circumſtances, and not from any alteration in the nature of the miaſmata. As a proof, the Peruvian bark is a ſpecific for a quotidian fever, as well as for a tertian, quartan, or thoſe of longer periods, and in continued itſelf, or other diſeaſes of the ſame claſs.

What we have ſaid of intermittents, may alſo be applied to other acute diſeaſes, which take on different forms under different circumſtances. The epidemic fevers in London in the years 1667, 68, and part of 69, were the ſame diſeaſe with the ſmall-pox, which reigned at the ſame time, and both gave way to the ſame

remedies,

remedies, except that the eruption which took place in the latter required an additional indication.

The diarrhoea which was epidemic in the year 1668, was likewiſe produced by the ſame cauſe as the fevers and the ſmall-pox, and was cured by the ſame means, that is, by blood-letting and the refrigerant plan. (Sydenham, Variol. regul. & feb. cont. Ann. 1667, 68, and part 69.)

Were the writers upon epidemics after Sydenham equally well acquainted with the *genius* of the diſeaſes, or did they adopt his practice? By no means. Their prejudices reſpecting the acrimony, &c. of the fluids, frequently made them ſtray from the path of truth, and led them into a plan which proved either uſeleſs, dangerous, or fatal.

Fevers ſometimes lay aſide their own character without any apparent cauſe, and aſſume a new form, which may impoſe upon those who have not a ſufficient ſhare of judgment and experience. Mr. Attwood, aged 60, who lived in Cheapſide, after having had the fever for three or four days appeared to be better, but was ſeized almoſt inſtantaneouſly with ſpaſms of the ſtomach and inteſtines, vomitings and extraordinary dejections per anum, as in the cholera morbus, which put him into a ſtate of imminent danger. Some medicines, amongſt which was liquid laudanum, abated the violence of the ſymptoms. When the paroxyſm was over, the phyſician ordered a preparation of bark, with a view to prevent a new attack, and in this he was not diſappointed, as the cholera never again returned, but the diſeaſe preſented itſelf under the form of a regular tertian. (Morton, de Protei form. feb. intermit. Gen. Hiſt. 7.)

It very ſeldom happens that a periodical diſeaſe changes its character to take on that

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of an intermittent fever; notwithſtanding, there are ſome inſtances of it, and I will mention one. Mr. Hamms, who lived in Bull and Mouth-ſtreet, had a lancinating pain in the right hypochondrium, without fever, cough, or any other mark of diſeaſe. The urine only was very high-coloured, which induced Morton to believe that the liver was affected; afterwards a variety of ſymptoms was preſented, as difficulty of breathing, fever, diarrhoea, &c. The patient had already taken many medicines without effect, when the phyſician diſcovered he was miſtaken in his idea of the diſeaſe, which was of the intermittent kind. "Hoc facto," ſays Morton, with a candour which does him honour, "docte eſt argute delirans quippe falſa principia ponens et apparentibus ſymptomatis deceptus, ægram febre, vigiliis, deliriis, ac dolore pene confectam ad orci fauces duxeram, donec febre quotidianis, et ſtatis periodicis paroxyſmis jam tandem typum, ac genium ſuum palam prodeunte, ſuſpicatus ſum fermentum febrile venenatum deliteſcens, et ſpiritibus implicitum, ſymptomati huic dolorifico admodum moleſto, anſam præbu iſſe. Feb. Int. genio. Exerc. 1, Hiſt. xx.

As the bark after the developement of the diſeaſe was evidently indicated, ſo it was preſcribed, and the patient recovered her health.

It is very common to ſee intermitting degenerate into continued fevers, and even of a malignant and dangerous nature, in conſequence of ſome irregularity on the part of the patient. Purgatives given at an improper time, diaphoretics and blood-lettings when they are not indicated, have the ſame grievous conſequences. With regard to blood-letting, it is worthy of remark, that if blood be drawn by cupping glaſſes, the evacuation is not ſo hurtful as when made by a large vein; and that it is ſometimes even uſeful, as has been obſerved by ſome practitioners, among whom we may count Ramazzini. An evacution made from a large vein is frequently followed by faintneſs and ſwooning, ſymptoms which announce a change in the nervous ſyſtem, and which does not happen when the blood is taken from ſmaller veſſels. It is to this change that the bad conſequences are owing. And this, if I am not miſtaken, is the reaſon why perſons who have been attacked by the plague in certain caſes, have expired immediately after having been bled in the arm, or during the operation.

But theſe diſorders themſelves, and very active medicines, may ſometimes give riſe to ſuch a favourable revolution in the ſyſtem, as to bring back the action of health. Many perſons are in this way cured of periodical fevers, and of other difeafes ftill more terrible. M. Ab. Elten difcovering that he had a peftilential bubo, fwallowed a decoction of tobacco, which threw him into an agony, and fuch lofs of ftrength, as to bring on a fwoon, from which it was thought he would never recover. Sometime after, however, he came a little to himfelf, vomited exceffively, and had afterwards feveral very confiderable evacuations by ftool. He was put to bed in fo weak a ftate, that he could not move, but his bubo had difappeared. He drank fome hot wine with fpice in it, flept well, and fweated much; on his awaking he drank more wine, fell afleep again, and in the morning found himfelf quite recovered. (Diemerbroeck de Pefte, Hift. 40.)

Nappeltern finding himfelf attacked with the plague, went into a convivial company in hopes of difpelling his apprehenfions, and getting rid of his complaint.

Having drank much, and being heated by the wine, he was ſeized with vomiting; when this went off, he rejoined his company, and reſumed his glaſs. Having drank ſo much wine as to exhilarate him, he returned home, went to bed, fell aſleep immediately, ſweat in his ſleep, and in the morning found himſelf very well, having neither bubo nor any other ſymptom of the plague. (Loco Citato. Hiſt. 55.)

In the caſes juſt mentioned, was the miaſmata deſtroyed? I do not ſuppoſe it— a miaſma is not eaſily deſtroyed. Was it then diſcharged out of the body? This is not more likely than the former. What idea are we then to form of this affair? The condition or diſeaſed ſtate of the nervous ſyſtem is changed, and the miaſma can no longer act upon it. This idea is not ſolitary, but ſupported and confirmed by a variety of facts and obſervations, of which we ſhall mention a few. Blows on the head, exceſſive fear, violent fits of paſſion, have frequently cured dangerous and obſtinate fevers. The great Fabius (ſays Van Swieten) being ſent againſt the Allobroges, and the inhabitants of Auvergne, was freed from a quartan ague by his attention to the ſword.

Vicq d'Azyr relates a fact which is not foreign to the ſubject. An ox attacked by the epizootic, which exiſted in France in the years 1774-5, was led to the ſide of the ditch in which it was intended to bury him, after having diſpatched him by a blow on the head. The operator being an aukward fellow, ſtruck him obliquely upon the ſide of the cranium; the animal became furious, broke the rope with which he was faſtened, and eſcaped immediate death, by flying into a neighbouring foreſt, where he was ſeen ſome days after. The doctor does not ſay whether

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this animal died or recovered, but he adds, that he ſaw many caſes where inſufficient blows had been given, in conſequence of which diſcharge took place from the noſtrils, and they got well of a diſeaſe which was conſidered as incurable.

It is well known that an emetic frequently cures intermittents, and theſe cures ought certainly not to be attributed to an evacuation of depraved humours from the ſtomach, for it is not there that the miaſma exiſts. Morton, ſpeaking of the cures of fevers effected by the Vinum Benedictum, attributes them rather to the orgaſm excited in the animal ſpirit during the action of vomiting, which orgaſm likewiſe frequently cures other nervous diſorders.

The courſe of diſeaſes in general is confined within certain determinate and fixed limits. Sydenham having made this obſervation, in order to explain it, had recourſe

recourſe to the effervefcence of the blood, which, in his opinion, only could be effected in a certain ſpace of time.

It is not to the fluids that the exiſtence of this phenomenon is to be attributed, but to the very nature of the nervous ſyſtem. If we pay attention to the affections of this ſyſtem, we ſhall find that they obſerve almoſt always an order and regularity in their periods, and we ſhall likewiſe ſee that many of the operations in the healthy animal obſerve the ſame laws. A ſingular obſervation is made by Sydenham, which is not at all favourable to his theory, viz.—That the uſe of clyſters and cathartics in the decline of continued fever, has ſometimes relieved the patient, and produced a complete apyrexia, but that in a day or two the fever has recurred with the ſame ſet of ſymptoms, attending its firſt attack, and its progreſs and duration were the ſame with the firſt. " Illud porro obſer-

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vatu dignum eſt, quandoque accidere, ut æger clyſterum, aliorumque catharticorum uſu circa declinationem morbi in tempeſtive præſcriptorum parum alleviare videatur, imo nonnunquam apyrexia omni moda frui, poſt diem unum alterumve ſentiens, non tam priſtinam febrem vires redintegraſſe, quam novam accendi, rigor nimirum atque horror ſubito invadet, quem mox excipiet calor atque febris, idem ſtadium (niſi forſan in intermittentium claſſem ſe reponat) decurſura, quod in præcedentibus monſtratum fuit. Cum ita ſe res habeat non aliter tractandus eſt æger, quam ſi antea febre non detentus fuiſſet verum quoad res agendas calcata jam veſtigia repetenda; deſpumatio enim quæ cæptæ jam ebullitioni debetur non niſi prædicti temporis, ſcilicet 14 dierum ſpatio peragetur, utcumque moleſtum fuerit ægro, a prægreſſo morbo ſatis jam debilitato, eouſque ſanitatem expectare." (Febris continua, An. 1661, 62, 63, 64.)

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If the effervefcence had been carried almoſt to its term, how could it begin afreſh, and go through the ſame courſe, so was to produce a fever in every reſpect ſimilar to that which preceded it?

This cannot be conceived. But we can conceive that the nervous ſyſtem gave riſe to this reproduction, becauſe its morbid diſpoſition ſtill exiſted. Suppoſe two men, juſt recovered, one from a tertian and the other from a quartan ague, be expoſed to the action of intenſe cold, they will both of them be again taken ill, one of his tertian and the other of his quartan. Is it not evident that this relapſe is owing to the diſpoſition of the nervous ſyſtem? Have we not many examples of this nature in nervous diſeaſes? An impulſion is only wanted to produce their evolution.

We ſhall here obſerve, that fevers do not always ariſe from miaſma or contagion, but are frequently the conſequence of a violent ſhock. Theſe fevers have likewiſe their regular periods and duration. Van Swieten gives the hiſtory of a girl, who, when in perfect health, was frightened at the ſight of a ſquirrel, and ſeized with a quartan ague. She was cured of it; but by ſeeing a dead ſquirrel was again attacked by a return of it. The Abderites were all taken ill of a fever, when the celebrated tragedian Archelaus repreſented the fable of Andromeda, and all were recovered of it on the ſeventh day.

When a diſeaſe ceaſes after having run through its ordinary courſe, it does not ceaſe becauſe the miaſma has loſt its power, but becauſe the nervous ſyſtem is no longer ſenſible of its impreſſion, or becauſe the morbific impreſſion made upon the brain ceaſes of itſelf. This propoſition may be conſidered a conſequence of the foregoing facts and caſes.

That a convaleſcent retains within him the ſeeds of the diſorder from which he

has

has juſt recovered, is demonſtrable from a well-known fact, viz. that he is capable of infecting other people by his breath, ſweat, or even by ſimple contact. Another obſervation, not leſs in point, is, that perſons lately recovered from an infectious diſeaſe, may, without riſk of being attacked by it a ſecond time, live with, and attend upon others ſtill labouring under its influence.

Relapſes are in general owing to ſome irregularity in the patient's mode of life, or to ſome violent affection of the mind, and happen during convaleſcence, when the nervous ſyſtem is not re-eſtabliſhed in its natural ſtate.

When the ſyſtem is once thoroughly re-eſtabliſhed, the ſame diſeaſe is not eaſily reproduced. Sometimes, even it does not again attack during the whole courſe of life, having left the living ſolid in a ſtate incapable of receiving any impreſſion of

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this kind. The ſmall-pox, meaſles, cynanche parotidæa, the ~~pian~~ yaws in the human ſubject, and the ſtrangles in horſes, and diſtempers in dogs, are ſufficiently illuſtrative of this fact.

Certain diſeaſes never return if their firſt attack be violent, or if they do, are of a much milder character. This is particularly obſervable of the plague. Thucydides ſpeaking of that which reigned in his time, ſays, "verum illi præcipue morientium, laborantium que miſerabantur qui ipſi evaſerant, quippe id jam antea experti, ac de ſe ſecuri: neque enim bis eundem morbus corripiebat, ut extingueret." (Lib. 2.)

If ever they experience any ſmptom of it, it is in the enlargement of ſome lymphatic gland, which, notwithſtanding, is not productive of any ill conſequence, and they may live without danger or apprehenſion in a country where the plague commits the moſt dreadful depredations.

If miaſmata exerciſed their deleterious power upon the blood, as was in former times generally believed, and ſtill is by a few, nothing would be more common than relapſes.

The blood is only a conductor of the miaſmata, and theſe only act when they find a favourable diſpoſition in the individual; and as in every individual the diſpoſition is not the ſame, ſo all perſons are not equally ſubject to feel its influence. Joab, a Jew, who practiſes as a phyſician at Smyrna, has never been attacked with the plague, although for many years he has treated people in that diſorder.

It is for the ſame reaſon that ſome epidemics only attack perſons of certain temperaments, or certain ſpecies of animals.

Hippocrates mentions a diſeaſe which attacked only the bond-men and maid-ſervants, and ſpared entirely the nobles

and

and free-men. The peſtilential fever deſcribed by John Morelli, exerted its violence againſt the nobles and upper claſſes of citizens, whilſt the peaſants and lower order of thoſe who lived in towns, eſcaped.

The miliary fever, which reigned in Italy in the year 1528, only ſeized very few old people, but a very conſiderable number of children and young people of robuſt temperament, fell victims to its power. (Fracaſtor.)

In the Roman Hiſtory by Dionyſius Halicarnaſſus (lib. 4.), we find a deſcription of a plague which confined itſelf to unmarried women. In the year 1690, an epidemic tertian fever attacked the inhabitants of the country round Modena, in 1691 invaded thoſe of the town, and in a third conſtitution, it was entirely confined to the citizens, and not one of the Jews in the city experienced any attack of it. (Ramazzini, Conſt. Epid. Mut.)

Doctor Kern obſerved an epidemic ſcorbutic affection, which ſhewed itſelf only among the women. (A. N. C. Cent. p. et 11 Obſ. c. 159.)

Cardinal Baronius makes mention in his Annals of an epizootic diſeaſe which reigned in Europe in the year 376, and which deſtroyed almoſt all the herds of horned cattle. Fracaſtorius likewiſe ſpeaks of another in the year 1514, which only attacked horned cattle. From the hiſtories of every age, it appears that horned cattle are more ſubject to epidemics than any other kind. Horſes have alſo their epidemics. Gregory, of Tours, ſpeaks of one which committed great depredations in the Bourdelois in 581. (De Mirac. S. Mart, lib. iii.)

Another ravaged the horſes in the army of Arnoul in Lorraine. (Ann. Fuld.)

Lanciſi recounts the hiſtory of one which deſtroyed the major part of the horſes in

Italy

Italy, 1712. (V. Recherches, par M. Paulet, which contain an account of the different epizootics which have made their appearance in Europe at different periods.)

Stegmann ſpeaks of an epidemic angina among dogs, and of another conſtitution which proved fatal to turkies, hens, pigeons, and geeſe. (Obſ. 169, et 170.) An epizootic itch was obſerved in Weſtphalia in the year 1672, which only affected cats. (Recherches Hiſt. et Phyſ. ſur les Maladies Epizootiques, par M. Paulet, t. i. p. 101.)

M. Adam has remarked, that the fiſhes in the river Dive, in Normandy, have experienced three ſeveral attacks of an epidemic ſince the year 1760. (V. Inſtruct. & Obſerv. ſur les Maladies des An. Domeſtiques, par M. M. Chabert, Flandrin, & Huzard.) It would be an endleſs taſk to recite all the hiſtories of this kind with which medical writers have furniſhed us.

I have here given only a very imperfect account of diseases, but the observations and facts contained in it are more than sufficient to demonstrate that miasmata only act upon the nerves, and that the seat of all acute diseases is in this system.

Of the Secretions.

The power, by means of which the different organs of the animal body separate different substances from the mass of fliuds, is a mystery in physiology.

This power, whatever it may be, is regulated and governed by the nerves. Every nerve possessing a specific sensibility of its own, is only excited by the contact of substances which are in relation to it.

At this excitement they impel into action the machine upon which they are expended, and which they govern.

Is a portion of the nervous electricity at this moment poured into the secretory vessels? Can

Can this matter be the cauſe of the attraction and repulſion which muſt take place in theſe operations?

Do the ſmells and flavours of the ſeparated fluids, or animal productions, derive from the ſame principle?

Some obſervations made by Abbé Nollet and Abbé Vaſſalli upon the electricity of animals, and the ideas of Profeſſor Sauſſure on the nature of this univerſal agent, will aſſiſt us in advancing a few ſteps in theſe enquiries; but as they require much time, and new experiments, I ſhall delay examining them till another time. At preſent I ſhall only relate a few facts reſpecting the influence of the nerves in the mechaniſm of the ſecretions, as it is by this that we are to become acquainted with the qualities of the fluid, by means of which the nerves maintain their empire.

An organ of ſecretion becomes more active when it is affected by acrid and ſtimulating ſubſtances, or when it experiences the nervous impreſſion in any other way. Saline matters diſſolved and applied to the nervous papillæ of the tongue increaſe the ſecretion of the ſaliva. Smoke and certain vapours provoke a copious flow of tears; a greater afflux of fluids into the inteſtinal canal is determined by cathartics; ſquills ſolicit and augment the action of the kidnies. The mouth of an epicure waters at the ſight of a delicious morſel. Fear frequently occaſions a diarrhœa. In a fit of melancholy, there is a great ſecretion of urine.

It frequently happens, that on one ſecretion being increaſed, another or ſeveral at once ſuffer a diminution.

In a colliquative flux, the tongue is parched, the perſpiration and urine ſcanty.

Again, ſometimes one ſecretion or ſeveral are ſuppreſſed or ſuſpended without an increaſe of any other. Opium, under cer-

tain

tain circumſtances and certain affections of the mind, produce this effect.

Again, the ſecretions are often entirely changed.

Irritation in the ſtomach induces exanthematous affections of the ſkin.

In an epileptic fit, the ſweat became very fœtid. (De Haen.) Pechlin ſays, that he had a maid-ſervant, who, during her menſes, infected the whole houſe with a ſmell like garlic. "Et vero ſunt," (ſays he) "quæ cum ita affectæ ſint, peculiarem fundunt et olidam atmoſpheram illo indicio cognoſcendo."

But there is not an inſtance of ſuch an alteration taking place in any of the ſecretions as that which the ſaliva is capable of taking on in an inſtant.

In the Philoſophical Tranſactions there is an account of a man, who, in a ſtate of deſpair from having loſt his money at play, bit himſelf on the wriſt, and died hydrophobic.

 Animals

Animals, which have bitten others whilſt in a fit of anger, have ſometimes communicated madneſs. Hoffman relates ſome obſervations of this nature. This great phyſician was perſuaded, that violent affections of the mind could create true poiſons.

We muſt notwithſtanding acknowledge, that ſometimes a ſimple wound of a nerve has excited terrible ſymptoms, which have been ſucceeded by death. Daily practice furniſhes us with inſtances of this, particularly in the tropical climates, where contuſions, punctured and lacerated wounds produce locked jaw and death.

Hipprocates mentions a caſe of a Captain of a veſſel, who being wounded by an anchor, was ſeized with convulſions and opiſthotonos, which terminated in death on the third day. But I am not acquainted with any caſe, in which hydrophobia has been in conſequence of a wound or other injury inflicted on the nerves.

I have made many animals ſuffer extreme pain, with a view to watch the changes the ſaliva would undergo. By laying ſome of this ſaliva on a wound in another animal, I could never produce the hydrophobia, but the wound has taken on ſuch a character, as to convince me that the ſaliva was of a poiſonous quality.

Fiſhes, by nature harmleſs, when very much irritated, have ſeparated a ſaliva which has proved mortal. (Diarium Eruditorum virorum Francof. An. 1612.)

The power the nerves have over the ſecretions being admitted, we cannot but acknowledge the exiſtence of a fluid in them, which muſt be extremely ſubtle, and extremely rapid in its motion, to put in action machines of ſo fine and delicate a texture, and to be able, *ictu oculi,* to change the ſecretions in ſuch a way as to create a poiſon.

Can we avoid being aware of the qualities of electricity in this fluid?

Plants,

Plants, it may be ſaid, have likewiſe their ſecretions, prepare their different juices, and notwithſtanding are not provided with nerves or nervous fluid. The faculty of ſecreting would then ſeem to be inherent in, and peculiar to their veſſels; and if ſo, why ſhould it not be inherent in the veſſels of animals?

In anſwer to this, I ſhould obſerve, that we have not any right to draw concluſions from things we do not know, and that we can only reaſon from known facts. We are ignorant of the ultimate ſtructure of plants, their ſprings and mechaniſm, and do not know whether they have or have not any nerves; but we know that an animal has nerves, and their influence upon the ſecretory organs is likewiſe demonſtrable.

Some phyſiologiſts, who have been convinced of the reality of this influence, have attributed to the nerves the whole proceſs of ſecretion: this is perhaps carrying it too far. We muſt however allow, that they do much in it.

This

This obſervation is far from being unproductive to a phyſician; on the contrary, it is of the greateſt importance to him. A phyſician, who has not any information upon this head, and who ſhould meet with a patient, whoſe ſweat is extremely offenſive, and ſtools liquid and and putrid, would pronounce, with a confidence which is the natural reſult of ignorance, that the maſs of fluids has a tendency to putridity and diſſolution, or that it is already in that ſtate.

If the ſame phyſician were to viſit a man ill of a fever, whoſe tongue is parched, ſkin dry, urine intenſely high-coloured and ſparing, he would give it as his opinion, that there ~~obtained~~ existed an inflammatory diatheſis of the blood. Theſe falſe principles will regulate and direct his practice, and be the ſource of fatal conſequences.

If he had known how much the nerves are capable of doing with the ſecretions, he would not have ſuffered himſelf to be impoſed upon with theſe ſymptoms, nor would he have committed faults which too prove irreparable.

Let us not however loſe ſight of our phyſician. He happens to reſide in a large town where are epidemic intermittent fevers. As he unfortunately enjoys a conſiderable degree of reputation, he has an opportunity of ſeeing ſeveral patients in the courſe of the day, amongſt whom, ſome are affected with fevers, others have not any fever, but are diſtreſſed with a diarrhœa, and others have an itch or eruption on the ſkin, all equally epidemic.

He begins by vomiting the fever-patient, then purges, and laſtly, though late, adminiſters the Peruvian bark, and he gets well.

A purgative is the firſt medicine employed in the caſes of diarrhœa, and it is repeated according as circumſtances appear to him to indicate it. He afterwards has recourſe to opium. Opium fails of producing the wiſhed-for effect. He then paſſes to the uſe of aſtringents, and here too he is diſappointed. In ſome of his patients, the diarrhœa takes on the character of dyſentery, in others the purging ceaſes, but dropſy ſupervenes, and his patients die.

Our phyſician employs his emetics and purgatives againſt the itch, and to them ſucceed mercurial and ſulphur inunctions. Some of the patients remain in the ſame ſtate, others are cured of the itch, and are attacked with intermittent fevers, and theſe are the moſt fortunate; in a great number of others the viſcera become affected, and many periſh.

The miaſmata, which excited the intermittent fevers, likewiſe gave riſe to the alvine flux and cutaneous eruptions, by acting upon the nerves which govern the

ſecretion

ſecretion of the veſſels, which open in the inteſtines and in the ſkin. A remedy, which would have put the nerves in a ſtate not to be ſenſible of the impreſſions of the miaſmata, would at once have ſtopped both the diarrhœa and the itch. This remedy would have been the quinquina, which put to flight the fevers occaſioned by the ſame cauſe as the other diſeaſes juſt mentioned.

The phyſician we have here brought forward, will be neither more judicious or more fortunate in his treatment of thoſe chronic complaints, which are generally attributed to an acrimony in the maſs of fluids. Being perſuaded that it is the blood, which is the generator and ſource of them, he will only attempt to correct the fancied depravation of it.

The ſolidiſts have declaimed much againſt this doctrine and practice, but even amongſt thoſe who have moſt exerted themſelves in attempting to reform the ſcience of medicine, we find ſeveral who have retained ſome of their ancient prejudices, and talked of acrimony in the blood, in the ſame way as it was ſpoken of in the old ſchool. I cannot avoid comparing theſe phyſicians to apoſtates in religion, who always retain ſome maxim of that in which they were educated.

I do not mean to deny the exiſtence of acrimony altogether. Acrimonies exiſt, but they are always in their origin the reſult of a vitiated ſecretion—Analogy, a great number of facts and obſervations ſerve as a baſis for this opinion. My Eſſay on Chronic Diſeaſes (Saggio ſopra diverſe Malattie croniche—Edizione di Pavia, An. 1792) has this principle for its foundation. Thus I am of opinion, that in phthiſis pulmonalis, the lungs themſelves form an acrimony of a particular kind; that in the rickets, the blood-veſſels, which are intended to nouriſh the bones, ſecrete a menſtruum or ſolvent for that portion which conſtitutes their baſis; that the ſcrophulous acrimony is likewiſe prepared by the lymphatic glands (Dr. Cullen is likewiſe of this opinion, although at firſt he ſuſpected a peculiar acrimony in the blood); that all the ſcrophulous affections, ſuch as the tænia capitis, eruptions in the ſkin, indolent ſwellings, &c. are the effects of a morbific humour, originally elaborated in the lymphatic ſyſtem, provided theſe affections are not hereditary or acquired by way of contagion; that other acrimonies which affect the ſkin, have likewiſe their origin in the lymphatic ſyſtem; that the ſpleen, under certain circumſtances, ſecretes a fluid which attacks the gums, or produces ulcers on the ſhins.

Hippocrates has aſſerted, that thoſe who have a large ſpleen, are ſubject to theſe ulcers if they had not any bleeding or fœtor of the breath.——

"Lienes tumidi si illis non veniant sanguinis eruptiones et oris graveolentia desinunt in ulcera prava tibiarum et turpes cicatrices."

This same observation has been made afterwards by many others, but no physician till now imagined, that a bad secretion of the spleen was the cause of that distemper.

This secretion is not made except where this viscus is enlarged, and, as Hippocrates properly observes, swollen.

If the spleen is enlarged from obstructions, schirrosities, tubercles, or abscesses, then there is not any secretion, and we find, under these circumstances, the patient has not the symptoms which we have attributed to it.

These symptoms in general disappear when an evacuation happens, by which also the spleen regains its former bulk. They do not likewise make their appearance if the evacuation is continual.

It is conſtantly obſerved, that in proportion as the tumour of the ſpleen diminiſhes by the application of moxa to its region, the ulcers on the legs aſſume a more healthy aſpect, take on a diſpoſition to ſkin, and cloſe entirely almoſt at the moment when the ſpleen reſumes its natural ſtate.

What I have now taken off in a very curſory manner on this ſubject, is demonſtrated in my Treatiſe before mentioned, by appearances on diſſection, by medical obſervations, and by experience.

After the theory we have given reſpecting the origin of acrimony, we muſt be convinced, that in order to cure the diſeaſes which ariſe from them, we muſt change the diſpoſition of the ſecretory organs whoſe functions are deranged. This is the method adopted by the ſolidiſts, pointed out by reaſoning, and confirmed by experience.

Of Nutrition.

The life of an animal depends upon the exercise of different functions, which physiologists have agreed to call *vital, natural*, and *animal*. Whilst these various processes are going forwards, the body experiences continual losses, which would soon put an end to it were they not quickly repaired. From hence arises a necessity for food, which the animal seeks with eagerness, being impelled to it at first by instinct, and afterwards pressed by instinct and a sensation of pleasure.

The substances, which are taken in as food, undergo two great processes, which are those of chylification and sanguification. When they are by these means once animalized, they become proper for nutrition.

Nutrition is the work of blood-vessels. Their number, as well as their minuteness, is extreme, and the power by which they ſelect the nutritive matter proper for every diſtinct part of the body is entirely unknown.

This power is certainly the ſame as that which produces the ſecretions, and like this too is equally under the dominion of the nerves.

Although this dominion is not denied, yet it will not be ſuperfluous to adduce ſome obſervations on this purpoſe.

In old age, when the nervous energy begins to grow languid, nutrition is ſparing, the muſcles become lax, and even the bones themſelves diminiſh in thickneſs and length.

A perſon, whoſe nervous ſyſtem is affected by ſtrong paſſions, has not that degree of embonpoint, which is the ordinary mark of tranquillity of the mind. To be convinced of this, we have only to caſt our eyes upon lovers, miſers, tyrants.

Emaciation ſometimes comes on in a very

ſhort time in nervous diſeaſes.—But perhaps in this caſe it rather ariſes from an increaſed action of the lymphatics; and this appears to be ſomewhat confirmed by the obſervation of Morgagni, who has ſometimes ſeen in the hydrophobia part of the body of the patient in a ſtate of emaciation, whilſt the reſt retained its natural plumpneſs.

A young man of my acquaintance, in whoſe family the phthiſis was hereditary, in conſequence of a fall became ill of this complaint. The waſting of his body was ſo rapid, that in the firſt days of the complaint he loſt his hair, which muſt be conſidered as very extraordinary at ſo early a period.

I am convinced that in the true phthiſis the nerves have not their ordinary influence in the proceſs of nutrition, and that from hence the conſumption principally ariſes. In confirmation of this, let us recollect that there are ſome ſpecies of phthiſis, in which the lungs or other viſcera are not ulcerated or otherwiſe diſeaſed, and that in other caſes, although the lungs are very evidently attacked, and hectic fever prevails, along with a copious ſpitting of purulent matter, yet the patients preſerve their corpulence to the laſt. We may likewiſe farther obſerve, that the paſſions of the mind develope this diſeaſe in ſubjects prediſpoſed to it, and that they exaſperate the complaint very conſiderably when it has once made its appearance: that the ſtimuli which call forth the nervous power, or which change the exiſting diſpoſition of this ſyſtem, become proper means for ſuſpending the progreſs of phthiſis, and ſometimes for curing it entirely: Madneſs, pregnancy, wounds, and eruptions have frequently produced thoſe ſingular effects. Laſtly, it has been proved, that riding on horſeback, an active life, good food, and tonic medicines are the means beſt adapted to oppoſe the attacks of this terrible diſeaſe.

We have daily examples of partial conſumptions, where there is not any doubt of the ſtate of the nerves being the cauſe. Thus it is very ſeldom, that a paralytic limb preſerves for any long time the ſame degree of plumpneſs with the correſponding healthy limb. In the ſciatica, the diſeaſed extremity waſtes very much; but no ſooner does nature or art change the morbid condition of the ſciatic nerve, but the limb regains its natural muſcularity.

Can the nerves increaſe the nutrition of parts under certain circumſtances, as well as contribute to diminiſh it? This is credible, as we obſerve, that in long continued intermittents, when the action of the nerves is determined to the ſpleen, this viſcus becomes enlarged. This enlargement of the ſpleen has been commonly believed to be in conſequence of obſtructions, but Doctor Rezia, Profeſſor in the Univerſity of Pavia, a great anatomiſt and phyſician, has combated this erroneous opinion, in a diſſertation publiſhed ſome years ago.

Nutrition may likewiſe undergo changes with regard to the quality and nature of the matter ſeparated; as for inſtance, we frequently find in different parts of the body bony productions, ſecreted and depoſited by the blood-veſſels. (Vide Haller, Opuſcul. Pathol. Obſ. xlvii.) In the hiſtory of Medical Obſervations, we meet with ſeveral inſtances of the hair of the head becoming grey in an inſtant, on hearing very diſtreſſing news.

After exruciating pain, the bones have been known to become ſoft and flexible. (Gagliardi, Morgagni.) The enlargement and caries of the teeth in the ſcurvy, are

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the effect of a depravated nutrition. After the work of Doctor Milman, no one will have the hardineſs to aſſert, that this accident, as well as all the others which accompany the ſcurvy, are owing to an acrimony in the blood. It has been demonſtrated by ingenious phyſicians, that in this affection there exiſts an atony in all the ſyſtem, and that this is the cauſe of the great derangement in the animal economy obſervable under theſe circumſtances. It is only by this atony or defect of nervous power, that the veſſels of the teeth prepare a fluid which has the property of deſtroying their organization.

It would be ſuperfluous to employ more time on the influence of the nerves in nutrition, as it appears ſo very obvious and plain.

If it were now to be aſked by what means the nerves act, we could only repeat

peat what has been ſaid in the article of ſecretion, viz.—That they act by means of a matter, the ſubtilty and velocity of which are exceeding great, and that this matter can only be electricity. If, inſtead of electricity, we ſhould ſubſtitute any other known agent, we ſhould have to encounter inſurmountable difficulties in the explanation of the ſeveral phenomena. If any one ſhould ſuppoſe a particular fluid, we ſhould aſk him to demonſtrate it. It is demonſtrated, he might anſwer, by its effects. Let him then analyſe theſe effects, and he will diſcover, in ſpite of every prejudice, the exiſtence of a matter identical with electricity.

In the foregoing obſervations I have endeavoured to determine both the quality of the nervous fluid, and its influence in the government of the animal economy; theſe two objects appearing to me as the baſis of physiology, pathology, and the practice of medicine. If I have not been so happy as to have succeeded in my design, I yet flatter myself this Essay will be found to contain some matters of use.

APPENDIX.

APPENDIX.

BOTH on the Continent and in London, the experiment of M. Galvani upon amputated limbs has been made with ſucceſs. I repeated it at the London Hoſpital along with M. Blizard, F. R. S. and the following is briefly what we obſerved. Having coated the nerve of an amputated leg with tin-foil, and eſtabliſhed a communication between the coating and the muſcles by a ſilver conductor, the limb did not move. The contractions of the muſcles were very ſtrong when the conductor touched the coating and the naked nerve.

On ſubſtituting gold or ſilver for the tin-foil, and making uſe of the ſame conductor, the electricity was not apparent. After having excited a glaſs cylinder, we brought it near the nerve, but it did not produce any change. The ſame happened with a ſtick of ſealing-wax. When half an hour had elapſed, the limb loſt its vitality, which could not be recalled either by immerſion in hot or cold water, nor was even a torrent of electricity directed againſt the nerves and muſcles themſelves capable of awakening the ſmalleſt motion.

The life of the nerve in the part which is coated is preſerved much longer in animals with warm, than in thoſe of cold blood, in proportion to the reſpective duration of the life of each claſs. Again, the life of the nerves in the former does not periſh by ſuch ſlow degrees as we have remarked in the latter, as in frogs.

If

If we take two prepared frogs, one coated, the other ſerving as chain, and eſtabliſh a communication between them by means of a metallic conductor, the former (I have ſaid in the courſe of my work) falls into convulſions, whilſt the other remains motionleſs. On making new trials, I find this is not the caſe. The frog which is not coated frequently gives ſhocks, and what appears more ſingular, ſometimes the other remains without motion. Several other curious accidents I have met with, which I ſhall communicate as ſoon as I ſhall have collected a ſufficient number.

In a letter I wrote to Dr. Simmons, I ſaid that electricity has probably a great ſhare in the production of animal heat. The following reaſons induced me to take up this idea.

The degrees of heat do not always correſpond with the velocity of the circulation.

Animals

Animals which have been kept a long time without food, retain their natural temperature, as has appeared to me in many experiments. Their pulſe was very ſlow.

Hyſterical affections are preceded by a chillineſs or cold; nor does the heat return in general until the fit goes off. The pulſe in the mean time is in its natural ſtate. (Sydenham, Diſſert. Epiſt.)

In a fainting fit, the heat diſappears all at once, and returns almoſt inſtantaneouſly when the patient recovers.

Paralytic limbs frequently loſe their natural heat, although the circulation is as well carried on in them, as in the reſt of the body. (De Haen.)

Mr. Hunter relates the hiſtory of a gentleman, who was taken with an apoplexy, in whom the whole body would in an inſtant become extremely cold in every part, continue ſo for ſome time, and in a ſhort

time

time become extremely hot. Theſe changes continued for ſeveral hours without any marked alteration in the pulſe. (Phil. Tranſ. A. 1775.)

I have found (ſays Doctor Currie) in certain diſeaſes greater and ſuddener variations than any mentioned, from the application of cold, very gentle in degree, and momentary in duration. (Account of the remarkable effects of a ſhipwreck—Phil. Tranſ. 1792.)

A man, whoſe body is in evaporation, reſiſts cold more powerfully when a bladder of warm water is applied to his ſtomach. For this beautiful and intereſting obſervation, we are alſo indebted to Doctor Currie.

The power of reſiſting cold, or of generating heat, increaſes conſiderably when the nervous ſyſtem is excited, and in orgaſm.

As in almoſt all the phenomena which

have

have been mentioned respecting animl heat, the influence of the nerves is very evident; I attribute them to this influence for want of any other more satisfactory explanation. But, how do the nerves exercise this influence, if it is not by means of the electricity they contain?

Does the electricity of the nerves contribute to the decomposition of the air in the lungs?

Is this electricity itself decomposed?

Is it by this decomposition that the blood is supplied by the inflammable principle and a portion of fire? To experimental physicians I submit the solution of these questions.

For my own part, I shall undertake a work which is connected with these queries, the execution of which requires neither genius nor great expence. The following is the outline of what I propose.

I ſhall make an aſſemblage of the moſt remarkable facts and obſervations with regard to the alterations of animal heat, which appear to militate agaiuſt the theories of Doctors Crawford and Vaccà Berlinghieri.

I ſhall endeavour to calculate at different periods the heat of animals deprived of all ſuſtenance; and at the ſame time to procure an exact account of the weight of their bodies.

I ſhall make ligatures upon, or divide the nerves of ſome limb in animals with warm blood, in order to aſcertain whether the temperature will remain the ſame as in the reſt of the body.

I mean afterwards to examine what takes place in poiſoned animals at the beginning, midſt and cloſe of their ſufferings.

Laſtly, as my own nervous ſyſtem is proper for affording ſuch circumſtances as I am in ſearch of, I ſhall make myſelf the ſubject of many experiments.

If there is a means of advancing in animal phyſics, it is certainly by that of experiment; but we muſt acknowledge that this field has, hitherto, been little cultivated by thoſe who have devoted themſelves to the ſtudy of the human body and of medicine.

THE END.

ERRATA.

Page Line

135 9 *for* Brouſſouet *read* Brouſſonet.

146 4 and 5 *for* common ſenſory, *read* nerves and brain.

— 5 *for* affected, *read* effected.

173 19 *after the word* here, *proceed to read,* the objection, &c. *page* 174, *line* 2.

175 5 *for* the, *read* their.

— 7 *After* affected, *add,* Now we conſider the fibres as electrical organs inſulated by the nerves.

185 8 *for* dedicated, *read* devoted himſelf.

187 15 *for* Boeraahve Praeect. *read* Boerhaave Prælect.

188 18 *after* Newton, *add,* and we alſo agree to it.

194 14 *for* lacrem, *read* lævem.

195 17 *for* acuter, *read* neuter.

200 10 *for* Languiſh, *read* Langriſh.

207 *laſt line, dele the words* "in oppoſition to its efforts to reſtrain them."

241 5 *for* reſerve, *read* reſume.

244 4 *for* ſenſorium, *read* brain.

253 17 *for* curement, *read* recovery.

— 22 *for* diſtinguiſh, *read* diminiſh.

254 1 *for* charge, *read* change.

256 11 *for* feel, *read* ſenſation.

269 6 *for* Nimega, *read* Nimeguen.

279 14 *for* miaſmata, *read* miaſma.

287 2 *for* pian, *read* yaws.

299 7 *dele* and.

ib. 17 *for* obtained, *read* exiſted.

315 3 *for* ſucceed, *read* ſucceeded.